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FOR THE NORTHWEST POWER AND CONSERVATION COUNCIL, COLUMBIA RIVER BASIN INDIAN TRIBES, AND NATIONAL MARINE FISHERIES SERVICE

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Review of the Comparative Survival Study (CSS) Draft 2023 Annual Report

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ISAB Review of the Comparative Survival Study (CSS) Draft 2023 Annual Report

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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 draft annual reports for the Comparative Survival Study (CSS). The ISAB has reviewed these reports annually beginning thirteen years ago with the evaluation of the CSS's draft 2010 Annual Report, and most recently the draft 2022 Annual Report.[1] This ISAB review of the *2023 Draft CSS Annual Report: Comparative Survival Study of PIT-tagged Spring/Summer/Fall Chinook, Summer Steelhead, and Sockeye* is thus the ISAB's fourteenth review of CSS annual reports.

II. Summary

This ISAB review begins with an overview of the latest report's findings (this section), which is followed by suggested topics for further CSS review (Section III). The review then provides general comments and editorial comments on each chapter of the draft 2023 CSS Annual Report (Section IV).

The annual CSS report is a mature product, typically including updates of analyses using the latest year of data and expansion of analyses when data are sufficient. Many of the methods have been reviewed in previous ISAB reports and now only receive a confirmatory examination. However, as more data are acquired some new patterns may emerge. The passing years may also bring scientific advances and perspectives, leading to new conclusions, and these are now the primary focus of our reviews. The ISAB appreciates the CSS's detailed responses to suggestions provided in previous reviews (e.g., CSS 2022 Annual Report, Appendix H), and we do not expect the CSS to necessarily respond immediately to new requests for further analyses.

The Fish Passage Center has developed a valuable long-term database on the hydrological performance of the hydrosystem and its effects on salmon and steelhead survival during their seaward migration as juveniles, at sea based on detections of tagged smolts and returning

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[1] 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, ISAB 2019-2, review of Chapter 2 of the 2019 Annual Report (ISAB 2020-1), ISAB 2020-2, ISAB 2021-5, and ISAB 2022-1.

adults (e.g., smolt-to-adult-return: SAR), and during their upstream migration as returning adults. The CSS reports since 1998 summarize the trends and provide analyses of the effects of the hydrosystem on salmon, steelhead, and other species in the Columbia River Basin. ISAB reviews from 2010 to the present have critically evaluated the analyses in the CSS reports and made suggestions for improved methods and interpretations.

In the following section of the Summary, the ISAB identifies major findings and regional issues that warrant attention and potential decisions and actions.

Effects of Bypass Actions and Spill Management

Analysis of juvenile bypass systems and spill management strategies to reduce mortality of juvenile salmon and steelhead and to increase the return of adults has been a major focus of CSS studies in recent years. The CSS has concluded that bypass systems result in hydrosystem-related delayed mortality (i.e., below Bonneville Dam, in the estuary, and early marine period). The CSS now has enough raw data available to refine their analyses and create summaries indicating the scale and longitudinal pattern of impacts of bypass events.

The study of the Effects of Juvenile Bypass Systems on Smolt to Adult Return Rates in Chapter 7 assumes that almost all fish not detected in the bypass systems passed through the spillways, and only a small proportion passed through the turbines. Estimates used for Fish Guidance Efficiency (FGE) in the analyses now have a wider range than earlier studies (Chapter 8). Is the assumption of constant FGE valid? It seems likely that conditions promoting bypass experiences would also promote turbine passage, as the two are related through FGE. This means that fish with more bypass experiences may also have more high-risk turbine experiences (up to a point), due to conditions favoring powerhouse passage, which may affect the interpretation of the results. For dams with PIT-tag detectors, the number of powerhouse passage experiences (PITPH) index will strongly depend on the estimates of FGE. The estimates of FGE are based on data collected two decades ago, and there is an opportunity to revisit the estimates considering more data and changes in the dam structures.

The 2022 and draft 2023 CSS Reports (Chapter 2) evaluated the initial responses of juvenile Fish Travel Times (FTT) and survival to the Flex Spill Agreement and the Preferred Alternative for the 2019 BiOp. The CSS will assess smolt-to-adult return (SAR) and the ratio of returns of fish transported to in-river migrants (“transport SAR divided by in-river SAR” p. 102, referred to as TIR). The analyses indicate that increased survival likely is related to increased spill. While these trends are consistent with the intended benefits to juvenile salmonids, models indicate that they are not sufficient to even meet the lower targets for SARs of the Columbia River Basin Partnership. Based on initial results, additional actions or other changes in conditions would be necessary to achieve SARs of 2% or greater. This ongoing study will provide critical information about flow management strategies in the Columbia River Basin.

Several aspects of the analyses and interpretation could be strengthened. The application of the covariance models to evaluate the Fish Travel Time model is reasonable, though it is not clear if explaining 40-50% of variation in juvenile survival for Chinook makes it a robust model. The CSS should explain what minimum proportion of the variation explained in these datasets should be expected for model performance, and discuss the uncertainties involved in using the results in management decisions.

The study of spill management in Chapter 2 recommends that future analyses should evaluate the impact of daily load following on juvenile fish survival and travel time. The ISAB concurs and recommends that the CSS should include this new recommendation in the Conclusions section. The CSS report makes very clear and compelling recommendations regarding the need for more spill, which they have recommended in past years. In its present form, the analyses show promise but could be improved in the "learning" part of Adaptive Management. We encourage the CSS to continue and expand its efforts to highlight the most recent body of evidence related to these issues for regional decision makers and researchers.

Smolt-to-Adult Returns (SARs) and Survival of Upstream Migrants

The SAR analysis provides important long-term data for the Columbia River Basin and management of the hydrosystem. Trends in SARs in the Columbia River Basin are a major concern regionally (Ford 2022). SARs for all hatchery and wild stocks of spring Chinook, fall Chinook, steelhead, and sockeye in the upper Columbia and Snake rivers are below the Northwest Power and Conservation Council's 2% minimum SAR objectives (Chapter 4). Only wild spring Chinook salmon and wild steelhead from the mid-Columbia (i.e., farther downriver) meet the minimum objectives in most years.

In addition to concerns about SARs, from 2008 to 2022, an average of 74% of adult Snake River Chinook salmon migrating upstream past Bonneville Dam survived to Lower Granite Dam, but in the warm year of 2015, only 52% of the adults survived from Bonneville to Lower Granite (Chapter 5). The frequency of warm years is likely to increase in the future, and the survival of adults may decrease more than recent averages illustrate. The collective ongoing poor survival of Columbia River salmon and steelhead warrants a comprehensive assessment of the long-term consequences of these trends and consideration of likely scenarios of climate warming.

Uncertainties and Risk Associated with Breaching of the Lower Snake River Dams

One of the major issues explored in the Columbia River System Operations Environmental Impact Statement (CRSO-EIS) and Biological Opinion in recent years has been the potential breaching of the four lower Snake River dams to increase the survival of juvenile and adult salmon and steelhead. CSS annual reports (2017, 2019, 2021), a recent peer-reviewed paper (Storch et al. 2022), and a NOAA Fisheries salmon and steelhead rebuilding report (NMFS 2022) have concluded that removal of the Snake River dams would have substantial benefits for salmon and steelhead. The CSS analyses have projected that substantial increases in SARs are only likely if the lower Snake River dams are breached and spill is maximized at the lower Columbia River dams. However, the causes of the declines in abundance of many Chinook salmon populations along the coast continue to be a topic of scientific debate (e.g., Atlas et al. 2023), as are the actions to reverse those declines (Welch et al. 2020, ISAB 2021-3). While scientists are weighing the effect of breaching on survival, major policy discussions are ongoing and influenced by these studies, so their importance cannot be overemphasized.

The premise of Chapter 6 in the 2023 CSS Draft Report was to examine the potential uncertainties and risks associated with dam breaching for salmon populations. The analysis was focused on some isolated effects of dam breaching and not any broader changes to the ecosystem or hydrosystem operations that would be expected to occur with dam breaching. Increases in juvenile freshwater survival, early ocean survival, and SARs were projected across management alternatives that included breaching the lower Snake River dams and increased spill levels at the remaining dams on the Columbia River. The Preferred Alternative of the CRSO, which did not include breaching, did not meet the minimum SARs objectives of 2%. The Executive Summary of the 2023 CSS Draft Report states:

> *"Analyses in this chapter shows [sic] that under poor ocean and flow conditions associated with climate change, the PA will not stop population decline, whereas the breach alternative is predicted to maintain SARs above 1% and avoid population decline. These analyses indicate that failing to implement the breach alternative presents the greatest risk to future population abundance and avoidance of extinction."* p. xxvii

The CSS reports since 2017 have modeled and assessed the projected responses of salmon and steelhead under various management scenarios, including No Action, the Preferred Alternative of the CRSO-EIS, the Flex Spill Agreement, and the breaching of the four lower Snake River dams. While the results of the CSS analysis are critically important, the ISAB considers the quoted statement that depends on the modeling results to be inadequately qualified and caveated.

The analyses of dam removal in the Snake River reported in Chapter 6 substantially underestimate the complexities and resulting uncertainties associated with dam breaching. A core assumption is that the system will immediately return to a state simply represented by changed model parameter values for water transit time (WTT) and PITPH and that other factors affecting SARs will remain the same. The ecological processes at play after dam breaching cannot be reduced to water travel time and exposure of smolts to structures. While general hypotheses about ecological recovery are well established and supported by case studies of moderate-sized or small dams (Bellmore et al. 2019), removal of large dams can have uncertain effects, particularly for ecosystem processes (vegetation recovery, predator and prey communities, water quality, primary production, straying and life history diversity, and others) in the years immediately following dam breaching. Further, variables representing the ecosystem condition may not be representative of a likely future state that involves greater climatic and ecological variability independent of the dam breaching. Consequently, quantitative predictions of the environmental conditions and effects on salmon will be uncertain.

Separate from whether or not the premise of dam breaching has been fully evaluated in Chapter 6, there were additional challenges with the formulation and assumptions behind the models. The current structure of the models and the assumed relationships may not accurately predict SARs and abundances after breaching. Predictions of SARs after breaching using models configured with and completely informed by data collected when dams are present must be viewed cautiously. In modeling terms, this means that one may not be able to realistically simulate breaching by simply substituting values for certain parameters in relationships based on data with the dams present. While it is not possible to exactly represent the physical and ecological conditions that will occur after dam breaching, the inherent uncertainty and implications for risk assessment should be clearly stated. The report should thoroughly describe the limitations of representing these changes by creating scenarios to represent the physical and ecological changes and explain how those conceptual representations are related to risk and uncertainty.

Further, within a narrow range of hydrological and climate conditions comparable to those in recent years, the model results may be more predictive, but outside the range of recent records, the model results have much greater (and unquantified) uncertainty. There is an implicit assumption that the observed record of conditions in the river and ocean adequately describes the likely range of future scenarios. Is this justified? A discussion on the consequences of climate change and its effect on model realism and how historical conditions can be used to represent future conditions is warranted. We encourage the CSS to explore approaches to incorporate the influence of climate-related and density dependent factors on the total life-cycle survival of Columbia salmon in future reports.

Given the exceptional importance and interest around the potential for breaching of the lower Snake River dams, the representation of breaching in the models requires further evaluation, and the limits of what is represented need to be clearly stated alongside the results. The CSS analysis alone does not constitute a comprehensive analysis of breaching, but it is an important component of a broader analysis that would also have to incorporate the other important geomorphological, ecological, and management changes that would occur with breaching. The ISAB supports and encourages these types of analyses by the CSS and others to examine critical and challenging topics such as the uncertainties and risks associated with the ecological responses to breaching the lower Snake River Dams. Despite some of the concerns raised through this review, the use of existing modeling results to explore this new application is an excellent example of the CSS looking forward and exploring important and challenging questions with their analyses.

Importance of Information in CSS Reports

The ISAB strongly emphasizes the importance of the CSS reports for effectively monitoring and evaluating salmon co-management and hydrosystem operation. There may be a tendency to consider the annual CSS reports to be just "more of the same" each year. With more than 25 years of data, the conclusions reached are now extremely valuable because the uncertainties in the results can be well estimated and outlier years can be identified. Moreover, annual data collection and analysis updating serves a potential "sentinel" role permitting a standardized detection of changes within a contemporary period. The physical (e.g., PIT tag detection arrays) and human capacity and expertise added over the 25 years are extremely valuable and the maximum benefits of these efforts should be obtained.

