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SCIENTIFIC
ADVISORY BOARD

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) 2025 Draft Annual Report

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ISAB 2026-1
October 16, 2026

ISAB Review of the Comparative Survival Study (CSS) 2025 Draft Annual Report

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ISAB Review of the Comparative Survival Study (CSS) 2026 Draft Annual Report

I. Background

The Columbia River Basin Fish and Wildlife Program calls for a regular system of independent and timely science reviews of the [Fish Passage Center's](#) (FPC) analytical products. These reviews include evaluations of the draft annual reports for the Comparative Survival Study (CSS). The FPC 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, and during their upstream migration as returning adults based on detections of salmon tagged as smolts (e.g., smolt-to-adult return: SAR). 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.

The ISAB and ISRP have reviewed CSS analyses and proposals periodically since the project's inception in 1997, and the ISAB has reviewed draft CSS reports annually beginning sixteen years ago with the evaluation of the CSS's draft 2010 Annual Report.¹ This ISAB review of the [2026 Draft CSS Annual Report: Comparative Survival Study of PIT-tagged Spring/Summer/Fall Chinook, Summer Steelhead, and Sockeye](#) is thus the ISAB's seventeenth consecutive review of CSS annual reports. ISAB reviews have evaluated the analyses in the CSS reports, noted the usefulness and applicability of the CSS's analyses, and made suggestions for improved methods, interpretations, and presentation of results.

CSS annual reports are mature products, containing analytical updates that use the most recent data available. Many of the draft reports' chapters include methods that were reviewed in previous ISAB reports. The ISAB reviews those methods for any changes and, as more data are acquired, whether new patterns in the results are emerging. The passing years may also bring scientific advances and new analytical approaches, leading to new conclusions, and these new analyses and chapters are now the primary focus of ISAB reviews. The ISAB appreciates the CSS's detailed responses to suggestions provided in previous reviews (e.g., Appendix H).

¹ See draft annual report reviews: [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](#), [ISAB 2022-1](#), [ISAB 2023-2](#), [ISAB 2024-3](#), and [ISAB 2025-3](#). For early reviews, see [ISAB 1997-2](#), [ISAB 1998-1](#), [ISAB 2006-3](#) and the ISAB and ISRP Review of the CSS Ten-Year Retrospective Summary Report ([ISAB/ISRP 2007-6](#)).

The ISAB recognizes and appreciates that there are financial, contractual, and staff availability constraints on the preparation and content of the CSS annual reports. We do not expect the CSS to respond immediately to all new requests for further analyses. The ISAB's approach is to assemble comments and suggestions into this single review document. Some suggestions will be important or easy to implement immediately while others may become useful later as analyses related to specific comments evolve and are revisited in subsequent annual reports. The complete listing of ISAB comments and suggestions is intended to help the CSS start to explore issues that are likely to emerge as critical topics in the near future.

To help understand the historical context of CSS activities, the ISAB refers readers to Chapter 7 of the [CSS Final 2022 Annual Report](#) "Hydrosystem Changes and Their Influence on CSS: Spill" and Chapter 8 of the [CSS Final 2025 Annual Report](#) "Have We Followed the PATH?" The ISAB also continues to suggest that the CSS include those chapters as electronic appendices in future annual reports. The ISAB found those chapters very useful for our review, especially for new readers.

This ISAB review begins with an overview of the latest report's findings (Section II), followed by suggested topics for further CSS review (Section III). The review then provides primary, minor, and editorial comments on each chapter of the [draft 2026 CSS Annual Report](#) (Section IV). Primary comments are those that could affect the analytical methods and results, while minor comments are mostly technical suggestions intended to improve existing analyses and increase clarity in the methods and the presentation and interpretation of the results. Editorial comments are related to grammar and consistency of the text, tables, and figures.

II. Summary

The [draft 2026 CSS Annual Report](#) includes the following chapters and appendices that the ISAB reviewed:

- Chapter 1. Introduction
- Chapter 2. Adaptive management evaluation of changes in hydrosystem operations on Chinook Salmon and Steelhead
- Chapter 3. Effects of the in-river environment on juvenile travel time, instantaneous mortality rates, and survival
- Chapter 4. Patterns in Annual Overall Smolt-to-Adult Return rates (SARs)
- Chapter 5. Upstream Migration Success

- Chapter 6. Examining Heterogeneity and Factors that Influence Rates of Smolt-to-Adult-Return for Spatially Dispersed Stocks of Spring Chinook Salmon and Steelhead in the Columbia River Basin
- Chapter 7. Steelhead Overshoot and Fallback Rates
- Chapter 8. Reassessment of Juvenile Migration Timing and Adult Return of Snake River Spring/Summer Chinook Salmon and Steelhead
- Chapter 9. Impacts of Water Transit Times and Flow on Water Temperature in the Snake and Columbia Rivers
- Appendix A: Survivals (SR), SAR by Study Category, TIR, and D for Snake River Hatchery and Wild Spring/Summer Chinook, Steelhead, Sockeye, and Fall Chinook
- Appendix B: Supporting tables – Annual Overall Smolt-to-Adult Returns

Chapters 1-7 of the 2026 draft CSS Report are largely updates of the 2025 CSS Report. Chapter 8 covers the topics of bypass effects and delayed mortality, and although a chapter on this was not included in the 2025 CSS Annual Report, the topic was covered in past CSS reports, most recently in 2023. Chapter 9 is a new analysis of the impact of various meteorological variables and water transit time on water temperature.

In this section, we summarize the content and some of our Primary Comments on chapters 2, 6, 7, 8, and 9. These chapters either had significant changes from earlier years or are new in 2026.

CSS Chapter 2. Adaptive Management Evaluations of Changes in Hydrosystem Operations on Chinook Salmon and Steelhead

Chapter 2 presents an adaptive management evaluation of changes in hydrosystem operations on spring-summer Chinook salmon and steelhead. This chapter provides updates to previous models characterizing variation in five response metrics: juvenile fish travel time, juvenile survival, ocean survival, smolt-to-adult return (SAR) survival, and the transport:in-river ratio (TIR). The figures and tables are updated to include 2025 data. The models themselves were reviewed in earlier ISAB reports. A new variable this year is an index of walleye predation. In addition, the use of the term “regime” needs some clarification, as it is used for periods of hydropower operations and to delineate time periods of different fish survival. The ISAB comments on how these regimes (as well as possible regimes based on fish predation) could be incorporated into the adaptive management framework and process.

CSS Chapter 6. Examining Heterogeneity and Factors that Influence Rates of Smolt-to-Adult-Return for Spatially Dispersed Stocks of Chinook Salmon and Steelhead in the Columbia River Basin

In Chapter 6, the long-term time series of SARs as estimates of survival are analyzed based on synthesized data for groups of salmon and steelhead across the Columbia River Basin. The analysis (termed basin-scale population model or meta-analytic approach) examines patterns of survival (SARs) and the variables that affect the SAR estimates of these groups. Chapter 6 also appeared in the 2024 report, and many of the ISAB's primary comments on the 2024 draft were addressed in the CSS 2025 report, including questions regarding the inclusion of jacks and the effects of the water transit time (WTT) and powerhouse passages (PITPH) covariates on the variance structure. However, this chapter remains a work in progress, and the authors should carefully review and revise it to integrate the various changes more consistently (e.g., using consistent notation across subsections, revising the variance components used in the models, and integrating model assessment with the other models used).

CSS Chapter 7. Steelhead Overshoot and Fallback Rates

Chapter 7 is a revised CSS analysis that examines the migration routes of wild adult steelhead in the Columbia River Basin, including those that migrate past (overshoot) their natal tributary, those that eventually fall back to their natal stream, and upstream strays that were never detected in their natal basin. The combination of overshooting and failure to fallback represents a loss to the natal population. The ISAB recommends improving the analysis to give individual years equal weight (rather than weighting by sample size in each year) and suggests that a Bayesian model approach can be useful when the data are finely partitioned by spawning year and population groups to deal with small sample sizes in each partition. The degree of straying of natural-origin and hatchery salmon and steelhead that do not return to their natal rivers have been ongoing concerns for Columbia River Basin hydrosystem management, especially in the Snake River for the four lower Snake River dams ([ISRP 2023-1](#), [ISRP 2025-3](#)). The ISAB commends the CSS for investigating this important management issue and encourages them to refine and continue this analysis.

Chapter 8. Reassessment of Juvenile Migration Timing and Adult Return of Snake River Spring/Summer Chinook Salmon and Steelhead

This chapter investigates the impact of Bonneville passage timing (and other covariates) on the probability that a fish returns to Lower Granite Dam, using standard generalized linear and generalized linear mixed models (logistic regression). An analysis from 2009 is replicated,

expanded to include more years, and then extended with additional covariates. The ISAB has no major concerns about the analysis and provides recommendations to improve presentation and integrate findings from Chapter 6 into the Chapter 8 analysis. More discussion about the interpretation of the results is warranted. This chapter is rich with interesting and useful results that should be further explored (perhaps as part of next year's report).

Chapter 9. Impacts of Water Transit Times and Flow on Water Temperature in the Snake and Columbia Rivers

This chapter is a summary of recent reports authored by the FPC. The chapter uses a regression approach to estimate the change in water temperature between the upper and lower parts of reservoirs as a function of input covariates such as WTT and mean temperature. There is little model assessment, which is important because the regression model using daily values likely suffers from high autocorrelation in the residuals which is known to underestimate the standard errors and overstate the statistical significance of coefficients. A functional data analysis (FDA) regression approach may be a better approach to this type of data.

Importance of Information in CSS Reports

As stated in our previous CSS reviews, we strongly emphasize the importance of the CSS reports for effectively monitoring and evaluating salmon co-management and hydrosystem operations. It is important to avoid any tendency to consider the annual CSS reports to be just “more of the same” each year. With more than 28 years of data, the conclusions reached are extremely valuable because the uncertainties in the results can be estimated and outlier years identified. Moreover, annual data collection and analytical updates play a sentinel role by permitting standardized detection of changes within a contemporary period. The physical (e.g., PIT tag detection arrays) and human capacity and expertise added over the 28 years are invaluable, and the CSS annual reports provide an effective and useful resource to many involved with the Columbia Basin.

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.

Long-term data are particularly critical when assessing years with extreme conditions, including low flows, warm temperatures, and other atypical seasonal patterns. Such cases, at the edges of the distributions, are expected to occur with greater frequency with climate change, and long-time series are needed to capture enough of these uncommon conditions

to make reliable assessments. In other words, what seem like extreme conditions from the perspective of the historical record must be increasingly viewed as part of a new but poorly understood normal. Improving that understanding requires long-term datasets. The ISAB continues to emphasize the need to maintain the CSS's long-term monitoring and analyses and to increase sample sizes and enhance detection probabilities at the dams and in the lower Columbia River.

III. Suggested Topics for Further Review

Since 2011, the ISAB has suggested topics that warrant further CSS or regional review, and they are listed in this report's appendix. The 2026 draft CSS report incorporates many past ISAB suggestions, and we greatly appreciate the CSS's effort to respond to our past queries. As stated above and emphasized in previous reviews, we do not expect the CSS to respond immediately to new requests for further analyses, and we understand that some of the requests might be beyond the existing mainstem passage focused scope or budget of the CSS and might require expanding the CSS scope or coordinating with other entities. Although other entities could potentially undertake some of these analyses, the CSS is well positioned to do many of the analyses, building on their substantial body of work.

ISAB 2026 Suggested Topic 1: Two chapters have started to include information on an index of non-native fish predator abundance (Smolt Monitoring Program walleye counts). Smolt and adult salmon predation by avian, marine mammal, and fish predators is a significant management concern in the Columbia River Basin ([ISAB 2019-1](#)). Improvements to the existing monitoring programs to ensure standardized sampling frameworks that inform the status and trends of these potential predators of concern should be developed (see [ISAB 2016-1](#)). The ISAB recognizes that the CSS is not responsible for this index but encourages them to coordinate with the relevant agencies to improve the sampling design to ensure that useful information can be extracted.

ISAB 2026 Suggested Topic 2: Many chapters use information theoretic methods (AIC) to fit and rank multiple models on a particular dataset. The authors then use model averaged coefficients and variable importance metrics to help interpret the results of multiple models that fit the data reasonably well. However, in Frair (2026), this approach is criticized and the editorial team at the *Journal of Wildlife Management* (JWM) officially no longer recognizes coefficient averaging or variable importance measures as an acceptable practice due to the narrow conditions required for valid statistical inference.

The ISAB recognizes that model-averaged predictions are useful, and tables of model averaged coefficients (e.g., Table 5.4 and Figure 5.4) and variable importance charts (e.g., Figure 5.7) do have a role to play because they account for model uncertainty (rather than simply reporting the results from the “best” model), are simple to compute, and can be easy to interpret. Frair (2026) suggests using partial effect plots, and there are more advanced methods (e.g., Penalized Regression such as LASSO- or ridge-regression methods) that could be used. The CSS should review their methodology in light of Frair (2026) by, for example, using an example dataset that compares the results from the current methods and the alternative methods to better understand the limitations and advantages of the different approaches and to prepare to pivot away from methods no longer deemed acceptable in peer reviewed publications.

ISAB and CSS dialogue regarding the ISAB’s 2025 Suggested Topics for Further Review

In [ISAB 2025-3](#), we recommended the following four topics (*italicized*) for consideration in future analysis. Each suggested topic is followed by the CSS’s responses in blue font and short responses from the ISAB to the CSS responses in text boxes.

