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Review of Chapter 2 of the Comparative Survival Study (CSS) 2019 Annual Report

Life Cycle Evaluations of Fish Passage Operations Alternatives from the Columbia River System Operations Environmental Impact Statement

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ISAB Review of Chapter 2 of the Comparative Survival Study (CSS) 2019 Annual Report

I. Review 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 (CSS) draft annual reports. The ISAB has reviewed these reports annually beginning ten years ago with the evaluation of the CSS's draft 2010 Annual Report and most recently the draft 2019 Annual Report (ISAB 2019-2).[1] This ISAB review focuses on the 2019 Annual Report's Chapter 2, *Life Cycle Evaluations of Fish Passage Operations Alternatives from the Columbia River System Operations Environmental Impact Statement* (CRSO-EIS), which was not available at the time the ISAB reviewed the draft 2019 Annual Report. Chapter 2 became available when the draft EIS (CRSO-DEIS) was released to the public on February 28, 2020. It is important to note that the ISAB is not reviewing the CRSO-DEIS but just the CSS analyses as reported in Chapter 2.

For context, the CSS's Chapter 2 abstract:

> At the request of the Federal Action Agencies, the CSS used the Grande Ronde Life Cycle Model and the cohort-specific model to analyze six federal operational alternatives for the Columbia River Systems Operations (CRSO) Environmental Impact Statement (EIS), using the 80-year water record. These six operational alternatives included the No Action Alternative (NAA), Multi-Objective Alternatives 1-4 (MO1, MO2, MO3, and MO4), and the Preferred Alternative (PA). In 2017, the CSS modeled several operational scenarios that were analogous to those developed for the CRSO-EIS. The CRSO-EIS federal alternatives included a power focused alternative (MO2) as a "bookend" but did not include a SAR [smolt-to-adult return] focused alternative. The 2017 CSS scenario of breach of the Lower Snake River dams and spill to the 125% tailrace TDG levels in the Middle Columbia River can be considered a SAR focused "bookend." Therefore, to provide this SAR focused "bookend" in the context of the CRSO-EIS scenarios, the CSS added a seventh alternative (MO34) to these analyses, also using the 80-year water record.
>
> There were several important findings from the CSS model analyses. For both CSS models, the non-federal MO34 alternative demonstrated the greatest expected improvements across all biological response metrics, compared to all of the federal CRSO-EIS alternatives. On average, the non-federal MO34 alternative exceeded the 4%

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[1] Also see ISAB 2010-5, ISAB 2011-5, ISAB 2012-7, ISAB 2013-4, ISAB 2014-5, ISAB 2015-2, ISAB 2016-2, ISAB 2017-2, ISAB 2018-4.

> average SAR regional goal. The lower end of the predicted SAR range for MO34 was above 1% for both Chinook and steelhead, indicating that further population decline would be avoided.
>
> Among the federal alternatives, MO3 (the four dam breach alternative with spill to the 120% tailrace TDG in the Middle Columbia River) resulted in the highest SARs and in-river survivals, followed by MO4 (the spill to the 125% tailrace TDG alternative). These two alternatives, among the federal alternatives, resulted in the highest likelihood of meeting the 4% average SAR regional goal. The lower end of the predicted SAR range for MO3 was also above 1% for both Chinook and steelhead but for MO4, the lower end of the predicted SAR was slightly below 1%, indicating greater risk of further population decline. The other federal alternatives (NAA, MO1, MO2, and the PA) did not meet the regional 4% SAR goal and the lower end of the predicted SAR ranges were well below 1%, indicating greater risk of further population decline under each of these alternatives. For all fish survival metrics, the PA resulted in only slightly better performance than the NAA and MO1, and had lower performance than both MO3 and MO4. Because the modeled datasets provided by the federal agencies used daily averages, the CSS results for the PA are likely overestimates.

The ISAB has previously evaluated versions of the models and analyses used in Chapter 2 in our reviews of past CSS annual reports.[2] We also considered the Grande Ronde life-cycle model during our review of the NOAA-organized Interior Columbia Basin Life-Cycle Modeling (see Chapter 4.a. Integrated population model of the Grande Ronde Basin and our review, ISAB 2017-1, page 49). Although the analyses presented in Chapter 2 were generated to inform the CRSO-DEIS, the ISAB did not review the draft EIS and only referred to sections of it for context and understanding of the analyses reported in Chapter 2. Again, this review is part of our long-standing regular review of Fish Passage Center and CSS analytical products.

II. Review Summary

The ISAB had difficulty reviewing this report ("Chapter 2") because it does not fit cleanly into the format of CSS annual reports reviewed previously. Some methods and reporting styles used in this chapter are likely a specific charge from the CRSO-DEIS steering committee, and the authors perhaps had less freedom to choose methodologies or the format for reporting and interpreting results. As such, it would be helpful for the authors to present the details of the charge they were given so readers can understand what was mandated for this chapter report and what was under control of the authors, as is the case in the CSS Annual reports.

The CRSO-DEIS analyses (and thus those reported in Chapter 2) use an 80-year modified flow dataset as input to the models. Modified flows are defined as the historical streamflows that

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[2] Supplemental materials for this review are listed in the Appendix at the end of this document.

would have been observed if current irrigation depletions existed in the past and the effects of reservoir regulation were removed (CRSO DEIS, Appendix I, page I-4-1).

The ISAB interprets that the modified flow dataset does not reflect projections of flow and environmental conditions that may occur due to future climate change. The results from the mandated task of using the modified flow dataset to compare operational alternatives are useful, but the results may not be indicative of future benefits. This is noted by the authors at the end of the introduction of Chapter 2: "Finally, it is important to carefully consider the lower end of the predicted ranges of biological response metrics, as anticipated consequences of climate change suggest poor river or ocean conditions may occur more frequently, which would mean that the lower end of the predicted ranges is likely to occur more often." This caution needs to be described more prominently in the report. Climate, ocean, and in-river conditions are unlikely to remain static in their current state or range of conditions.

The CRSO-DEIS appears to contain an examination of the potential impacts of climate change (Chapter 4 and Appendix V) on the freshwater environment, ocean environment, and life histories of salmon and steelhead. These would provide a useful framework for future modeling and understanding the limitations of the current analysis. Chapter 2 should summarize these findings in more detail for the reader rather a single sentence as noted above.