Long-term records of fish abundance and environmental conditions are extremely difficult and expensive to develop. The survival of salmon and steelhead during parts of their life cycle is affected by the hydrosystem, and these data are essential for the Fish and Wildlife Program. This is particularly critical when assessing years with extreme conditions, such as low flows, high temperatures, or other atypical seasonal patterns. These "edge of the distribution" cases may occur more often under climate change, and a long-time series is needed to capture enough of these uncommon conditions to make reliable assessments.

Editorial Comment

The 2023 CSS Annual Report contains numerous acronyms and technical terms, many of which are not defined or explained. The CSS reports since 2017 do not include a glossary of acronyms and technical terms. For example, several chapters use two acronyms for Bonneville Dam (BON and BOA) and McNary Dam (MCN and MCA). The 2010 CSS Report contained a useful glossary and defined BON as Bonneville Dam and BOA as Bonneville Dam adult fish ladder, with similar explanations for MCN and MCA. The CSS reports from 2010 through 2017 contained a helpful Glossary of Terms, which included acronyms and definitions of technical terms. We recommend that the previous Glossary of Terms be updated and included in the 2023 Report and future reports. Each acronym needs to be defined when it is first used, and in general acronyms should be used only when needed, as they are inevitably less clear than the longer but complete name for the organization, dam, report, or process.

We continue to encourage the CSS to include in future reports an “overview” chart showing when various chapters are added and deleted. Such a chart would help the reader understand the history of chapters, know if a chapter is no longer being updated because the issues are settled, or if a chapter’s number has changed because other chapters have been added or deleted.

III. Suggested Topics for Further Review

Since 2011, the ISAB has suggested topics that warrant further CSS or regional review, and they are listed here in Section V as an appendix. The latest CSS report incorporates many of our past suggestions, and the ISAB greatly appreciates the CSS’s effort to respond to our past queries.

1. Building upon the 2019 model comparison, Basin Partnership 2022, and Chapters 2 and 6 in the 2023 Report, continued analysis of the benefits, uncertainties, and risks of breaching the lower Snake River dams is warranted. The breaching scenario assessment reported in Chapter 6 pushes (and may exceed) the capabilities of the present models to simulate the complex ecological responses expected after breaching, and we suggest that a more comprehensive effort to predict these responses is warranted. There is much room for more detailed and inclusive evaluation of the sources of uncertainty (e.g., implementation uncertainty, realism of existing models for no-dam conditions) under present-day and plausible future (climate change) environmental conditions. The framework started in Chapter 6 on uncertainty and risk has much room for further work, and its use within a decision-analysis framework should be rigorously pursued using established available approaches. The ISAB views this as a critical effort going forward, as the issues to be addressed likely involve changes to models, adding sources of uncertainty not previously considered, and using modified models to perform new simulations.

2. With the long-term data available and changes in some of the dams, additional dam-specific information is available to include in the analyses. First, what are the most important differences among dams that affect passage and survival? This approach would benefit from a model that looks at survival considering different configurations of the various dams during a fixed period of years when the dams were not changed substantially. Second, did the modification of dams increase survival? For example, the

estimates of FGE are now 20 years old and they might be revisited in light of the new data and changes in the dam structures.

3. The CSS could consider how to incorporate the influence of climate-related and density dependent factors on the marine survival of Columbia River salmon in future reports.

4. Given the value of the time series for comparative analyses, a useful addition would be a recurring chapter that synthesizes similarities and differences between hatchery and wild fish in SARs, FTTs, PITPH, and other response variables. The synthesis could draw from the results for hatchery and wild fish from throughout the report. It would be very information to assess the effects of body size and seasonal timing at initial tagging on in-river survival and return. There is good evidence that survival varies with smolt size and timing, which often co-vary, and often differ between fish of wild and hatchery origin. With the long-term data, the CSS could explore the interplay between origin, size, and timing on survival of salmon and steelhead.

In ISAB 2022-1, we recommended the following two topics (*italicized*) for future reports. After each recommendation, we summarize the status of the work to address them:

1. *Given that the Council's SAR targets are generally not being met, this could imply that the populations are more or less destined for functional extirpation sometime in the future. Chapter 6 [of the 2022 CSS Annual Report] identifies SARs needed to attain the higher goals of the Columbia Basin Partnership, and their analysis indicates that "higher SARs than seen in most recent years are needed to attain and maintain higher natural-origin abundances, rather than mere persistence." Factors related to attaining the recommended SARs need to be explained with respect to the suite of actions implemented under the Fish and Wildlife Program.*

The response from the CSS was, "Evaluating the Fish and Wildlife Program is outside the scope of the CSS." There may be some confusion because our language was not specific enough. We did not intend the CSS to evaluate the entire Fish and Wildlife Program. We were referring to major management actions associated with the hydrosystem in the Columbia River. Chapters in the 2023 Draft Report discuss responses to such management actions (e.g., spill, hatchery releases, flow modification, passage alternatives, dam removal).

2. *Although the CSS is an empirical modeling effort, can the FPC and CSS Oversight Committee expand upon previous analyses to identify further evaluation and data needed to address the "breaching" proposals for the four lower Snake River dams more fully? This has been a critical regional issue for more than two decades and is currently being discussed in state and federal documents (NMFS 2020, Columbia Basin Bulletin 2020, 2021, 2022, NMFS 2022; also see Storch et al. 2022). Insights provided by the*

> *depth and scope of CSS analyses over the last 27 years will be important in these regional discussions. Chapter 2 of the 2019 CSS Annual Report included breaching as one of the EIS alternatives considered and found that it resulted in the highest SARs of the alternatives examined. Is breaching an all or nothing proposition, or can significant gains be expected with fewer dams being breached?*

The CSS provided a thoughtful and informative response to this comment. They indicated that the Action Agencies did not select the two-dam removal option for the CRSO-EIS, but ODFW, the Yakama Nation, and Nez Perce Tribe requested an analysis of that option. The CSS conducted an analysis (https://www.fpc.org/documents/memos/50-22.pdf) and concluded that "a 2-dam breach option will not be adequate to avoid continuing salmon and steelhead population decline, particularly under climate change conditions occurring now and predicted for the future." The outcomes resembled the MO4 125% spill alternative of the CRSO-EIS.

The CSS pointed out their funding limitations and the requirement for the CSS Oversight Committee to prioritize analytical work. The ISAB recognizes these very real constraints, but we have a responsibility to suggest research directions and new analytical approaches. We do not expect the CSS to adopt our suggestions, hence we call them Suggested Topics for Further Review rather than ISAB Recommendations for the CSS.

IV. Comments on New or Updated Analyses in the Draft CSS 2023 Annual Report by Chapter

IV.A. Comments on the Executive Summary and Chapter 1. Introduction

The 2023 CSS Report includes three new chapters. Chapter 6 describes the risk and uncertainty in adaptive management related to recovery of Snake River salmon and steelhead, which may be useful for the ISRP's review of the steelhead program of the Lower Snake River Compensation Program in 2024. Chapter 7 provides an analysis of inferred delayed mortality resulting from bypass/collection system passages in the hydrosystem, updating analyses presented in 2010 and 2016 CSS Annual Reports. Chapter 8 describes estimates of powerhouse passage proportions at hydropower dams, updating estimates included in the 2015 CSS Report.

The 2023 CSS Report does not include the chapter of the 2022 CSS Report that describes the history of the hydrosystem, development of management of spill to benefit salmon and steelhead, and the formation and evolution of the CSS study to inform state, federal, and tribal fisheries managers. This was a valuable foundation for readers of the CSS reports, especially for those without extensive prior knowledge of the system, and the ISAB encourages the CSS to include it in the 2023 Report and all future CSS reports.

The ISAB recommends the authors of the CSS include a paragraph or two that explains plainly what the CSS is and is not designed to address. Such a description may help new readers and those with less depth and familiarity to understand the focus of the report and analyses. The CSS is a study of the survival impacts of the hydrosystem engineering and operation. The main parameters that are examined are WTT, PITPH, total dissolved gas (TDG), spill, and transportation effects, and therefore the CSS and FPC activities focus specifically on these. Broader biological and ecological issues mechanisms (e.g., predation, trophic cascade, disease, ocean ecology, etc.) are potential factors in the analyses but are not primary considerations. While the FPC and tagging data may be used to address biological questions related to these factors, their primary purpose is to monitor survival through the hydrosystem at seaward migrating smolt and returning adult stages of the salmon life cycle. The CSS authors could add a paragraph or two about their mission for background.

IV.B. Comments on Chapter 2. Adaptive Management Evaluation of Changes in Hydrosystem Operations on Salmon and Steelhead Survival and Travel Time

This chapter reports the second analysis of the results of the Flex Spill experiment. During 1998-2016, on average approximately 40% of the discharge at the lower Snake River dams and the Columbia River dams was spilled. This increased to 66% at the lower Snake River dams and 55% at the Columbia River dams under the Preferred Alternative in 2020-2022. Powerhouse passage index (PITPH), the expected number of powerhouses encountered by migrating juveniles, decreased from 3.3 (1998-2006) to 0.6 (2020-2022). Because not all fish have returned from the cohorts affected (2019-2022 brood years), only the Fish Transit Time (FTT) and in-river survival of the affected cohorts are examined. Analyses of early ocean survival, smolt-to-adult return (SAR), and Transport: In-River ratio (TIR) will be presented after all fish in each cohort have returned. The data will be particularly important for evaluating the effectiveness of the spill strategy and continued analysis as presented in this chapter is critical.

The CSS analysis (a) fits a model for the FTT and in-river survival using the 1998-2018 (pre-experimental conditions), (b) uses the fitted model to predict the FTT and in-river survival using the conditions present during the Flex Spill experiment, and (c) compares the FTT and in-river survival from the Flex Spill years estimated from field data with the model-predicted values. The analysis indicates that the spill regime for 2020-2022 reduced fish travel time by 0.7 days for Chinook salmon and 1.4 days for steelhead compared to travel times for spill during 2007-

2019. Low flow in 2021 and increased reservoir elevations in John Day Dam (2020-2022) and lower Snake River dams (2021, 2022) adversely affected FTT and possibly survival, thus the spill regime could have had greater benefits under normal flows and reservoir elevations. The results confirm prior analyses indicating that the primary factors influencing juvenile fish travel time and juvenile survival are ordinal day, water transit time, and the number of powerhouses experienced during the migration. Analyses of data from 1998-2022 indicated no association between the observed total dissolved gas (TDG) over a range of maximum concentrations from 117% to 132% and juvenile mortality rates.

The application of the covariance models to evaluate the Fish Travel Time model is a reasonable approach, although it is not clear if explaining 40-50% of variation in juvenile survival for Chinook makes it a robust model. What minimum proportion of the variation explained in these datasets should be expected for model performance and application in management decisions?

The chapter points out that the Flex Spill Agreement would result in greater survival than the CRSO-EIS Preferred Alternative. The flex spill increases daily load following at Columbia/Snake River hydroelectric projects, particularly in low flow periods. It provides an estimate that the flex spill reduced powerhouse flow by 60% during the spring migration compared to an 18% reduction under the Preferred Alternative. The text recommends that future analyses should evaluate the impact of daily load following on juvenile fish survival and travel time. The ISAB concurs and urges the CSS to include this new recommendation in the Conclusions section.

The CSS report makes very clear and compelling recommendations regarding the need for more spill, which they have recommended in the past. The premise of the chapter is excellent and the analyses are important for evaluating the effectiveness of the operation plan for spill under the Flex Spill Agreement. In the future, adult salmon and steelhead return will allow the CSS to assess the consequences of spill operations on SARs. The ISAB encourages the CSS to continue such analyses. Conducting analyses as soon as the data become available is recommended, as that will allow the analysts to improve the process in advance of further data. In its present form, the analyses show promise but could be improved in the “learning” part of Adaptive Management. We encourage the CSS to continue and expand its efforts to highlight the most recent body of evidence related to these issues for regional decision makers and researchers.

Minor Comments

Our previous review suggested some additional plots to help the reader assess the performance of the model. The CSS team tried to follow this suggestion, but they judged the plots too “busy” and hard to read. Consequently, the report provides coefficients of determination. Figure 2.4 is an example of the new plots (which is good), but it hard to tell, for example, if high values tend to be underpredicted consistently. The ISAB suggests making an additional simple plot of observed vs expected values, pooled across all years and cohorts, colored by the out-of-sample indicator. This should show a 1-1 average relationship (high coefficient of determination as shown in Table 2.1) but could help identify the types of data values for which the models have difficulty predicting.

Editorial Comments

It would help to explain what is meant by “spill level” and “load following” at first usage.

