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Review of the Comparative Survival Study (CSS) Draft 2018 Annual Report

Also see February 6, 2019 addendum re: CSS Chapter 8

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ISAB Review of the Comparative Survival Study (CSS) Draft 2018 Annual Report

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ISAB Review of the Comparative Survival Study (CSS) Draft 2018 Annual Report

I. Background

The Columbia River Basin Fish and Wildlife Program calls 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 draft annual reports. The ISAB has reviewed these reports annually beginning eight years ago with the evaluation of the CSS's draft 2010 Annual Report and most recently the draft 2017 Annual Report (ISAB 2010-5, ISAB 2011-5, ISAB 2012-7, ISAB 2013-4, ISAB 2014-5, ISAB 2015-2, ISAB 2016-2, ISAB 2017-2). This ISAB review of the draft 2018 CSS Annual Report is the ISAB's ninth review of CSS annual reports.

II. Summary

This ISAB review begins with an overview of the latest report's findings (this section). It moves on to suggested topics for further CSS review (Section III) and then general comments and specific editorial comments on each chapter of the draft 2018 CSS Annual Report (Section IV).

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 CSS's detailed responses to suggestions provided in previous reviews, and we do not expect the CSS to necessarily respond immediately to new requests for further analyses—see the CSS response (Appendix J) to the ISAB's 2017 review.

Chapter 2 (Life-cycle modeling) uses life-cycle modeling for a new system—Upper Columbia spring Chinook populations from the Wenatchee and Methow populations, which migrate above McNary Dam past an additional three and five public utility district (PUD) projects respectively. A state-space formulation is now used (rather than the likelihood framework of previous models) to account for and separate process variation (time-specific effects) and measurement error (e.g., from sampling). Few of the dams upstream of McNary have PIT-tag detection arrays installed, and so there is little direct information on dam-reservoir survival for the dams upstream of McNary. Two power-house contact indices (upper and lower river) are used to measure the exposure of the juveniles to the power houses. No evidence was found for an effect of the power house contact index upstream of McNary because of low contrast in spill upstream of this dam. Because this is the first year of development for this model, further extensions and estimates of impacts of hydrosystem modifications are proposed but not yet implemented. The ISAB looks forward to seeing more work on this model in future years.

Chapters 3 (Effect of the in-river environment on juvenile travel time and survival), 4 (Patterns in SARs), 5 (SARs and productivity), and 6 (SARs for Snake River subyearling Chinook) are updates from previous years. The ISAB is concerned that the overall pattern of low SARs of Upper Columbia and Snake River spring/summer Chinook and steelhead in 2015-2016, as shown In Chapter 4, is likely to continue, particularly in light of the apparently poor early ocean survival of juvenile salmon in 2017 and unprecedented ocean conditions in 2018 in the Northern California Current and Gulf of Alaska. The ISAB appreciates that the CSS's continuing analyses have responded to past annual ISAB reviews, and we agree with CSS that these analyses provide a sound basis for developing quantitative planning objectives.

Chapter 7 (CSS adult success) is a revision of the corresponding chapter from last year that looks at the relationship between survival of adults upstream of Bonneville and travel time, temperature, and arrival date. A Bayesian imputation method is used to account for the fact that travel time is, by definition, not available for fish that are not detected at the last upstream dam. The analysis seems to be well formulated and executed. However, information about the distribution of the latent travel times is very indirect, and so the primary concern that the ISAB has is with the sensitivity of the results to a different choice of latent distributions for travel times.

Chapter 8 (PIT-tag and coded-wire-tag effects on estimates of survival and SARs) reports on an experiment to investigate effects of different types of tags on various estimates of the population processes. Several different Cormack-Jolly-Seber models are used to estimate the various parameters. Unfortunately, lower than expected returns implies that the power to detect effects has been reduced compared to original plans. Given the reduced sample sizes, it was not surprising there was no evidence of an effect of tag-type on apparent survival or SARs. There was evidence for a decrease in PIT-tag retention over time once the adult fish entered the holding tanks. The ISAB is concerned about the effects of heterogeneity in overall survival when age 3, 4, and 5-year-old adult fish are pooled in the analysis, and some potential impacts of over-dispersion on the results.

Chapter 9 reports on development of methodology to estimate the detection probability (and abundance) of smolts passing Bonneville Dam. A variant of the Shafer (1951) stratified-Petersen model is used. This work is preliminary, and the chapter could be improved by making it more self-contained with definition of parameters and statistics, a more formal development of the statistical model, and incorporation of more information from smolts released/detected upstream of Bonneville Dam.

III. Suggested Topics for Further Review

The latest CSS report incorporates many of our past suggestions. For example, the life-cycle models have been extended to more populations and the effect of tag-type (i.e., PIT vs CWT) on SARs and estimates of survival are now being investigated (2016 #4; see Section V below). As noted above, the ISAB appreciates the CSS's effort to respond to our past queries.

Some of the past recommendations from the ISAB appear to be beyond the current scope of the CSS (see several from 2017) but will become increasingly important in the future. Some of our earlier and current recommendations may seem repetitive and unachievable within a year to inform the next report, but they deserve some advance planning as these issues will become much more pressing in the future. In particular, if there are data gaps, these gaps should be identified for potential new data collection procedures. When life-cycle models are modified, the modification should be flexible enough to incorporate these issues. This is reflected in our recommendations for future work below.

In 2018, we recommend the following topics for future reports:

1. The ISAB previously recommended that combined and interactive effects of populations be studied (2016 #3; 2017 #2). This recommendation is still applicable to the life-cycle models for the Wenatchee and Entiat populations. Currently, the populations from the two rivers are analyzed separately. The CSS indicates in Chapter 2 that they are considering a single model for both populations. This would allow sharing of information between the two populations (e.g., on detection probabilities) but also then allows modelling interactions. Given that there are only two populations modelled, this seems to be a logical place to start with a combined population model.

2. Chapter 2 should be extended to investigate potential benefits on survival of management actions on the hydrosystem, such as spill modifications, as has been done in previous CSS reports. The CSS indicates in their report that it is under active investigation. We look forward to the results.

3. The ISAB recommends expansion of ocean survival estimates to additional salmon and steelhead populations with sufficient data, and collaboration between CSS and NOAA

investigators (Project 199801400, Ocean Survival of Salmonids) to address relevant questions about salmon ocean survival in an adaptive management framework.