The modeling presented examines effects of large-scale management actions, such as Snake River dam breaching, and various spill and flow scenarios that may incorporate other bioengineering improvements such as high capacity ("fish friendly") turbines or powerhouse surface passage. Based on the modeling, scenarios that include dam breaching and maximum spill result in the greatest SARs. However, key outputs of models such as variability in projected SARs across years and associated measures of uncertainty for summary performance measures are not presented. Was presentation of results for this Chapter 2 dictated as part of the charge to the authors for the CRSO-DEIS? Could additional modeling results beyond those dictated for the CRSO-DEIS be reported as part of Chapter 2 of CSS annual report? Measures of process and population uncertainty around the point estimates will be difficult to develop because simulations do not include demographic stochasticity; i.e., the same set of inputs will always lead to the same number of fish surviving the hydrosystem, surviving the ocean, and returning to spawn. However, variation in the model response measures (e.g., SARs) reflective of the 80-years of hydrology would be useful to report to aid in interpreting the differences among the scenarios (alternative management actions). A single point estimate of the average/median response without considering year-to-year variation across different flow regimes and without considering demographic stochasticity is much more difficult to interpret. In addition, caveats about interpreting the results in Chapter 2 should be stated much more prominently.

Components of the cohort-specific (CSS-CS) model are not all based on the same years of data, and the years generally stop by about 2013, perhaps reflecting complete brood year records. However, partial brood year records are still useful for estimating juvenile survival as was done in Chapter 3 of the Annual CSS reports. It was unclear to the ISAB why data series were truncated for the CSS-CS models. The CSS Grande Ronde life cycle (CSS-GR) model uses data from 1966 to 2010 (much longer than the CSS-CS), but more complete brood-years appear to be available. How different are the datasets used for model fitting in terms of conditions experienced by fish? For example, does one set of data include drought years while the other does not? If so, this may affect the performance of the two models differently. Reasons and implications for the choice of fitting data should be stated and discussed more fully.

SAR and marine survival may be sensitive to smolt body size, timing of emigration, and date of marine entry. Are these expected to remain the same under all operational alternatives? Do other variables used in the model, partially or completely, capture these effects? The ocean is the source of the most important and highly variable mortality—how well have the models captured variation in ocean survival as a function of measurable variables that can be forecast into the future? Rationale for the exclusion of fish-specific covariates needs to be presented and justified.

The present analyses have a rich model-generated dataset based on the modified flow record to identify processes important to determining the variation and magnitude of SARs. Yet, only point estimates of the mean/median response are reported. A rationale for the sole use of means/medians needs to be included. In addition, the report could be improved with an analysis to provide deeper understanding and implication of WHY the scenarios performed as they did. The chapter should address two questions. First, how do the results at different life-stages vary across the alternatives? These are shown for the CSS-CS models, but only SARs are presented for the CSS-GR models. Second, are the internal life stages available for review from the CSS-GR model? The yearly outputs across scenarios should be sufficient to tease apart more mechanistic (life stage and process-level) understanding of why the models generated different SARs and relative abundances and why alternatives produced differences in SARs.

For such an important study, caveats of interpreting current findings as representing future benefits should be more fully presented in the chapter's Discussion section. For example, not only may the flow record change with climate change, but maturation schedules, conversion probabilities (what fraction of adults survive from Bonneville to the spawning ground), harvest rates, habitat improvements, fish passage improvements, ocean conditions, and such may change from those used in the models. A chart listing these other factors and their impact on the results would permit a side-by-side comparison and consideration of outcomes. For example, would changes in conversion probabilities modify the results positively or negatively, by a large or small amount, or would they change the order of the results across alternatives?

Chapter 2 implicitly assumes that the reader is familiar with the many years of the CSS reports and the DEIS, and many key terms and data used in the chapter are not fully explained. For example, little explanation is provided in how the changes in model inputs from the alternatives actually affect specific life stages and processes within each of the models. Chapter 2 presumably relies on the other chapters in the 2019 CSS report and the DEIS for details. Nonetheless, many readers will look at "Chapter 2" in isolation and will need some guidance to fully understand terms and data employed, results, and implications. The Chapter 2 document should provide sufficient information so that that the reader does not need to make extensive reference to these other reports. Chapter 2 either needs to be better integrated into a final version of the 2019 CSS Annual Report, or if this report is a standalone document, it needs to be more self-sufficient.

III. Suggested Topics for Further Review

In our 2018 and 2019 reviews, we suggested the following topics related to Chapter 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. (ISAB 2018-4)

- Smolt-to-adult survivals (SARs) continue to be very low. Do we have enough information to suggest changes to hydrosystem operations that could improve SARs? Is there now enough information to estimate how much improvements in habitat and other “controllable” aspects of the hydrosystem are needed to improve SARs? (ISAB 2019-2)

These two topics have been at least partially addressed in this chapter.

The ISAB recommends that:

1. Recent years, such as 2015, have experienced very low summer flows and warm temperatures. The world’s five warmest years in the 1880 to 2019 record have all occurred since 2015 with nine of the 10 warmest years occurring since 2005 (NOAA Climate.gov). Future projections based on the modified flow dataset are therefore likely to be overly optimistic about survival. A sensitivity analysis needs to be performed to investigate the impact of climate change by, for example, only using water-years that have similar patterns to what may be observed in the future due to climate change or by developing likely future flow regimes, such as those described in Chapter 4 on Climate Change in the CRSO-DEIS. Such a sensitivity analysis will also need to account for changes in the maturation schedule, conversion probabilities due to warming water, and changes in the capacity in the Beverton-Holt spawner-recruit relationship for the CSS-GR model, habitat improvements, etc. Do relative rankings of the alternatives change if future climate scenarios (even if oversimplified) are represented?

2. A more detailed comparison of results between different types of flow years would be useful as a first step toward meeting ISAB recommendation (1). Demographic and other stochasticity (80 years of hydrology is a great start) should be included in the models so

that year-to-year variation in the output measures is more reflective of the response from different operations.

3. Both models do not incorporate the relationship of individual fish characteristics—such as body size, body mass, and condition factor, and date of ocean entry—to survival. The current literature is confusing (e.g., Faulkner et al. 2019 vs the rejoinder in Appendix G of the 2019 CSS Annual Report). It would be beneficial for both groups to collaborate on joint analyses and use a common data set to resolve this issue.

IV. ISAB Comments on Chapter 2 Sections

A. Introduction (pages 2-4)

Presently, as noted above, Chapter 2 assumes that the reader knows the models, the system, and is familiar with the many earlier reports that are cited by the authors for model descriptions and other details. Throughout this ISAB review, we ask a number of clarifying questions. Many of these may be answered with additional concise or descriptive text. Others, however, highlight a need for more thorough attention to assumptions or rationale for why information is omitted. One question to consider is, “Who is the audience?” While the authors should not and cannot repeat ALL details of the analyses in this chapter (e.g., detailed model descriptions), the present level of presentation is terse. A balance needs to be found between so much detail that readers lose the important information and perspective versus a document that is so vague or insufficiently self-contained that many readers are not sure what was done. The chapter needs additional information to ensure that methods and results are sufficiently described for readers to understand the analysis without requiring extensive reading of the other sources.