IV.C. Comments on Chapter 3. Effects of the In-river Environment on Juvenile Travel Time, Instantaneous Mortality Rates and Survival

This chapter is an update of the previous data for Chinook (subyearling and yearling), sockeye, and steelhead smolts in three reaches of the Columbia and Snake rivers: 1) the upper Columbia River migration corridor, from Rocky Reach Dam (RRE) to McNary Dam (MCN), 2) the Snake River migration corridor, from Lower Granite Dam (LGR) to McNary Dam, and the common migration corridor from McNary Dam to Bonneville Dam (BON). Most of the text is identical to the four previous CSS Annual Reports. In the 2023 Draft CSS Report, analyses of the effect of TDG on instantaneous mortality rates are restricted to years when the average TDG concentrations exceeded 115% because reduction or elimination of spill in years with both low survival and low TDG conditions can produce apparent positive relationship between TDG and survival. Overall, the results are sensible.

In 2020 and 2021, the ISAB raised questions about the unusually low survival probabilities reported for several cohorts of subyearling Chinook in the MCN-BON reach, which is a critical reach for understanding the survival of salmon and steelhead in the Columbia River and evaluating the effects of the hydrosystem. The CSS found that low and variable survival probabilities were associated with cohorts having low sample sizes. The 2022 report stated that several actions were needed to estimate survival in the future of subyearling Chinook in the MCN-BON reach: 1) increased number of subyearlings that are PIT tagged upstream, 2) increased detection efficiency at McNary Dam, 3) extended PIT-trawl sampling below Bonneville Dam through the end of August, or 4) a combination of these actions. Have BPA and the Action Agencies responded to this recommendation?

The 2023 CSS Draft Report noted that “For wild yearling Chinook salmon in the LGR-MCN reach, we observed that instantaneous mortality rates during 2020-2022 were higher compared to the observations in previous years.” The authors again identify small sample sizes as a possible cause for the higher and more variable estimates and add that “daily load following operations, with daily periods of high and low powerhouse flow in 2020-2022, may be resulting in increased mortality rates." The CSS plans to further investigate this trend in the LGR-MCN reach. We note that a similar pattern seems apparent for hatchery-origin subyearling Chinook (H CH0, Fig. 3.5), but it is difficult to tell from the figure. Has the CSS assessed the trends in mortality rates of these fish? Additional analysis and future assessment may be warranted.

One of the reasons for low sample sizes is that the PIT-tag trawl sampling below Bonneville Dam was shortened from the end of August to mid-June. This period later in the summer (i.e., through August) is associated with lower juvenile survival because of increased temperatures and longer fish travel times. Exclusion of data from late June through August inflates annual survival estimates and omits critical information for hydrosystem management. In response to the ISAB's recommendations, the CSS added a conclusion related to this issue in this chapter of the 2022 CSS Annual Report, and the Executive Summary of the 2022 CSS Report stated:

> *In response to recommendations from the ISAB regarding fall chinook survival, analyses in Chapter 3, show that detection of PIT tags below Bonneville Dam is critical to the CSS evaluation of hydrosystem operations. Funding for and operation of the PIT tag trawl has been limited in recent years. These analyses indicate that adequate funding of the operation of the PIT tag trawl below Bonneville, through the spring and summer migration is critical to estimation of juvenile fall chinook survival.*

This statement is included in the Discussion of Chapter 3 but has been removed from the Conclusions at the end of the chapter. Have any management processes and decisions addressed this critical need? The ISAB highlights the need for full season PIT-tag trawl data and recommends that the CSS add the conclusion in the 2023 CSS Annual Report.

Minor Comments

Details of the PITPH index are not provided in Chapter 3. The methodology for developing the PITPH index was described first on page 20 of Chapter 1 and then again in Chapter 8. It would be helpful to cite those sections of the report that provide additional information about the PITPH index. The text states that CSS developed the PITPH building on the results of McCann et al. 2015 (Appendix J). Did the method used in 2023 differ from the method described in Appendix J of the 2015 CSS Annual Report? Was the same methodology used in Chapters 3 and 8, which used 7-d and 14-d windows, respectively? In general, it is more helpful to briefly explain methods, past results, and so forth rather than referring the reader to other reports.

In the section describing the use of random effects in the mixed effects model, please correct the notation and clarify the explanation of equation 3.5. The narrative appears to indicate that random intercepts and slopes (with ordinal day) were specified for year (i.e., the effect of ordinal day on FTT is allowed to vary among years). However, the effect of ordinal day is allowed to vary among cohorts, which have different ordinal days (assumed to be $X\_\{1,y,j\}$).

The text states that b_y represents the yearly random intercept, but unfortunately uses b_j to represent the cohort random effect. We assume that each cohort has a unique label so that cohort 1 in year 1 has a different label than cohort 1 in year 2, etc., so there is no need to specify "nesting" for the cohort number within a year (it will be "automatic").

Confusing notation is also used when specifying the random effects (e.g., σ^2_Y is the YEARLY random variance and not a separate σ^2 for each year). The same is true for use of σ^2_j to represent a COHORT variance (one value) and not a separate variance for each cohort. Better would be σ^2_Y and σ^2_j so it is explicit that these represent a single term.

The marginal and conditional r^2 values were interesting. Please consider adding discussion about the variance attributed to the random effects, as this could have some bearing on future studies. In the Methods, please clarify how the different random effect structures were combined when evaluating models of S.

On page 63, the first paragraph first indicates that seven environmental variables were evaluated. Later in the paragraph it states that "In cases where all six variables were applicable, there were 64 possible model combinations of the predictor variables." The authors noted that the quadratic effect of ordinal day was limited to the yearling Chinook salmon fish travel time models. Presumably, the authors used six environmental variables for all species and age classes other than yearling Chinook salmon, for which they used seven. Please clarify the use of the environmental variables in the model.

Editorial Comments

p. 45-48. Estimated and predicted values are shown in Figures 2.5 to 2.7 of FTT and Z. Please clarify which model is used to generate the predictions (e.g., the top model?).

p. 66. Text refers to Table 3.3, but seems to mean Table 3.2; indeed, there is no Table 3.3.

IV.D. Comments on Chapter 4. Patterns in Annual Overall SARs

The analysis of overall SARs provides important long-term data for the Columbia River Basin and management of the hydrosystem. The ISAB has reviewed these data and analyses in the chapter in previous annual CSS reports. Most of the text, tables, figures, and Conclusions are identical to recent CSS Reports, updated with 2022 data. One consequence of this long history of reports is that important methodological information is not presented, relying on citations to previous work. Notably, it is difficult to discern exactly how the survival in the first year in the ocean “S.o1” was calculated (p. 104). This estimate is important as it is the most plausible period when delayed mortality might occur. The primary reference cited, Petrosky and Schaller (2010) itself refers to previous reports and estimates but seems to rely on Ricker’s (1976) estimate of 0.8 for annual survival after the first year at sea. Given the research since Ricker’s (1976) review, how valid is this assumption, and how sensitive are the outputs to the deviations from 0.8 that certainly occur? Moreover, Petrosky and Schaller (2010) refer to Wilson (2003), who used fixed age at maturity schedules. This assumption ignores the scientific evidence of variation and overall decline in age at maturity resulting from multiple factors at sea and freshwater. Future CSS reports might explain the assumptions and provide tables of key input values such as annual marine mortality rate and age structure.

SARs for wild Chinook and wild steelhead for the Tucannon were recently included in CSS reports. Estimates ranged from 0.0% to 0. 86% for wild spring Chinook from 2015-2020 and 0.12% to 1.5% for wild steelhead in the Tucannon River. Adult returns to the Tucannon River in 2021 were too low to allow estimates of SARs. These results underscore concerns raised in the recent ISRP review of the Lower Snake River Compensation Program (ISRP 2023) about the status and management options for spring/summer Chinook salmon in the Tucannon River.

As the ISAB stated in previous reviews, the take-home messages in the Conclusions section are well crafted and useful for managers, but they are essentially identical to the past Conclusions and do not highlight any new results, insights, issues, or concerns. Except for wild spring Chinook salmon and wild steelhead from the Mid-Columbia, SARs for all hatchery and wild stocks of spring Chinook, fall Chinook, steelhead, and sockeye in the upper Columbia and Snake rivers are below the NPCC’s 2% minimum SAR objectives. The long-term consequences of this collective ongoing poor survival of Columbia River salmon and steelhead warrant a major conclusion and recommendation to regional managers and decision makers. CSS models and NOAA life cycle models could be used to estimate the quasi-extinction probabilities for Columbia River stocks of salmon and steelhead for the next century under scenarios of both current conditions and future climate change. These data and their implications warrant an immediate comprehensive assessment across the species and stocks of the Columbia River, including consideration of the extent to which they reflect coast-wide processes vs. processes specific to the Columbia River basin.

The ISAB reviews of the 2021 and 2022 CSS Annual Reports suggested that more sophisticated analytical tools may be available and could strengthen the analyses. Current reports still include estimated correlation coefficients and use simple statistical models, albeit with intricate data manipulation. More sophisticated statistical tools might be able to characterize underlying patterns in the 20+ years of data, perhaps using time series approaches. There is an apparent long-term decline, but also some evidence of a potential periodicity in these data. Once these trends are accounted for, would a pattern in the residuals become more apparent? The CSS response to our review in the 2021 Annual Report indicated that they would consider this suggestion in the development of CSS analyses for future annual reports, but the CSS did not respond to our comment in the 2022 CSS Annual Report. How does the CSS plan to address the ISAB's suggestion in the near term?

Minor Comments

As noted in the Summary comments at the start of this review, the 2023 CSS Annual Report does not include a glossary and does not explain differences between the similar acronyms. For example, the 2010 CSS Report defined BON as Bonneville Dam and BOA as Bonneville Dam adult fish ladder. In this regard, "BON-Ad" or some similar modification that includes the whole acronym for the dam with an addition would be clearer than changing the acronym. The CSS report from 2010 through 2017 contained a helpful Glossary of Terms, which included acronyms and definitions of technical terms. The previous Glossary of Terms used in the CSS reports from 2010 to 2017 should be updated and included in the 2023 Report and future reports. The Columbia River basin is inherently complicated because of its physical geography, political boundaries, multiple levels of agencies, and history of development and management. Use of jargon and acronyms may be efficient for those intimately familiar with the system but can thwart efforts of newcomers to understand documents, so any reduction in jargon and clarification of acronyms will be helpful. This need, of course, extends far beyond the CSS.

The ISAB notes with appreciation that the CSS report (e.g., p. 105, Figure 4.1) indicates that jacks are included in returns for the purposes of estimating SARs. Inclusion/exclusion of jacks varies among reports by other entities in the basin, and many fail to indicate how this sometimes-large fraction of the males was handled. Wilson (2003, cited by Petrosky and Schaller 2010) only included females in the model, and they are much less variable in age-at-maturity compared to males. Some brief comment on the effect of models based only on females, including males but not jacks, or all adults might be useful in the future.

Chapter 4 cites many papers but does not provide references for them, as is done in other chapters. Indeed, a single set of references for the entire report would be more helpful than separate ones for each chapter.

Appendix B: Supporting Tables for Chapters 4 – Annual Overall SARs

There have been no major changes in Appendix B. Values for 2021 or 2022 have been added and overall averages or totals have been updated.

IV.E. Comments on Chapter 5. Upstream Migration Success

Chapter 5 pertains to upstream migration success (also referred to as “conversion”) of spring and fall Chinook salmon and steelhead given PIT tags above Lower Granite Dam as smolts that were detected as adults at Bonneville Dam, returning to the Snake River. It is updated from the previous CSS annual reports to include data for 2022. The contrast in inter-dam survival probabilities is small, so the results are not surprising. The ISAB has raised questions about the low survival from Bonneville to McNary in previous reviews. The CSS examined this more closely this year in this chapter and found that history of being transported as juveniles and temperature in this reach were associated with low survival probabilities across the stocks and species, as well as origin (hatchery-origin having lower survival than wild). Temperature has greater effects on survival once spring Chinook reach the Snake River reaches because fish experience higher temperatures at the upper extent of their migration. The CSS included degree days, a cumulative measure of temperature, in this year’s analyses. Degree days, which reflects both time and temperature, often was a better predictor of conversion than temperature. This is a useful addition to the modeling of adult migration success in the Columbia and Snake rivers.

The Results and Discussion sections clearly explain the results of the modeling of survival of spring, summer, and fall Chinook salmon and summer steelhead for the three major reaches of the Columbia and Snake rivers. The consistent importance of factors that reflect the cumulative effects of conditions the fish experience throughout their migration, such as history of transportation and degree days, demonstrates the need to consider the full life history of salmon and steelhead in management decisions.