ISAB 2025 Suggested Topic: *The ISAB continues to call for the CSS and/or others to develop a focused assessment of the influence of climate-related and density-dependent factors on the marine survival of Columbia River salmon. Climate and density-dependence will increasingly play a role in influencing population responses to hydrosystem operation and management actions and further assessment will improve understanding of causal factors of trends in SAR values. The CSS’s past and ongoing (e.g., Chapter 6) analyses of the effects of climate on specific aspects of the life histories, including the life stage survivals and recovery of salmon and steelhead, is already available and could provide valuable content for future syntheses. The CSS could also undertake new analyses to illustrate specific important climate and density-dependent relationships identified in its previous studies and provide a context based on recent literature.*

CSS Response: *The CSS is a management-oriented project with a study design, which includes analyses of variables that account for climate (e.g., ocean conditions, in-river conditions, etc.). We are addressing climate-related variables within this scope of the CSS and we plan to continue to examine the climate-related factors influencing ocean survival and SARs in Chapters 2 and future versions of Chapter 6. These focused, contemporary analyses, using the full time series of available data and accounting for both freshwater and marine factors, provide the strongest basis for inference on climate-related effects on ocean survival and SARs. Past analyses, that relied on fewer years of*

data, have lower power to identify the most important climate-related factors and quantify their effects on ocean survival and SARs. Rather than a chapter with the general topic of climate-related factors on ocean survival, we think that the focused, contemporary time series analyses in Chapters 2 and future versions of Chapter 6 are the most appropriate location and format for conducting and presenting the results of those analyses.

ISAB 2026 Response to CSS's Response: The ISAB acknowledges the gains made by the CSS to understand the impact of early marine factors on SARs. The effects of marine factors occurring later in the ocean phase, such as interspecific competition with other salmonids, remains unresolved. The ISAB urges the CSS to continue to explore and expand their analyses that explicitly consider effects in the marine environment, including the role of competition with large and sometimes increasing numbers of hatchery-produced salmon (e.g., further analyses in Chapters 2 and 6).

2. *ISAB 2025 Suggested Topic: The current monitoring of the hydrosystem above Bonneville Dam is now well developed and mature. While studying fish in the estuary is somewhat beyond the current scope of the CSS, the ISAB recommends that the CSS conduct a brief review on the methods currently and previously used by others and potential new methods that could be used to better understand smolt and adult survival in the estuary below Bonneville Dam, in the North Pacific, and throughout the Basin.*

***CSS Response:** To some extent this request is beyond the scope of the CSS, and to some extent the CSS is already evaluating methods used by others to understand adult survival in the estuary, and the ocean. We have adapted new juvenile survival methods in the past two years (see Chapters 2, 3, 6, and 9).*

ISAB 2026 Response to CSS's Response: The ISAB appreciates the geographical boundaries of the CSS effort and encourages the exploration of the influence of ocean conditions as has been done in recent CSS reports. To the extent feasible, including key processes and habitats (areas) outside the currently specified scope would add important information to understanding hydropower effects in the context of the entire life cycle in this highly connected system.

3. *ISAB 2025 Suggested Topic: The ISAB appreciates the CSS analysis of overshoot, fallback, and straying, which is an extremely important management concern in the Columbia River Basin. The extent to which these are observed and their relationships to hydrosystem operations (and other management actions) warrant examination. As data sets and associated analyses become more robust, the CSS and others in the basin should be able to test the influence of hydrosystem operations on straying rates. We encourage the CSS to refine and continue these analyses and include other stocks and species, such as hatchery steelhead and spring Chinook salmon. Future analysis by the CSS or others could also examine overshoot and straying at a finer resolution within basins or around specific dams or river confluences.*

CSS Response: We plan to continue to develop and refine these analyses.

ISAB 2026 Response to CSS's Response: As the CSS continues these analyses, the ISAB suggests that the CSS expand the ongoing analyses to develop a stronger link of the results to management issues. These behaviors occur naturally and have been further affected by hydropower operations. This prevents easy identification of historical (natural) rates. How would overshoot, fallback, and straying affect the estimation of SARs and hinder the performance of hatcheries and recovery programs? Linking the ongoing analyses to evaluate the importance of overshoot, fallback, and straying to these types of management actions is a logical next step.

4. *ISAB 2025 Suggested Topic: The CSS increasingly uses multiple models (e.g., Chapter 9) to bound the uncertainty in predicted responses. Two general cases of multiple models seem to occur with the CSS analyses: (1) alternative assumptions and explanatory variables within a hierarchy of similarly structured (often statistical) models, and (2) models based on fundamentally different assumptions and thus they can appear to have little direct relationship to each other. Using multiple models can be a very powerful approach but is also a complex endeavor and can easily lead to mis-interpretations of results. The CSS should develop a "multi-model guide" to help standardize the approach and documentation (for example, see [ISAB 2023-1](#) for a review of multiple models used for Willamette Valley System analyses). This will allow readers to better understand analyses that use multiple models and to properly interpret differences and similarities in predictions.*

CSS Response: Chapter 9 includes a table and text to illustrate similarities and differences between two models. See Chapter 9 responses below for details.

ISAB 2026 Response to CSS’s Response: The ISAB appreciates the CSS responding to this suggested topic with additional information for specific analyses. Such multi-model analyses can occur in various chapters and likely will be used in new chapters. We encourage the CSS to continue on this path and develop guidelines so that all analyses that use multiple models, which the ISAB supports, do so in a consistent way in terms of presentation and interpretation of the results.

IV. Comments on Updated or New Analyses in the draft CSS 2026 Annual Report by Chapter

IV.A. Comments on Chapter 1. Introduction

Chapter 1 introduces the CSS Annual Report. It is very similar to the introductions in past CSS reports, reflecting the additional data collected, the resulting updated analyses, and any new chapters. The ISAB appreciates the chapter summaries and, as noted in a footnote above, the continued inclusion of the table (and the web table) showing the history of analyses and chapters over the years, as well as the authors’ previous positive responses to other ISAB suggestions for this chapter.

Primary comments

Page 1. It might be helpful to provide more explanation following the statement: *“The study was implemented to address the question of whether collecting juvenile fish at dams, transporting them downstream of Bonneville Dam (BON), and then releasing them was compensating for the effect of the Federal Columbia River Power System (FCRPS) on the survival of Snake Basin spring/summer Chinook salmon that migrate through the hydro-system.”* For example, a sentence can be added: *“Since its inception, the study has been broadened to include X, Y, and Z, because additional information and data are evaluated that do not necessarily fall under this specific umbrella but are germane to understanding better how the hydrosystem and its operations are affecting salmonids.”*

Figure 1.1. Please add a statement about the ways management actions could affect ocean survival to give a more complete picture as there are actions taken by resource managers that affect salmon in the ocean besides salmon harvest management. Some examples include decisions about hatchery production levels of pink salmon that are partly driving record-high ocean salmon abundance and affecting Columbia Basin salmonids through competition, and non-salmonid fisheries that affect salmon prey, competitors, and predators. Also, as the CSS has documented, the experience of smolts surviving their river migration phase influences their later performance in the ocean.

The section on PITPH² (middle paragraph on page 21 and top of page 23) provides useful information about the definition and importance of PITPH, which appears as a variable in many analyses in the other chapters. However, while the definition of PITPH seems simple in principle, the narrative description is confusing because it is not clear exactly how bypassed fish are handled and whether they are handled in the same way across all dams (e.g., surface vs. powerhouse bypass systems). PITPH is also described in various ways across chapters. Clear description of PITPH is important to ensure the covariate is used and interpreted properly. For example, the predictive equations for PITPH include flow and proportion spilled as covariates. If spill proportion is estimated using data on flow, a potential concern is that the use of a composite variable can lead to statistical issues due to multicollinearity between PITPH and flow itself as a covariate.

We suggest that the most recent work to derive the dam- and species-specific predictive equations for PITPH be cited in the glossary (i.e., Chapter 8 of the CSS 2023 report). In addition, a graphic showing the equation and the routes via spill and powerhouse (turbine and bypass) for one dam could help to explain this metric.

Editorial comments

The chapter covers a lot of ground from study objectives to methodology, terminology, and some results, but often these are mixed together. Future reports could benefit from an improved organization into separate sections that cover terminology and graphics, aspects of the study design (including maps), and sampling efforts across time and space.

² In the CSS's report glossary and on page 23, PITPH is defined as an "Indicator variable that indexes the relative proportion of fish passing dams via the powerhouse. The variable was developed to improve upon the average spill proportion index variable that assumed 1:1 spill volume to fish passage via spill. The PITPH variable accounts for greater than 1:1 spill passage efficiency that occurs at many dams particularly with the implementation of surface passage structures.

Figures 1.2-1.5. It is hard to pick out the location of the dams on these maps. Can the clarity be improved?

IV.B. Comments on Chapter 2. Adaptive Management Evaluations of Changes in Hydrosystem Operations on Chinook Salmon and Steelhead

Summary from 2026 draft CSS Annual Report: *“Chapter 2 presents an Adaptive Management evaluation of changes in hydrosystem operations on spring-summer Chinook salmon and steelhead. This chapter provides updates to previous models characterizing variation in five response metrics: juvenile fish travel time, juvenile survival, ocean survival, smolt-to-adult return (SAR) survival, and the transport:in-river ratio (TIR) using all years of available data. The chapter also presents analyses comparing these response metrics across four spill management regime periods, 1998-2006, 2001, 2007-2019, and 2020-2025.”*

Chapter 2 presents an Adaptive Management evaluation of changes in hydrosystem operations on spring-summer Chinook salmon and steelhead. The figures and tables are updated with 2025 data. The models have been reviewed by the ISAB in previous years, and the CSS has been very responsive to our previous review comments. A new variable this year is an index of walleye predation. The use of the term “regime” needs some clarification, as it is used for periods of hydropower operations and for delineating time periods of different fish survival rates. The ISAB also comments on how these regimes (as well as possible regimes based on fish predation) should be incorporated into the adaptive management framework and process.

Primary comments

The ISAB commends the CSS for exploring potential new factors affecting survival of juvenile Chinook and steelhead and attempting to include an index of walleye abundance in the analyses. The ISAB recognizes that the new results are preliminary and has a number of comments and suggestions related to this aspect of Chapter 2. Overall, the adaptive management framework as applied to hydrosystem operations (spill and powerhouse-passage) demonstrates an effective adaptive management program that leads to improvements in management.

1. Adaptive management framing and the emerging ecological regime shift

The chapter currently uses the word *regime* in two different ways: (1) to define the four periods of differing spill and powerhouse-passage conditions, and (2) in the phrase *regime shift* to describe an apparent change in juvenile salmon and steelhead survival that may reflect broader ecological changes. These are different concepts. The first is an operations focused categorization of different water management frameworks and the second is a hypothesized ecological change in the system. The use of the same word “regime” in these two contexts can create confusion that can be addressed editorially.

More importantly, the persistence of the discussion of an ecological regime change across three CSS Annual Reports (2023, 2024, 2025) should be considered as a new phase in the ongoing adaptive management process. This distinction is more than editorial because it would also strengthen the adaptive-management framing by showing how unexpected biological responses can generate new hypotheses about underlying ecosystem mechanisms. In some ways, this ecological regime shift may represent a slow variable change in the system related to press disturbances from landscape-scale changes (e.g. changing climate) compared to a fast variable response related to hydrosystem operations. These distinctions are important because operational capacity may be more able to integrate fast variables into an adaptive management program than slow variables.

Framing the hypothesized ecological regime shift within the adaptive management context may help clarify how these newer hypotheses relate to the original hydrosystem conceptual models and where the effects may be confounded. The existing CSS adaptive management framework is well suited to the original purpose of evaluating biological responses to changes in spill and hydrosystem operations. The five response metrics have now been measured across contrasts in spill and powerhouse passage, and the analyses in Chapter 2 show how those contrasts can be used to evaluate fish responses under changing operational conditions. However, the hypothesized ecological regime shift is a different adaptive management question. Recent changes in juvenile fish survival have generated new hypotheses within the CSS involving changing habitat conditions and increasing abundance of predators including invasive walleye. Identification of new hypotheses is an important outcome of the existing adaptive management process and should be recognized and encouraged. This is because the observations from the original monitoring program have identified responses in fish survival that are not completely explained by the original conceptual model that has informed the adaptive management program, and these responses have now generated new hypotheses that could be included within the adaptive management process. That is an example of an adaptive management success.

The ISAB suggests that the chapter explicitly recognizes the ecological regime shift idea as a second, overlapping adaptive-management loop or next phase of the program. The original adaptive management loop was asking: “How do salmon and steelhead respond to alternative spill and hydrosystem operations?” While the new ecosystem regime shift loop is asking: “What other ecosystem processes are contributing to recent changes in salmon survival, how do those processes interact with hydrosystem operations, and what management contrasts are needed to separate the competing hypotheses?”

Critically, the ecosystem regime shift hypotheses can be viewed as evidence that the adaptive management process is working as designed and the monitoring and evaluation framework presented in the CSS has generated new understanding and revealed an important uncertainty. The next step is to update the conceptual model and monitoring or experimental design so that the new competing hypotheses can be tested and evaluated.

2. Walleye, confounding, and adaptive-management design

The walleye analysis in Chapter 2 illustrates why the distinction between hydrosystem management regimes and broader ecological regimes is important. Adding a walleye covariate may improve model fit and identify an important association, but it does not show whether the relationship is caused by walleye predation, changes in spill or PITPH, time, or other ecosystem changes. During the recent period when walleye counts increased, spill has generally increased and PITPH has declined, making these effects difficult to isolate. In addition, the available walleye data are incidental counts from the Smolt Monitoring Program at a specific location, rather than a system-wide population abundance index. The report should clarify what these data represent and whether other information indicates that walleye abundance has increased more broadly across the system.

From an adaptive management perspective, this lack of contrast is not just a statistical problem. It also reflects how the existing adaptive management program was designed. The existing program was built around uncertainties in how fish populations responded to changes in the hydrosystem and has taken advantage of contrasts in hydrosystem operations (see Section 1: Hydrosystem Operations and the Adaptive Management Framework). These contrasts are what make it possible to evaluate the original spill/PITPH hypotheses.

The identification of the walleye and broader ecosystem hypotheses (including marine survival) can be viewed as an adaptive management success. Learning from the existing program has identified new questions that were not central to the original framework. These new hypotheses show that the adaptive management program has generated enough

understanding to recognize that additional or other ecological processes may need to be considered.

These emerging hypotheses, including the ecological regime shift, will likely require different kinds of contrasts to distinguish among possible mechanisms that could influence juvenile salmon survival. For example, separating spill and walleye effects ideally requires information spanning combinations such as high spill–low walleye, high spill–high walleye, low spill–high walleye, and low spill–low walleye. Some of those combinations are likely rare or not possible from a management perspective (such as increasing walleye populations experimentally as a treatment), then the independent effects of spill and walleye cannot be easily separated by including both variables in the same regression model.