A modified 80-year water record is being used to evaluate the models, but the derivation of this water-record is not transparent and therefore difficult to determine. Many readers may erroneously assume that it is the historical water record, but it is not. The chapter needs to clearly explain how the constructed flow regime was developed, and how it is used in the modeling without the need to delve deeply into the DEIS. A more complete description of the development of the water record is available in the CRSO DEIS, Appendix I, but a summary should be given as part of Chapter 2 or that appendix included as part of this chapter. For example, the summary should indicate that the flow record is based on the period from 1929 to 2008 that is used by the Action Agencies in the Columbia River Systems Operations Draft Environmental Impact Statement (CRSO DEIS).

The following description is from the CRSO DEIS, Appendix I, page I-4-1 which could serve as a useful description of the modified flow dataset for Chapter 2.

> The base source of water inflow time series into reservoirs and stream reaches, used in both HYDSIM and ResSim modeling, is the 80-year Modified Flow dataset (Bonneville 2011). This dataset is computed for the Columbia River hydroelectric system and associated tributaries, and used by the numerous internal and external groups for a variety of studies. Bonneville, Reclamation and the Corps perform hydroregulation studies of the Columbia River basin for analysis of environmental impacts, changes to operation criteria from BiOps, power revenue forecasts, FRM studies, operations planning, downstream benefit calculations, and effects of new projects or plant data. A wide range of other regional organizations, including the NWPCC, Northwest Power Pool, Pacific Northwest Utilities Conference Committee, fishery agencies and organizations, universities, research organizations, contractors, and public interest groups also need a consistent and accepted regional streamflow dataset. Modified flows are defined as the historical streamflows that would have been observed if current irrigation depletions existed in the past and the effects of reservoir regulation were removed (except at the upper Snake, Deschutes, and Yakima basins where current upstream reservoir regulation practices are included). Because irrigation practices and evaporation rates have changed since the historical flows were observed, historical streamflows need to be adjusted to account for current levels of irrigation depletions and evaporative losses. The 2010 modified flow study includes 80 years of flows (1929 to 2008) adjusted to 2010 irrigation depletions and evaporation rates.

A table that reports the types of years in the 80-year modified flow record (percent drought, low-flow, 25% to 75% percentile of flows, and floods) would help to understand the range of hydrologic characteristics represented in the 80-year flow regime used to compare management alternatives. It was unclear if a discussion on how future regional climate changes could change the hydrologic characteristics was outside the charge to the authors. Are there existing subsets of years in the constructed hydrological record similar to expected future conditions? How does the 80-year record compare to the future climate projections in the CRSO-DEIS?

Smolt-to-adult return (SARs) can be computed in many ways, but the actual computation must be inferred from a careful reading of the chapter. This should be more prominent in the chapter (e.g., in a table with definition of many of the terms in this report). For cohort-specific models, it appears (pages 10-11) that computations are based on juveniles at Lower Granite Dam (LGR) and adult returns at Bonneville (BON). Yet, the Grande Ronde model uses LGR for both juveniles and adults (McCann et al. 2017). Various methods for SARs computation should be explicitly defined in the introduction. Definitions should make clear whether SARs have been adjusted for ocean harvest, or if ocean harvest is implicitly included in ocean survival. For example, on page 11, the text explicitly states that SARs are based on adults detected at Bonneville, so this seems to indicate that ocean harvest is combined with natural mortality. Is this correct? Additional clarification is needed.

Harvest is one variable that can be controlled through fisheries management to improve returns. Similarly, have adjustments for marine mammal predation and control efforts below

Bonneville Dam been included? If not, then the natural mortality (modeled to emphasize the early ocean period) is combined with fishery interceptions (likely to most heavily affect older fish) and marine mammals. This would seem to hamper interpretation of changes in the river designed to benefit juveniles.

The term "SAR focused alternative" (MO34, as a bookend to contrast the power focused alternative of MO2) implies that a certain alternative mainly considers SARs as a significant input for decision making. How are SARs used in the DEIS document for decision making? For example, page 3 of Chapter 2 states the DEIS includes predicted metrics of abundance, juvenile passage, and SARs but provides no information on the relative weight of each outcome in the decision process for selecting a preferred alternative. If one alternative has much better juvenile survival within the river and worse SARs compared to another alternative, then how is the choice among alternatives made? Similarly, on page 8, it states that the CSSOC 2017 used these models to evaluate the effects of the CRSO-DEIS operations alternatives on juvenile fish travel time, juvenile fish survival, TIR, ocean survival, and SAR, but again provides no information on weighting. Are SARs the ultimate metric of success? Should the reader focus mostly on the results for SARs? This may be beyond the mandate of the authors for this chapter, but some assistance in how results are intended to be used or interpreted, without necessarily reaching any conclusions beyond the scope of the chapter, is needed for the reader.

The PITPH variable is used extensively in this report. Its computation is described in detail in Appendix J of the 2015 CSS Annual Report, and a summary is available in Chapter 1 of the 2019 CSS Annual Report. It is an index of the total number of powerhouses encountered ranging 0 – 8 (e.g. Figure 2.1) based on the proportion of fish that encounter a powerhouse at each dam in the migration route. Later in the report, the term “powerhouse passage event” (page 6) is used for PITPH. A consistent terminology would be helpful. Many readers will read this chapter as a report in isolation from these other documents, so it would be useful to refer these readers to Chapter 1 in 2019 CSS Annual Report or provide a list of acronyms and definitions.

With regard to the options, what does it mean to “breach” a dam? Does this mean to demolish it, cut a notch, etc.? There a number of engineering approaches to return or mimic a “normalized” hydrograph – are the differences important or otherwise relevant? The abstract and introduction need to explain the conditions and actions that are represented by the term “dam breaching.” Perhaps these are well established or defined elsewhere.

The chapter states, “Federal parties expressed their concern that the 2017 CSS alternatives were not based on the full 80-year water record, and so they would not be applicable to the CRSO-EIS analyses.” Even using an 80-year historical water record includes some basic assumptions that it includes the range of future conditions. It would be helpful if this could be explained in greater detail especially given that on page 8, the Chapter 2 analysis still uses a random 10-year period of the water record when using the CSS-GR model.

MO34 is a blend of alternatives MO3 & MO4, which the CSS Oversight Committee (CSSOC) created ostensibly to make this report comparable to scenarios analyzed in the 2017 CSS report. The rationale for this new alternative is confusing, and the report should specify which 2017 modeling results were used to inform and design the new alternative.