4. Add information on mini-jacking. The ISAB/ISRP reviewed several studies that showed an increase in mini-jacking over time. Is the current CSS system able to track this? This shift in the age-distribution of returning fish has serious implications on estimates of SARs. Has this been incorporated in the current analyses?

5. Chapter 8 is an important chapter because PIT-tagging is used throughout the Basin. A more in-depth treatment is warranted for Chapter 8 including a discussion of how the results compare to those of Knudsen et al. (2009). Also, Knudsen et al. (2009) report reduced growth in adults that were PIT-tagged as juveniles. Why is such a comparison not included in this study? We assume that the hatchery has data that allow for a similar comparison. The major problem with these studies is that very large sample sizes are required and, consequently, no one study is likely to give definitive results. It would be an opportune time to do a meta-analysis of the many different studies that have looked at this issue to help guide managers on this important topic.

6. Chapter 9 is important because estimates of abundance are needed when investigating compensatory and interactive effects among stocks. The authors estimated the probability of detection at Bonneville Dam which forms the basis of the estimator for abundance. A similar approach may be applicable for each dam in the hydrosystem. The feasibility of extending the analysis to the other dams should be explored.

In 2017, we recommended the following topics (italicized) for future reports (ISAB 2017-2, pages 2-5). After each recommendation we summarize the current status of the work to address them:

1. *Modeling flow, spill, and dam breach scenarios is very useful for policy makers. Consequently, it is important that all assumptions be clearly stated and that the results are robust to these assumptions. For example, rather than using a single year to represent future flow conditions, variable flow conditions should be used to study the impact of flow/spill modifications under future climate change and examine correlations between Pacific Decadal Oscillations (PDOs) and flows. What assumptions are being made about in-river predation under dam breach scenarios? What assumptions are being made about harvest under dam breach scenarios? Why are there discrepancies between the results of the Life-Cycle Model (Chapter 2) and the COMPASS model (refer to #4 in 2016)? The same scenarios should be run through both models and discrepancies resolved.*

This analysis is pending for the new life-cycle model in the new Chapter 2.

2. *Include other important processes in the life-cycle models. In the current CSS analyses, each modeled population does not interact with other modeled populations as they migrate through the hydrosystem. Interactions among the various populations, including compensatory responses, are important and whenever possible should be folded into future modeling efforts, particularly if restoration actions increase the abundance of out-migrants.*

   This is still applicable to the life-cycle models for the Wenatchee and Entiat populations. Currently, the two populations are analyzed separately. The CSS indicates that they are considering a single model for both populations. This would allow sharing of information between the two populations (e.g. on detection probabilities) and also allows modelling interactions. Given that there are only two populations, it seems logical to develop a combined population model.

   Similarly, there has been a great deal of interest in the impact of predator control programs on salmon returns, especially northern pikeminnow, birds, and pinnipeds. Are these programs effective? Are there compensatory responses?

   Is there evidence in the existing data about either issue? What type of data would be needed to address these issues and include them in the life-cycle models? This recommendation builds upon our previous recommendations (#1 in 2015; #3 in 2016).

   The CSS responded that data availability is a key issue in attacking this problem. They will continue to monitor for the availability of suitable data.

3. *There appear to be sufficient data to try to elucidate reasons for shifts in the age distribution of returning spring/summer Chinook (# 5 in 2016). We suggest doing so.*

   This is still pending.

4. *The graphical analysis of the impact of TDG could be improved using direct modeling to deal with potential confounding effects of spill, flow, TDG, and temperature.*

   The CSS added a more formal model of these effects in their revision to the 2017 CSS report in response to our comments.

5. *The (new) modeling of adult survival upstream of Bonneville Dam should be continued and improved to identify the limiting factors to adult returns. Once these factors are identified, are there modifications to the hydrosystem operations that could mitigate some of the factors?*

   The current CSS continues development of this model.

6. *The CSS report is a mature product and the authors are very familiar with the key assumptions made and the impact of violating the assumptions. These should be collected together in a table for each chapter to make it clearer to the readers of the report.*

   This has been partially addressed but more work is needed to make it clear to readers the critical assumptions what impacts those assumptions could have on the results.

In Section V below are the lists of topics we recommended in our reviews from 2013-2016. As noted above, many of our recommendations were incorporated in subsequent CSS analyses and reports, and some recommendations require advance planning and coordination with other entities.

IV. Comments on New or Updated Analyses in the draft CSS 2018 Annual Report by Chapter

IV.1. Chapter 1. Introduction

Chapter 1 is well written overall and is basically identical to previous years. This chapter forms the basis for the entire report, so careful definitions of tagged and release groups are critical so that readers new to the work have a clear understanding of the analyses and results that follow.

Editorial comments

p. 1. Last year's report included data for SARs of Snake River wild fall Chinook for 2006-2011, but these were not included in this year's report. Why were they removed?

p. 1 and 15, and perhaps elsewhere. The terms 2-salt, 3-salt returns, and the like may be confusing. Will all readers understand the meaning of these terms?

p. 11, first sentence of first full paragraph. The wording is confusing. Perhaps "…from smolts passing downstream of BON to adults returning back to LGR…"

p. 12, top paragraph continuing from the previous page. The description of tagged groups is quite confusing. They cannot be understood sufficiently to read further without reading Appendix A, which is not ideal. A more logical outline and flow might be:

> 1. PIT-tagged smolts are preassigned randomly to be either transported (T) or returned to the river (R) - [Note: we suggest avoiding use of "monitor-mode" unless this is critical because it is not intuitive and neither word relates directly to the study]
>
> 2. Most untagged smolts were transported, so Group T reflects the experience of these fish.
>
> 3. PIT-tagged fish are either bypassed, if transportation is not occurring, or routed according to their pre-assigned grouping (T=transported; R=returned to river) when transportation is occurring.

p. 12, next paragraph (first full paragraph), 4th sentence. This is also unclear. Were smolts not transported when detected in a bypass system because transportation was not occurring at that time?

p. 13, line 8. A minor point is that "tractable" is statistical jargon, and the meaning may be unclear to some. Perhaps "…can be calculated..." would be clearer.

Table 1.3. It would be best to also define RIS for readers who don't know the meaning.

Figure 1.6 caption. It is unclear how the proportion transported was calculated for the years 2007-2016, given that only <2006 and 2017 are addressed.

p.19. Figure 1.6. Bottom panel has "Proportion transported" in title, but the values are "Percent transported." Make this consistent.

p. 20-21. Chinook and River are proper nouns in certain places and need capitalization.

p. 21. Chapter 8 appears to have been omitted in the review of content of the chapters so that Chapter 9 is referred to as Chapter 8.