The ISAB recommends that the CSS report use the ecosystem regime-shift hypotheses as the next stage of the adaptive management process. A useful next step would be to develop a new conceptual model (i.e., box and arrow diagram) of the hypothesized key drivers of salmon survival, including both the hydrosystem and more emergent factors identified by the CSS. This conceptual model could then be used to identify what types of contrasts, monitoring data, or management actions would help distinguish among the competing hypotheses involving water transit time, temperature, changing habitat conditions, and invasive fish species including walleye. It would also be useful to determine whether these factors are changing together because strong correlations among the factors will make it difficult to learn from the existing data. The goal is not to replace the existing adaptive management program, but to build on its success. The existing program has produced learning that now points to a broader set of questions. Developing those questions as testable hypotheses is an extension of the existing adaptive management process.

3. Technical Issues with the walleye variable

In the Discussion, the potential usefulness of the new walleye index needs further clarification. The chapter states, “The increase in walleye is likely indicative of other recent changes in the freshwater environment that could negatively affect the survival of juvenile salmonids.” This could be interpreted as meaning the walleye index is correlated with some causal factor other than the walleye abundance but could also mean that other factors are exacerbating the potential effects of walleye, potentially synergistically. Relatedly, the chapter states, “Invasive predatory fish populations could be affected by other ecosystem changes as well. Higher water temperatures could result in increased abundance of warm-water invasive predatory fish such as walleye.” But walleye could also just have a greater impact due to greater consumption, bioenergetically possible (and necessary) under warmer temperatures.

Setting aside these conceptual issues of how a walleye variable should be interpreted and how it should be included in analyses where it is potentially confounded with other variables known or thought to be important, the ISAB shares the CSS's concerns about the specifics of how the walleye variable is created. The CSS cautions that the walleye variable has some limitations:

“This preliminary trial should be considered with a significant degree of caution for multiple reasons including that these walleye counts do not take sampling rates into account. Therefore, the SMP incidental sample of juvenile walleye at McNary and Little Goose cannot be considered a population abundance index of walleye because SMP sample counts do not account for sample rates or changes in project operations, which influence the number of fish routed into the juvenile bypass systems.”

And then again in the Discussion on page 61:

“Until further evaluation has been completed, caution must be used when considering any implications of these results. However, the inclusion of incidental sample catch of juvenile walleye at McNary and Little Goose improved model fit (i.e., lower AICc, higher R^2 , less patterned residuals) and helped explain patterns in variation in juvenile survival, especially for Chinook salmon, so we included it in the models for this year as a preliminary trial.”

A follow-up on the reference as to where to find the data for incidental catch of juvenile walleye at McNary and Little Goose dams within the Smolt Monitoring Program does show that such caution is warranted. The effort to collect a daily count of juvenile walleye data is highly variable among months within a dam and very different for each dam. For example, at McNary Dam in 2023, daily counts of juvenile walleye were available for a total of 11 days spread over 2 months (July, August), but the counts there in 2025 were available for a total of 18 days spread over 3 months (July, August, September). Sampling intensity was very different at Little Goose Dam where in 2023 daily counts of juvenile walleye were available for a total of 98 days spread over 7 months (May-November), and the counts there in 2025 were available for a total of 93 days spread over 9 months (March-November). These temporal issues are then compounded by using the raw total count for each year, without considering incomplete sampling of the unknown distribution of total catch expected throughout the 365-day cycle, such as one might do with a statistically valid stratified sampling plan.

Caution was appropriately advised about using the walleye data, but the reader has been given little further information on the validity of the data used. A reference was given for finding the walleye data, something that was not provided for the ichthyoplankton community composition index data. The initial attempt to include walleye catch data and the ichthyoplankton community composition index in this year's CSS report is a good start. The ISAB

recommends that both datasets be more fully described including collection methods, spatial and temporal coverage, how the different indexes are calculated, and how methods and effort change over time and space. This would help the reader better understand the data and their limitations to inform the reported trends or analyses.

Furthermore, using catch of juvenile walleye as a proxy for the standing crop of walleye capable of predation on smolts may produce misleading results. For example, walleye are well known for variable strength of year classes, largely determined after hatching and before they reach the size where they would prey on smolts (Nate et al. 2011). Therefore, the number of juveniles caught in a given year may or may not be directly related to the predator population of walleye and the smolts lost to predation in that particular year.

4. Other covariates

The adaptive management framework and process could be helped by creating and updating a more comprehensive list of potential covariates that might be important drivers of the biological responses of interest. Each covariate should be described in sufficient detail, with an explanation of its purported mechanism, references from the literature, details of its calculation and data underling it, caveats, and ways it might be included in analyses. Correlation plots could be presented to identify any covariates, and time series plots could be useful in identifying short vs longer-term patterns as hydrosystem operations and landscape scale changes have occurred.

5. Quality of model fits

The fits of the models are satisfactory but not great (e.g., Figure 2.3, 2.4, 2.9, 2.10, 2.13, 2.15, 2.16, 2.18, 2.19). It would be useful to discuss possible explanations. The ISAB suggests that the CSS add a section in this year's report and briefly describe plans over the next few years to revisit this analysis and explore how the models could be improved. This can involve revising existing covariates, considering more covariates, and trying alternative statistical approaches. How will the walleye covariate be explored to establish how models that include it should be interpreted? Similar comments apply to the ocean covariate(s). Ocean variables have been discussed in previous ISAB reviews multiple times, and the authors seem hesitant to explore this important covariate. Perhaps a strategy is needed on how to explore these newer covariates so the confidence level in their use can be factored into the interpretation.

Approaches can be used to bound model results rather than requiring a high level of confidence before they can be included. Section 2 has some ideas that could serve as a start on this.

Minor comments

p. 61. Just under equation 2.2, the walleye variable was found to be not useful in predicting FTT for Chinook or steelhead. This was also noted in the discussion on page 61. A limitation of measuring FTT is "survival bias," i.e., only those fish that survive to be measured downstream can be used to measure FTT. See p. 66 of Chapter 3 for a longer discussion of this. The potential survivor bias should be mentioned here.

The Chapter 2 conclusion that the long-term adaptive management program provides useful evidence regarding the biological responses associated with changing hydrosystem operations is a program strength. The ANCOVA analysis explicitly evaluates the five response metrics across operational periods while accounting for WTT and other modeled covariates. Adding text to highlight this adaptive management program success is recommended.

p. 44. Please add citations to the statement, "The removal of avian predators on fish may have also benefited walleye population growth." Also, a potential mechanism for the relationship is not offered until page 61 and this should be presented with the statement.

p. 61. The statement, "Higher water temperatures could result in increased abundance of warm-water invasive predatory fish such as walleye," conflicts with the general notion that walleye are a cool-water fish species. Please clarify.

A description of all the oceanographic variables used in the analyses – including their time frame – would be helpful to include in a table or appendix. It appears that all of the variables used pertain to the salmon and steelhead's early ocean residency and not to the entire ocean period. Please clarify this in the text.

It could also be helpful in future analyses to consider ocean variables affecting later life at sea. Since the species under consideration in this chapter migrate to the ocean, measures such as ocean temperatures and primary production in different seasons, regions and years could be informative, as could information on pink salmon abundance (e.g., stocking levels or other measures of pink salmon year-class strength). While these factors are beyond hydrosystem managers' control, poor ocean survival would negatively influence the performance of Columbia Basin Chinook and steelhead, and such effects are important for management.

Regarding the statement, "These decreases in spill during the evening and nighttime hours likely resulted in greater powerhouse passage proportions than would be predicted based on

the PITPH equations,” further investigation of the magnitude or impact of this bias might be useful for future reports.

In general, it would be useful to present conditional R^2 values to assess the amount of variance accounted for by random effects.

From Figure 2.20, it looks like transportation is generally beneficial, but there is little acknowledgement or discussion of this. Please add a short statement about this apparent result to the text.

Editorial comments

p. 45. Regarding the statement, “The Walleye incidental sample variable increased AICc, so were not included to model FTT for either Chinook salmon or steelhead,” please add information about how much the AICc increased (i.e., 5+, 10+ etc.).

Table 2.4 and 2.5. Indicate in the table legend what FW means (freshwater base model) and what terms are included. Why does the left column exclude FW+ while the right column includes FW+ in the actual table values? The freshwater base model was also used in the models on the left column. Table 2.4 uses “Freshwater only” while Table 2.5 uses “Freshwater” to indicate models with no additional covariates. Please make the tables more self-explanatory and consistent.

Table 2.11 and 2.13. Please explain in the Table legend how to interpret that there are two letters (b,c) for the final row.

Table 2.12. Why as the letter “b” used (rather than “a”) when there was no evidence of a regime effect?

Many figures are now missing tick marks that were present in previous annual reports. For example, Figures 2.3 and 2.4 need tick marks on the X-axis to show where, for example, 2003 is located. See Figure 2.2. for an example of good labeling.

Our earlier comment about the different uses of the term “regime” could be resolved by referring to the four operational categories consistently as spill-management periods (recognizing this is a change from previous use) rather than spill-management regimes. The phrase “regime shift” would be used for the broader ecological phenomenon as this is the standard ecological phrasing. This distinction would help clarify that the apparent regime shift is not synonymous with the most recent spill management period but that it instead may reflect processes occurring within and maybe across management periods, including slower water

transit time, changing reservoir conditions, temperature, and invasive predators including walleye.

Revise “The index of the relative proportion of fish passing dams via the powerhouse (PITPH)” to ensure that PITPH is represented as a number of dams (not a proportion of fish). In general, it may be useful to refer to PITPH when considering a single dam and Σ PITPH when considering multiple dams.

Check citations to Figure 2.11 and Figure 2.12.

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

Summary from 2026 draft CSS Annual Report: *“Chapter 3 updates the time series of data on juvenile travel time, instantaneous mortality, and survival with data from 2025. Survival estimates continue to use the Bayesian hierarchical modeling framework that was first introduced in Appendix I of the 2024 CSS Annual Report (McCann et. al., 2024). Fish travel time models evaluate the effects of water transit time, the probability of powerhouse passage, and ordinal day, with an additional regime-shift indicator included for yearling and subyearling Chinook in the Lower Granite to McNary Dam (LGR-MCN) reach. Models of instantaneous mortality assess water transit time, water temperature, the probability of powerhouse passage, and original day, again including the regime-shift indicator for yearling and subyearling Chinook in the LGR-MCN reach. The species evaluated include juvenile yearling Chinook salmon, subyearling Chinook salmon, sockeye salmon, and steelhead as they migrate through the reaches from LGR-MCN and McNary Dam to Bonneville Dam (MCN-BON). The Bayesian methods allow estimation over multiple cohorts in a single season, borrowing information from those cohorts with more data to inform estimates for cohorts with less data. This is especially important in recent years because of very low detection probabilities in reaches potentially causing bias to reach survival estimates.”*

This chapter is an update from previous annual CSS reports that the ISAB has reviewed extensively since 2009. Most of the text is identical to recent CSS Reports, and most of the

figures are identical to previous graphs in this chapter but are updated with 2025 data. The ISAB appreciates the CSS's responses to previous ISAB reviews and offers only minor and editorial comments on this draft 2026 CSS report chapter plus some recommendations to consider in future work.

Suggestions for future work

The Bayesian approach provides the capability to borrow across cohorts, and the ISAB encourages the CSS to continue to try this and other approaches for evening out sample sizes.

The analysis of survival and travel time should be done in a single analysis of the PIT tag capture histories along with the covariates of interest. This would overcome issues that arise from using model-based estimates from the mark-recapture analysis as data in further statistical analyses. Moreover, capture-recapture models incorporating both spatial and temporal variation can overcome issues of assigning covariate values to individual capture histories and the censoring problem of estimating travel time from survivors when mortality depends on travel time as well as distance. Also, teasing apart the temporal and spatial components of mortality could be informative. Until recently, capture-recapture analysis involving both spatial and temporal variation was difficult if not impossible, but recent and developing advances in modeling software and computing power will make this increasingly feasible. For example, the USGS Columbia River Research Laboratory has developed the nimbleTIMS R package that will be released in late 2026 as a USGS data product that could be useful. The ISAB recommends that the CSS monitor these developments and consider conducting future analyses within such a framework. Developing a robust and interpretable fish predator index will be a continuing need for several of the analyses.

Minor comments

p. 91 and Figure 3.27. This chapter presents the observed count of walleye, but unlike other chapters, does not include a walleye variable in any analysis as a predator index. A brief explanation should be added explaining why the walleye covariate was not included in this analysis.

The effect of temperature on mortality is likely non-linear. Consider showing a plot of mortality rate versus temperature. Also, could variation in mortality rate that is being attributed to ordinal day (especially when transformed) be absorbing variation that could be explained by temperature? This might be examined by comparing a model using ordinal day (linear or quadratic) but not temperature to a model with temperature (linear, quadratic or perhaps a spline) and not ordinal day.

It would be helpful to include a model comparison table along with the variable importance plots.

In addition to describing what was done with correlated predictors, it would be useful to show their correlations in a multi-correlation plot. How does excluding correlated predictors ensure model averages include “true relationships” (page 68)?

When discussing impacts of WTT, causation is sometimes attributed to its component parts (discharge and reservoir elevation/volume). To support these discussions, consider adding trends in discharge and reservoir elevations to the report.

In considering management actions to improve survival, can the analyses show how much FTT could be improved via manipulation of WTT?

Please add a brief explanation as to the reasons that fish experiencing more powerhouse exposures have slower travel times. Is this just due to time spent in powerhouse, or is there a carry-over effect?

All numerical predictors were standardized, which allows for better comparisons of variable importance. Please make it clear how effects were estimated (e.g., back-transformed to native units?).

In contrast to the narrative, it appears that TDG most often had moderate importance (often similar to the importance of WTT and PITPH). However, this is difficult to reconcile with the lack of any consistent effect on mortality. Please discuss this further.

Consider adding discussion of species differences in the drivers of FTT and survival.

Editorial comments

There are a few cases where “survival rate” is used rather than “survival probability.” Please be consistent in the chapter.

p. 64. “Multiple regression models were employed...” Given that the current set of models now include indicator variables and random effects, a more accurate description would be “Mixed-effect linear models were employed...”

p. 66. “All numeric predictors were standardized by subtracting...” It would seem that the regime indicator variable was not standardized and is an exception?