The ISAB can sympathize with the authors with the near impossibility of fully describing a complicated suite of scenarios and models in a report of finite length. In general, they have done a good job of providing citations where further details may be found. A good outline of the workflow (text or graphics) showing the relationships connecting data sources and management alternatives with specific model input variables, connecting those input variables to the life stages and processes in the different models, and defining how outputs are computed in the models would provide a useful roadmap for uninitiated readers.

B. Methods (pages 4-13)

The report cites CSSOC 2017 for the cohort-specific models, but in each description refers to similar models from the CSS annual reports (McCann et al. 2015). Are these the same type of models? Do they have similar findings (i.e., coefficients in the same direction and magnitude)? Why aren’t the results from Chapter 3 of the annual CSS reports used directly? What is the relationship between Chapter 2 and Chapter 3?

The report needs a discussion on how the CSS-GR model and CSS-CS models are similar and different, and what are the advantages and disadvantages of each modeling approach? How do the life stages and processes map between the two models? When processes co-occur in the models, how are they represented in each? Does one model include processes not in the other? What are the differences in the temporal resolution of the two models? The approach of multiple (two in this case) models is powerful as long as there is clear understanding of the similarities and differences between the models.

The report needs to be much more explicit about the simulation methodology as this has implications on how to summarize and interpret the findings. Here is the ISAB’s understanding of how the simulations were conducted:

- All CSS analyses of CRSO-DEIS operations alternatives used results from Action Agencies’ hydromodeling of flow, spill, and reservoir elevations, over the 80-year water record. The results from the Action Agencies’ hydromodeling were summarized for input into the CSS models. The inputs for the CSS models include values of Water Transit Time (WTT), the number of powerhouse passage events (PITPH), and the proportion of smolts transported.

- For the CSS Grande Ronde (CSS-GR) model, McCann et al. (2017, page 21) indicated that samples were drawn from the posterior distributions of the parameter estimates. The number of simulated samples was never clearly specified. For each set of parameter values, the population is initialized (with the same values for all simulations?). Then the CSS-GR model was run using the 80-year modified flow record (is there a burn-in phase so that the CSS-GR model is stable?) giving an 80-year predicted numbers of smolts, spawners, etc. There was no demographic stochasticity in the CSS-GR model, so the same set of inputs will give the same outputs. A random 10-year period from the 80-year predictions was taken and the average SARs was computed for the 10-year periods and reported for that simulation.

- For the CSS-CS models, Chapter 2 indicates that 10,000 samples from the posterior distribution of the parameter estimates were generated. For each simulation, the CSS-CS models were used to predict the average FTT, juvenile survival, ocean survival, and SAR for each water year. There is no demographic stochasticity in the CSS-CS models so that the same set of inputs (parameters and water year record) will generate the same outputs. It was not clear if how the 80-year predictions were summarized—all 80 years of values used or subsampling like with the CSS-GR model?

In both cases, was the covariance among the parameter estimates calculated and reported? For example, were simulated values for each parameter sampled independently (marginally) or were the parameters sampled as a set (using the covariance matrix for the CSS-CS models and a multi-variate normal distribution or using the multivariate posterior sample for the CSS-GR model)?

Similarly, more details on how summary measures are obtained are needed (Tables 2.3 onwards). The ISAB understands that:

- For the CSS-GR model, the median value (across all simulations) of the average prediction of a random sample of 10 years (for each simulation) was obtained.

- For the CSS-CS models, the description of how these summary values were estimated is vague in the Chapter 2 document. Presumably, the median (across all simulations) of the average of a random sample of 10 years of the average predicted response was obtained. Table 2.7 appears to report median predictions (based on the fact that inter-quartile ranges are presented), but this is not explicitly stated. The text on page 21 uses the phrase “MEAN SARs” rather than the implied median.

The components of the cohort-specific models are not all based on the same years of data, and the years generally stop by about 2013. Why were the submodels of the CSS-CS models not fit to exactly the same years, and why were data more recent than 2013 not used? Was the idea to get the best fit for each submodel (i.e., use all available data) at the sacrifice of consistency of responses across submodels? Are only complete brood year records used? Partial brood year records are still useful for measuring juvenile survival (see Chapter 3 in the CSS annual reports). Why are these not used?

The CSS-GR model parameters were based on data from 1966 to 2010. How does the use of two different time periods for the CSS-GR and CSS-CS models, with only some overlap, affect the results? If all models are fit for the same years of data, then data may have to be truncated, but year-to-year effects are common across all models. If different sets of data are used for different models, then the results may not be directly comparable (e.g., some data sets may include drought years while the others do not). The chapter should discuss the comparability of the years of data used to fit the different submodels comprising the CSS-CS models and between the CSS-CS and CSS-GR models.

1. Operational Alternatives Modeled by the CSS (pages 4-6)

A table that summarizes the seven alternatives would help the reader quickly compare their differences and supplement the narrative in the text. For example, the table could have columns for spill, other flow modifications, transportation, high capacity (“fish-friendly”) turbines, powerhouse surface passage routes, dam removal, with brief descriptions for each.

For each of the alternatives except the No Action Alternative (NAA), the authors state, “It is unclear whether these changes were incorporated in the Action Agencies’ modeling of the 80-year water record.” The CSS should confirm if this has occurred, or if not, at least discuss the implications of this uncertainty in greater detail. Does this statement indicate there really was such loose coordination of the hydrologic models with the biological models? Further information is needed, and this should be included in the Discussion.

The authors state, “However, even at 30% efficiency, the installation of Powerhouse Surface Passages (PSPs) had very little effect on modeling results. Here, we only present model results with no PSPs installed (i.e., 0% efficiency).” Does this decision introduce bias into the results? By using 0%, the analysis effectively censors the PSP-related actions in the alternatives. If the authors feel that PSPs have a negligible effect, then they should provide evidence; otherwise, state this clearly as a model assumption. The report (pp. 33-34) cites an FPC memo (FPC 2019b). A quick review of that memo supports negligible effect of PSPs except for scenario MO2, but the memo provides insufficient documentation of the methods used to come to this conclusion. Also, the Draft EIS states, ”Consequently, the team chose Powerhouse Surface Passage efficiencies for sub-yearling and yearling Chinook, and Steelhead at 30, 40, and 50% respectively for purposes of the CRSO EIS effects analysis” (CRSO DEIS, p. E-1-6). Why were the other efficiency levels not included in the CSS analyses reported in Chapter 2?