IV.2. Chapter 2. Life Cycle Evaluation of Upper Columbia Spring Chinook

Chapter 2 is well written, and the methods are clearly presented. The model is similar to life-cycle models in previous reports and the analysis of the Snake River and Grande Ronde basins in the *Interior Columbia Basin Life-Cycle Modeling Report* from NOAA in 2017. It now represents the Wenatchee and Methow populations as a state-space Bayesian model. Productivity and survival through the hydrosystem are represented separately in this model and are then combined for the ocean phase of their life histories. Standard techniques are used (i.e., Markov Chain Monte Carlo [MCMC] simulation and Bayesian approaches). The model development is limited by the quality of the PIT-tag detections and smolt trap data in the upper Columbia River and the paucity of ocean survival data. Overall, this is a solid approach that does a good job with limited data. A comparison of the results from those from the Snake River models would be useful, e.g. compare dam-reservoir survivals in parts of the hydrosystem common to both population models.

Specific comments

Equations [2.1] to [2.3] have a stochastic component that the CSS often calls "observation error," but this terminology is misleading. Equation [2.1] is a prediction for the (latent or hidden) state variable for the actual number of smolts produced. Then, Equation [2.17) relates the observed smolt count to the actual underlying smolts produced with another stochastic term. The latter is more properly an "observation error" while the former is a "process error" representing year-specific (hidden) effects.

Equation [2.2] has a stochastic component which is called "measurement error." But again, this is a process-error (year-specific effect) because no actual measurements of juveniles surviving the trap to McNary are taken. The same is true for Equation [2.3].

Equations [2.20] to [2.22] use a log-normal distribution on the empirical survival probabilities based on the mean = log(observed survival probabilities). This could lead to survival probabilities that are greater than 1. Perhaps a normal distribution on the logit(survival probabilities) would be better?

In Figures 2.3 and 2.4, the models seem to have difficulty predicting age-3 recruits in late 1960s, but not the age-4 and age-5 recruits. Why? This seems odd given that there is a separate $s_{1,t}$ term for each year that should have compensated for year-specific effects in predicting the age-3 recruits?

p. 30. Something appears to be missing in Equation [2.8]. An autocorrelation term usually has this year's stochastic component plus the correlation with the previous stochastic error.

The data for the Wenatchee and Methow populations both fit the Ricker stock-recruitment model poorly. The discussion should describe possible causes for the lack of fit.

Minor comments

p. 24. Provide explanation of the term "gravel-to-gravel."

p. 24. Provide explanation of "the effect of spill to total dissolved gas" for readers not familiar with the relationship.

p. 25. The acronym PITPH would be more intuitive if preceded by "PIT tag powerhouse passage index (PITPH)…"

p. 26. In the second full paragraph, define conversion rate for those who may not know the meaning of this technical jargon.

p. 28. The Methods section is a nice simple description of the basics of MCMC simulation in a Bayesian analysis for many readers not versed in Bayesian statistics.

p. 31. Is there any rationale for selecting the ranges of the coefficients described in the paragraph after Equation 2.22? Are these default ranges?

p. 32. Why not use a uniform (0,1) for the prior for rho directly?

p. 32 and elsewhere. "95% confidence intervals" should be "95% credible intervals."

p. 32. "… sampled the chain 10,000 times to obtain posterior density estimates." Is this thinning?

p. 33. "A positive smolt prediction bias can be seen in the Wenatchee smolt relationship, where a negative prediction bias is seen in the Methow model fit." The positive bias for Wenatchee is evident, but the negative bias for Methow is difficult to discern. Please describe how this can be seen.

p. 34. Why are observed values of SAR not included in the lower right plot?

p. 35. "… show no variation over the time-period" is likely a consequence of the small variation in UPH shown in Figures 2.1 for the upper dams.

p. 33. For the results, it would be useful to at least refer to Figure 2.1 or even move Figure 2.1 adjacent to Figures 2.2 and 2.3 to make some of the results statements easier to follow. The same attribution to appropriate figures and tables would make the Discussion section clearer.

p.43. Explain what is meant by the phrase "… owing to confounding in stage abundance and survival uncertainties."

Editorial comments

The term "rate" commonly is used to express change per a unit time. This is often not the case in the text and using the term "probability" would be better (e.g., adult conversion probability; juvenile hydrosystem survival probability, etc.).

p. 24. Second paragraph. It is unclear if this paragraph was about the Snake River populations. It seemed like it must be because of the last sentence, but it would be helpful to clarify earlier.

p. 24. "Using statistically validate models…" should be "Using statistically validated models…."

p. 24. In the third paragraph, "river" should be capitalized.

p. 24. Insert "the" between "given nature."

p. 25. In the first incomplete paragraph, spell out "upper C" model

p. 25. In the first full paragraph, grammar in the fourth sentence is incorrect.

p. 25. In the last paragraph, perhaps reword as "…by subtracting the sum of all fractions of water spilled at each dam from the total number of dams..."

p. 26. The last sentence in the first partial paragraph is not related to the rest of the paragraph (spill and dam passage).

p. 26. In the last paragraph, hyphenate "four-year old and five-year olds." Also, the term "average conversion rate net of harvest" is unclear.

p. 27. Table 2.1. Should the lower case "b" instead be a capital beta to conform to the Ricker equation?

p. 29. Definition of lambda is not consistent. On page 29, it is defined as the "Adult migration conversion rate"; on page 31 it is defined as the upriver survival rate.

p.29. The term "prediction error" is used for R_t, but on page 30 following equation 2.8, it is called "process error."

p. 30. "fish that return to spawn at five years of age" should be "fish that return to spawn at four years of age."

p. 31. In the paragraph after Equation 2.11, H_t is described as being hatchery fish, but in Table 2.1, H_t is described as harvest. Is H_t a measured parameter?

p. 32. "… the process error term." This appears to be an observation error and not a process error. Change to "observation error" as in the other equations on this page.

p. 32. The damma () prior is not an "uninformative prior" in the technical sense. it would be better to use the term "diffuse prior."

p. 32 "The tau are inversely proportional … statistical relationships … with JAGS." This repeats what is said at the top of the page but changes "statistical relationships" to "express variability in the normal and log-normal distribution in many Bayesian statistical packages."

p. 32 "dnorm (0, 1000)" is likely "dnorm (0, .001)."