Table 3.1. Just below Table 3.1, there is a paragraph that shows exclusions of estimates by reach from 1998 to 2025. Perhaps a table of exclusions would be a better way to list exclusions rather than as text.

Table 3.2. Rather than using “untransf,” perhaps use “linear” to contrast with “quadratic” in other lines in the table.

Figure 3.10 and 3.11. Rather than using “untransformed” to describe the ordinal day variable, perhaps use “linear” to contrast with “quadratic” in other models.

Note that if the quadratic terms are computed using the standardized ordinal day (rather than standardizing the day² separately), the estimate of the linear coefficient is more valid across linear and quadratic models.

PITPH is sometimes referred to as a probability or rate, but it is a number. Please be consistent when referring to PITPH.

p. 65. Revise “Temperature and WTT were calculated based on fish travel time” for clarity.

p. 65. Please elaborate on “To account for differences between Columbia River and Snake River flows, adjustments were made for the McNary pool.”

IV.D. Comments on Chapter 4. Patterns in annual overall SARs

Summary from 2026 draft CSS Annual Report: *“Chapter 4 updates the time series of overall smolt-to-adult return rates (SARs) for wild and hatchery salmon and steelhead populations from the Snake River, Mid-Columbia and Upper Columbia regions. Overall SARs of Snake River wild spring/summer Chinook and steelhead fell well short of the Northwest Power and Conservation Council’s (NPCC) 2% - 6% SAR objectives, while those from the mid-Columbia region generally fell within this range. For Snake River populations, none of the passage routes (in-river or juvenile transportation) have provided SARs within the range of the NPCC objectives; the relative effectiveness of transportation decreases as in-river conditions improve and survivals increase. SARs of wild and hatchery populations were highly correlated within and among regions, suggesting that common environmental factors were influencing survival rates.”*

The analysis of overall SARs for wild and hatchery salmon and steelhead populations from the Snake River and Mid-Columbia and Upper Columbia River provides important long-term data for the Columbia River Basin and management of the hydrosystem. The chapter topic has been presented in CSS reports dating back to 2002 and reviewed by the ISAB in the previous 16 years. Much of the text and the Conclusions are identical to recent CSS Reports, updated with 2025 data. The ISAB offers a suggestion for analysis and some editorial comments for the CSS authors to consider.

Minor comment

The ISAB suggests a short exploratory analysis that groups populations by their ocean migratory strategy (e.g., time of entry, general ocean rearing area). Such an analysis, if feasible, may provide information on how the ocean environment and how shifts in ocean migration in the future may affect SARs.

Editorial comments

p. 99. "...bootstrapped SARs and confidence..." The bootstrapping only applies to the confidence interval, so perhaps reword as "... SARs and bootstrapped confidence..."

p. 128. The SARs for Winthrop in 2023 were about "average," but the SARs in 2023 for Entiat were depressed. Any obvious reason why?

Table 4.2. Reduce number of decimal places given that standard errors are relatively large compared to the estimates.

Figure 4.26. Fix the R^2 to read R^2 .

p. 136. Please provide a reference for the statement: "In general, the relative effectiveness of transportation has been observed to decline..." and indicate that the survivals of 0.70, 0.84, and 0.50 were annual in-river survival from LGR to BON.

p. 137. Please explain what is meant by "comparable" in the sentence: "Increases in PIT-tag detection capability in the Columbia River upstream of MCN will make regional monitoring of overall SARs more **comparable** to the SARs for salmon and steelhead populations in the Snake River and Mid-Columbia regions."

p. 137. Consider revising the wording of the following sentence, "The high degree of inter-regional correlation in SARs of wild and hatchery spring and spring/summer Chinook populations indicates that common environmental factors are influencing survivals from outmigration to the estuary and ocean environments."

Perhaps: “The high degree of inter-regional correlation in SARs of wild and hatchery spring and spring/summer Chinook populations suggests that some common environmental and marine factors play a significant role in determining survival during outmigration and early residency in the ocean environment.”

p. 138. Specify which study in the sentence: “The study detected a reduction in PIT-tag retention over time after fish entered the hatchery holding ponds, likely due to females expelling PIT tags as eggs ripened.” Perhaps it is citing McCann et al. (2018)?

p. 138. Suggested wording change to the sentence “Lower survival rates for spring/summer Chinook were associated with warmer ocean **conditions**, reduced **upwelling in the spring**, reduced ichthyoplankton, slower river velocity during the smolt migration, and multiple passages through powerhouses at dams (Petrosky and Schaller 2010; Schaller et al. 2014, CSS Oversight Committee 2017).” Replace “conditions” with “temperatures”; add “spring” before “upwelling”; and delete “in the spring.” Whatever changes are made should also be done to the next sentence.

p. 138. In the sentence, “In addition, evidence of positive co-variation in mortality rates between the freshwater and subsequent marine-adult life stage for each species, **suggests** that factors affecting mortality in freshwater **partially affect** mortality during the marine-**adult** life stage (Haeseker et al. 2012).” replace “suggests” with “suggesting”, replace “partially affect” with “influence subsequent,” and delete “adult.”

IV.D.1. 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

Summary from 2026 draft CSS Annual Report: “**Appendix A** updates the CSS time series of juvenile in-river survival from LGR to BON (termed S_R), SARs by study category, and TIRs and D for Snake River hatchery and wild spring/summer Chinook, hatchery and wild steelhead, and hatchery sockeye. For an overview of the groups and years for which these data are available, and where to find them, see Table 1.4 (S_R), Table 1.5 (SARs by study category), and Table 1.6 (TIR and D). Prior to the 2012 CSS Annual Report, these data were presented in Chapter 2 (S_R) and Chapter 4 (SARs, TIR, and D). Patterns of TIR and in-river survival probability are also updated for Snake River wild spring/summer Chinook, steelhead, and fall Chinook.”

Appendix A updates the CSS time series of juvenile in-river survival from LGR to BON (termed S_R), SARs by study category, and TIRs and D for Snake River hatchery and wild spring/summer Chinook, hatchery and wild steelhead, and hatchery sockeye. Patterns of TIR and in-river survival probability are also updated for Snake River wild spring/summer Chinook, steelhead, and fall Chinook.

As stated in our 2025 review, the ISAB has no specific comments on Appendix A. The presentation of the information supports the concepts of open data and increases transparency.

IV. D.2. Appendix B: Supporting tables for Chapters 4 – Annual Overall Smolt-to-Adult Returns

Summary from 2026 draft CSS Annual Report: *Appendix B contains tables of the overall SARs that are presented in Chapter 4 along with methods for estimating overall SARs. For an overview of the groups, reaches, and years for which these data are available, and where to find them, see Table 1.7 (Snake River wild and hatchery Chinook), Table 1.8 (Snake River wild and hatchery steelhead and hatchery sockeye), Table 1.9 (Snake River fall Chinook), Table 1.10 (Mid-Columbia Chinook and steelhead), and Table 1.11 (Upper Columbia Chinook, steelhead, and sockeye).*

Appendix B contains tables of the overall SARs that are presented in Chapter 4 along with methods for estimating overall SARs. There have been no major changes in Appendix B. Values have been added and overall averages or totals have been updated.

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

Summary from 2026 draft CSS Annual Report: *“Chapter 5 updates the analyses of PIT-tag-based adult passage success from Bonneville Dam upstream. Date of passage, flow and spill conditions as well as juvenile passage experience (transport or in-river migrant) are used to predict passage success. The analysis estimates the relationship between temperature, juvenile transport, and salmon survival using models in generalized regression, mixed effects, and Cormack-Jolly-Seber (CJS) framework.”*

Chapter 5 continues analyses of upstream migration success and explanatory variables affecting upstream migration success for spring, summer, and fall Chinook salmon, sockeye salmon, and steelhead. For all but the last section, this chapter is an update from previous annual CSS reports that the ISAB has reviewed extensively over time with most text and figures being simple updates from previous years. The Chapter is well organized, and we appreciate the continual updating of this important series. We focus our comments on the Discussion and the Chinook Supplement.

Primary comments

The terms “conversion” and “cumulative conversion” need clear definitions (possibly mathematical). The difference between “conversion” and “survival” is not clear from the text.

In most time series, the survival fraction showed very little variability from year-to-year (e.g., Figure 5.1). The exception is sockeye salmon that showed significant inter-annual variation. For the time series with little variation, the effect sizes of covariates would be very small – how much effect can a covariate have if the range of survivals is 0.71 to 0.76 (exact ranges of values can be obtained from Tables 5.3, 5.6, and 5.9)? The range of survival fractions is also less for others (e.g., Figures 5.2 and 5.3) – all values near 1.0. It is possible the effects of individual covariates are larger, and there is some “cancelling effects” or an antagonistic interaction. The analysis, especially the interpretation, would be strengthened by (1) presentation of the time series of the covariates and any correlations among them, (2) reporting of effect sizes of the covariates, (3) justification for examining effects when there is little variation in the response variable, and (4) performing an extreme value analysis between years as was conducted in the Fall Chinook Supplement.

The Chinook Supplement appears (at least for this year) to replace the previous Sockeye Supplement and uses similar methods. It makes a good contribution of identifying the effect of very low flows on temperatures and thus passage survival.

The Summary Conclusions, as written, are confusing. Some patterns are relevant to the entire data series over years, but others appear relevant just to 2025 compared to previous years. The data series used should be made explicit in each summary bullet.

Two new conclusions have been added to the Summary Conclusions. The first was: “Adult fall Chinook experienced an unusually large passage delay that resulted in substantially reduced adult conversion between The Dalles and John Day. Three-day TDA-JDA conversion was 53.5%, 30.5% below the 2018–2024 proportion, and both year-end PIT-tag and count based conversion were well below historic averages (80% vs 93% avg, and 61% vs 76% avg, respectively).” Given

such dramatic declines in conversion, it would seem that the role of temperature would be stated in this bullet in the Summary Conclusions.

The second new conclusion in the Summary Conclusions was: “Passage declined as thermal conditions intensified. Modeling showed that the interaction between JDA tailrace temperature and the maximum ladder temperature differential was central to describing three-day conversion rates. Conversion rates declined from 76% at tailrace temperatures of 70.2°F to 45% at 73.5°F.” This is an extension of previous conclusions about temperature and should be moved up one bullet.

The Discussion could benefit from a discussion about whether there are actions that can mitigate the negative temperature effects.

By itself, a table like Table 5.2 (detections of PIT-tagged spring Chinook adults at four dams and “Above GRA”) is hard to interpret without accounting for detection probabilities. The ISAB understands from the 2025 response to the ISAB request to report detection probabilities that these are generally quite high, but nonetheless, it would be beneficial for readers to have that information in this table. The ISAB suggests the CSS consider ways to present the detection probabilities either in tabular (expanded Table 5.2 or a new table) or graphical form. As an example, the idea is to show how all detections at BOA would be new. However, detections at MOA are likely to be a mix of those detected at BOA plus some additional new detections. Furthermore, detections at ICH would likely be composed of those previously detected at BOA only, those detected at MCA only, those detected at both BOA and MCA, and those newly detected at ICH.

The ISAB notes several issues that should be addressed about the series of stock-specific tables showing “Model average effects and SE results...” followed by three reach-specific figures (e.g., Table 5.7 for Spring Chinook salmon followed by these averages and SEs graphically displayed in Figures 5.13, 5.14, and 5.15). Much clarity could be gained by using the same order of the variables listed in the table as listed in the figures (e.g., if Day is listed first in the table and is a non-“NA,” it should be the first effect listed in the subsequent figure). In addition, there is not always a 1-to-1 correspondence between what is in the tables versus what is graphed in the figures (e.g., “Spill” in the tables becomes “Spill max” in the figures; For SPCH BON_MCN in Figure 5.4, “Spill %” appears in the figure but is shown as “NA” in Table 5.4; for sockeye, “Age” appears as an effect in SO MCN_ICH in Figure 5.41 but is not listed in Table 5.16). Other inconsistencies between these tables and figures include: in FACH, Age appears as an important factor in the analysis presented in Figure 5.25, but Age does not appear to be a factor tested for in the analysis presented in Table 5.10. Same goes for SUST about the Age factor. Contrast this

to SPCH and SOC, where Age appears across the series. In general, all tables and related figures should be carefully checked to ensure 1-to-1 correspondence in ordering or presentation and labeling.

Furthermore, the decision to ignore the NAs in the figures may gloss over equally important negative results. It also causes inconsistencies between figures whereby differing number of factors are presented from one graph to another. We recommend that the NAs be graphed and the factors graphed in the figures should occupy a consistent space from one figure to another.

Minor comments

p. 141. It is not clear how the analysis using capture-recapture differs and complements the analysis on the individual detections. Presumably, the capture-recapture analysis ignores all covariates and includes modeling detections, while the “survival” analysis does not adjust for detections (perhaps call it *apparent survival* to make this clear because non-detection above the dams is not modeled). In previous reviews, the CSS team pointed out that detections are nearly 100%, so this should be added here.

p. 141. “All covariates were standardized prior to model fitting.” Does this mean that the quadratic effect of temperature was standardized separately from the linear effect of temperature? The linear effect of temperature should first be standardized, and then this is “squared” for the quadratic effect of temperature.

p. 142. Variable names in first paragraph do not match the names in Table 5.1.

Figure 5.38/5.39; Table 5.15. A confidence interval of 0 to 1 generally indicates that a quantity was not identifiable which can affect estimates of survival in the other dam intervals, i.e. only the product may be estimable? Additional exploration of this issue is needed to ensure that results are properly interpreted. These seem to occur in years with very small numbers of returns. Perhaps do not compute estimates if the number of fish initially detected at BON is < 10 to avoid these issues? Alternatively, 0-1 intervals sometimes arise due to using an approximation for the interval that fails when the point estimate is on the boundary, and more accurate methods (e.g., profile likelihood) could avoid this.

p. 159. Some of the bullets specify species and others do not. This should be consistent.

p. 184. “For the five species that we evaluated, the largest and most consistent effects on upstream survival success were the differences between hatchery and wild stocks, and those that were transported as juveniles vs those that migrated in-river.” Please provide slightly more details to provide some context, i.e., is the difference in survival on the order of 1%, 10%, etc.?