Across all species, stocks, and reaches, history of being transported was the most common predictor of conversion and had the greatest effect (negative) on migration success.

The 2021 and 2022 ISAB reviews suggested that it would be useful to produce an overall model for Bonneville to Lower Granite dams with the same variables and compare it with the reach-specific model results to see where the largest effects occur and how these differ by species or run. The CSS disagreed, explaining:

> *“An overall reach model (BON-LGR) would remove context from the processes that influence survival in different sections of the river. ISAB has long encouraged finer-scale analyses to better link environmental conditions to individual fish experience. Aggregating over BON-LGR results in loss of fine-scale detail and management applicability. The reaches used reflect the reaches focused on by the fisheries co-managers. Specifically, survival and passage metrics from IHR-LGR are extensively used in management decisions by the fisheries co-managers,*

> *combining this reach with the lower river would conflate many of the metrics for this section."*

The ISAB appreciates the thoughtful response to our suggestion. To clarify, we did not intend to replace the reach-specific analyses but rather present an analysis for the total distance of the hydrosystem. An alternative that might meet the perspectives of both the CSS and ISAB would be to create a context for the reach-specific analyses by reporting the total survival or mortality for each stock and species from Bonneville Dam to Lower Granite Dam. While this information is available in other sources or documents, it would provide a spatial context for the overall migration success of salmon and steelhead. Additionally, the mortality rate per mile of river could be reported to give a clearer understanding of the overall mortality rate as a function of distance and a spatially normalized comparison of the three modeled reaches (see Minor Comments below).

Additionally, a more complete discussion of the mechanisms by which transportation of smolts could reduce the success of the fish migrating upstream years later as adults is warranted. It is plausible that stress experienced during downstream migration might affect early ocean survival (i.e., delayed mortality), but it seems unlikely that this would affect adults that survived to return to the river. Given the evidence that transportation and rearing can affect homing, and the recent studies on fall-back, delay, and straying, the report would benefit from interpretation of these patterns. In contrast, the discussion of the effects of temperature is more thorough (p. 199-200).

Minor Comments

As noted in the Summary comments at the start of this review, the 2023 CSS Annual Report does not include a glossary and does not explain differences between the similar acronyms. For example, the 2010 CSS Report defined MCN as McNary Dam and MCA as McNary Dam adult fish ladder. The CSS report from 2010 through 2017 contained a helpful Glossary of Terms, which included acronyms and definitions of technical terms. The previous Glossary of Terms used in the CSS reports from 2010 to 2017 should be updated and included in the 2023 Report and future reports.

A specific term that is confusing for most readers is “conversion" (e.g., top paragraph on p. 194). It would be helpful to define the term in the report for new readers of CSS reports. The 2017 CSS Report and many before it explained it as follows:

> *“Conversion rates represent adult losses net of harvest, e.g., a conversion rate of 0.5 means that 2 adults would need to return to the mouth of the Columbia so that 1 adult could make it to the spawning ground. Those losses represent all factors not related to harvest, including predation loss, pre-spawn mortality,*

> *adult passage related mortality, and other causes. In recent years, conversion rates have been fairly high, and historically they were comparatively low because less passage infrastructure was in place."*

If the essence is "loss" or "migration success" then perhaps one such word might be used rather than "conversion" which is less intuitive.

In the review of the 2020 CSS Annual Report (ISAB 2020-2), the ISAB noted that use of the term "rate" is incorrect. Conversion is not a rate (per unit time) but rather a simple probability. We recommend using the terms conversion probability or probability of migration success (or loss) rather than conversion rate or survival rate.

Given the apparent effect of temperature on survival (p. 176), is it assumed that dam removal would or would not affect temperature, and if so, how?

Distance is a variable in the models. It would be helpful if the Methods section indicated the lengths of the three reaches in the analyses—145.9 river miles for BON-MCN, 31.7 river miles for MCN-ICH, 97.8 river miles for ICH-LGR (data from DART website). For spring Chinook as an example, the mortalities for 2022 are 21% from BOA-MCN, 2% from MCN-ICH, and 5% from ICH-LGR. However, the mortality rates per mile are 0.14%/mi for BOA-MCN, 0.06%/mi for MCN-ICH, and 0.5%/mi for ICH-LGR. The mortality rates per mile in the upper two reaches are 2 to 3 times greater than the rate from BOA-MCN but are not 4 to 10 times greater as would be expected from the rates not adjusted for distance.

Editorial Comments

In general, it is more helpful to state the nature of relationships rather than merely the fact that there is one. For example, the last paragraph on p. 194 states that "hatchery or wild origin was highly predictive in the lower reach..." Why not just say that wild fish had higher or lower survival or detection, and indicate the magnitude of the difference? This issue is repeated in the Discussion section (p. 199) where it states that transportation was predictive. Why not state the relationships and magnitude of the effect?

It would be simple to provide information to help busy readers. For example, the first sentence in the Results section on p. 182 starts with survival of fall Chinook salmon but why not remind the reader that these are upstream migrating adults? Likewise, it would be clearer to say survival from a specific dam to the next rather than "between," which is ambiguous as to direction of movement.

p. 193. Replace the period with a comma after juvenile in the first sentence of the paragraph.

p. 194. Delete the comma after transport in the first sentence.

IV.F. Comments on Chapter 6. Exploring Uncertainty and Risk in Analyses of Recovery Potential for Snake River Salmon and Steelhead

One of the major issues explored in the CRSO EIS and Biological Opinion in recent years has been the potential breaching of the four lower Snake River dams to increase juvenile and adult salmon and steelhead survival. CSS annual reports (CSS 2017, CSS 2019, CSS 2021) have concluded that removal of the Snake River dams would have substantial benefits for rebuilding salmon and steelhead populations. The CSS analyses have projected that substantial increases in smolt-to-adult returns (SARs) above maintenance levels are only likely if the lower Snake River dams are breached and spill is maximized at the lower Columbia River dams. Chapter 6 in the 2023 CSS Draft Report examines some of the potential uncertainties associated with the assessment of the consequences of dam breaching (and other alternatives such as the Preferred Alternative) for salmon populations. Frequently, questions about uncertainty and potential risks are raised for such studies, and this analysis examines a narrow set of those uncertainties.

Chapter 6 is intended to assess the risk and uncertainty in analyses of the recovery potential for Snake River salmon and steelhead. The analysis used Cohort Models for SARs of spring/summer Chinook salmon and steelhead for the Snake River and the Grande Ronde Life-Cycle Model for spring/summer Chinook salmon in the Grande Ronde River. Five alternatives were evaluated, using the framework developed in McCann (2019), which was reviewed by the ISAB (ISAB 2019-2). The analyses used the 80-yr discharge dataset (1929-2008) developed by the Action Agencies for the CRSO EIS.

The analyses and results presented in this chapter were extracted from previous analyses, most recently using the modeling conducted for the CRSO EIS. What is new in this chapter is the use of the results specifically to assess **a limited set of *uncertainties and resulting risks*** associated with the responses to the alternatives, with a focus on the breaching and preferred alternatives. The use of earlier analyses, including even identical text from earlier reports being repeated in some places of Chapter 6, for a new (albeit related) purpose creates documentation challenges and technical issues. An illustration of challenges a reader faces with the documentation is that Chapter 6 refers the reader to four earlier CSS reports for details on the modeling. New technical issues arise because, while the modeling approach and model outputs reported are the same as earlier, the results are now being interpreted for a new question about uncertainty and risk across the alternatives. Use of prior results to answer a new question warrants a new review aimed specifically at evaluating the methods, assumptions, results, and interpretation in the new context.

While the methods and results reported in Chapter 6 were reviewed previously for the EIS and by the ISAB as part of earlier CSS Annual Reports, the ISAB provides a complete review below focused on how the modeling was used in this Chapter 6. Analogous to the challenges to documenting the methods and results when old results are reused, combining the old ISAB reviews (and CSS responses) and relating them to the new use of the modeling is also challenging. Therefore, to ensure clarity, the ISAB's review presented below is standalone and does not attempt to cross-reference to earlier reviews.

The ISAB supports and encourages these types of analyses to examine critical and challenging topics such as the uncertainties and risks associated with the ecological responses to breaching of lower Snake River Dams. Despite some of the concerns raised through this review, the use of existing modeling results to explore this new application is an excellent example of CSS looking forward and exploring important and challenging questions with their analyses.

Documentation

The chapter serves as a vital entry point and further provides important information on the potential uncertainties and risk with various alternative, including the consequences of dam breaching and the Preferred Alternative. Given the intensity of the discussions and stakeholder interests in dam-breaching, it is critical that the analyses, interpretation, and conclusions are rigorous and valid. Therefore, the structure, organization, assessment framework, and integrated discussion of the outcomes require substantial improvement to inform decisions regarding potential breaching.

A much greater degree of synthesis and integration is needed in the writing and presentation of results to explain and justify the methods without needing to look through multiple CSS annual reports and other cited documents. Greater use of supplemental information and figures/tables showing the modeling steps and analyses applied to model outputs for both modeling approaches (for example, a side-by-side comparison) is warranted. This would provide readers with sufficient details needed to understand the modeling formulations, key assumptions, and results in the context of uncertainties and risk of breaching compared to the preferred and other alternatives.

Chapter 6 has two main parts, with the first part further having two independent subsections. In the first part, a Cohort Model simulation study is used to estimate the mean expected SARs assuming that historical hydrological and ocean conditions can be used to evaluate the CRSO-EIS alternatives into the future. The second subsection of the first part describes how field observations from Snake River Chinook and steelhead, and wild Chinook from the Yakima and John Day River (2014-2020), are used to compare variation in empirically derived SARs. These two subsections (Cohort Modeling and empirical estimates) seem to be independent of each other – readers are left to speculate how the simulation results inform the latter and vice versa. The Cohort Model results are used to estimate a very simple definition of risk (e.g., proportion of simulations with SAR less than 1%).

The second part describes a simulation study, based on the Grande Ronde life-cycle model that was used to predict mean SARs (analogous to the Cohort Modeling), spawning abundance, and probability of exceeding thresholds. The same hydrological time series and the same alternatives are considered as with the Cohort Modeling. The simulated output from the Grande Ronde life-cycle is used in a risk-based decision tree (Figure 6.18) to evaluate two of the alternatives (non-breach versus breach) for the Lostine population only. The likelihood of successful implementation is unknown. Therefore, two methods of assigning probabilities of successful implementation were used: i.e., a random “coin toss” case and a “breach-confident” case. Ultimately, Grande Ronde life-cycle model analysis produced a weighted estimate of mean success under the two alternatives.

Because both sets of modeling analyses were from previously-reported analyses and much of the text was reused, and two parts seemingly concatenated into a chapter, connections between the Cohort modeling and the Grande Ronde modeling are insufficient. The Cohort Modeling and Grande Ronde Life Cycle Modeling share the same historical hydrology and design of the alternatives and then proceed with their own approaches to the analyses. The minimal cross-referencing between the two modeling analyses hinders understanding how uncertainties are propagated within each of the models. The different treatments of the outputs further confounded differences in methods with differences in estimated SARs. This makes interpretation difficult for each model’s results (specifically, role of uncertainties and estimating risks) and further prevents comparisons between the two modeling results.

The order of information in the text could be presented more clearly. Common information used in both models (e.g., management scenarios, hydrologic dataset) could be described before presenting the two sets of analyses for the Cohort Models and the Grande Ronde model. The two modeling analyses should be structured more similarly and using a parallel structure. The Cohort Models part does not include a Discussion section and provides four brief conclusions about the potential risk to salmon and steelhead but does not discuss how the analyses illuminate the role and effects of uncertainties. The Grande Ronde modeling part includes a more informative Discussion than the section on the Cohort Models. Overall, the chapter provides limited discussion or conclusions about the sources of uncertainty and their effects on risks. A final Discussion section that integrates the results of the two analyses would provide more coherent conclusions. Previous annual reports (e.g., McCann et al. 2019) provided a more coherent integration of the Cohort Modeling and the Grande Ronde life cycle modeling.

Methodology

The approach of using previous analyses also presents challenges in terms of the methodology used to assess uncertainties and risk. While the use of existing results is efficient, a disadvantage is that analyses previously completed for a different purpose, along with their documentation, may not be ideal (i.e., sufficiently robust) for the next set of questions. The use of different years in many model inputs and differences in the simulation methods and use of predictions to estimate risk between the Cohort and Grande Ronde model analyses adds to the uncertainty of the results. Some or all of these differences may be scientifically sound or conversely may be because the modeling (as with all modeling) was subject to modeler decisions. The documentation does not present the reasons for differences between the two modeling analyses and does not present an assessment of the uncertainty associated with these model and analysis differences.