2. CSS Grande Ronde Life Cycle Model (pages 6-8)

The ISAB has reviewed the CSS-GR model in previous years and is generally pleased with the modeling effort. However, the report discusses “validation” against historical data but does not describe the methods used in this validation. The only methods described are that “… the model validation involved comparing predictions against empirical data until the best fitting model satisfied criteria that ensured the best fit across all abundance and survival data.” This sounds like a description of the model estimation procedure, not validation. How does this provide evidence that the model structure is reasonably realistic? No validation process is described in McCann et al. (2017). For example, the CSS-GR model could be fit by successively deleting specific years of data and then comparing predictions for the “deleted” years vs. actual data for the “deleted” years. Alternatively, the model could be used to forecast years outside the modeled flow years and compare the predictions with actual events. Perhaps this is just a terminology issue and the term “validation” should be clarified.

As noted earlier, “In the CRSO-EIS analyses, the two metrics were averages over a random 10-year period in the 80-year simulations for each simulated outcome. This is in contrast to always taking the average of the last 10 years in the 2017 CSS Annual Report (McCann et al. 2017).” What is the reasoning behind this change (i.e., why use a random 10-yr period instead of averaging over the whole series)? In the introduction, one objection to the results in McCann et al. (2017) was not using the full flow dataset. Isn’t this still a problem? Note that if the simulation was conducted as noted above, then reporting the averages from random samples of 10 years is basically equivalent to taking the average over the full 80-year predicted values. This is because a similar set of parameter values will be generated many times and a different random sample of 10 years will be selected and all 80 years eventually will be covered for a particular set of parameter values. However, estimates of variability (10-year mean values versus individual-year values) may differ between the two approaches.

It was unclear where the equation for the logit(probability of transportation) came from and how it was derived. A reference or explanation of how this was derived is needed.

The report states, “The CSS life cycle model predictions for a free-flowing river are also consistent with empirical in-river survival estimates from wild spring Chinook from the John Day River, which averaged 81% during 2000-2012.” More information is needed on the John Day River study to ensure that it is a surrogate for the Columbia River. Was the survival standardized based on distance traveled or time elapsed? The conditions in the rivers are different, so it is important to know how the results from the John Day River were used to inform model performance for the Columbia River. Recent studies in the Fraser River have observed substantial mortality of smolts in this large, free-flowing system, and its tributaries.

The discussion of in-river survival also needs to be coupled with ocean survival. For example, under a WTT of 30 days, the fish are older and larger when they exit the river, and so perhaps much of the "mortality" already has taken place compared to a WTT of 3 days when fish arrive at the ocean earlier and so may experience the rest of the mortality in the ocean rather than in-river.

3. CSS Cohort-specific Models (pages 8-13)

The CSS-CS models are a set of independent calculations that model juvenile travel time, juvenile survival, ocean survival, and other responses using individual cohorts (e.g., typically releases over two-week periods) but using a different set of years for estimation. These models are NOT integrated over an entire life cycle. As a result, combining results from each model may not equal predicted SARs from a full life-cycle calculation (e.g., predicted juvenile survival from juvenile survival model x predicted ocean survival from ocean survival model plus adjustments for transport). This is unlike the life-cycle models where results must be congruent across all life-stages within each year and also over multiple generations.

It was unclear how these models differ from those considered in the CSS Annual report (e.g., Chapter 3). Some details would be helpful.

The estimated coefficients and measures of uncertainty for each model appear to be illustrated in Figure 2.2. An appendix with the numerical values would be helpful.

If mortality partly depends on duration in the river and also varies from year to year, then does the calculation of fish travel time become dependent on mortality? That is, if the slower fish tend to die at a higher rate than the faster fish, will years with overall higher mortality rates show faster apparent travel times because more of the slow fish died? Alternatively, do the effects even out? This was raised in earlier ISAB reviews of the CSS annual report series, and the answer to our query should be summarized in this part of the chapter.

The chapter and models focused on the effects of flow. The chapter states that effects of environmental factors were represented in a logit equation based on day, WTT, and PITPH. This does not account for temperature. How do these variables (implicitly) reflect temperature? Some of the warmest water temperatures and earliest maximum water temperatures occurred in 2015, but the flow record for 2015 was not a record low-flow year. How does the model account for the effects of temperature and projected regional warming on juvenile and adult survival in the freshwater environment? Additional information on temperature data and its influence on the model would be helpful.

Under “Ocean survival models,” the text indicates that adults to Bonneville Dam is the index of survival at sea, based on the number passing Bonneville Dam on their way to sea. This seems to neglect the effects of fisheries along the coast and removals in the river below Bonneville Dam. The extent of these interceptions will vary considerably based on species and population. Steelhead catches at sea will be far lower than those of Chinook salmon, and Chinook salmon from interior populations, especially those with stream-type life histories compared to lower river and ocean-type populations, which generally have lower rates of interception at sea relative to the catches in terminal areas (Sharma and Quinn 2012). Moreover, the proportion taken in these fisheries may also vary from year-to-year—this information is likely available from the Chinook Technical Committee. Mortality from marine mammals below Bonneville is apparently not explicitly represented, and this has been increasing over time. If and how is this accounted for?

In the equations for the models, it is not clear if the variable $\mathrm{Day}_{i,y}$ refers to the start-date or mid-date of the cohort migration/tagging dates.

For all of the CSS-CS models, aspects of body size, body mass, or fish condition were not included in the models. Furthermore, date of ocean entry was not considered in the ocean survival model. The literature is not consistent; more work is needed (e.g., Giorgi et al. 1997; Scheuerell et al. 2009; Evans et al. 2014; Faulkner et al. 2019; the rebuttal for Faulkner et al. (2019) in Appendix G of the 2019 CSS Annual Report). It would be helpful to discuss this issue in the description of the models and why variables and processes were not related to fish size, mass, or condition factor, as well as how ocean entry was represented.

The CSS-CS models equations used the results from the CSS annual reports as starting points and then refit those using different data. How similar are the results (i.e., are the slopes of variables consistent and of the same magnitude)? Did the models have the same level of fit (e.g., R^2) as in the CSS annual reports? A summary comparing the CSS-CS models fit with those in the CSS annual reports would be helpful.

The different CSS-CS models used different sets of data (e.g., 1998-2011 was used for the juvenile fish travel time model, but 1998-2005 was used for the juvenile survival model; no information was presented on dates for the TIR model). Why are different datasets used for the different CSS-CS models? Why are more recent data not used?

When modelling steelhead, how was residualization of smolts handled (if applicable)?

a. Juvenile Fish Travel Time Models (pages 8-9)

Models described in McCann et al. (2015) have consistently shown that juvenile fish travel time is primarily a function of water transit time (WTT), proportion spill, and seasonal effects (day of the year). Changing climate/changing flows could substantially affect the results of these models and for the juvenile survival models, so the model based on historical data may not be applicable for the future.

b. Juvenile Survival Models (pages 9-10)

No additional specific comments.

c. Ocean Survival Models (page 10)

Which upwelling index was used (monthly or daily, and at what latitude)? While including upwelling and ichthyoplankton indices may have improved model fits for the short data sets, this may not be a good approach in the long term. There is a growing literature indicating that statistical relationships between organisms and climate indicators are valid for relatively short periods of times but are not useful for long-term predictions (e.g., Litzow et al. 2019).