With large sample sizes, it is possible to detect small effects that may not be biologically meaningful. Is this the case here? The same applies for the effect of transportation.

p. 185. “conversion ... 79.6%...” Here and elsewhere, report percentages to integer values only.

p. 185. Please be consistent about using “conversion” versus “conversion rate.” For example, in Figure 5.47 are these “conversion rate” or just “conversion”?

p. 194. Just below the table. If a model includes an interaction term, it must include the linear terms, even if not “statistically significant.” The text appears to suggest otherwise. Please clarify.

p. 194. Is the “three-day conversion probability” just another way to describe conversion? Please clarify.

Table 5.1 All tables should be self-contained. The variable name descriptions found in the first paragraph of p.142 should be contained in the legend to the table. This table uses H/W as the variable name for hatchery vs wild, but the first paragraph on page 142 says the variable name is *Wild*. The same applies to temperature and flow. Sometimes Spill% is listed as Spill%.

Table 5.2 and elsewhere. Presumably, the number of fish detected should generally decline as you move up the system, but there are a few cases where more detections were seen higher up (e.g. 2023, ICH vs GRA). Perhaps the table should be modified to include for each dam the number of fish that were not detected at a dam but were detected at a higher dam indicating it must have passed the first dam. Explain any cases with unusually large number of such fish.

Tables. Numbers of fish should be right justified in each column rather than centered (<https://ux.stackexchange.com/questions/24066/what-is-the-best-practice-for-data-table-cell-content-alignment>).

Table 5.3, 5.6, and elsewhere. Be consistent in decimal places shown, i.e. report 0.80 rather than 0.8. If upper bound of a confidence interval is 1, report it as 1.00 rather than simply 1. Center headers above respective columns.

Table 5.4, 5.7, and elsewhere. Note that *Age* is a categorical variable, so the estimates represent a difference from the reference category of *Age* which was never specified. Add the reference category to the table legend. The same applies to the *Trans* and *Wild* variables.

Table 5.19. Include a column for the number of parameters. What terms does the “Tailrace x maximum south ladder delta T” include? Does this include a linear effect of Tailrace and a linear

effect of the delta T? Does “Tailrace” mean the same as “Tailrace temperature”? Use standard syntax conventions to describe models.

Figures 5.1, 5.2, 5.3, and elsewhere. Adjust x-axis labels so they do not run together. Either show every second year or tilt the tick mark labels (e.g., see Figure 5.37)

Please ensure that graphs and figures make consistent use of tick marks, confidence intervals, variable names, etc.

p. 142. It is stated that computing time is a limiting factor in the analysis, but no further description is offered. For legacy reasons, it would be wise to document this to a fuller degree in terms of time and expense. Computing time may or may not be limiting in the future.

For FACH, Figures 5.26 and 5.27 are presented but are not referenced in the text. Similarly, for SUST, Figures 5.34 and 5.36 are presented but are not referenced in the text.

Editorial comments

p. 141, first paragraph, second sentence: “All PIT tagged above Lower Granite Dam and subsequently detected as adults migrating upstream in the Columbia or Snake Rivers” is an incomplete sentence. Suggest rewording the beginning of this sentence to: “All fish used in the analysis were PIT tagged as juveniles above Lower Granite Dam.” Subsequently, the first sentence in each “Data” section is now redundant with the sentence here and can be deleted.

p. 142, last paragraph: “modeled” should be “model.”

Table 5.16 has confusing columns and column labels. Some further formatting would help indicate that there is a list of covariates for each of three reaches, etc.

P. 140, fourth bullet, second sentence: “With higher temperatures throughout the river predicting lower survival through the FCRPS.” Please re-word this sentence.

Throughout, when the term differentials is used, please follow it up with “between ladder entrances and exits,” or, at a minimum, do this at the first use in different sections.

p. 140, bullet 4, please reword, perhaps to “consistent negative effect of temperature on survival.”

p. 140, bullet 9, suggest moving up above bullet 8.

p. 195, paragraph 1, the phrase: “and ladder temperature moderated.” This is awkward wording, perhaps change to “were lower.”

IV.G. Comments on Chapter 6. Examining Heterogeneity and Factors that Influence Rates of Smolt-to-Adult-Return for Spatially Dispersed Stocks of Spring Chinook Salmon and Steelhead in the Columbia River Basin

Summary from 2026 draft CSS Annual Report: *“Chapter 6 continues analyses of the long time series of SARs generated by the CSS, in response to recommendations by the ISAB. Updated analyses seek to model directly variation in SARs owing to both freshwater and marine covariates, where models are applied to the most recent time series available.”*

Chapter 6 is a longstanding chapter that continues SAR analyses. Most of the ISAB comments on the 2026 report are in response to changes to analyses or new analyses done by the CSS. The ISAB wants to acknowledge that the comments on a longstanding chapter are a good sign that analyses continue to advance.

ISAB Comments and CSS Responses in 2025 with Additional 2026 ISAB Responses to CSS

Included below are ISAB comments of particular concern and CSS response dialogue from the 2025 review that were not fully addressed or addressed but the changes made by the CSS triggered new comments.

ISAB’s Comment in the 2025 Report

The presentation and description of the models evaluated as part of model building need to be revised to ensure consistency and clarity. There are notational issues that make it difficult for readers to follow the development and fitting of the models without looking at the actual R code. The model description (p. 253) needs a careful review and correction for clarity and consistency. For example, in Equation 6.2, the thetas have (i,j) subscripts, but the right-hand side of the equation has (j,k) subscripts. The group subscript k represents a combination of reach and species (Table 6.1) with 8 values. Then, the text introduces a subscript l (“el”) to represent species, but there is no such subscript shown in the thetas. To further complicate the description, R-syntax is used in the tables (e.g. Table 6.2) that use terms not clearly linked to description of the models

in the text. The ISAB suggests that clear and consistent syntax be used, such as starting with (j,k,l) to represent (year, reach, species) or avoid subscripts and use a shorthand model notation such as:

$$\text{logit}(\text{SARs}) \sim 1 + \text{SampError}$$

$$\text{logit}(\text{SARs}) \sim 1 + \text{Reach:Species(R)} + \text{SampError}$$

$$\text{logit}(\text{SARs}) \sim 1 + \text{Reach:Species(R)} + \text{Obs(R)} + \text{SampError}$$

$$\text{logit}(\text{SARs}) \sim 1 + \text{Reach:Species(R)} + \text{Obs(R)} + \text{Year(R)} + \text{SampError} \dots$$

$$\text{logit}(\text{SARs}) \sim 1 + \text{Reach:Species(R)} + \text{Obs(R)} + \text{Year(R)} + \text{Year:Species(R)} + \text{SampError}$$

*SampError is the uncertainty in each SAR's values (related to the binomial-type variance) with **known** variance and so is NOT estimated in the model fitting but is needed to allow estimates of SARs that are more precise to have higher weight than estimates with lower precision. Note that this new style notation shows that a residual error is never fit (as commonly done in regression models) unless the Obs(R) term is included.*

***CSS Response:** We have revised model descriptions to better reflect the processes that are being characterized.*

ISAB 2026 Response to CSS's Response: The issue of notation is repeated this year because the notation used in the 2026 report needs further clarification in the text, figures, and tables.

ISAB's Comment on the 2025 Report

The CSS added an analysis to address the ISAB's comment about further exploration of the variance components. The added analyses are a reasonable approach and provide useful information but would greatly benefit from additional analyses. First, how well do the data support the assumption that each of the stock random effects are independent of each other? Intuitively, this seems unlikely because there are two effects from each Origin corresponding to the species analyzed and thus a more reasonable assumption would be that year random effects have autocorrelation. The analysis of changes in the variance component for stocks (p. 261) suggests that WTT is more influential than

PITPH. However, stocks located higher up the basin are farther away and hence have larger WTT values; impact of WTT is therefore confounded with the any Origin effects. A more refined analysis is needed to evaluate the importance of these assumptions in the new analyses about variance components.

CSS Response: We have revised the (base) model so that origin is included irrespective of species. We have also modeled the (nested) migration year random effect as an AR(1) process (see revised chapter).

ISAB 2026 Response to CSS's Response: This issue from last year on how to represent the variance structure in the modeling is both important and complicated. The ISAB appreciates the effort by the CSS to address this in the 2026 report. We repeat the comment here because we further explore this topic as a Primary Comment of the 2026 review. We offer suggestions in this year's comment to help support further exploration by the CSS.

Primary comments

The Summary conclusions are very helpful. This chapter uses a relatively complicated set of analyses. Relating the conclusions to the results presented in the main text will be challenging for readers. It would help the reader to add either a short paragraph on the methods or a sentence with some of the conclusions that illustrates how modeling informed the conclusion. This can serve as a guide linking the specific methods and results that led to each conclusion.

This chapter uses ordinary linear mixed effect regression models on the empirical logit of the SARs. This should be clearly stated to avoid readers thinking that a version of a logistic regression model was used.³ As part of this, the rationale and logic for using the `rma.mv()` function would be helpful. All of the n and x values are essentially integers, and so a generalized linear mixed model (logistic regression with random effects) could have been used. Such a model will automatically incorporate sampling error (as in `rma.mv()` function) and will use a proper logistic transform. Please explain and describe the model used, including the justification for the use of the `rma` package rather than a generalized linear mixed model on the untransformed data.

At least three different modeling notations are used: (1) mathematical equations with subscripts (e.g., Equations 6.4-6.6), (2) a simplified modeling syntax (e.g., Table 6.2), and (3) R

³ For more details, please refer to Donnelly and Verkuilen. 2017. [Empirical logit analysis is not logistic regression](#).

syntax (e.g., Table 6.3). The ISAB notes that this comment was also stated in the 2025 review, and effort should be invested to make the notation consistent and intuitive throughout the chapter.

As this chapter has advanced and grown over the years, the ISAB suggests that this year or next is a good time for CSS to take a step back and reorganize and provide more synthesis across analyses within the chapter. Either some sections can be merged (not simply concatenated) to achieve synthesis, or each section can be re-focused and new sections added specifically for synthesis and integration. In particular, the methods and results need further integration.

The ISAB appreciates the efforts of the CSS in addressing the issue raised last year about the structure of the variance component used in analyses. We suggest further exploration and analyses are needed to fully resolve the ISAB's 2025 comment on variance structure. We refer the CSS to the 2025 comment and further add that the treatment of variance could be more hierarchical and done to better match the structure of the data (e.g., locations stripped across years; split with species). The variance structure has implications on which variance components would be included in the models. The ISAB provides more information about the issues and some possible approaches to the variable structure in the text box below.

More details and suggestions on modeling the variance structure

p. 201. Equation 6.4. The model contains a variance component for years nested within species. Presumably, a more hierarchical approach would start with random effect of years (e.g., warmers years affect SARs for both species and in all locations). Then add a random component for years nested within species to allow the year effect to differ slightly between the two species with a year, then add an observational random effect.

p. 201. Equation 6.6. A clear definition of $\zeta_{(ii)jk}$ and $\zeta_{(iii)k}$ is needed. The () notation is usually used for nested effects, but what are *ii* and *iii*? The definition for the former says it is an observation-level random effect. The definition of the latter says that it represents the origin-group random effects representing among group heterogeneity, but that seems to match the definition of ζ_k ? Are two symbols needed for the same quantity? Please clarify.

Table 6.5 shows that models with a random effect for species were considered. Why is species a random effect rather than a fixed effect? The analysis is not sampling the two

observed species from a population of a larger number of species. Please explain the rationale for including MCS2, where species is not included as a fixed effect.

p. 201. Equation 6.4. The model contains a variance component for years nested within species, but there does not seem to be a component for the year level.

Table 6.1 shows that not all species are measured in every combination of year and location. Please create a table showing which combinations of location and year have data. For example, here is a sample table where C and S indicate if there is a SARS value for a Chinook/Steelhead respectively, for each combination of year and location.

	<u>mig.yr</u>	<u>LGR-BOA</u>	<u>JDA-BOA</u>	<u>MCN-BOA</u>	<u>RRE-BOA</u>
1	1994	C			
2	1995	C			
3	1996	C			
4	1997	C S			
5	1998	C S			
6	1999	C S			
7	2000	C S	C	C	
8	2001	C S	C	C	
9	2002	C S	C	C S	
10	2003	C S	C	C S	
11	2004	C S	C S	C S	
12	2005	C S	C S	C S	
13	2006	C S	C S	C S	
14	2007	C S	C S	C S	
15	2008	C S	C S	C S	C S
16	2009	C S	C S	C S	C S
17	2010	C S	C S	C S	C S
18	2011	C S	C S	C S	C S
19	2012	C S	C S	C S	C S
20	2013	C S	C S	C S	C S
21	2014	C S	C S	C S	C S
22	2015	C S	C S	C S	C S
23	2016	C S	C S	C S	C S
24	2017	C S	C S	C S	C S
25	2018	C S	C S	C S	C S
26	2019	C S	C S	C S	C S
27	2020	C S	C S	C S	C S
28	2021	C S	C S	C S	C S
29	2022	C S	C S	C S	C S
30	2023	C	C	C	C

This example illustrates that in 2001 and 2002, one of the steelhead SARS could not be computed. This changes the number of useable SARs values to 173 from 175.

This table also nicely shows the “design” of the study. Locations are stripped (<https://www.statisticshowto.com/strip-plot-block-design/>) across years; and then location.years are split by species. Locations and year are “blocking” variables and so the model must contain a random effect of location, a random effect of year, and a random effect of their interaction. Not all location.year interactions are available because there are missing cells above. It is important to ensure proper blocking in an analysis when considering “species” effects.

Examining species effects can be done in two ways. First, notice the pairing in many location.year combinations. A “paired” test (intra-block analysis) could be computed by finding the difference in logit(SARS) for the two species within each location.year present and testing if the mean difference is 0. Alternatively, a modeling approach (combined intra- and inter-block analysis) could be computed by fitting a model with location (R), year (R), and location.year (R) effects to create the location.year blocking and then adding the species effects. If the design was balanced (i.e., every year, location, species combination present) both analyses are equivalent

(<https://people.math.ethz.ch/~meier/teaching/anova/incomplete-block-designs.html>). In cases where the designs are not balanced, the latter approach can recover slightly more information than the former. Any model that includes species (or covariates that operate at the observational level) must include the year, location, and location.year variance components to ensure that the blocking structure is respected. Now species.group and species.location random effects can be added to the model as well. Observational random effects are the species.location.group interaction. Finally, sampling error can be added when using the rma package.