For example, in the description of the simulation process for the Cohort Modeling (p. 207-208), bullet 5 refers to upwelling and ichthyoplankton indices, but they are from different sets of years. Were the values paired within years or treated as independent of each other? Further, the SAR estimates are from 1998-2013. What are the implications of drawing from years prior to the fish survival data? For the Grande Ronde modeling, PDO and UPW covariates used in the simulation were estimates for the 1929-2008 period, even though the authors later describe non-stationarity in ocean conditions (page 222). Given that poor ocean conditions may persist (p. 238), more extensive treatment of this non-stationarity beyond comparing good and bad time periods within the simulated historical period is warranted.

The ISAB also has some concerns specific to the Cohort Modeling about the treatment of the multiple cohorts that are simulated within each year. Currently, each cohort within a year is treated as independent of other cohorts in a year and each cohort in a year is given equal weight when the summary statistics are computed. First, this ignores potential correlation in the SARs among cohorts in the same year due to year-specific effects. Second, each cohort does not “represent” equal number of fish from the outgoing total in a particular year. For example, one cohort may represent 50% of the emigrants whereas another cohort may represent 25% of them, so their SAR values represent different numbers of fish. Current methodology used elsewhere in the CSS sums results across cohorts within a year assuming that sample sizes of cohort are approximately proportional to the number of emigrants; this may be a more reasonable assumption than weighting the SARs from each cohort in a year equally. The simulation unit and analysis unit should be the yearly total over all cohorts within a year, rather than individual cohorts.

While not presented as such, the Grande Ronde analysis is an example of a Bayesian analysis with the assignment of prior beliefs for some parameters that are not well-informed by data. As well, the analysis on page 235 only uses “means” rather than the full posterior distribution available from the simulation model. Consequently, no measure of “uncertainty” about the overall results is available. It may be helpful to implement the decision analysis directly using a Bayesian paradigm using the posterior distributions from the simulation, and full uncertainty in the prior beliefs, so that the results include a measure of uncertainty, and then are easily updated as more information becomes available. A Bayesian analysis also lends itself to conclusions about the posterior belief that returning numbers are above certain thresholds rather than reporting only on weighted-averages.

The focus is on PIT tag data to provide the empirical SAR values used for model fitting, which are ideal for assessing survival of smolts from point of release to one or more dams, and upriver progress from Bonneville Dam to their destination. However, they are not the only source of information on survival, and notably lack consistent fishery (harvest) data. To the extent that conclusions rely on comparisons among populations, it would greatly increase confidence in the conclusions by reducing uncertainty, if other forms of data such as CWT and Parental-Based Tagging (PBT) could be brought to bear by way of comparison. Elsewhere in this report, there is mention of comparisons, but they might be repeated here in this context.

The physical and ecological consequences of dam removal have not been fully explored. The four dams have been in place for decades and many physical and ecological changes have happened upstream and downstream, and not all are related to the dams themselves. What management changes will be made once the dams are removed, and at what cost or benefit? For example, will removing the dams expose the stream reaches above the dams to increased nonnative plants and fish? When and if northern pike colonize down from the Upper Columbia, for example, does removal of the dams expose the Snake River to an invasion of this (or other) nonnative fish predator? The ecological processes at play after dam removal cannot be reduced to water travel time and exposure of smolts to structures. As identified by Tullos et al. (2016), it will be important to consider the types of potential changes in the physical and ecological processes that are likely to happen when, how, and if the dams are removed.

Predictions of SARs in post-dam years from models configured and informed with data collected in the presence of dams are therefore highly uncertain. In modeling terms, this means one may not be able to realistically simulate breaching by simply substituting values for certain parameters in relationships based on data with the dams present. The representation of breaching in the two models requires further evaluation.

The assumption about marine survival being a single year for the Cohort Model and assumptions about harvest for both models should be described. Inclusion of marine conditions is warranted. The Cohort Modeling assumes that ***early*** marine survival is critical to represent and ignores marine survival affecting older fish. These effects on older fish can arise from the spatial variation in marine conditions and from potential responses of other species and, in some situations, harvesting. For example, Snake River salmon and steelhead spend much of their ocean lives far from the Columbia River. Salmon abundance in the North Pacific is near all-time high levels in large part due to releases of hatchery salmon (Ruggerone and Irvine 2018). There is increasing evidence of interannual variability in marine survivals as a result of density dependent competition including with pink salmon, aggravated by ocean warming (Ruggerone et al. 2023). We encourage the CSS authors to consider how one might incorporate the influence of climate-related and density dependent factors on the total marine survival of Columbia salmon in future reports. Likewise, many populations of Chinook salmon have shown decreases in mean age and size at age, and the influence of these patterns on projections should be considered.

The first subsection of the Cohort Modeling part that presents the modeling results is not connected to the second sections that uses field data. The text in the second subsection on use of field data states: *“To assess our previous predictions based on the 1929-2008 time period, we report SARs, water transit times, PITPH, river water temperatures and ocean conditions that occurred 1998-2013 compared to 2014-2020. For context, we also report SARs, … for wild populations from the Snake River.”* Then, a statement about how the NAA continued through 2019 and the PA operations started in 2020. Yet, the text seems to interpret the data as showing that the PA has been less effective than expected, while it did not start until after the data ended in 2020. Figures 6.3 to 6.5 seem important, but the text does not sufficiently relate them to the modeling from the previous subsection. The rationale, methods, and results of the data-based subsection of the Cohort modeling need to be explained and related to the cohort modeling more clearly, especially in the terms of informing the cohort modeling, uncertainties, and risk.

The rationale for why SARs for only Yakima and John Day Chinook stocks were used in the comparison with Snake River Chinook SARS (Fig. 6.7) was not clearly stated. This section of the chapter suggests that the higher survival of Yakima and John Day stocks relative to Snake River stocks occurs because the former have lower WTT and PITPH values. The comparison is used to support the breaching alternative. The validity of the comparison of SARs and abundances across alternatives rests on the assumption that the only important differences between these populations and their migration and survival patterns are associated with the dam passages. While this may be true, there would be greater confidence in the conclusions if this issue was very clearly identified and evaluated. Differences in body size distribution, migration timing, marine distribution, and such might all exist and if so, might affect SAR values.

Why were other mid- and Upper-Columbia yearling Chinook stocks (with WTT and PITPH) not used to bolster the comparison with Snake River populations? Figure 5 of Welch et al. (2020) indicates that Yakima and John Day stocks have SARS well above other mid-Columbia populations. This figure indicates that SARs for Snake River populations are similar to those in the upper Columbia and for 3 of 5 populations in the mid-Columbia. Using the full suite of Chinook stocks could negate or weaken the support for the conclusion that higher WTT and PITPH is the reason SARs for Snake River stocks are low. The comparisons among the Snake, Yakima, and John Day rivers are further complicated through the use of (presumably) different periods for the SARs. Comparisons of SARs across the same intervals may help identify the key stage where stocks differ.

Use of Historical Hydrology and Climate Change

The Cohort modeling used random draws of years from time series values of hydrology and different (but somewhat overlapping) years for WTT, PITPH, ichthyoplankton index (ICH), and upwelling index (UP). The river condition variables were selected by year to match the hydrology. However, ocean conditions were chosen independently of the river conditions and independently of each other. However, there may be associations between the freshwater and marine conditions and among the ocean condition variables. For example, the 1998-2020 data presented in Figure 6.5 suggests that sea surface temperatures were consistently elevated in recent years and ichthyoplankton biomass was depressed. The assumptions of independence of conditions, as represented by random sampling of variables, is tenuous and can greatly affect the uncertainty of model predictions. There is considerable research on climate change's effects on ocean conditions in the North Pacific and that current warmer conditions tend to reduce ichthyoplankton density and marine survival rates of salmon and steelhead. The Cohort simulation model does not acknowledge this possibility and will therefore potentially overestimate SARs for all scenarios. A positive bias in SAR projections would result in predictions that provide too rosy a picture of Snake River dam breaching scenarios with respect to SAR targets (e.g., MO3 is predicted to have a 57% and 68% change of SARs > 2% for Chinook and steelhead, respectively, Table 6.2).

The Grande Ronde modeling simulated the 80 years using estimated annual matched-by-year values of its inputs (hydrology, transport, PITPH, WTT, PDO, UPW). It would be helpful to see a direct comparison of the input variables across the two models, as well as the empirically-derived values reported in the data subsection of the Cohort modeling.

There is an implicit assumption (p. 206) for both modeling analyses that the observed record of water conditions and ocean conditions adequately describes the likely range of future scenarios. Is this true under climate change? If not, perhaps a more selective choice of appropriate water years/ocean conditions like those expected under climate change could be used rather than the full 80 year observed record? The authors recognize the problem on page 210:

> *"The lower ends of the predicted ranges for SARs represent years with poor water transit times that occurred in the 1929–2008 time period and/or poor ocean conditions, both of which are expected to increase in frequency due to climate change effects. "*

The ISAB suggests that the CSS reports might benefit from selecting water years/ocean conditions more similar to those expected under climate change and documenting why they reflect likely future conditions. The Grande Ronde analysis attempts to address some of this by comparing two periods that had favorable and unfavorable conditions, assuming the unfavorable period is more likely to occur into the future. How well the unfavorable period represents future conditions is not assessed.

The authors mention non-stationarity in terms of river and early ocean conditions and population trends, but that can also be thought of in terms of relationships among the covariates (affects their coefficient values) and between the covariates and SAR and abundances. It is possible that the relative importance of different covariates can change under climate change. This is a particular concern if climate change predictions about rising tributary and mainstem temperatures, an altered hydrograph from earlier runoff and lower snowpack, a widening distribution of non-native piscivores, and increased production rates from hatcheries are realized. Thus, the Cohort and Grande Ronde Models may be overly optimistic. A discussion on the consequences of climate change and its effect on model realism is warranted.

There are several options for addressing the non-stationarity issue. The authors should add a caveat to statements about the benefits of dam breaching, indicating they are conditional on the unverified assumption that historical ocean conditions represent the future, which underlies their analyses. They also could perform additional simulations to address how alternatives will perform under future conditions that differ from historical ones. One approach is to use time periods that represent likely future conditions, as was done with the Grande Ronde model, but with a rigorous demonstration that the selected time periods resemble reasonable expectations of future conditions. However, using the current approach with stratification of years into favorable and non-favorable sets is not a complete solution. SARs in the non-favorable set would still be overestimated through the random draws of long-term historic ocean conditions.

Another approach is to model the ichthyoplankton and upwelling time series (e.g., lag-1 correlation model) and use those time-dependent modeled estimates to generate values that resemble future conditions. In terms of variability that affect risk, one can replace ICH and UP components of the SAR model with estimates of lag-1 random year (marine) effects and use this revised model to simulate future marine effects for the simulation. These two options will have two effects on simulation results. First, the early years of the simulation may have lower SARs that are below SAR targets. Second, the uncertainty in random marine effects will increase in the middle and later years of the simulation. It would be useful to communicate the simulated trends in SAR for the early and full simulation period to decision makers so they understand how sensitive the predicted probability of exceeding target SARs are to future marine conditions.

Set-up of the Problem to be Addressed

Consideration of potential breaching of the Lower Snake River dams is of great societal importance, and robust analysis that can inform conversations about that infrastructure is critically needed. However, if Chapter 6 model results are to be used to make conclusions about breaching, the Introduction should be expanded beyond the approach to include discussion of the fundamental problem and the uncertainties with the alternatives, especially with breaching, as well as the model uncertainties. The analyses indicate that dam breaching will reduce or eliminate some key stressors that contribute to the continued decline of salmon and steelhead. However, other important ecological and management changes will co-occur with breaching, and these changes are not reflected in the results presented. Thus, the modeling methods and results must be adequately caveated and explained so conclusions are properly interpreted.

The authors should consider changing the title of the chapter to better match the analyses and results. A more precise title that better reflects that the chapter is an exploration of the uncertainty of model-predicted SARs to hydrology, hydrosystem operations (water transit times, PITPH, transport), and early marine factors.