p. 33. Use decimal number for the estimates of early ocean survival (i.e., 0.05 to 0.20 rather than 5-20%) to be consistent with the Upper/Lower Columbia hydrosystem survival probabilities immediately above this sentence.

p. 33 "… of 2 year old recruits, but also a slightly negative bias for 1 and 3 year old recruits." This should likely read 4, 3, and 5 year-old recruits respectively.

p. 34. Explain the dots in the upper frame of Figure 2.2. They apparently are observed values.

p. 34. The fourth sentence in the captions for Figures 2.2 and 2.3 is confusing. "Lower right panel shows the median (line) predicted SAR as measured from smolt detection …" The term "measured" is incorrect if it is predicted.

p. 40. Convert to proper decimal notation or proper scientific notation rather than computer output.

p. 41. Provide references to figures and tables to support the points in the Discussion.

p. 41. "the estimated distributions are similar." This is presumably a comparison between the two basins. This comparison is difficult to make because the scales differ in the two plots (see next comment). The distribution of σ_S from that of the other variance components also looks different once the scales are adjusted.

p. 41. Use the same scale for the comparable plots between Wenatchee and Methow posterior plots to make the comparison between the two systems easier.

p. 41. "survival ffis not accurate..." Change to "survival ff is not accurate..."

p. 43. In sixth sentence of the second full paragraph, is the suggestion that later entry "predicts" shorter travel time referring to a model or an observed relationship? Provide references to support this.

p. 43. Four lines from bottom, the term “increase” is missing.

p. 44. The differences between the Wenatchee and Methow migration survivals might be realistic and therefore do not need to be resolved. This should be discussed. (Note typo on “resolved” in this sentence.)

p. 44. In the second sentence of the last paragraph, add “the notion” before “that changes to hydrosystem operations.”

p. 45. The last paragraph discusses findings about the effects of PDO and upwelling on survival, but there is no analysis of this relationship in the chapter. Should this sentence be deleted?

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

Chapter 3 is an annual update of previous versions of this chapter. The chapter is well written, and the analysis is understandable for technical audiences. The addition of a conclusions section was helpful. The chapter is appropriate for a scientific audience, but it is complicated and uses a lot of technical jargon. A simpler, more easily understood version might be useful for decision makers and the public for future reports

Minor comments

p. 60. In the last sentence, an r-squared value of 0.34 does not indicate that a high degree of the variation in mean FTT of RIS+MCN sockeye was captured.

p. 62. In the first sentence of second paragraph, we question that there was generally high accuracy for survival shown in Table 3.4. R-squared values of 0.13 and 0.19 are not that high. Also, r-squared is only one measure of representativeness; it does not capture bias or deviation from the mean.

p. 62. In the last paragraph, the wording about “generating predictions of fish travel time, instantaneous mortality rates, and survival probabilities that would have been expected for sockeye in 2015 through 2017 (Figures 3.2-3.4)” is confusing. Are the open circles in Figures 3.2-3.4 for those years on the sockeye panels the “generated predictions”? If so, use a different symbol or notation so that they don’t look just like the other open circles on those panels. We assume this text is explaining why there is so much difference between the “estimates” and “predicted” sockeye results shown in those panels, but it is difficult to see what is being referred to in Figures 3.2-3.4. If this text is not explaining those differences, the discussion should describe those differences.

p. 65. In the fourth sentence of the third paragraph, indicate whether you mean increased water temperature or decreased water temperature when "water temperature" is mentioned.

p. 67. In the last bullet, the Discussion should provide possible explanations for why the highest instantaneous mortality rates and reduced survival probabilities occurred in 2015-2017 (e.g., extremely high water temperatures in 2015 and its effect on subsequent years).

The 2017 report included an analysis of the effects of total dissolved gases (TDG). The ISAB's review of the 2017 report suggested a different statistical approach for analyzing the data. Nothing about TDG is included in this year's report. Will the effects of TDG be reanalyzed or is this modeling effort to assess TDG being abandoned?

Editorial comments

p. 55. Move Table 3.4 to the beginning of the results section if the results shown in Figures 3.2-3.4 are produced by the models shown in Table 3.4. A statement to that effect could be made at the beginning of the results section.

p. 57. In Figs. 3.2-3.4, the Y-axis label numbers lie on top of the axes.

p. 57. In Figs. 3.2-3.4, the X-axes of the graphs do not extend to the extent of the records being presented. The graphs should be updated to represent the range of time illustrated.

p. 57 – p. 59. For sockeye in Table 3.4, the text indicates that the models were for hatchery and wild sockeye, yet the labels for the sockeye panels in Figures 3.2-3.4 indicate only hatchery sockeye (H SOX).

p. 59. In Figure 3.4, it is difficult to assign which tick mark belongs to which year. Can these figures be improved to make it clearer (e.g., place year at an angle against each tick mark)?

p. 66. In the second full paragraph, will readers understand the meaning of "dissolved gas limit spill operations on a 24-hour basis…"?

IV.4. Chapter 4. Patterns in annual overall SARs

This chapter was updated by the use of newly acquired data, primarily for the 2015 (steelhead) and 2016 (Chinook and sockeye) juvenile migration years, and the inclusion of new population groups, including Kooskia Hatchery (Snake River) spring Chinook (2014-2016, Fig. 4.4, Table B.29), Springfield Hatchery (Snake River sockeye, 2015 and 2016, Fig. 4.9, Table B.50), and Umatilla River (Mid-Columbia) wild steelhead (2011-2015, Fig. 4.13, Table B.80). As in past reports, SAR estimates were compared to the 2%-6% SAR objective from the Northwest Power and Conservation Council's [2014 Columbia River Basin Fish and Wildlife Program](#).

The annual (2011-2015) SARs (JDA to BOA) of Umatilla wild steelhead met the NPCC 4% SAR objective and exceeded the 2% minimum SAR objective in three of five years (2012-2014). However, the ISAB considers the large drop from 4.78% SAR in ocean entry year 2014 to 1.46% worrisome in light of poor and unprecedented ocean conditions during the 2014-2018 (*see* Ocean Ecosystem Indicators of Salmon Marine Survival, www.nwfsc.noaa.gov/research/divisions/fe/estuarine/oeip/index.cfm).The new estimate of overall SARS (LGR to GRA) of Snake River wild steelhead in ocean-entry year 2015 also is concerning because it was the lowest SAR (0.15%) on record (1997-2015; Table B.35).