Covariates can operate at different levels. For example, WTT and PITPH operate at the location.year.species level. Tests involving these covariates must again include the location, year, and location.year random effects. Marine covariates operate at the year level and so require random effects of year to be present. Covariates such as distance operate at the location level and so require the random effect of location to be present.

The only variance components that may be pooled are the species:location (R) and species:year (R) sources that may be pooled with the residual (observational) error if the underlying variance components are small or zero.

The principle of hierarchy (often called the principle of marginality or heredity) states that if a statistical model includes a higher-order term—such as an interaction (A:B) or a quadratic/polynomial component (Day²)—it must also include all corresponding lower-order component terms (the main effects *A* and *B*), regardless of whether those lower-order terms are statistically significant. In mixed-effects linear models, this principle applies to both fixed effects and random slopes/intercepts. Models, for example, that include a random effect for species.year, must include terms for species and year in the model.

An initial model can be fit to estimate the proportion of total variation associated with each part of the “experiment.”

Logit(SARS) ~ Location (R) + Years (R) + Location.Years (R) +
 Species (R) + Location:Species (R) + Years:Species(R) +
 Years:Location:Species(R) + SamplingError(R)

This model will first provide a variance partitioning showing where the major variation in logit(SARs) lies. For example, a table like:

	Factor	Variance	prop.var
1	factor(orig_grp)	0.39606583	0.38
2	factor(mig.yr)	0.25622639	0.24
3	orig_grp:mig.yr	0.14353592	0.14
4	factor(spp.code)	0.04622093	0.04
5	spp.code:orig_grp	0.03165161	0.03
6	spp.code:mig.yr	0.14072001	0.13
7	spp.code:mig.yr:orig_grp	0.04088214	0.04

would show that the majority of variation in SARS comes from the 4 locations (orig_grp) and very little from species effects.

Then fixed effects for species, WTT, PITPH, and marine covariates can be added. Models that assess additivity can also be examined in the same structure.

This approach will also prevent fitting models that may not be realistic. For example, consider the first model of Table 6.3. The *R* syntax is complex but can be expanded as:

{1|origin.grp\obs.id; 1|spp./mig.yr} =>

(1 | origin.grp) + (1 | origin.grp:obs.id) + (1 | spp) + (1 | spp:mig.yr) =>

Origin.grp(R) + Obs.id:Origin.grp(R)+ Species (R)+ Year:Species (R)

This model includes a random effect of origin.grp (i.e., location), but then obs.id appear as interactions with each origin.grp. This seems to be incorrect. Observations are at the location.year.species level. Similarly, there is now a random effect of species and then migration year nested within species. But a hierarchical structure would enforce that a year random effect must also be included.

Similarly, consider the last model in Table 6.4. Presumably this is the first model in Table 6.3 because the likelihood and number of parameters match. Then, the next to last model, includes a fixed effect of species. If the model includes a fixed effect of species, the model should drop the species random effect (models would not have both a fixed and random effect of species), and so the number of parameters would remain the same.

A careful review of the variance component structure and models fit is needed that corresponds to the “experimental” design.

The ISAB encourages the CSS to continue exploring ways to include the marine environment in these analyses. Two aspects that are likely important are density-dependent growth and mortality, and the influence of hatchery releases. The ISAB recognizes that such analyses would be exploratory and further work is needed to identify how to include them and how to interpret the results. The issue of marine survival will likely continue to increase in importance. There is widespread agreement that not only is the marine ecosystem changing (overall warming and increased variability) but also that salmon abundance in the ocean is high compared to historical levels. There is growing evidence that the continuing large releases of hatchery salmon (primarily from Russia, Japan, and Alaska) are having negative effects on overall salmon survival. This is being actively investigated farther north in British Columbia (see [Pacific Salmon Foundation Bottlenecks to Survival Program](#)) and may also be germane to the Columbia system. Some recent review papers include Connors et al. 2025; Ruggerone et al. 2022, 2023; and Ruggerone and Irvine 2018. The ISAB strongly encourages that the CSS continue to analyze new data streams, review the results of analyses by others, and consider different statistical methods for including the marine effect in the SARs analyses.

Additionally, the ISAB suggests that the CSS explore the feasibility of using the BON-BON SARs to try to separate direct mortality within the hydrosystem from carryover effects and ocean effects. We already know that WTT and PITPH have some direct effects during freshwater migration, and those are probably best estimated from the analysis of PIT tag detections during

the juvenile migration.

Minor comments

p. 216. A brief discussion of the similarities and differences in the marine migration patterns among the Chinook populations would be helpful to a reader. Columbia Basin stocks differ in where they spend their time after arriving in the coastal ocean, and different physical, biological, and human processes operate in the subarctic North Pacific versus the Alaska and California Currents.

Figure 6.1. The implicit assumption is that only coastal processes during early ocean residency matter for the ocean. Some scientists are increasingly concerned that later stages, such as over-winter survival, might be just as important. Depending on the population, these impacts could occur in the California Current, Alaska Current, or the North Pacific, and appropriate environmental indicators should be considered.

The analyses demonstrate statistical relationships between WTT, PITPH, and SAR. Some of the Discussion goes further by interpreting these relationships as the expected effects of management actions. A short paragraph should be added that emphasizes and reminds the reader that the analysis is based on observed variation rather than an experimental manipulation of either WTT or PITPH.

Adding the marine covariates improves model agreement and reduces the migration-year random-effect variance (Figure 6.7). This suggests that marine conditions explain an important portion of the interannual variation in SAR. This clarifies how the marine-related indices should be interpreted. The indices used are best interpreted as indicators of broader ocean conditions rather than as direct measures of a factor linked to the mechanisms affecting marine survival. Pointing out this distinction will ensure that readers view the results on marine indices correctly.

p. 212. The final Discussion raises the important issue of the practical value of identifying marine predictors because managers cannot control ocean conditions. Even if ocean conditions cannot be manipulated, they may still help explain annual variation in SARs and provide useful context for evaluating freshwater management actions. In addition to helping explain variation in SARs, one could also explore if the effects of freshwater factors (WTT, PITPH) depend on the early marine factors. This can be done by considering interaction terms between freshwater factors and early marine factors as fixed effects. If freshwater factors are exacerbated by early marine factors, then early marine factors can potentially influence the freshwater management actions that are taken, or the importance of taking these actions. There are likely already hints

of plausible interactive effects because extended migration affects travel time and exposes fish to different early marine conditions. These uses of marine information, beyond whether management can control marine survival, should be acknowledged in the discussion.

p. 200. There is some confusion about “groups.” Table 6.1 shows 8 lines. These are labeled as “zone” in the first column, but “zone” is not used except on page 199. Are there 8 groups? Or are there 4 groups (locations) x 2 species? The first paragraph on page 199 indicates there are 4 groups. The subscript k references groups and l (el) references species, so the 4 groups x 2 species appear to be the relevant interpretation. The document also uses the term “origin group” but never defines what this is – the 4 locations shown in Table 1? Table 6.5 shows that the origin-group has 4 levels. The issue of defining groups also applies to Equation 6.4.

p. 200. “As necessary, we added a small constant to cell entries where values equaled zero to ensure responses were defined” What was the constant that was added?

p. 200. Equation 6.2 uses the subscripts ijl but the definition below that uses the subscripts jkl . Please correct this.

Equation 6.2. An empirical logit is being used in the analysis. It is customary to add a small constant to the x and n values to avoid bias. See Equation (1) in

<https://www.sciencedirect.com/science/article/abs/pii/S0749596X1630167X>

Equation 6.4. Why is there no species effect in the base model? *A-priori*, are SARs expected to be equal for the two species of fish in a location.year, given their life history characteristics?

p. 207. “...this congruence is due in part to the implementation of an observation-level random effect.” Even if the fixed effects had no predictive ability, one would still get a good fit using an observation-level random effect. The rightmost column of Figure 6.4 does not provide very useful information. Of more interest are why are there observations so far from the $x=y$ line in the right column of Figure 6.4?

Editorial comments

The narrative should ensure consistency with the emphasis on early marine factors. In the Introduction, an assumption is made that “out-migrating fish experience comparable estuarine conditions and are subject to a common suite of physical and biological processes in the northeast Pacific Ocean.” However, this is only true for early marine factors (and also is not quite true if there is variability in marine entry time). It is also possible that groups differ in the number of years they spend in the ocean, which is not something addressed in this chapter.

p. 202. “...several appears to suffer from overfitting (indicated by singularities).” This needs elaboration and clarification. For example, such a singularity would occur if a variance component had an estimate of zero. This does not mean that the model was overfit – rather that the variation in the data was insufficient to estimate variation. The model is still valid. An example of this is found in the text on page 205 when the variance component for group-level effects is reduced to close to zero when covariates are added. On the other hand, extreme collinearity could result in a singularity but now the model is invalid.

Equation 6.1 seems to be a missing subscript on the SAR term.

Table 6.2. This is a clear model description. Please consider using a similar notation for the other modeling tables.

Table. 6.5. Keep number of decimal places consistent, i.e. if 0 report 0.0000. See <https://ux.stackexchange.com/questions/24066/what-is-the-best-practice-for-data-table-cell-content-alignment>

Table 6.7. The models with a “non-nested observational level” random effect and “nested observational level” random effect are identical. The only reason these appear to be different in Table 6.7 may be due to differences in the estimated variances? This needs to be checked.

Figure 6.3. Why are there no confidence intervals for the effect of WTT?

Figure 6.7. Why is color used to differentiate the bars?

Table 6.3 Notation for models differs from equations and Table 6.2. Please be consistent in use of variable names in models through-out this document.

Code Review. A brief review of the code showed the use of deeply nested ifelse() functions. Such as:

```
Diskrm <- ifelse(
  css.grp == 'EMCR' | css.grp == 'EMWS',
  529,
  ifelse(
    css.grp == 'AGCW' | css.grp == 'AGWS',
    461,
    ifelse(
      css.grp == 'ROSA' | css.grp == 'YAKS',
      236,
      ifelse(
        css.grp == 'JDAC' | css.grp == 'JDAS',
        113,
```

```

        NA
      )
    )
  )
)

```

These can be prone to error (https://lintr.r-lib.org/reference/nested_ifelse_linter.html). For example, adding a `css.grp` will break this code. It is better to use a lookup table, which is then merged with the dataset; or code consisting of a series of non-nested `if()` statements such as

```

diskrm <- NA
if(css.grp %in% c(EMCR, EMWS)) diskmr <- 529
if(css.grp %in% c("AGCW", "AGWS")) diskmr <- 461
...
If(any(is.na(diskrm)) stop("Missing rkm for some observations").

```

IV.H. Comments on Chapter 7. Steelhead Overshoot and Fallback Rates

Summary from 2026 draft CSS Annual Report: *“Chapter 7 examines the timing of various detections of adult wild steelhead as they migrate upstream in the Columbia and Snake Rivers and how that timing differs between fish that exhibit overshoot and fallback behavior. Specifically, we examine adult detections at Bonneville Dam, the closest downstream mainstem dam for each population group, the first overshoot dam, and the first detection within a group’s natal watershed, looking for potential differences between overshoot and non-overshoot fish, or fish that fall back or fail to fall back. In addition, we compare the fallback rates for fish from each population group that overshoot various number of dams, including a group that does not overshoot any mainstem dams.”*

This was a new analysis in 2025, and much text has changed in the 2026 draft report. This chapter examines the migration routes of adult steelhead within the Columbia River Basin and movement of wild steelhead that overshoot their natal tributary, those that eventually fall back to their natal stream, and upstream strays that were never detected in their natal basin. Overshoot and straying have been ongoing concerns for Columbia River hydrosystem management, especially in the Snake River for the four lower Snake River dams. The ISAB commends the CSS for further advancing this analysis in 2026 and encourages them to refine and continue this analysis going forward.

Primary comments

The focus on timing as response variables needs some additional background and presentation of evidence for why it is important to analyze. In particular, description of how the analysis results can inform the estimation of SARs and management would be helpful. Also, a brief section on other analyses that have been done in the Columbia Basin on the same topic of fallback and overshoot, and how the CSS analyses is similar and different from those other analyses is important to add (Waterhouse et al. 2020, Min et al. 2025, See 2026).

A schematic showing example time histories of different fish trajectories (i.e., capture histories) and how detections at various locations lead to estimates of arrival times and the timing of overshoots would greatly help the reader follow the subsequent analyses in the chapter. This issue about pathways comes up again with Figure 7.7, which is about fallback percentages. The figures showing the results of analyses use highly aggregated data and are informative but the link from fish histories to these figures is difficult to envision. The supplement presents summaries of the intermediate data, and example histories could be added to the supplement rather than disrupting the flow of the chapter.

Figure 7.2 should be more thoroughly explained in the text. Figure 7.4 follows the same format. Further, Figure 7.5 is similar but not identical (now there is False/True and No/Yes), and Figures 7.3, 7.6, and 7.7 use different formats. Once the reader carefully examines and links the figure back to the methods, each figure makes sense for the question being asked, but it is difficult to follow the changes in figures. Why are they similar but not identical and, in some cases, why are they very different in their formats? Maybe a table listing the figures, what is plotted, and why the results are plotted as they are would help a reader.

Figure 7.8 is an interesting plot and would benefit from more than one sentence of text. Any ideas why the pattern is so noisy? Why are populations clustered? Is that simply a function of the total number of dams? The legend says that the data were grouped by spawn year and natal population, but where are the spawn years shown?

The statistical analyses performed are a good start and provide the basis for refinement as these important issues continue to be explored. For example, in the analysis of detection dates at the three dams (Bon, Downstream, Natal), a t-test was used where data were pooled over years to compare the mean arrival date. Sample size differences across years could impact these comparisons, as the mean difference can be more influenced by years with larger sample sizes. An alternative worth investigating would be a variant of a block design with years as blocks, e.g., $\text{ArrivalDate} \sim \text{Year(R)} + \text{Overshoot}$ for each population group. This would

automatically adjust for any sample size effects across years and would give each year equal weight in computing the mean difference.