Do these models account for the warm water mass that has been developing in the Pacific? To what extent would phenomena like “the blob” (Bond 2014) be represented in the logit equation for ocean survival? The upwelling anomaly index and the ichthyoplankton biomass index are general ocean indices for the west coast. More localized, but significant, patterns of environmental factors and food abundance could have major impacts that are not presently represented in the model. The ocean model cannot be easily subdivided spatially to explicitly capture the more localized effects, but the implications of this generalized representation of the ocean conditions should be noted and discussed.

These estimates do not seem to explicitly account for fishery interceptions at sea or in the river below Bonneville Dam, which can be substantial. Moreover, the models of survival seem to assume constant mortality (described in papers following Ricker 1976) in the years after ocean entry, so it does not seem surprising that the results point to environmental influences in the year of entry. Are the modeling results simply a reflection of the assumptions? Also, Ricker (1976) gives a number of methods for estimating marine mortality, all of which have problems of one sort or another. How could the concerns expressed by Ricker (1976) affect the modeling conclusions reported in Chapter 2?

d. Smolt-to-Adult Return (SAR) Models (page 11)

How do the results from SAR model compare to the product of the juvenile survival model and ocean survival model (adjusted for transportation)? This would be a good diagnostic to identify cohorts where something unexpected has happened.

As noted above, how have the SARs been adjusted for oceanic and in-river fisheries or removals by sources, such as marine mammals? The Methods section further clarified that mini-jacks and jacks were not counted as adult Chinook salmon (though 1-salt steelhead were included). The incidence of these life history patterns (and also males that do not migrate to sea at all) is closely related to smolt size, and therefore to wild versus hatchery origin, source population, and other attributes. Mini-jacks and jacks can be a substantial fraction of total returns, and especially for male salmon. The current treatment of mini-jacks and jacks, and their dependence on size that is related to other attributes, should be discussed and how alternative assumptions would affect the results should be described.

e. Transported: In-River (TIR) Models (pages 11-12)

No additional specific comments.

f. Influences of River and Ocean Variables on Life-Stage-Specific Survival (pages 12-13)

Was date of the year examined independently from water travel time? They could co-vary because discharge has a pronounced seasonal pattern. Is there a reason why so few ocean factors, including temperature, were included? Please provide a list of all factors considered and if specific factors were considered but not included because they were deemed not sufficiently influential (i.e., the rationales for inclusion of certain variables and exclusion of candidate other variables).

C. Results (pages 13-30)

Throughout the Results section, mean or median effects without measures of variability are emphasized. In complex systems, average effects do not give a complete picture, and often give a misleading picture when there are strong nonlinearities. The first thing that should be done (as mentioned specifically below) is to present the range of observed effects, either as prediction intervals in tables or using box or raindrop plots of results. Do the rankings of the alternatives change among years? Time-series plots like Figures 2.3-2.5 for the biological results would help clarify this.

As noted earlier, some of the important details of how the simulations were performed and summarized are missing or unclear. The models do not include demographic stochastic components, so the range in predictions is likely understated. This is crucial when reporting statistics on the range of SARs or the proportion of time the SARs is less 1%. The results about the ranges or probabilities need to be revisited and strongly qualified that many stochastic events have not been modelled. The chapter should describe exactly what the reported variability means relative to the different models and to variability expected in nature so that results can be properly interpreted and compared across models and among management alternatives.

a. PITPH (pages 14-15)

The PITPH variable is an annual value for the CSS-GR model and cohort-specific values for the CSS-CS models. Please provide a graph showing the range of PITPH from the cohort-specific models relative to the annual values.

We assume that PITPH was generated by simulating the operational alternatives for each year of the 80-year modified flow record. In that case, we would expect that the estimates for the different alternatives would be strongly correlated due to annual flow regimes affecting each alternative similarly. This is true of all the alternatives except MO2, which does not have the same strong peaks and valleys that the other alternatives show, in particular in years 1945-1970 and after 1990. Is there something about this scenario that limits the variability of PITPH?

Similarly, Figure 2.3 shows that the pattern of PITPH is roughly consistent across the alternatives (i.e., all of the alternatives tend to move up or down in unison). However, there are some obvious times when this is not true (e.g., in 1998, the PITPH for MO2 decreases compared to a sharp increase in PITPH for the other alternatives). Why does this disconnect occur? Perhaps a single graph showing the deviations in PITPH for each alternative from their respective mean would highlight years where something unexpected has happened.

The 95% confidence intervals in Table 2.1 are likely too narrow because of autocorrelation over time in the PITPH values so the annual means are not independent of each other. Report the estimated autocorrelation and adjust the 95% confidence intervals to account for this autocorrelation.

The report states "… we summarize the seasonal average estimates of PITPH from the CRSO-EIS alternatives and MO34 (Figure 2.3, Table 2.1). It is important to note that these are the seasonal average PITPH estimates that were utilized by the CSS life cycle model (Chinook only)." As Table 2.1 includes a steelhead column, we assume that table is for the cohort-specific models and is not for the same data as in Figure 2.3. Please clarify.

Table 2.1 should also present information about the actual year-to-year and within-season variation (i.e., a numerical summary of Figure 2.3 of the range or standard deviation for each alternative over the 80 years).

b. Water Transit Time (pages 15-16)

Why is there no table of results as there are for PITPH and Transport Proportions?

c. Transport Proportions (pages 16-17)

The 95% confidence intervals in Table 2.2 are likely too narrow because of autocorrelation over time in the transport proportion values. Report the estimated autocorrelation and adjust the 95% CI to account for this autocorrelation.

1. CSS Grande Ronde Life-cycle Model (pages 18-20)

No measure of uncertainty is presented in Tables 2.3 through 2.6. This may be difficult to determine because the CSS-GR model is deterministic (i.e., same input will give the same outputs) and sufficient simulations can always be generated to reduce uncertainty in the median value to essentially zero. However, smolt production and survival are not deterministic processes, and this variability has not been captured. If MO2 is worse than the NAA, are MO3 and MO34 actually different? Is the very small range in mean SARs among the stocks (lines) in Table 2.3 realistic given the high range of abundances in Table 2.5? Was the choice of limited summary measures, and how it is presented, mandated by the charge to the Chapter 2 authors for the analysis of CRSO-DEIS alternatives? Expansion of the presentation of results would seem warranted.