Estimated 2015 and 2016 SARs for Springfield Hatchery (Snake River) sockeye were 0.00%, Fig 4.9, Table B.50). As reported by CSS, the lack of adult returns from this release group is thought to reflect water quality issues in the hatchery. In response, the CSS reports that two new acclimation strategies used in an adaptive management framework have resulted in good survival to LGR in 2018 (IDFG, unpublished, as cited in the draft CSS 2018). Although results are unpublished, this appears to be a fine example of collaboration between CSS and regional agencies to resolve salmon survival issues.

On p. 100, the bootstrap confidence intervals for Salmon River and Asotin Creek wild steelhead in 2015 have 0 width (*see* Appendix Tables B.39 and B.40). This is an artefact of no returns from the tagged fish. However, the tagged fish are only a sample of all fish from this stock, and so there is still uncertainty in the estimates. In cases with 0 returns from tagged fish (or very low counts in general), the usual way to compute confidence intervals won't work. In these cases, alternate confidence intervals will give an approximate way to show uncertainty. Refer to http://influentialpoints.com/Training/limits-when-p-or-f-are-zero.htm for details.

The use of a bootstrapped confidence interval works well when there is much data, but performs poorer with small sample sizes. Sample sizes are difficult to find for each analysis, but perhaps an indication in the tables (e.g. an asterisk (*)) should be added to alert the reader that sample sizes are small.

Chapter 4 includes two new conclusions:

1. "PIT-tag SARs for Upper Columbia wild spring Chinook and steelhead have fallen short of the NPCC 2%-6% objectives since CSS monitoring began in 2006."

The ISAB considers the overall pattern of low SARs of Upper Columbia and Snake River spring/summer Chinook and steelhead in 2015-2016 to be particularly concerning and likely to continue in light of apparently poor early ocean survival of juvenile salmon in 2017 and unprecedented ocean conditions in 2018 in the Northern California Current and Gulf of Alaska.

2. "These continuing analyses respond to annual ISAB reviews and provide a sound foundation to continue and develop quantitative planning SAR objectives for the next amended NPCC Columbia River Basin Fish and Wildlife Program."

The ISAB appreciates that the CSS's continuing analyses have responded to past annual ISAB reviews of this chapter, and we agree that these analyses provide a sound basis for developing quantitative planning objectives, as in the case of Snake River hatchery sockeye discussed above.

Ocean survival (S.oa and S.o1) has been estimated only for Snake River wild spring/summer Chinook and steelhead, and CSS is exploring estimating S.oa and S.o1 for mid-Columbia and upper Columbia wild spring Chinook and steelhead. The ISAB encourages expansion of ocean survival estimates to these and other salmon and steelhead populations with sufficient data. The ISAB also encourages greater collaboration between CSS and NOAA investigators (Project 199801400, Ocean Survival of Salmonids) to address relevant questions about salmon ocean survival in an adaptive management framework.

Appendix B: Supporting tables for Chapters 4 and 6 – Annual Overall SARs

Appendix B provides 108 tables (107 tables in CSS 2017) with SAR data supporting the results presented graphically in Chapters 4 and 6. The 2018 report updates many tables with 2015 (steelhead) and 2016 (Chinook) SAR estimates and includes a new table with overall SARs for Umatilla River Basin (Middle Columbia) wild steelhead (Table B.80).

The SAR estimates of 0.00 will implies that the geometric mean SARs are not defined (see Tables B.39 and B.40). Some thought needs to be given on how to handle this problem. On some tables, there is a footnote that the sample size for a particular year is too small to be reliable, but the cutoff for an adequate sample size doesn't appear to be given.

Editorial comments

The table headings and footnotes in this appendix should define the acronyms used and reference the figures in Chapters 4 and 6 that correspond to the data presented in Appendix B tables.

IV.5. Chapter 5. SARs and productivity

Chapter 5 updates and extends two previous analyses: (1) a comparison of population replacement rates to brood-year 1998-2010 SARs for the John Day River Major Population Group (MPG) within the Mid-Columbia River spring Chinook ESU, and (2) a comparison of Snake River steelhead brood-year SARs to population productivity for Fish Creek (Clearwater MPG), Rapid River (Salmon River MPG), and the Pahsimeroi River population (Salmon River MPG), including new data for Joseph Creek steelhead (Grande Ronde River MPG). Methods used for deriving Joseph Creek spawner abundance data differed (red counts in 1996-2010 and 2014, mark-recapture in 2011-2013 and 2015-2016). The results generally support, the NPCC (2014) 2%–6% SAR objectives.

Analyses for Snake River spring/summer Chinook were not updated or repeated in this report because run reconstructions are not complete (updated only on a five-year schedule for NOAA ESA status assessments).

The ISAB appreciates that the analyses in this chapter are intended, in part, to address past ISAB review comments, and we encourage continuation and further expansion of analyses to additional populations and MPGs having adequate data time series.

Editorial comments

Figures 5.1 and 5.3. Data points obscure some of the value labels on the horizontal axis. For Fig. 5.1, we suggest using different symbols for each population as in Fig. 5.3.

IV.6. Chapter 6. Estimation of SARS, TIRS and *D* for Snake River Subyearling Fall Chinook

This chapter was updated with 2012-2015 data. Results and conclusions were similar to last year's CSS report. Methods are reported in other chapters. The length of the chapter was shortened by moving data tables to the appendices. The patterns of SARS for recent migration years (2014-2015) for all fall Chinook release groups were low (near or less than 1%), and, as discussed above by ISAB, likely reflect poor ocean conditions during those ocean-entry years.

The introduction to Chapter 6 states that SAR estimates will be developed for other wild and hatchery fall Chinook groups in the Mid-Columbia River as information becomes available. The ISAB appreciates this continuing effort. In addition, the CSS in collaboration with the Nez Perce Tribe has a pilot effort to develop PIT-tag SAR estimates for a few release groups of Snake and Clearwater fall Chinook. Are there any plans or ongoing efforts to develop PIT-tag smolt-to-adult survival information for the lower Columbia River populations?

Specific comments

p. 125. Figure 6.2. If SAR is 0, then the standard method used to estimate confidence intervals will not work properly. See comments in Chapter 4 about this issue.

p. 127 and 128. Figures 6.3 and 6.4. The addition of standard error (SE) bars would make the graphs easier to interpret whether the differences are real or just due to sampling variation. The abbreviations “TX” and “C0” for symbols used in the figures should be defined in the figure captions.