The authors seem to underestimate the complexities and resulting uncertainties associated with dam breaching. A core assumption is that the system will immediately return to the state represented by model parameter values for WTT, PITPH, etc. and other factors impacting SARs will remain the same. The ecological processes at play after dam removal cannot be reduced to water travel time and exposure of smolts to structures. While general hypotheses about ecological recovery are well established and supported by case studies of moderate- to small-sized dams (Bellmore et al. 2019), removal of large dams can have uncertain effects, particularly for ecosystem processes (vegetation recovery, predation, water quality, primary production, straying and life history diversity) in the transition years. Quantitative prediction of the transitory and final ecological state is still very early in development. The current structure of the models and the assumed relationships may not accurately predict SARs and abundances after breaching. Model predictions of SARs using models configured with and informed by data with dams only are therefore highly uncertain. In modeling terms, this means one may not be able to realistically simulate breaching by simply substituting values for certain parameters in relationships based on data with the dams present. Given the exceptional importance and interest around the potential for breaching of the Lower Snake dams, the representation of breaching in the two models requires further evaluation and the limits to the scope of what is represented need to be more clearly stated alongside the results. This issue becomes especially important with the presentation of the breaching alternative's benefits. The assumption of the analyses that the only important consequences of dam breaching for salmon would be in the lack of interaction with bypass or other structures and the effects on water travel times takes on much greater importance. Myriad ecological changes would almost certainly occur as well, with potential beneficial and deleterious effects on survival (e.g., changes in the density, species composition, food webs, sediment mobilization, and feeding efficiency of predatory fishes and birds). This source of uncertainty should be made very clear.

The wording for statements about the efficacy of Snake River Dam breaching in the introduction of this chapter was a bit ambiguous (e.g., likely to promote recovery, will support the recovery). Do these statements indicate that models predict that only a marginal gain in SAR will occur due to breaching (i.e., any action that increases survival, even by a small amount, promotes recovery), or do they mean a substantial increase in SAR, so it falls within the SAR recovery goal range? If low marine survival conditions persist, will gains in freshwater survival through breaching be large enough to offset low marine survival to achieve target SARs?

Use of Modeling Results for Decision Analysis

The Cohort Modeling analysis was not by itself a conventional decision analysis. The Grand Ronde modeling analysis came closer to a decision analysis with its presentation of a decision tree but also did not follow the standard decision analysis framework. For example, the uncertainty in simulations is used to estimate risk, but there is no uncertainty in the risks of outcomes (e.g., how well do we know the probability SARs being below 1%), and many sources of uncertainty are ignored or underestimated. The idea of explicit treatment of uncertainty and risk in a decision analysis framework, as proposed in Chapter 6, is a promising and relevant approach. The ISAB encourages the authors to revise their approach following the guidelines and structure suggested by Peterman and Anderson (1999), perhaps more clearly incorporating the eight components they consider essential in applying decision analysis to ecological issues. For a relevant example, see the treatment in Pestes et al. (2008) of a single population of sockeye salmon in British Columbia, and there are many other relevant examples.

Interpretation of Results

The analyses conclude that the breaching of the dams is the "best" alternative for achieving target SARs and abundances. The ISAB considers this to be overstated and premature based on the methods and results presented in Chapter 6 for several reasons. First, the documentation does not allow for easy critical evaluation of the many assumptions and results. Second, the breaching scenario, as was conducted in the Chapter 6 analyses, pushes (and may exceed) the capabilities of the present models to simply simulate the complex ecological responses expected under breaching. Third, the possible mis-specification of implementation uncertainty, especially the beaching alternative assumed to have the lowest implementation uncertainty. Finally, the use of the results in previous applications used the breaching results in a more comparative mode where predicted SARs were compared to the predicted SARs for the NAA and to other alternatives, while Chapter 6 relies completely on the absolute predictions of SARs and abundances to draw conclusions.

The predictions from the models are interpreted as if they included all major sources of uncertainty. In reality, only a very limited subset of uncertainties was explicitly included. The authors seem to put too much confidence that variability from the empirical modeling captures major sources of uncertainties. Such confidence is especially challenged when the models are used to predict outside of the range of the data used for their estimation, such as with breaching. Further, then only certain parameters were randomly varied in simulations and model predictions were highly aggregated into summary measures thereby essentially ignoring the variability in their predictions. Of particular importance is the assumption that the breaching has very low implementation uncertainty.

The authors position the chapter as "exploratory" or an "illustration" of a methodology but then make conclusions as if the analyses were definitive and robust. The analyses are better viewed as an illustration of how data may be more comprehensively modeled. Therefore, the Chapter's conclusions about the benefits of breaching should be more appropriately caveated until a more comprehensive effort is completed. An example would be using phrasing that more precisely reflects what was simulated: "When considering only travel times and exposure to structures, model results indicate that breaching can benefit salmon recovery by increasing SARs."

Further, unnecessary uncertainty is added to the other sources of uncertainty because the two models were applied to different years of SAR data (e.g., SARs from 1998-2013 for Cohort versus 1964-2008 for Grande Ronde). If there are good reasons for these differences, they should be explained. If the analysis results are intended to make conclusions about breaching, then the modeling methods should be re-visited (rather than simply using "old" results) and a coordinated effort conducted to apply both models to minimize any uncertainties from modeler decisions and to ensure a sufficiently realistic representation of breaching effects in the models.

Uncertainty and Risk

The presentation of uncertainty and risk is confusing. In decision analysis, risk refers to the probability of an outcome and uncertainty is how well we know that probability. The authors use uncertainty, sometimes with an adjective like "implementation," to mean a variety of types and further then assume that the response (SAR) uncertainty is entirely due to variation in the relatively few model inputs (e.g., hydrology), even under a breaching scenario. The only explicit treatment of risk is with a decision tree (only in the Grande Ronde application) that assumes the probabilities are known with absolute certainty. The chapter does not explain the types of uncertainty and risk explored in its analyses. Literature on uncertainty analysis and risk assessment is extensive, and many approaches are available. The chapter does not explain why the current approach was used for this assessment. Simply, uncertainty and risk need to be carefully defined and considered in the analyses and text.

The Cohort modeling limits its estimates of risk to the probabilities (proportion of model runs) of SARs being <1% and >2% from Monte Carlo simulations. The part of the chapter on the Grande Ronde modeling used the model results to evaluate risk based on two types of decision trees—a coin toss approach and a breach confident approach (breach is known to have zero PITPH at each dam). It also used these two approaches to evaluate risk under 10-year periods of favorable SARs (1963-1972) and unfavorable conditions (1989-1998). [Note that these are different periods than the favorable and unfavorable periods used in the Cohort modeling.] The chapter requires substantial revision to create a consistent format and content for its sections and a more thorough discussion of the analysis of uncertainty and risk.

Minor Comments

p. 215. We are uncertain whether travel times for Snake River smolts could be reduced to less than 8 days with dam breaching as stated at the bottom of p. 215. The 2000-2020 WTT value for the Yakima Chinook and steelhead populations is ~ 8 days (Fig. 6.7). Presumably the WTT in the Snake River without dams is more than zero days, so why is the total WTT not more than 8 days?

p. 236. Why would PITPH=0 for the breaching alternative, given there would still be four dams remaining in the Columbia River?

We are not clear on why theoretical examples are given for probabilities of failure/success for each alternative (coin toss or PMO34=0.75). Why not use the probabilities of reaching a target SAR predicted by the models for each alternative to set the probabilities of failure/success?

Why does the text refer to the various outcomes starting on p. 208, but the table that defines them is not until p. 221? It would have been much more helpful to put that table in the Introduction and explain them there. Instead, the Introduction (p. 204) gives us the conclusion in advance *“... alternatives that breach the four lower Snake River dams will support the recovery and sustainability of populations ..."* The information on p. 221 needs to be moved up and highlighted.

How does the magnitude of year-to-year variation in SARs shown in Figure 6.3 (roughly 10-fold) compare to the magnitude of variation associated with in-river effects? For example, if the fish did not encounter any dams, there would still be mortality and it would still vary from year to year. Can these forms of variation be compared?

p. 207. From the description of the simulation procedure, the authors simulated multiple cohorts from the same year independently of the other cohorts in the same year. But this would ignore any correlation among the SARs from the same years due to year specific factors that affect all cohorts simultaneously (e.g., warmer than usual). Shouldn’t the simulations attempt to keep these within-year correlations during the simulation study?

It appears that the authors computed simple summary statistics over the 3.2 million simulations for a particular EIS alternative and species (p. 207). However, while this approach is appropriate for a simulation approach that treats each cohort within a year independently of other cohorts (see previous comment), it will ignore the correlation in SARs from the 4 cohorts in a given water year if a different simulation method is used. To avoid future problem, it is preferable to summarize the data using an “averages of averages approach” by first finding a weighted average of the 4 cohort SARs for a particular water year (weighted by population that each cohort represents which may not match the number of smolts released). This gives a single SAR for each water year for a species. Now summary statistics can be computed on these summary measures. This may not affect the mean SARs reported in Table 6.1 but will certainly affect the computation of the probability of having a SAR below/above various thresholds when within-year correlation structures are introduced. The simulation unit and analysis unit should be the yearly totals of fish, rather than individual cohorts.

p. 207. “*80-year water record (1929-2008)*.” Might this reference period of record be expanded to include the last decade, at least?

p. 208. Step 6. The fitting process will have generated posterior samples for the random year effects and the over dispersion effects. Why were the random year effects and the over dispersion effect taken from Table 3.1 and 3.2 of a previous report rather than from the posterior?

p 208 and Tables 6.1 and 6.2. More clarity is needed on how the authors computed the responses over the 80 water years and 4 cohorts per year. Are these just pooled, even though the results for the 4 cohorts within a water year may be associated? How would this intra-year correlation in SARs among cohorts affect the interquartile ranges seen in Table 6.2 and the probabilities seen in Table 6.2?

p. 210. "*To assess our previous predictions based on the 1929–2008 time period, we report the SARs, water transit times, PITPH, river water temperatures and ocean conditions that occurred 1998–2013 compared to 2014–2020*." More context is needed here. Do the 1998-2013 years represent good water years; do 2014-2020 represent poor water years? Which time periods reflect NAA versus PA?

p. 212. Figures 6.3 to Figure 6.5. These appear to represent actual data (not modeled). It would be more interesting to use the model to forecast the SARs expected under the water/ocean conditions from 2013 onwards and compare to the actual values. If there is a good match, this would increase the credence of the model results.

p. 215. The authors recognize that "*These data clearly show that other factors besides ocean conditions are influencing SARs of wild populations across the Columbia River Basin*." If this is true, then how much credence should be placed on the results of the simulations?

p. 217. The actual data from wild Chinook and steelhead population were used to identify conditions leading to good SARs. How were the simulation results used to inform this decision? If the simulation results are not used, why was the simulation done?

p. 222. Upwelling was randomly selected from the entire period. If so, this seems to ignore temporal autocorrelation in the upwelling index.

p. 223. Many values for survival were fixed, reducing the variability in the predicted SARs seen in Figure 6.9.

p. 226. Will the fixed parameters (see page 223) reduce the variability in the SARs and abundances and so distort the probabilities of exceeding the Low/Medium/High goals?

p. 229 and Figure 6.15. What will be the impact of fixing many parameters (see page 22) on the proportion of years that fall above the target abundance?

p. 237. The sentence in the middle of the first Summary paragraph is unclear. What does it mean to say that "*the timing in relation to the favorability of environmental trends can affect whether objectives can be met within a desired timeframe*." Please revise this to clarify the meaning.

Editorial Comments

p. 202. Line spacing in third paragraph is inconsistent with report format.

p 207. Line spacing in second paragraph is inconsistent with report format.

p. 206. "*The SARs were calculated as the number of adults detected at Bonneville Dam divided by the number of smolts detected at Lower Granite Dam*." Presumably, the adults are from those smolts detected at Lower Granite Dam and do not include other adults.

p. 206. "*Data from juvenile outmigration years 1998–2013 were used in these analyses*." Why did the series stop at 2013? Presumably, more years of data are available.

p. 207. The description of the simulation process needs editing. On p. 207, it is stated that for each cohort and water-year, 10,000 random simulations are done. But for each simulation, the full 80-year water record is used again (Step 3)? Is each cohort simulated independently of the other cohorts in a year, even though all four cohorts commonly experience correlated water conditions?

p. 209. Table 6.1. Why present the inter-quartile range which corresponds to the middle 50% of values? Would not the 2.5th and 97.5th percentile ranges be more useful as a measure of uncertainty in the predicted SARs?

p. 209. Table 6.1 indicates that MO3 (Wild) predicts the greatest SARs for both Chinook and steelhead. However, none of the alternatives has an analysis focused solely on wild fish. There are likely reasons for this; however, an explicit explanation is not provided. Regardless, it is important to note that "wild" fish alone yield greater return rates than artificial and natural production together. This may be because of hatchery effects or because of differential harvest effects, but in any case, it requires some explanation.