The ratio of SARs relative to the NAA in Table 2.4 doesn't quite match the ratio of abundances relative to the NAA in Table 2.6. This needs some explanation.

The summary measures need to be interpreted VERY carefully. For example, in the CSS-GR, it appears that a mean SAR is computed from a random 10-year period and then the median of the average SAR is presented. This is NOT the same as the median predicted-SARs. How were values for P(SARs < 1%) obtained?

2. CSS Cohort-specific Model (pages 21-30)

The ISAB has similar concerns about the lack of measures of uncertainty about reported summary measures as noted in the previous section for the CSS-GR model.

As also stated above for the CSS-GR model, the summary measures reported for the CSS-CS modeling needs to be interpreted VERY carefully. For example, in the CSS-CS models, it appears that a mean SAR (and other measures) is computed from the full 80-year modified flow record (unlike a random sample of 10-year for the CSS-GR), and then the median of the average SAR is presented. Why is a different method of summarizing the simulations used for the two models?

There is a discussion for the CSS-CS models on the range of the SAR values. For example, on page 21, Chapter 2 discusses the "predicted SAR range." Where is this presented – perhaps page 21 is supposed to read "predicted SAR inter-quartile range." Similarly, in the discussion on page 31, Chapter 2 states that "... the lower end of the predicted SAR range for MO34 was above 1% ..." Presumably this refers to Table 2.10, but Table 2.10 shows that the lower IQR exceed 1% and not that the range of SARs exceeds 1%. Are these "ranges" across years, across simulations, across both?

As well, the discussions of the “range” need to be very carefully interpreted, for a number of reasons:

- The predictions from the CSS-CS models are for the AVERAGE juvenile survival, ocean survival, and SARs for a particular set of input variables and water year (similar to regular regression where predictions are for the AVERAGE response at new value of X).
- As noted earlier, Chapter 2 is not clear how each of the 10,000 simulations (reflecting the uncertainty in the parameter values) is summarized. If a random sample of 10 years is used to compute an average SARs, then for each simulation you get an average (over 10 random years) of an average prediction (from the model).
- The CSS-GR model is deterministic (i.e., the same set of inputs give the same set of outputs), and no stochasticity in smolt production or survival is introduced. Similarly, the CSS-CS models predicts the average response and so no demographic stochasticity is introduced.

Does the IQR refer to the inter-quartile range of this average of average SARs? If so, the IQRs are NOT ranges on individual SARs and cannot be interpreted as such. There are many statements on page 21 about the range of SARs that need careful description and interpretations.

The conclusions about P(SARs<1%) or P(SARs>2%) in Table 2.12 and Figure 2.18 also need much more detail. No information is given on how these are computed. Are these values the mean (over simulations) of the proportion of water-years in each simulation that have SARs <1%? Alternatively, is this the proportion of simulations where the median/mean SARs is < 1%? The results from each simulation are not individual SARs, so these probability statements likely have no, or very complicated, meanings.

Summarize how the thresholds (SAR < 1%, SAR > 2%) relate to the statements regarding population decline/increase that are presented in the NPPC SAR objectives.

D. Discussion (pages 30-36)

Coordinated use of multiple models is a powerful approach answering difficult ecological questions. The use here of a common hydrological input, management alternatives, and a common output predictor (SAR) potentially provide added confidence to modeling results and conclusions. When reporting results from multiple model analyses, it is very important for the reader to know how different the models actually are in terms of variables/parameters that drive their outputs. Should the reader interpret similarity of predictions and differences in predictions as two independent answers or do the models overlap in their assumptions, formulations, and data used in their estimation? In the latter case, agreement across models is an artefact rather than representing convergence of truly independent projections. Without sufficient information and proper guidance, many readers will assume that the models are completely independent and place too much confidence on similarities. The discussion needs to guide the reader through this tricky aspect of using multiple models to address the same question.

The present analyses have a rich model-generated dataset that can be mined to find out which processes and inputs are important to determining the variation and magnitude of SAR. There are 80 years of model output matched with a range of values of many model inputs. This should provide a relatively easy way to add a more process-based interpretation to the modeling results. Such interpretations add understanding to the results that lead to more informative conclusions.

There is little explanation as to WHY the models generated the differences across scenarios. How do the results at different life stages vary across the alternatives? These are shown for the CSS-CS models, but only SARs are presented for the CSS-GR models. Are the internal life stage-specific results available from the CSS-GR model? The spread in outputs across management alternatives should be sufficient to tease apart why they generated different SARs and relative abundances.

The use of the 80-year modified flow record assumes that the past continues to the future. The Introduction includes the statement, “Finally, it is important to carefully consider the lower end of the predicted ranges of biological response metrics, as anticipated consequences of climate change suggest poor river or ocean conditions may occur more frequently, which would mean that the lower end of the predicted ranges is likely to occur more often.” The authors should discuss potential effects of changing climate (see Chapter 4 on Climate Change in CRSO-DEIS) on model results, unless there is a good reason to not (e.g., outside their mandate).

Additionally, the focus on the models being developed with “80-year water records” ending in 2008 is a possible limitation, especially given the fact that many parts of the country are already experiencing altered precipitation (and air temperature) that translates into altered flows (and water temperature). It seems reasonable that the authors should be able to discuss such changes have been detected in the region, and if so, the use of 80-years of water records may be limited for projections and decision making. A summary of conclusions about the impact of climate change on the water record should be included.

Other effects due to climate change may also occur in addition to those effects on the water record. For example:

- The Beverton-Holt spawner recruit-curves are assumed to be unchanged for these scenarios. A discussion of potential impact of changes is needed (e.g., a summary of

McCann et al. 2017 and other reports that discuss the impacts of improved habitat and other factors).

- The maturation probabilities are assumed to be fixed for the 80-year water record. However, the ISAB has reviewed many reports that indicate decreasing mean age of return (i.e., more mini-jacks, more jacks, more young age-classes of adults).
- Conversion probabilities (e.g., probability that an adult at Bonneville returns as a spawner) also are assumed to be fixed. However, with warming waters, conversion probabilities may decline for adults.

How might these changes affect the results?

1. CSS Life Cycle Analysis of Productivity, Abundance, and SARs (pages 30-31)

The models predict that increasing spill will at least double the response (“2 – 2.5-fold increase in SARs and return abundance”) and breaching four dams will increase SARs by “up to 4 times higher.” Despite all the earlier caveats in the chapter that results are only relative and not absolute, the above statement seems very strong. These are only modelled results, and there are many examples where model predictions, especially of abundances, are not fully realized due to the inherent limitation that models are a simplification of reality.