IV.7. Chapter 7. Adult salmon and steelhead upstream migration

In this chapter, a revised model is created to estimate the effect of travel time on fish conversion (survival to reach the upmost dam) using a Bayesian logistic regression model. However, travel time is available only for fish that convert—fish that do not convert have missing travel time information. The Bayesian model imputes a travel time for these fish selected from a mixture distribution. This is a generalization of simple imputation methods where the missing value is replaced by the mean but now correctly incorporates additional uncertainty in the estimates of the parameters from the imputation process.

The authors careful examined the fit of the model as presented in the chapter’s Appendix. This greatly improves “faith” in the results. The only item that should be explored is the impact of a different model for the distribution of travel times because information on the latent distribution is very diffuse. For example, what happens if a single-component distribution is fit or a log-normal distribution is fit (similar to Figure 7.14)? Do the results change very much? Do the goodness-of-fit tests detect a lack of fit? The mixture distribution worked well for spring/summer Chinook but not for the others. While the authors propose to try other distributions, it wasn’t clear what they thought might work other than simply using a uniform distribution. They do propose to model continuous and erratic travel times separately, and the ISAB concurs that is worth doing, especially given the imputation of some travel time. We note that the proportion of imputed travel times was much lower in the species units that worked best under the mixed distribution, but there was no discussion of this.

The chapter may be difficult for many readers of the CSS report to understand. The text appears to be written with an assumption that readers are expert Bayesian modelers and are already familiar with terminology used by the authors. For example, an explanation of “conversion” needs to be made early in the Chapter. It is unclear if the authors are using “conversion” and “conversion rate” interchangeably. In Table 7.2, 7.4, and 7.6, no explanation is given of what “Eff size” and “R hat” are, so the relevance of those numbers will have no meaning to many readers. Better spell-checking and proofreading would reduce typos and missing words, making it easier to read and review the document. It would also be helpful to number equations to make reviewing easier.

Minor comments

p. 131, paragraph 3. “For this reason, inferences regarding travel time from our past analysis were limited to fish with successful upstream passages and likely to misrepresent unsuccessful fish. Recognizing potential bias, we did not attempt to assess survival-travel time relationship, which relied on information for both successful and unsuccessful fish.”

It is clear that “unsuccessful fish” are those not detected at upstream sites. Perhaps “undetected” is a better descriptor. How does “unsuccessful” relate to “We categorized fish in our data into two groups: fish with continuous travel and fish with interrupted, delayed travel” on page 134, 3rd paragraph?

p. 134. More details are needed on the permutation test. For example, did you permute fish only within return years or across all years?

p.134. What criteria did you use for eliminating variables using AIC? For example, eliminating all models more than 2 AIC units from the best fitting model?

p. 135, first sentence. “Out of 56 spring/summer Chinook that were detected outside of their main migration route, 48 (0.6%) were transported as juveniles, and 8 (0.1%) migrated in-river.” The percentages do not match with any comparisons that we can find. As the sentence incorrectly reads, 48 of 56 = 0.6%. There are similar sentences at the beginning of results for each species and similarly the percentages don’t match the numbers presented.

p. 135. How was the estimate for transportation derived? Model terms are on the logit scale, so do they represent log-odds rather than differences in probabilities? It appears that the model was used to make predictions at specific values for the covariates. What were the values used? Some clarification is needed here.

p. 136. What is the Bayesian model? For example, is it on the logit scale? What are the units of $mu_{1,vel}$ and $mu_{2,vel}$? What priors are used for each parameter? How many iterations, burn in, thinning, etc.? The Stan program uses a Hamiltonian system for doing MCMC, so some of these questions may not require a direct answer, but more details are needed. Some of this information is given in Supplemental Material A on page 151 but could be included in the text.

p.136. Where is the estimate of the mixing proportion for the travel time distribution?

p. 149 Figure 7.11 and text in Discussion page 148. Are there any statistics to support the statement that the mixed distribution is more appropriate than the unimodal? For example, what are DIC values? Some caution must be taken when computing the DIC to ensure that it has been computed with the correct focus.

Editorial comments

p. 131. If "conversion rate" Is a synonym for "upstream migration success" used elsewhere in the chapter, please be consistent in usage.

p. 131, last sentence: "And" should be "An"

p. 131, first word: Add "fish" after "undetected"

p. 131, first complete sentence at end of first paragraph: The "conversion rate" rationale is not very clear. Are there some words missing here? Could this be written more clearly?

p. 131, second full sentence: Remove comma after "Although"

p.132. "Iterations of sampling and simulations produced multiple versions of imputed data. At the end, we averaged the multiple sets to get estimates of imputed data and parameters." This sentence appears to be misleading. It is not necessary to create multiple datasets. Just run the Bayesian model for a long period of time and sample the posterior rather than averaging. This sounds like a mixture of classical and Bayesian methods. Based on the rest of the chapter, especially Supplemental Material A, the sentence is misleading and should be deleted.

p.132 "missing travel time of unsuccessful fish." This is a strange concept. Presumably it is a theoretical travel time for an unsuccessful fish had it not been censored by another cause. Average speed may be a better quantity because this is still defined if a fish does not survive.

p. 132 line 6. "fewer" parameters not "less" parameters.

p.133. Figure 7.1. It appears that the Ice Harbor-McNary temperature relation diverges at temps > 20 C. Is this significant to the analyses? What is the odd point in Figure 7.1? It would be helpful to show the correlation coefficient for the comparison in the figure or an r-squared-type value.

p. 133, Snake River Sockeye: Which Chinook are referred to by "Chinook"? The two previous italicized items are referring to spring/summer Chinook and fall Chinook.

p.133. Give the model as an equation rather than text.

p. 134, last sentence before Results. The meaning of this sentence is not clear.

p. 135 (and elsewhere). Fix Table/Figure references, such as the references to Table 2 (which should be Table 7.2). Figure 2 should be labelled as Figure 7.2, etc.

p. 135, Table 7.1: It would be helpful to include the years covered in the dataset. Also, “summarize” should be “summarizing.” This comment applies to Tables 7.3, 7.5, and 7.7 as well.

p. 135, Figure 2: The term “middle 95%” is unclear. Suggest rewording. This comment applies to Figures 7.4, 7.6, and 7.8 as well.

p. 135, Figure 2. What is the grey area? Does this represent all of the lines from the MCMC procedure?

p. 135, Table 7.1 and similar tables. It would be helpful to have totals for columns and rows to match the numbers in the text in the beginning of the section.