Also, for Table 6.2, is there an accepted or acceptable threshold probability of the < 1% or > 2% which is important? All are non-zero, but when is it acceptable?

Table 6.2 seems to list the alternatives in order from least to most favorable. Is this correct, and if so, can this be stated in the caption?

Is the information in Figure 6.2 the same or different from that in Table 6.2? If they are the same, then this should be stated in the captions/legends?

p. 210. "… *both of which are expected to increase in frequency due to climate change effects*." We do not necessarily question this conclusion; however, some referenced justification is warranted. What is likely to be the range of future conditions?

p. 215. Font differs from text style.

p. 218. Figure 6.8 – this is a very interesting and helpful way of presenting the WTT and PITPH impacts.

p. 220. Because 80 years of historical water conditions were used to calibrate the Grande Ronde model, it is important to know how confident the future conditions will fall within the past ranges. As such, if the future conditions exceed the historical extremes, can we safely extrapolate beyond lower/higher extremes? Will the predictions still work?

p. 221. This may be addressed previously or elsewhere, but the non-federal MO34 has a slightly higher TDG than MO3. Why is the 125% significantly better than 120% TDG for spring spill in downriver dams (rather than incrementally)? This is at first counterintuitive as higher TDG should reduce survival, correct?

p. 222. How well does the model perform in terms of a priori prediction versus observed outcomes for abundance and survival? A table or graphic comparing these would go a long way in establishing confidence in the model without the reader needing to track down such comparisons from multiple earlier reports.

p. 222. Also, for Equation 6.4, Su is not defined as are the other terms.

p. 222. "*SARs to Lower Granite (LGR) dam*." Does this mean the LGR -> LGR SARs or LGR-BON SARs computed in the previous analysis? Throughout this chapter, clarify which periods are used in the calculations of SARs.

p. 223. Equations 6.5 to 6.7. SR is a proportion and not a logit. The authors likely wanted to write that logit(SR) is a function of covariates. Similarly, for equations 6.6 and 6.7.

p. 225. The models use a 10-year running average survival prediction. Why 10 years rather than the average generation time (or 2x or other) for each species? Is this based on a previous analysis? If so, provide a reference and if not, describe more fully.

p. 225. The effect of the alternatives on the in-river survival is understandable, but how do the alternatives affect the first-year ocean survival? From Equation 6.6 this appears to be a delayed response to PITPH. This needs to be explained, or at least reference to Chapter 8 of this year's CSS report.

In each chapter, abbreviations such as MPG, CBP, NPCC should be spelled out in first use as readers may not progress from beginning to end in sequence. For example, CBP is used early in the chapter, but first time spelled out is for the heading for Table 6.4.

p. 227, Figure 6.11 and Figure 6.12. Why use the default definition of the whiskers? The simulated data is known, so why not extend the whiskers to the 2.5th and 97.5th percentiles directly?

pp. 231 and 232. Table 6.5 and associated paragraph on p. 231 are good presentation of simulated outcomes.

p. 238. "… *future will contain more years resembling unfavorable metrics produced in this analysis.*" A published reference or link to such is needed to support this contention.

Also, "*The CBP Task Force identified other limiting factors* …." Such as?

IV. G. Comments on Chapter 7. Effects of Juvenile Bypass Systems on Smolt to Adult Return Rates

Logistic regression was used to estimate the impact of number of bypass events on adult return of smolts detected as juveniles at BON. This chapter is an update to the analysis done in 2016[2] and appears to be well done, and we have limited substantive comments along with the minor and editorial comments.

Given the demonstrated cumulative impacts of juvenile bypass systems, it would be useful to provide a general description of a typical bypass system in this chapter or cite elsewhere in this

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[2] See CSS 2016 report, Appendix J (page 16) for ISRP comments on Chapter 7 and CSS response.

report. In addition, there have been several major papers published on the topic that should be incorporated into the chapter (see ISAB 2021-1).

The authors now have enough data that some raw data summaries could be useful to give the readers a sense of the scale of impact of bypass events. For example, they might group fish by number of bypass events and compute a simple “raw” survival probability for each number of bypass events that represents an “average” over multiple years, environmental conditions, etc. Then superimpose a fitted line to show how these fit. This would give more credence to the “linear” effect of number of bypass events rather than a quadratic or step function.

As indicated in Figure 7.4, most steelhead reported in this study experienced about one bypass event in recent years, as did hatchery-origin Chinook, and wild Chinook experienced about 1.5 events. Does this mean that the entire effect on survival results from only 1-2 events in most cases? How good is the model for predicting effects at such low frequencies?

How closely do the assumptions driving the models in this analysis follow assumptions of Tuomikioski et al. (2010) and McCann et al. (2016)? The authors mention that a weight of evidence approach was followed, but more information on specific assumptions would help.

While the results are clearly presented, there is much room for further interpretation. Two key points of interpretation would be helpful:

> a. Very broadly, why are these bypass systems contributing to delayed mortality? The recommendation to increase spill to reduce bypass interactions seems sensible, but could operations or infrastructure otherwise be changed to reduce the delayed mortality? As noted below, perspectives in the 2002 paper by Budy et al. would be helpful here.
>
> b. Why do wild Chinook experience more bypass events than hatchery Chinook? Differences in body size or migration timing, perhaps? Does the interaction between Year by Rear (versus the additive term of Year and Rear for steelhead, who do not have the difference in wild versus hatchery for the number of bypass events) tell us anything meaningful?

The second paragraph of the Discussion states, “*This study clearly exhibits that juvenile bypass systems are one mechanism that results in hydrosystem-related delayed mortality*.” Given the strength of this statement and the obvious implications for the debate on the broader topic of Snake River dam breaching, it is incumbent on the authors to clearly state how other covariates of individual fish such as body size and timing as well as covariates of the populations being compared could affect the results. There is simply no “before-after-control-impact” (BACI) comparison to be made here. We are asked to base our expectation of what will happen in the future in one area (the Snake River) from what happened in the past elsewhere, in the John Day River and Yakima River populations.

An assumption is made that the vast majority of fish not detected in the bypass systems passed through the spillways, and only a small proportion passed through the turbines. McCann et al. (2015) is cited, presumably for the high estimates of FGE (they ranged from 0.76 - 0.95, depending on the species and dam). However, in Chapter 8, updated values of FGE range from only 0.52 to 0.95. Thus, it seems likely that conditions promoting bypass experiences would also promote turbine passage, as the two are related through FGE. This means that fish with more bypass experiences may also have more high-risk turbine experiences (up to a point), due to conditions favoring powerhouse passage. Please consider this possibility and how it may affect the interpretation of the results.

With the long-term data available and changes in some of the dams, additional dam-specific information is available to include in the analyses. How have dams changed during the historical record and what are the differences among the dams? Their approach would benefit from a model that looks at survival considering different configurations of the various dams. Was the modification at a dam followed by higher survival? Also, the estimates of FGE are now 20 years old and there is an opportunity to revisit them considering more recent data and changes in the dams.

Minor Comments

The Introduction begins with overly simplistic statements that can be viewed as “the dams went in, and the runs went down.” What is not stated is that wild runs of Chinook and steelhead in many places along the coast also went down over the same period, and many remain low. The opening sentence should be phrased more carefully to reflect the available evidence.

The paper by Budy et al. (2002) is an excellent review of possible direct and indirect mechanisms for delayed mortality, and the Introduction’s text on page 243 would benefit from more detail on these proposed mechanisms, drawn from that paper.

In the Introduction, authors state that operational and configuration changes at the FCRPS dams were implemented between 2009 and 2013. Could those be briefly described here? Similarly, it is stated in the following paragraph that between 2014 and 2019, dam operations continued to evolve. Again, it could be helpful to describe these. [Note: operations during these time periods seem to be inconsistent with the description of when NAA and PA were operating in Chapter 6.]

Paragraph 2 defines SARS as smolt to adult survival rates. This chapter is on smolt to adult returns (i.e., SARS in Chapter 6 and as explained in Appendix A). Please clarify/correct the definition in paragraph 2 and check to see the definition of this important term is consistently applied throughout.

In the Methods, please indicate if all juveniles were yearlings, and clarify that adults could be 1-salt, 2-salt, or 3-salt fish, as seems to be indicated in the second paragraph on p. 244.

In the denominator of equation 7.3, the indexing should be by j, not r.

When reporting the rear x year interaction effect for Chinook salmon, it may be worth stating that year-specific differences usually were of the form W > H.

The assessment of low SARs in recent years is clear for steelhead but less so for Chinook salmon. Perhaps temper the statement.

Given the suggestion of higher in-river mortality among later-migrating cohorts (most evident for steelhead, Chapter 3), the role of day of the year (ordinal day) in the analyses of BON-BOA SARs should be explored.

Figure 7.3. Can the dips in proportions of fish experiencing bypass events in 2010 and 2015 be explained? Moreover, was there a downward trend in the proportion of fish with bypass events? Visual examination suggests that this might be the case. Also, the wild and hatchery origin Chinook seem to differ, but not the steelhead. Why might this be the case? And why would wild Chinook values be higher than hatchery origin fish, but closely tracking among years?

Figure 7.4. Similar to Figure 7.3, can the dips in numbers of bypass events experienced in 2010 and 2015 be explained?

The number of parameters (k) In Table 7.2 seems too low. For example, in the Year + Reach model there should be 14-year effects (2006-2019) plus one hatchery rearing effect, resulting in estimation of 15 parameters (k=15, not 9)?

There is very large uncertainty in the model-specific and model-weighted bypass effect odds ratios in Tables 7.4 and 7.5. In all cases, the standard errors of the estimates exceed the most likely estimates (i.e., coefficient of variation is > 1). While we might expect this for dam-specific rates due to lower sample sizes (fewer fish go through the bypass system at any one dam compared to the cumulative number of bypass events across all dams), the ISAB was surprised to see such high levels of relative error for total bypass and Snake/Columbia bypass effects (and error levels similar to dam-specific estimates). It is inconsistent that there is strong support for bypass models from the AIC analysis; yet, the bypass effects are so poorly determined. Perhaps the SE for the odds ratio is calculated incorrectly?

Explanation is needed why bypass effect odds ratios were nearly identical across dams (Table 7.4). The uncertainties were also very similar.

The model-weighted bypass odds ratios are confusing. There are only two models that estimate dam-specific bypass effects (4 and 8). For Chinook, there is virtually no weight assigned to model 4 (Table 7.3), so we expected the model-weighted bypass odds ratio in Table 7.5 to be very similar to the result for model 8 (Table 7.4).

Figure 7.5 reveals roughly 10-fold variation among years within any species-origin groups (ca. 7% to < 1%), consistently higher survival for wild versus hatchery Chinook, a smaller effect for steelhead, and a downward trend in all species-origin groups. These patterns should be noted and explained.

Please show a figure or figures showing the effect of bypass experiences on estimated SAR, or otherwise evaluate the assumption of linearity.

Editorial Comments

p. 248. Table 7.2. The column for k needs to be checked. There are now 14 years of data which is an increase from the 2016 analysis, yet the number of parameters has not changed from the tables in the 2016 analysis (Table 7.3).

p. 249. Equation 7.2 does not seem to allow for over dispersion because it cannot be modeled for Bernoulli data. Please add a note to the text.

p. 249. Equation 7.2. Is Year treated as a fixed or random effect?

p. 249. The "Effect" term is not typeset properly. You need to specify it as \textit{} to avoid LaTeX (the equation editor in Word) from typesetting it as a function.

p. 249. "*These estimates were computed by model-averaging bypass related parameters across all models sharing the same Year by Rear interaction to ensure contrast consistency*." It is not clear what is meant by "contrast consistency" here. Regardless of the type of rearing interaction, the impact of the bypass parameters has the same interpretation. Predictions for year specific probabilities can still be computed with and without the year-rearing interaction and the model weights will give appropriate weights to the predictions. For Chinook, including all models will not have any real impact because the models in the other "interaction set" have such low model weights. For steelhead, the impact will be lower because the model weights are low, but not negligible in the other "interaction set."

p. 249. "*The odds ratio is simply the probability of an event occurring divided by the probability of that event not occurring (i.e., the ratio of survival to adulthood to not surviving to adulthood)*." This is not correct. This is the odds of an event. The odds ratio is a ratio of odds under two scenarios. For example, in a logistic regression, the coefficient for the "rearing" effect represents the change in log-odds between fish reared as wild or hatchery. A difference in the log-odds, converts to an odds-ratio when exponentiated.

p. 250. The authors now have enough data to explore non-linear effects of number of bypass events (e.g., quadratic or a step function). Some exploratory plots (see above) may be helpful.

p. 250. Why include model 7 in the Chinook models with a weight very close to 0. Why were some models for steelhead excluded even though they had substantial weight? This needs to be explained better.

p. 252. "*wild-reared Chinook salmon were 1.3 times more likely …*" These are the odds ratio, so some rewording is needed along the lines of "the odds of a wild-reared Chinook salmon smolt returning as an adult are 1.3x larger." Similar changes are needed for the steelhead conclusion.

p. 252. "*These values are reported as decimal odds*." What does this mean? Odds are a pure number, despite common vernacular that an odds value of 1 represents a 1:1 split.

p. 253. The confidence interval for the odds ratio all seem to include the value of 1 indicating no effect. This is consistent with Table 7.3 showing that the model with dam specific bypass effects has very low effect and should be explained in the legend. Or, as indicated previously, the SE have been computed incorrectly when back-transforming the estimates.