Some discussion of the model assumptions and limitations, especially as they affect the comparison of management alternatives, would be warranted in the final Discussion. Especially needed is discussion of the adequacy of a retrospective simulation of the past as a proxy for predicting future operational alternatives, given that the climate and other conditions in the basin have changed substantially over that 80-year record. Perhaps this could be addressed by examining the performance metrics for selected years in the past that reflect different conditions that might better reflect anticipated future conditions (high flow vs. low flow, late vs early peak flow, high PDO vs low PDO). Again, the ISAB is unclear if this is outside the charge to the authors.

2. CSS Cohort-Specific Models of Biological Responses (page 31)

Statements about the range of values of SARs should be re-visited to ensure that they accurately reflect how the outputs of the two models were summarized. This arises partially from the lack of detail in the document in how the simulations are conducted and summarized and also because of the complicated ways the 80 years of results were subsetted and resampled and that differ between models.

3. CRSO Actions with Highly Uncertain Benefits and Assumptions (pages 32-34)

The discussion of actions with uncertain benefits and assumptions was comprehensive and detailed. However, this needs to be expanded to explain the major factors that affect juvenile and adult survival that are not explicitly represented in the model. Essentially, these factors are “black-boxed” in the survival models based on WTT, spill, and seasonal effects. This assumes that the large number of mortality factors inherently are captured in the past data and their relationships will not change in the future. The lack of explicit representation of environmental factors such as temperature should be noted. The consequences of the interaction of temperature and WTT could produce substantially different results for different management alternatives that are not represented in this comparison. Increases in WTT during years of higher river temperatures would likely reduce SARs much more than the current models indicate. In addition, the effects of other factors—such as marine mammal predation, avian and fish predation, commercial, sport, and Tribal harvest—are significant, and their representation (explicitly or implicitly) in the models is unclear. Again, the intersection of the environmental, harvest, and predation factors could create conditions where quasi-extinction thresholds would be reached. Perhaps a ranked list of features of the model and sensitivity of the results to changes in these features is needed.

a. Powerhouse Surface Passage Structures (pages 32-34)

No additional specific comments.

b. High-Capacity (i.e., “Fish-Friendly”) Turbines (page 34)

No additional specific comments.

4. Preferred Alternative Considerations (pages 35-36)

This is a potentially important section and should have more prominence. If the spill response is non-linear, then the difference between daily average and hourly data will bias the results for the PA (and to a lesser extent, the NAA alternative that also has day/night spill differences in some time periods). This is mentioned in the Abstract (final sentence) but should be noted in the results (especially PITPH section) as well. If PITPH is the only way spill enters the models, then Figure 2.19 suggests that the relationships are not very non-linear, so it may be a small effect. This warrants more discussion, including a better evaluation of the magnitude of the effect.

V. Editorial Comments

All pages. The CSS Grande Ronde Life Cycle Model is referred to in different ways in Chapter 2. The terms “CSS Grande Ronde Life Cycle Model”, “CSS life cycle model”, “Grande Ronde life cycle model”, “life cycle model” appear to be used interchangeably. The ISAB suggests a common term be used (e.g., consider using CSS-GR for this model). Similarly, make sure that the “CSS Cohort-Specific model” is consistently named throughout the report (e.g., consider using CSS-CS for this model).

All pages. TAFS recommends that the term “Julian Day” not be used for “day of the year” (Wilimovsky 1990).

p. 6. The text states that “we set the WTT value to 3 days” for the example, but Figure 2.1 shows WTT = 0, 5, 10, …. Please clarify.

P.7. Equation for log(FTT) for Chinook salmon is missing subscripts on the Day^2 term.

p.9. (and elsewhere). Not all terms are explicitly defined (e.g., $S_{R,i,y}$, $S_{O,i,y}$).

p.10. The report states “The SAR was calculated as the number of adults detected at Bonneville Dam divided by the number of smolts detected at Lower Granite Dam.” Presumably this is the number of adults detected at Bonneville Dam over all returning ages **of those** smolts in a brood year detected at LGR, rather than all smolts detected and all adults returning.

p.10. The ichthyoplankton index has a reference, but it would be helpful if a brief description were included.

p.11. It is unclear how the SAR estimates are derived from the model equations. Is $SAR_{i,y}$ identical to $p_{i,y}$? If not, how is $SAR_{i,y}$ calculated? Perhaps a table of variables, units, and definitions, would be helpful?

p.12. The paragraphs under “Influences of River and Ocean Variables on Life-Stage-Specific Survival” should be moved to the Results section.

Figures 2.8-2.17: Can these multiple graphs be combined? They are very similar. For example, plot the relative performance of all indices joined by a line to show how the different indicators all move in the same direction across alternatives?

Figures 2.8-2.17. Keep the order of the X-axis the same across figures. It is better to keep the order on the x-axis the same even if trends get reversed.

p.32. “While these data are not directly comparable to the aforementioned acoustic studies…” The authors did not specify what studies were acoustic. Adams & Rondorf (2007) presented data and analyses that were based on radio telemetry rather than acoustic monitoring.

p.34. “These studies indicate the increases in survival developed from bead [head] strike studies and physical modelling efforts are overstated.”

p.35. The reference to Figure 2.18 should be to Figure 2.19.

p.35. “This results in an overestimate of the benefits of the PA in the CSS analyses, because documented fish behavior shows that powerhouses are more efficient at catching fish at night than during daytime hours.” This statement needs citations to support the assertion.

p. 38. the citation for Petrosky and Schaller (2010) in the Chapter is not complete, as the journal was omitted. It should be:

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 19:520-536.

VI. ISAB References

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Wilimovsky, N. J. 1990. Misuses of the term "Julian Day." Transactions of the American Fisheries Society 119:162.

VII. Appendix: Supplemental Review Material

In addition to reviewing Chapter 2, the ISAB considered the following material for context:

- Detailed documentation of life cycle model methods are included in Chapter 2 of each of the 2014, 2015, 2016, and 2017 CSS Annual Reports (McCann et al. 2014, McCann et al. 2015, McCann et al. 2016, and McCann et al. 2017). The ISAB's reviews of those draft CSS annual reports are available through the following links: ISAB 2014-5, ISAB 2015-2, ISAB 2016-2, ISAB 2017-2.

- Detailed model documentation and model coefficients for the CSS cohort-specific models are included in "Documentation of Experimental Spill Management: Models, Hypothesis, Study Design and response to the ISAB," submitted to the ISAB on May 12, 2017 (CSSOC 2017) (ISAB related review: ISAB 2014-2).

- *Final* 2019 CSS Annual Report and the ISAB's review of the *draft* report (ISAB 2019-2).

- Chapter 4.a. Integrated population model of the Grande Ronde Basin and the ISAB's review, ISAB 2017-1, page 49.