p. 136, Table 7.2. It is often helpful to define a statistical equation before the symbols appear in the text, tables, or figures. The model does appear in the appendix, but the reader has to refer to the appendix to understand the meaning of the mean and variance parameters in the last five lines of the table. What is the estimate of the mixing proportion?

p. 136. Table 7.2. Make the decimal places similar everywhere for R-hat. “Eff Size” is “Effective sample size.” This comment applies to other similar tables.

p.136. Table 7.2. It is not clear on what scale the estimates are reported--presumably on the logit-scale for the majority of the terms; days for the means and standard deviations of the travel distributions? Is the transported term coded as 1=transported and 0= not transported? Is the “summer run” term coded as 1=summer run, 0=spring run? The last row in the table has strange text (“lp_____” ?) in first column, Supplemental Material A (pdf page 166) indicates that continuous covariates were standardized, so how do we interpret some of the coefficients?

p. 137. Figure 7.3. Overall this is a nice figure, but the secondary y-axes for the panels is missing. Should that be numbers of fish? This comment also applies to Figures 7.5, 7.7, and 7.9.

p. 145. Discussion items for spring/summer Chinook and fall Chinook are mostly just restating results, so perhaps they can be deleted. More discussion could be made about the implications of these results. The analogous discussion was better for sockeye and steelhead.

p. 148, First sentence of last paragraph. The term “skewed right” should be stated more clearly regarding the variable being described (i.e., “skewed to higher fish travel time” if we are interpreting the text correctly).

p. 149, Figure 7.11: “compare” should be “comparing”

p.149. Provide a Q-Q plot to check distributions. It is too hard to see the improvement when moving to a mixture distribution.

NOTE: The following comments apply to the section on spring/summer Chinook, but most also apply to each of the sections on fall Chinook, sockeye, and steelhead.

p. 151, next to last sentence. Many readers won’t understand the relevance of R-hat being close to one and what an adequate effective sample size is. We suggest having an explanation of these items in the main document, since these values are given in the tables in the main document.

p. 151-154, Figure 7.12. Readers who are not familiar with this modeling approach may find the caption for Figure 7.12 to be very unclear about what the figure was showing.

p. 154. first equation shown on this page: What are $n^{in\text{-}river}$ and n^{trans}? Are these numbers of fish? We had a similar question for the n-values in the next equation.

p. 155. What is the difference between μ_{1vel} and μ_{2vel} (same question regarding sigma)? Are these different years, different cohorts, or different models? This presumably describes the mixture distribution for the travel times, but a reader may not have the technical expertise to know this. Provide some additional details

p. 155, Figure 7.14. What does “Density” mean on the y-axis? This figure seems to be about fish travel time distributions. We also didn’t understand the last sentence of the caption about the plot being one randomly selected outcome. Is this referring to the “predicted” output shown? Again, a reader who is unfamiliar with the output of Bayesian methods may have difficulty interpreting the figure.

p. 1*55*, Figure 7.14. What are the units of the y-axis?

p. 155, last sentence. A reader who is unfamiliar with Bayesian predictive p-values may not understand why this p-value of 0.307 shows a reasonable relationship? Some additional information may be helpful.

IV.8. Chapter 8. PIT tag and coded-wire tag effects on smolt-to-adult rates for Carson National Fish Hatchery spring Chinook salmon

This is a very important study, which appears to be well conceived and executed, but, unfortunately, the reduced returns (compared to projections) reduced the utility of the study.

The original power analysis performed by the authors assumed that SAR values would average 0.37% (0.0037). Instead, for the years they examined, SARs were one-half to one-third of expected (~.0015. or so). This reduces the power to detect effects. While their original power analysis was designed to detect a minimal (relative) difference of 25% in SARs with a power of near 80%, even a 10% effect would be concerning, but an effect of this size will now be very difficult to detect. Indeed, the final estimates of the effect of tag type (Figure 8.9) have very wide confidence bounds on the effect size.

The Discussion needs improvement. The Introduction states: "Knudsen et al. (2009) found that smolt-to-adult recruit survivals (SARs) were on average underestimated by 25% for PIT tagged fish due to tag loss and reduced survival of tagged individuals. Further, they found that after correcting for tag loss, mortality caused by tagging was on average 10.3% and as high as 33.3%." It seems that you would want to address the discrepancy in your results as compared to Knudsen et al. (2009). Further, Knudsen et al. (2009) reported differences in growth of PIT-tagged vs non-tagged fish. Does Carson NFH collect lengths and weights at spawning that would allow the CSS to address that question?

Rather than simply reporting that no evidence of an effect was found, please also present point estimates and confidence bounds. For example, Figure 8.9 shows estimates of effect sizes with confidence bounds, but a reader would be hard pressed to interpret the results. All the confidence bounds in Figure 8.9 seem to run from about -1 to 1. Presumably these are on the logit-scale and so represent bounds on the odds-ratio (?) and so the confidence bounds on the effect size are somewhere between exp(-1)=0.36x to exp(1)=2.7x! These are very wide, but many readers may not appreciate just how wide.

The major problem with studies of the kind in this chapter is that very large sample sizes are required and consequently, no one study is likely to give definitive results. It would be an opportune time to do a meta-analysis of the many different studies that have looked at this issue to help guide managers on this important topic.

The chapter fits several Cormack-Jolly-Seber models to selected parts of the data. Is there any advantage to creating a single omnibus model?

The ISAB has the following suggestions and questions to improve the statistics and modeling in the chapter:

p. 176. Figure 8.3 seems to indicate that fish of age 3, 4, and 5 are all pooled to estimate a single "survival" probability. But these three age groups will have quite different survival probabilities from Bonneville (as juveniles) to Bonneville (as adults). This heterogeneity may affect subsequent estimates of survival. Fortunately, the vast majority of fish are age 4 so any bias is likely to be small. This should be mentioned in the report, and an estimate of potential bias produced.

p. 177. The estimate of tag loss needs a model to justify that these are the estimates of the relevant parameters.

p. 178. Was there evidence of over-dispersion? If so, how was this incorporated into the results?

p. 179. Doesn't the proportion of release groups in each rearing vessel type add to 1? This would make these three X-variables co-linear with the intercept. How was this handled?