The use of the KS test to compare distributions is also influenced by sample size. One suggestion is to perform the KS test on the residuals from the previous model to adjust for year effects. Another avenue to pursue is to use the Anderson-Darlington (AD) test, which can be preferred over the KS test. The AD test gives more weight to the tails while the KS test focuses on the maximum vertical difference in the center. Similar comments apply to some of the other comparisons made in this paper. Finally, new analyses are also possible. For example, a Bayesian analysis that borrows/pools information across small cohorts may be useful to even out the sample sizes. Another possible approach would be to use overshoot or non-overshoot as the response in a logistic regression and use the detection times as the covariates. Or possibly a multinomial logistic regression could be considered, with outcomes dead, on target, and overshoot. Further analysis is warranted.

No methods were described to compare the number of dams that were overshoot. This could be examined using the previously described models, or contingency tables and chi-square tests (see Table S.5), or as shown in Figure 7.8, a logistic regression. The logistic regression could be improved by using random effects for major population group (to give each group equal weight) and for spawning year (to give each year equal weight).

Table S.2 and others. Perhaps a better model could be used here rather than performing so many statistical tests. It is not clear what information is to be gleaned from the percent of tests that were significant. Presumably, it must be 5% or greater. This proportion would be highly dependent on sample size – so what can be concluded by comparing this number across population groups? Box plots of effect size (e.g., difference in mean arrival dates) would be more informative.

The number of populations shown in any one graph or table jumps around according to stipulations, mainly based on whether they showed a significant test or not. This results in populations not showing significance not being shown. This makes comparing across figures difficult and it results in not seeing any of the data displayed from the populations showing non-significance. Better to show the full matrix of 12 populations in each figure and table for comparisons, consistency, and legacy. Figure 7.8, which depicts data from 7 populations may be an exception, but the sentence in the caption that reads “These data come from only those populations with a documented substantial overshoot rate” needs a more definitive qualifier (i.e., the rate that constitutes substantial). Going forward, it will likely be just as important to investigate and understand why some populations have low to no overshoot behavior as investigating and understanding why some populations exhibit substantial overshooting.

In the summary sentence of the first paragraph of the Discussion, it states that “It warrants further investigation in the future whether conditions in the mainstem river shift during the migration season and could be tied to why fish who arrive sooner tend to overshoot.” The Introduction can lead one to believe (unless read very carefully) that the effect of conditions in the mainstem river (and the tributaries themselves) will be addressed in this chapter. It should be made clear which effects are considered in the subsequent analyses.

Minor comments

Some discussion about the possible effects of repeat spawning and straying (even if they occur at low levels) on the analysis would provide further context for interpreting the results.

p. 214. In the Methods, it is stated: “We excluded fish tagged as adults if their size at tagging was greater than 400 mm or the life-stage at tagging was indicated to be an adult,” but no reason is offered as to why these fish were excluded. Other groups of excluded fish (e.g., fish tagged at a mainstem dam lower in the watershed, fish tagged as juveniles at Rock Island Dam, and some fish that were detected at Bonneville Dam as adults between November and February) are provided with a reason why they were excluded.

Also in the Methods, it is stated: “We pulled out fish known to be tagged in the Wenatchee (*sic*) and Entiat within the Upper Columbia, and fish known to be tagged in the Tucannon within the Snake River, because fish from those three basins have the opportunity to overshoot past a mainstem dam within their DPS, whereas populations upstream of Wells Dam (e.g. Methow, Okanogan) or Lower Granite Dam (e.g. the rest of the Snake River DPS) do not. For these analyses, we excluded fish tagged upstream of Wells Dam and Lower Granite Dam.” The words “pulled out” suggest they were excluded, but when the word “excluded” appears in the last sentence, it suggests that fish “pulled out” were actually those that were included. This needs to be made clear. Perhaps using “accepted” instead of “pulled out” would help.

Editorial comments

p. 213. “...discernible difference in the arrival timing at Bonneville Dam...” should read “...discernible difference in the MEAN arrival timing at Bonneville Dam.” Similar changes may be needed in other bullet points.

Figure 7.1. The map is not very legible.

Table 7.1. Perhaps add a column that indicates the number of known repeat spawners included in the analysis. As suggested on page 216, “N tags” is not equivalent to the number of unique

tags because, for example, a three-time spawner would potentially be considered three different data points. Also, another addition to the table to consider is showing in what years these fish were originally tagged. In the first paragraph under Capture Histories, it is stated that the spawning years were 2011-2026.

p. 216. The text states: “First, the initial wild steelhead adult detection at Bonneville Dam was identified, based on it being at least **at year** since the date of tagging.” Simply change “at year” to “a year.”

p. 217 (and elsewhere). “...a non-parametric difference...” The differences themselves are neither parametric nor non-parametric. Rather, the KS test examines differences in the distribution between the two groups in a non-parametric fashion.

p. 218 (and elsewhere). “...little difference between the arrival timing...” should read “little difference between the MEAN arrival timing...”

Figure 7.2 (and elsewhere). The Y-axis tick marks need the date in Jul, Jan, or Jul, i.e. 1 July, 1 Jan, 1 July, etc. In addition, rather than showing box plot with outlier dots, perhaps plot the raw data (jittered) with the box plot overlaid (and outlier dots removed). This way the entire distribution can be easily seen.

Figure 7.2. Red stars are preceded by blue stars, but in the caption, the definition of blue stars precedes that for red stars. Better to consistently align the order of these, and best to keep blue before red, which follows the wording in the Methods.

Figures 7.2 and 7.4. Consider stacking the red and blue stars at the midpoint line between boxplots of each category. As is, it is too easy to think that the red star goes with the left box plot within the pair and the blue star goes with the right box plot of the pair.

Table 7.4 (and elsewhere). Please format the last column better by aligning various features. See <https://ux.stackexchange.com/questions/24066/what-is-the-best-practice-for-data-table-cell-content-alignment>

Table 7.5. Why is there a column for “significance level” that is not present in other similar tables? The legend says that rows with a statistically significant difference are bolded – but the bolding is not apparent.

Figure 7.3 (and elsewhere). The Y-axis are integer counts and the tick marks should also be integers.

p. 223. The sentence “Fish in all populations that exhibited overshoot behavior showed a general pattern of fewer fish overshooting more dams” needs a rewrite. The use of “fewer” without reference to what this is relative to (i.e., who is being compared so there are fewer) is confusing.

p. 223. In the last sentence of the last paragraph, Substitute “chance” for “change”

Figure 7.6. The Y axis label says percent, but it is actually a proportion. What do the error bars represent? Variation across years?

Figure 7.7. Why rotate the axis in this figure? The labels for the pathways are difficult to read.

p. 217. The term Julian day is used only here, and not correctly. Year day, day of year, or ordinal day would be preferred, and whichever is used should be used consistently throughout the entire report.

Figure S1 (and others). Perhaps color the dots by year? As noted in the earlier comments, a comparison of distributions should not be impacted by different sample sizes across years. The more general issue of uneven samples sizes across years in the analysis hinders interpretation of the results.

IV.I. Comments on Chapter 8. Reassessment of Juvenile Migration Timing and Adult Return of Snake River Spring/Summer Chinook Salmon and Steelhead

Summary from 2026 draft CSS Annual Report: *“Chapter 8 starts with a reproduction of the analyses presented in Scheuerell et al. (2009), using the same species-specific migration years, migration window, and the original timing and environmental covariates. This original analysis is then expanded to include the full 26-year juvenile migration time series (1998-2023) to determine whether the direction and interannual variability of the relationship between juvenile migration timing and adult returns remained consistent over this longer record. The analysis is then expanded to include hatchery and wild fish and juvenile bypass history to determine whether those factors explained additional variation or modified the estimated timing/survival relationship.”*

Chapter 8 is a new chapter for the CSS Annual Reports and the ISAB appreciates the effort by the CSS as part of this chapter to revisit earlier published analyses. The repeating of the earlier analysis is a nice bridge to the new analyses that use greatly extended time series and expand on the earlier methods (e.g., adding hatchery fish, bypass history).

Primary comments

The chapter is logically organized, which makes it easy for a reader to follow. What would help is consistent use of the labels (reproduce, extended, and expanded) in all text, tables, and figures. A table or flowchart showing the pathway of analysis for each of these can easily be added (it is presently described in words spread over paragraphs). There is the reproduction of the Scheuerell analysis, and then it gets more complicated with the extended and expanded analyses: inclusion of 2001, centered covariates, original fixed versus mixed model options, etc. In general, making the methods and results more parallel in their presentation would also help, which should include parallel subheadings dividing up the methods and results.

It would also be useful to clarify the ways that fish can pass dams (turbine, powerhouse bypass, spill, barge, others?), how and how well bypass encounters are detected, and what this means for estimating return probability. Presumably, bypass history is an individual level covariate and does not account for detection probability (unlike PITPH), but this should be clarified. Finally, if bypass history does not account for detection probability, then it should be described as representing the minimum number of bypass encounters.

The figures and tables should avoid overuse of labels and shorthand – once shown in complete form in the table or figure, a single set of labels can be defined and used elsewhere. Showing the original Scheuerell model equation is also useful.

The chapter presents a mixture of information theoretic (AIC) and frequentist methods (p-value). This is discouraged (<https://stats.stackexchange.com/questions/104152/how-to-refer-to-aic-model-averaged-parameters-and-confidence-intervals>) because they are different paradigms. This issue should be discussed and presentation of a mix justified, or subsequent analyses can use a model averaging approach (with the caveats noted above in “Suggested Topics for Further Review”) and remove the p-values.

It would be helpful to see plots of the key explanatory variables. These are shown for the significant variables of the final models, but seeing the other variables is also useful since so many models were fit and explanatory variables with similar influences could have been dropped whereas they could come very close to providing the same variation. The change in fits

(delta-AIC) between subsequently ranked models can be small in some cases (<2 , Table 8.3 and 8.4). Do these alternative models fundamentally change the conclusions? The results can be included in a supplement.

The use of centered covariates in later analyses versus the early analysis needs to be explained further. Perhaps showing the actual equations would help here.

The probability of adult return likely varies with the life history strategy (outmigration age and age at return), but age is not considered. At minimum, please present information on the dominant age classes that are represented in the data and discuss the implications this has for interpreting the probability of return.

More discussion about the interpretation of the results is warranted. This chapter is rich with interesting and useful results that should be further explored (perhaps as part of next year's report). Are there any ideas or conjecture about the shapes of some of the relationships? There are multiple sets of contrasts possible. The differences between Chinook and steelhead, between fixed and mixed models, between hatchery and wild, etc. are often but not always striking. The text seems to emphasize the similarities (especially between the original and extended analyses), which is valid and useful, but there is also rich information on differences that would also be useful to explore. The chapter provides model-selection results and predicted curves, but there is little direct assessment of how well the models reproduce the observed data. The ISAB suggests that more information be provided on model performance. For example, the empirical adult-return rates can be added to the year-specific timing figures for Chinook and steelhead in a simple way using the existing Figures 8.4 and 8.8. For each migration year, juveniles could be grouped into weekly Bonneville arrival intervals, using observed return rates calculated separately for hatchery and wild fish. Plotting these observed values on the existing year-specific model prediction curves would give readers a simple way to see how well the predictions match the data across the years. More sophisticated methods can also be considered. For example, it is not possible to directly test for overdispersion with individual (vs. grouped) data. A simulation approach is likely needed for models with random effects such as is done in the DHARMA (R) package.

More information on the method used for model averaging and how the results should be interpreted is needed. The top models for Chinook are generally similar, so when models are similar, the model averaging is easier to interpret. But when the candidate models differ in covariates, interactions, and other ways, the "averaged" prediction may not represent the relationship estimated by any single model. For example, for steelhead, some of the top models have bypass history and others do not, and the temperature effects seem different. Does model averaging mean that the model averaged predictions represent "different descriptions of how

the world might work” than any of the original models that were averaged? This may be addressed by providing some additional information on how the model-averaged predictions were calculated and how the resulting curves should be interpreted. A comparison of predictions from the highest-supported model and the model-averaged predictions would also be useful. This could be a small table or figure and would show whether model averaging supports and changes the main biological interpretation.

The analysis provides evidence that juvenile arrival timing and cumulative bypass history are associated with subsequent adult return. However, this is an observational analysis and does not isolate the causal effect of migration timing, bypass passage, a particular hydrosystem operation, or other type of management action or natural factor. The Discussion does a good job recognizing this limitation and points out that arrival timing integrates several processes, including migration initiation, fish travel time, hydrosystem conditions, environmental conditions, and timing of ocean entry. But some of that caution seems to get diluted or understated in the Conclusions and in the general management part of the discussion. This comment is just a reminder to be clear with the distinction between association and causation, which can get lost as results are increasingly summarized.

The expanded Scheuerell et al. analysis includes origin, migration timing, temperature, CUTI, bypass history, and migration year, but it is not clear whether population, subbasin, release group, or other sources of population structure were considered in the expanded analyses. This could matter if fish from different populations migrate at different times and also have different adult return rates. In that case, part of the apparent timing relationship could reflect changes in the composition of fish passing Bonneville through the migration season.

Scheuerell et al. included supplemental figures (Figures S2 and S3) showing the timing and number of juvenile Chinook and steelhead detected at Bonneville from each source subbasin. One idea is to develop a graphical representation that is similar for the extended time series used in this chapter. This would help show whether some subbasins consistently contribute more fish at times of the migration season and whether those patterns have changed over time. Because the original bubble plots can be difficult to interpret (e.g., Figure S2, year = 1999), a heat map may work better for the extended record. This information would also help evaluate whether some of the estimated migration-timing relationship could reflect changes in population or subbasin composition through the migration season. At minimum, the chapter should discuss whether changing population or subbasin composition could contribute to the estimated timing relationship.

p. 238. The Scheuerell et al. paper found a 0.96 or higher correlation between day of arrival and water temperature. It is hard to believe that any approach will be able to differentiate between

the effects of these two variables. What was this covariate correlation in the extended and expanded analyses? Is it equally strong in all years?

p. 239. The models included random effects of year and a year by linear slope for arrival term (e.g., models that include the terms $Y(R) + D + D^2 + Y:D(R)$ where Y is the (random) year effect, and D is arrival day). However, the quadratic term was not allowed to vary by year. This induces a strange consistency in the curve shape across years and needs further explanation and justification. The suite of models should include terms such as $Y:D$ and $Y:D^2$.