IV.H. Chapter 8. Estimating Powerhouse Passage Proportions at Hydropower Dams

Overview

Additional data were added to an existing analysis that uses regression analysis to derive functions to estimate PITPH. For dams with PIT-tag detectors, the empirical logit of the bypass passage probability is related to environmental variables (flow, proportion spilled), which is then used with an estimate of fish guidance efficiency (FGE) to estimate PITPH. For dams without PIT-tag detectors, they directly model PITPH. This is an update of a previous analysis reviewed by the ISAB.[3] We have limited substantive comments along with Minor and Editorial comments.

For dams with PIT-tag detectors, the PITPH index will strongly depend on the estimates of FGE. The methods used to estimate or acquire dam-specific values of FGE are provided in the narrative, and it would also be helpful to summarize the data used in a table (e.g., years used, type of estimation, source of data). In future years, this table could be updated if new year's (e.g., with low spill) are included in analyses.

For example, p. 261 states that estimates at Bonneville and The Dalles dams were based on fish with radio transmitters rather than PIT tags. Transmitters are typically larger than PIT tags, so were the body sizes of the fish tagged similar to those with PIT tags, and to untagged fish?

In addition, given the importance of understanding the uncertainty in the estimates of PITPH, it would be helpful to present the estimates of uncertainty in the FGE values presented in Tables 8.1 - 8.3. We also note that in many cases, the values of FGE are lower compared to earlier estimates (especially for LMN), and they now range from 0.52 to 0.95. Thus, is the assumption of constant FGE valid?

The model fits (e.g., Figure 8.3) in many panels appear to have high leverage data values with a cluster of points at low passage proportions, for which the model cannot resolve differences within. Some of the fits indicate the model is only capable of predicting low or high values, and the high values would have high uncertainty due to few data points. If these few data points in the upper right corner are removed, the fit may be quite different. While maintaining the same statistical modeling has advantages for continuity, some of the fits suggest that the fitted models are highly sensitive to a few points and cannot predict differences among low to moderate proportions (i.e., points are a cloud). Exploring this modeling further is warranted to determine if alternative strategies for model building and formulations may improve the fitted models.

The recommended approach to computing PITPH when spill proportions are low (<0.15) seems ad hoc (assume PITPH = 1 – spill proportion) and inconsistent with the conclusion that the model-based approach to estimate PITPH is better than the spill proportion estimate. This approach could lead to a discontinuity between modeled estimates of PITPH at a spill proportion of 0.16 (model-based) compared to a value calculated from 1-0.15 at a spill proportion of 0.15. Can the model be reformulated so that it predicts PITPH=1 when the spill

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[3] See Appendix J in the 2015 CSS Annual Report.

proportion is zero? One option is to use a zero-intercept model with only the PropSpill covariate (e.g., PITPH= 1 - b1*PropSpill).

Minor Comments

Please define PGE in terms of numbers when this abbreviation is initially introduced. In general, throughout Chapter 8 (text and figures), better clarity could be achieved by using consistent terminology or abbreviations for PITPH, bypass passage proportion, etc. It would be helpful if PITPH is used in place of "total powerhouse passage" in the exemplar calculations.

Please clarify the calculations used to derive the complete reach (LGR to BON) PITPH values (Figure 8.10). Also, please clarify what is shown on the y-axis.

Why is there a sharp bend in predicted PITPH in some of the lower panels of Figs. 8.4-8.6 for TDA and BON?

The initial description of the methodology (page 287) would be easier to follow if only equation 8.2 was presented (this is the equation used to predict PITPH). It is also confusing to use the term “detection probability” for bypass passage probability. Can PIT-tagged fish going through the turbines also be detected at some facilities? Multi-dam CJS models will predict that a PIT-tagged fish passed a dam even if it went over the spillway.

Editorial Comments

p.262 “*Logistic regression was used to evaluate the association between environmental conditions and passage route probabilities*.” This is technically not correct. A logistic regression would refer to a model where the response variable is 0/1 such as in Chapter 7. In this chapter, an ordinary regression analysis is done on the empirical logit of the passage probability. This is described in https://esajournals.onlinelibrary.wiley.com/doi/full/10.1890/10-0340.1

p. 262. Several cohorts from a year were included for some species/dam combinations. The detection probabilities of multiple cohorts within a year may be correlated, yet the model in Equation 8.4 does not have a random year component. Why not?

p. 262. Each species/year/cohort detection estimated passage probability will have different precision (SE) because of differing sample sizes? How variable are the precisions of the estimated passage probabilities on the original and logit scale? Would a weighted regression (weighted inversely to SE) be more appropriate? Or perhaps the variation in precision is small enough that it doesn’t matter. This needs to be investigated further.

p. 264. Legend to Table 8.2 indicates “logistic regression” was used. This needs to be fixed in all tables.

p. 264. There are two tables both captioned Table 8.2, so renumbering is needed here and for the following table.

p. 264. Estimates should always be reported with SE. This is especially important given the discussion about uncertainty in the estimates of PITPH.

p. 265. The example uses PERCENT spill, but the variable names are PROPORTION spill as is the definition on page 263 “$PropSpill$ is the proportion of total flow.” Be consistent throughout the chapter.

p. 265. There is an extra “is” after “1-0.38=0.62.”

p. 265. *“Also, predictions should be restricted to the range of zero to one by setting any predictions above one to one and any predictions less than zero should be set to zero.”* The response variable is on the logit-scale that restricts predicted probabilities to be between 0 and 1. Is this related to PITPH being a ratio? Please explain.

p. 268. Plots of observed versus predicted usually have the predicted values along the X-axis and the observed values along the Y-axis. The plots in these figures (and other similar plots) are “reversed.” See https://www.sciencedirect.com/science/article/abs/pii/S0304380008002305

p. 269. Figure 8.6. See previous comment about model fits and leverage points.

p. 270. Figure 8.3. See previous comment about model fits and leverage points.

p. 271. Figure 8.4. See previous comments about model fits and leverage points. Very concerned about plot in row 4, column 1.

Please fix mislabeled figures and tables, and improve the resolution of figures to improve readability. It is especially important to see the estimates of R2.

IV.I. Comments on Appendix A: Survivals (S_R), SAR by Study Category, TIR, and *D* for Snake River Hatchery and Wild Spring/Summer Chinook, Steelhead, Sockeye, and Fall Chinook

There have been no major changes in Appendix A. Values for 2022 have been added and overall averages or totals have been updated.

V. ISAB Appendix: Suggested Topics for Further Review 2011-2022

ISAB 2022-1, pages 5-7

1. Given that the Council's SAR targets are generally not being met, this could imply that the populations are more or less destined for functional extirpation sometime in the future. Explain factors related to attaining the recommended SARs with respect to the suite of actions implemented under the Fish and Wildlife Program.
2. Although the CSS is an empirical modeling effort, can the FPC and CSS Oversight Committee expand upon previous analyses to identify further evaluation and data needed to address the "breaching" proposals for the four lower Snake River dams more fully? Is breaching an all or nothing proposition, or can significant gains be expected with fewer dams being breached?

ISAB 2021-5, pages 4-7

1. Provide a more robust introduction section that includes a summary of major findings, highlights new analyses, and describes recommendations for potential management applications of findings. Describe changes in annual report structure from year to year, including why chapters and analyses were dropped or added.
2. Describe major applications of the CSS data that have been published or reported over the last few years and briefly highlight the important findings that are based on CSS data.
3. Consider recent analyses conducted outside of the CSS to identify possible new analyses that would inform issues raised by these external analyses. Step back, decide on the core results that need to be presented, identify the major uncertainties in the results and how these could be addressed.
4. Explore analytical methods to adjust for biases for smolts captured and tagged at Rock Island to maintain a longer period of information.
5. Address the unusually high mortality rates of subyearling Chinook in the MCN-BON reach and include major recommendations in their Conclusions.
6. Form a working group to explore how newer computer technology could reduce the human cost of updating and reporting the CSS report.

ISAB 2020-2, pages 3-7

1. Expand the annual report's introductory section to highlight 1) an overall summary for the survival of Chinook salmon, steelhead, and Sockeye salmon in the Columbia River

basin and how the SARs for the year compare to the long-term means, 2) new analyses included in the report, 3) major changes that may signal emerging management concerns, and 4) major recommendations for management of the hydrosystem that substantially alter or reinforce previous decisions or concerns.
2. Consider ways to address the spatial and temporal aspects of the effects of total dissolved gas (TDG) on acute and long-term survival, as we also recommended in 2019.

ISAB 2020-1, Review of 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), pages 5-6:

1. Perform a sensitivity analysis to investigate the impact of climate change for potential future flow regimes.
2. Compare results between different types of flow years and include demographic and other stochasticity in the models so that year-to-year variation in the output measures is more reflective of the response from different operations.
3. 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). Collaborate on joint analyses and use a common data set to resolve this issue.

ISAB 2019-2, pages 3-4:

1. Include information about the effects of mini-jacks on estimates of SARs and other relevant parameters.
2. Investigate implications of very low smolt-to-adult survivals (SARs) to hydrosystem operation alternatives and explore whether there is enough information to estimate how much improvements in habitat and other "controllable" aspects of the hydrosystem are needed to improve SARs.
3. Continue the work on the integrated life-cycle model looking at smolt-to-adult survival.
4. Continue to model adult salmon and steelhead upstream migration and consider adding information on individual covariates.
5. Consider ways to address the spatial and temporal aspects of the effect of TDG on survival.
6. Continue work on methods to estimate numbers of outgoing smolts at Bonneville.

ISAB 2018-4, pages 3-6:

1. Develop models for multiple populations that include combined and interactive effects.

2. Use the life-cycle models to investigate potential benefits on survival of management actions such as spill modification.
3. Expansion of ocean survival estimates to additional populations.
4. Include an analysis of mini-jacking and impact on SARs.
5. Include a more in-depth analysis of the PIT/CWT tagging experiment.
6. Improve the model for estimating abundance of juveniles at Bonneville.

ISAB 2017-2, pages 2-5:

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. Work on testing assumptions was suggested.
2. Include other important processes in the life-cycle models such as compensatory responses and predator control programs.
3. Elucidate reasons for shifts in the age distribution of returning spring/summer Chinook Salmon.
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.
5. The (new) modeling of adult survival upstream of Bonneville should be continued and improved to identify the limiting factors to adult returns.
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.

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 Salmon declines of four and five-year olds and increases in three-year olds.

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.

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

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

ISAB 2012-7, pages 2-3:

1. Evaluate if the NPCC's 2-6% SAR goals and objectives are sufficient to meet salmonid species conservation, restoration, and harvest goals.
2. Development of technology to improve PIT-tag recovery in the estuary.
3. Review estimation methods for smolt survival below Bonneville Dam through the Columbia River estuary using PIT-tags, acoustic tags, and other methods.
4. Examine measurement error in SAR estimates associated with PIT-tags.

ISAB 2011-5, page 2:

1. Influence of mini-jacks on SARs.
2. Effects that differential harvest could have on the interpretation of hydropower, hatchery, and habitat evaluations.
3. Extent to which PIT-tag shedding and tag-induced mortality varies with species, size of fish at tagging, tagging personnel, and time after tagging.

VI. References

Note on references to ISAB and CSS reports: The review above includes dozens of references to ISAB reports and CSS reports. Full citations are provided below only for the most recent ISAB reviews and key CSS reports. Hyperlinks are provided for ISAB reports. All ISAB reports referenced are available at https://www.nwcouncil.org/fish-and-wildlife/fw-independent-advisory-committees/independent-scientific-advisory-board. CSS reports are available at https://www.fpc.org/documents/Q_fpc_cssreports.php.

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