Editorial comments

p. 171. It states: "… with a power of near 80%." Please give the effect size here rather than making the reader refer to past reports.

p. 174, 1st full paragraph. It is not clear when this holding period took place. Was it at the time of smolt release or at the time of tagging?

p. 174. It states: "… these fish were removed from the PTES database ..." It is unclear why these fish had to be removed from the database? Of what is PTES an abbreviation?

p. 180 (and elsewhere). It states: "… do not indicate significant differences between the two PIT tagged groups." This should be reworded as "indicates that there was no evidence of a difference in survival between the two PIT tagged groups." Similar changes are needed in other conclusions in the chapter.

p. 184, Table 8.4. How can the number of surplus fish be negative in 2014?

p. 184, 1st paragraph. How can the total percent returns by age = 104% (7% returned at age-3, 90% returned at age-4, and 7% returned at age-5)? The total number of returning dual tagged fish in Table 8.5 is 77, not 76.

p. 186, Figure 8.9. The X-axis needs units (logits?).

p. 187, Figure 8.10. Please place the brood-year of the group of fish next to the corresponding point.

IV.9. Chapter 9. Preliminary Development of an approach to estimate daily detection probability and total passage of spring-migrant yearling Chinook salmon at Bonneville Dam

An estimate of abundance is found using a variant of the Schaefer (1951) estimator. The approach looks promising, but the chapter needs additional details so that a reader can follow the development.

The development of the estimator seems somewhat ad hoc. What is needed is a formal development of the methodology with definitions of parameters, a sample dataset, a statistical model, and an explanation of the estimators. A table similar to Table 2 of Sanford and Smith (2002) showing the probability and expected values of each history would be helpful (i.e., PIT-tagged detected or not detected at Bonneville and detected or not detected in the trawl, plus the number of untagged smolts found in the collector).

The chapter also did not consider a variant of a Petersen-type estimator (Seber 1982). For example, suppose that detection probabilities are constant over time so that the counts for each day in a year can be pooled. Then, let *N* be the total abundance of smolts passing Bonneville; λ represent the proportion of the run that is PIT tagged; p_B represent the collection efficiency at Bonneville (all PIT tagged fish that are collected are assumed to be read), and p_t represent the detection probability in the trawl (PIT-tagged fish only). We then have:

n_u=number of smolts without PIT tags collected at Bonneville with expectation $N(1-\lambda)p_B$

n_{10}= number of PIT tagged smolts detected at Bonneville and not in the trawl with expectation $N\lambda p_B(1-p_t)$

n_{11}= number of PIT tagged smolts detected at Bonneville and in the trawl with expectation $N\lambda p_B p_t$

n_{01}= number of PIT tagged smolts not detected at Bonneville, but detected in the trawl with expectation $N\lambda(1-p_B)p_t$

Then the Petersen-like estimator

$$\frac{(n_{01}+n_{11})(n_{10}+n_{11}+n_u)}{n_{11}}$$

has an approximate expectation of *N* (after substituting in the expected values of each term).

Because the proposed estimator appears to be a variant of Schaefer (1951), it likely has the same properties as Schaefer (1951) and unfortunately, is likely no better than a pooled-Petersen estimator (shown above) (Schwarz 2008). This chapter should also evaluate the performance of a pooled-Petersen estimator for comparison. Results in Figure 9.1 may be a simple artefact of the Schaefer estimator forcing the daily probabilities to be similar.

An alternate estimator is a time stratified Petersen estimator. Methods proposed by Bonner and Schwarz (2011) as implemented in the BTSPAS package in *R* may be applicable by running the estimator in "reverse" (i.e., pretending the smolts swam from the trawl to Bonneville). The investigators should investigate this alternate estimator in this chapter as well.

Finally, the chapter ignored information about detection at Bonneville Dam from fish detected above Bonneville Dam and also detected in the trawl. The statistical model should incorporate information from these additional fish to improve precision of the estimates.

This is a very good first step toward the development of a predictive model to estimate smolt passage at Bonneville Dam. The continued development is encouraged.

Minor comments

p. 192. The variable set is sufficiently small that an all subsets regression can be used rather than a stepwise procedure.

p. 197. Figure 9.2. "Standard deviation" in the legend should be "standard error"?

p. 199. Figure 9.4. There appears to be overestimation happening for larger predicted values. That is puzzling. It suggests a non-linear model would work better. This may indicate important cofactors are not being modeled

Editorial comment

p.191. The notation U_j is a poor choice because it represents observed counts (capital letters in the capture-recapture literature represent population values)

New references

Bonner, S. B. and Schwarz, C. J. (2011). Smoothed estimates for time-stratified mark-recapture experiments using Bayesian p-splines. Biometrics 67, 1498-1507

Schwarz, C.J. & Arnason, A. N. & Kirby, C. (2008). The Siren Song of the Schaefer Estimator - no better than a pooled Petersen. Available at https://www.researchgate.net/publication/238733181_The_Siren_Song_of_the_Schaefer_Estimator_-_no_better_than_a_pooled_Petersen

Seber. G. A. F. (1982). The estimation of abundance and related parameters. Blackburn: New York.

IV.10. Appendix A: Survivals (SR), SAR, TIR, and *D* for Snake River Hatchery and Wild Spring/Summer Chinook Salmon, Steelhead, and Sockeye

The ISAB does not have any comments on Appendix A.

IV.11. Appendix G. Snake River Adult Success Rates for Transported and In-river Out-migrants and for the Run as a Whole

In Table G.3., steelhead success is lower than Chinook in almost every year. In 7 of the 16 years the difference is 0.100 or more. Might that be because the Chinook estimates do not include 1-salts (jacks) or is there some other factor(s) at play?

V. ISAB Appendix: Suggested Topics for Further Review 2013-2016

In 2016, we recommended these topics (ISAB 2016-2, pages 5-6)

1. Use variable flow conditions to study the impact of flow/spill modifications under future climate change, and examine correlations between Pacific Decadal Oscillations (PDOs) and flows.
2. Examine impact of restricted sizes of fish tagged and describe limitations to studies related to types/sizes of fish tagged
3. Modify life-cycle model to evaluate compensatory response to predation.
4. Comparison of CSS and NOAA in-river survival estimates.
5. Examine factors leading to spring/summer Chinook declines of four and five-year olds and increases in three-year olds.

In 2015, we recommended these topics (ISAB 2015-2, pages 4-5):

1. Use SAR data to examine both intra- and interspecific density dependence during the smolt out migration and early marine periods
2. Propose actions to improve SARs to pre-1970s levels
3. Explore additional potential relations between SARs and climate and ocean conditions
4. Consider ways to explore the variability of inter-cohort response

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 2014 review]
3. New PIT/CWT study

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

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
5. Publication of a synthesis and critical review of CSS results