It is not clear why the extended analysis used year as both a fixed and random effect. The former seems to be redundant when the mixed linear effects model is also used. We recommend that the CSS consider removing the analysis with year as a fixed effect in the extended and expanded analyses.

The analyses would be better supported with graphical presentation of the covariates in each analysis and information on covariation among them. The discussion about temperature and arrival day leading to use of residual temperature needs graphical and other support about the resulting covariances. Temperature and residual temperature are then both included in the models. They provide different interpretations but how does this help with the covariance between temperature and arrival day? Some plots would help. In addition, the data could be binned by weeks and the empirical logits plotted over time to investigate if a quadratic effect was needed and the consistency of the quadratic effect over years.

This chapter and Chapter 6 look at SARs in two different ways. Chapter 6 uses tag-release groups and looks at influences of other covariates (including WTT). This chapter uses arrival timing, which is presumably influenced by WTT (among other variables). Can the results of Chapter 6 help to improve this chapter by suggesting new models to fit?

Minor comments

Return probability as a function of migration timing is limited to being either linear or quadratic. Some preliminary investigation using selected analyses and a more flexible approach (e.g., GAM and spline on arrival) could allow assessment of the realism of the linear or quadratic assumption.

p. 240. Is “turning point” the peak of the quadratic?

Please consider the abundance of migrants as a covariate.

It would be helpful to add some discussion of what could be driving the u-shaped relationship for hatchery fish (e.g., Figure 8.5)? Perhaps predator swamping or prey switching?

It seems this analysis could be used to tease apart the different effects of passage delay and late arrival, since the former are controlled for. How much of the impact of the hydrosystem is due to passage delay versus other latent effects?

Is there a relationship between the strength of the timing effect and the level of overall conditions, as expressed in the year effect?

Another way to foster earlier juvenile migration would be to accelerate development, trigger migrations (effect of flow and flow variation on migration timing do not seem to be examined in the report), and earlier reproduction. Could this be achieved by flow and temperature manipulation and improving adult passage at dams?

The relationship with PITPH and bypass history also needs clarification. Presumably, bypass history is an individual level covariate and does not account for detection probability (unlike PITPH), but this should be clarified. Finally, if bypass history does not account for detection probability, then it should be interpreted as the minimum number bypass encounters. The choice of explanatory variables needs further elaboration, and checks should be made to ensure consistency between Chinook and steelhead. For example, the Chinook models allow for (day + residual_T) or (T), but the steelhead models allow for (day + T). The inclusion of ordinal day and temperature in the same model is problematic owing to collinearity.

Further justification is needed on the choice of the seasonal time window of interest; ideally this would be a biological justification. This would negate any need to explore a different time period (which is not exactly a sensitivity analysis).

When interpreting effects, please back-transform coefficients to show the odds, and ensure that readers can interpret effects based on original units of the explanatory variables.

Please present conditional and marginal R^2 for the generalized linear mixed models.

Residual temperature was not considered to be important for steelhead because it was not in the top model, but it was in the second best model with a delta AIC of < 2 . This issue of multiple models having very similar AIC values is discussed above.

The chapter would benefit from a clearer explanation of how the candidate model set was developed. In particular, explanation of why some models include particular random slopes or

interactions while others do not, and how those structures relate to the biological hypotheses being evaluated.

The Methods state that the best-supported expanded models were also fitted using all available Bonneville arrival dates as a sensitivity analysis. Did the results from this sensitivity analyses make it into the chapter? If not, consider adding them in a way that does not interrupt the already complicated flow of the chapter.

The year-specific figure captions should state that the y-axis scales vary among panels. Some of the figures should use standardized scales to enable easy comparison. For example, Figure 8.5 is designed to compare Panel A and Panel B. The two panels use similar but not identical scaling of their x-axis (arrival date) and y-axis (adult return probability). The same scaling for both panels is used in Figure 8.6. Consider using a common scale where practical, or at least, maybe just 3-4 scales because some of the scales are pretty similar that are being produced now. This can help in interpreting the comparisons across the years.

Figure 8.5. The Chinook model includes temperature, so it would seem a surface would be plotted.

It would also help to state more clearly how confidence intervals around the model-averaged predictions were calculated.

p. 238. The original analysis excludes fish tagged in 2001, but the extended and expanded analyses include fish from 2001. Scheuerell et al. explain that 2001 was omitted because adult returns were very low, with only 7 Chinook and 1 steelhead returning. The chapter should explain why migration year 2001 was not included in the repeat of the Scheuerell et al. analysis and why it is included in the extended and expanded analyses.

Table 8.1. Consider creating a larger table showing the number of fish detected and the number of returns by species for each individual year.

Figure 8.4. A similar graph for the 26-year dataset with random year effects would be helpful to see.

Editorial comments

As with the scaling of axis, the labeling of the panels in figures should be consistent (e.g., a difference is illustrated between Figure 8.5 for Chinook and Figure 8.6 for Steelhead). The legends say they are the results from the same step in the analysis and are comparing the same results (except Figure 8.5 adds the results from the original study).

The year-specific figures within the report would also be easier to compare if they used a smaller number of common y-axis scales. The current panels use many slightly different scales, which makes differences among years harder to interpret.

Consider using the term “outmigration timing” when referring to arrival timing at BON. Whichever term is used (outmigration timing, arrival timing, migration timing), please do so consistently.

p. 240. Here and elsewhere, report model weights to integer percentages only.

p. 288. DOI for Scheuerell paper may be incorrect by missing the .x at the end.

p. 240. “The positive linear and negative quadratic arrival date coefficients were both statistically significant.” If a model contains a quadratic effect, then it must contain a linear effect regardless if the linear effect is statistically significant. There is no point in examining the p-values for the linear arrival effect.

Table 8.2. (and others). Right justify delta the AIC values within the column.

Table 8.2. (and other similar tables) should include a column for the number of parameters estimated.

Table 8.3. There are 2 panels labeled “A”?

Figure 8.5. Make the graphs the same size with common y-axis tick marks to make them easier to compare.

Figure 8.5/8.6. There are multiple figures with the same labels.

Figure 8.8/8.9. The graphs are labeled as “Section 3.” A more informative title is needed.

Figure numbering needs to be checked throughout the chapter.

In the Methods, “we fit linear models in Program R” should probably be changed to “we fit generalized linear models” or “we fit logistic regression models,” since the models use a binomial distribution and logit link and that is important.

Explain the || notation used for the random effects. It has a specific meaning in the R package lme4 related to random effects and should be defined clearly in the Methods and probably the Table header.

Use consistent terminology for “wild,” “natural-origin,” and “hatchery/wild origin” fish throughout the chapter.

IV.J. Comments on Chapter 9. Impacts of Water Transit Times and Flow on Water Temperature in the Snake and Columbia Rivers

Summary from 2026 draft CSS Annual Report: *“Chapter 9 is a continuation of previous FPC analyses; it includes a suite of environmental covariates to evaluate the relationship between water transit time and river temperature in June and July for the Lower Snake and Mid-Columbia River. The results confirmed that longer water transit times substantially increase summer water temperatures in the Lower Snake and Mid-Columbia rivers, particularly when air temperatures are much warmer than the river. Because water transit time is directly influenced by dam and flow operations, reducing transit times during periods of high thermal stress could provide a meaningful management strategy for reducing river temperatures and improving conditions for migrating salmon.”*

The CSS has demonstrated that low flows and high temperatures are major factors depressing in-river survival of migrating juvenile and adult salmon in the Columbia and Snake rivers, and the ISAB commends the CSS for beginning investigations into how flow affects temperature and thus how hydrosystem operations affect conditions in the migratory corridor. This chapter is an analysis that follows up on recent reports by the FPC (FPC 2025a, FPC 2025b, FPC 2026). The trends and effects revealed by the analyses are extensive and complicated but also well described, and the ISAB views this analysis as an important contribution to understanding the temperature and WTT problem, with some areas for clarifying and improving analytical approaches.

Primary comments

The chapter may be hard to understand for many readers without reference to the original FPC documents. The summaries of the three earlier analyses are too brief. More information should be provided because understanding the earlier analyses is important to the subsequent analysis presented. Such information can be added partially to the main text (which is quite brief now), supported by a supplement, appendix, or as a text box to maintain the flow of the methods. How do results differ and what results are new?

Much of the ISAB’s comments below concern the details of the regression-based (statistical) approach used in this chapter. We recognize that many choices must be made to conduct such an analysis. Many readers will wonder why these choices were made over similarly reasonable alternatives, and whether the results are sensitive to these choices. We acknowledge that adequately addressing such concerns requires a

significant amount of work but believe this is worth the effort given the importance of the results. We suggest that the CSS initiate such documentation for inclusion in this year's report and for future reports.

An example of choice was to use a statistical approach rather than a more mechanistic modeling approach. A complementary approach that would avoid some issues (while raising others) would be to make use of existing physics-based models of this system (e.g., CE-QUAL-W2, <https://www.cee.pdx.edu/w2>). Such physics-based mechanistic models allow for the estimation of the relative importance of the various elements of the heat budget for a reach of interest and can be used to explore how alternative operational scenarios would affect temperatures under various climatological conditions, including those outside of the observational record. Also, by enforcing conservation of mass and energy, such models could be used to explore trade-offs between spill, PITPH, WTT, and temperature and resulting impacts on in-river juvenile survival and carryover survival. In future CSS work, the ISAB recommends that the CSS explore application of existing physics-based hydrodynamic and water quality models. Strategic use of both approaches together offers a powerful analysis. While development of a new model is beyond the scope of the CSS, there are likely existing models that could be leveraged through a cooperative approach (with moderate effort) and used to mechanistically derive general relationships between variables of interest (e.g., outflow, volume and temperature), if the expertise exists within the FPC to utilize these models.

The chapter would benefit from further description of how WTT was calculated, and the adequacy and accuracy of the methods for representing WTT across the reservoirs. For example, the mass balance method can lead to a reservoir having the same WTT while in a low-flow, low-volume state or a high-flow-high volume state. However, all else equal, the reservoir will heat less at a given WTT when it has a higher volume. Was this ignored because reservoir volumes are close enough to constant? If not, perhaps consider including a measure of reservoir volume as a covariate. Please add a discussion of how flow and volume can affect WTT, as well as providing some indication of the error and confidence in the WTT estimation approach.

Further discussion is warranted about how autocorrelation in the observations is considered in the analysis. Using daily values likely introduced autocorrelation in the residuals, which are known to cause underestimation of the standard errors and overstatement of the statistical significance of coefficients. Very high autocorrelation could seriously affect the results. A functional data analysis (FDA) regression approach

may be a better approach to this type of data

(https://en.wikipedia.org/wiki/Functional_regression;

https://functionaldataanalysis.org/chapter_05.html). The delta-temperature is a continuous function over time, as are the other covariate values. A functional data approach does not require the discretization of the functions to daily values. Rather it models the relationships directly between the functional response and functional covariates.

It is important to include more assessment of model performance, such as time-series plots of model fit to the observations and residuals. The models provide some very specific numbers with respect to the effects of WTT and temperatures. What is the appropriate level of confidence for the absolute value predictions? Would an uncertainty or sensitivity analysis help? And how important is it to have precise numbers? This is a fundamental question about how the models and their generated relationships will be used. Presentation of model performance and guidance on interpretation should be added to the chapter.

It is not clear how to define an “observational unit.” For example, Equation 9.1 defines a daily average input temperature, and Equation 9.2 defines a daily difference in temperature. However, this seems arbitrary. One could also define a 12-hour change in temperature or a 2-day average difference in temperature. Because of possible diurnal variation, are the daily averages computed using ordinary clock time (e.g. from 0:00 to 23:59) or from noon-to-noon (e.g. 12:00 to 11:59)? Similar questions apply to determination of the “daily” covariates to be applied to an observation. Further explanation as to why the regression analysis used “daily” values would help.

How is the lag time (WTT) determined to be used in Equation 9.2? This is missing from the chapter. While WTT has been widely used by the CSS in the report, some explanation of its derivation would be helpful here. The WTT is described as measured in days. Is this a real or integer variable?

Equation 9.5 only shows fixed effects, yet the paragraph above the equation says the linear mixed models were fit. Please clarify this.

Equations 9.7 and 9.8 appear to model a random slope intercept model allowing for random variation in these parameters across years. What were the estimated coefficients?

The analysis that combined the two reaches needs further explanation in how it was computed, and the assumptions needed to allow the two reach values to be added together. Both models were for June and July, yet they are treated as additive and sequential. Does delta T depend on

temperature? Was the temperature of the downstream reach updated by the delta T of the upstream reach?

The chapter says that an information theoretic approach is used, but no details on the multiple models fit is provided. Table 9.1 presents regression coefficient estimates. Are these from the top model or model-averaged over all models? Predictions appear to be based on the highest ranked model rather than model averaged.

The interaction effect between WTT and air-water gradient is very weak in Figure 9.5 but quite strong in Figure 9.7. Understanding this is a new analysis, the results could be more thoroughly explained and interpreted. Why do the models differ in some key ways between the two reaches?

Figure 9.2: These relationships look reasonable, except in the hottest of years (2015, 2021). How should one interpret that the discharge-weighted relationships are less predictive in the most extreme years?

p. 262 (bottom). Please briefly explain how water transit time across a reservoir is dealt with in the estimation. Methods are not described. How confident are we in these estimates, and does error in WTT have any significant effect on the key findings, or are the temperatures somewhat steady during this time?

p. 265. The comparison plot between modeled and observed for CNFL versus MCN (similar to Figure 9.2) would have been helpful here.

Eq. 9.11 and Figure 9.5: How important is it that these values for each thermal gradient are exact? What is their intended use? Is the point to demonstrate the key processes (WTT and thermal gradient), or is it expected to be used to change operations to modify WTT?

The Snake model (page 266) describes the variability well (73%) and thus is likely capturing the key factors. However, the Lower Columbia model explained a much lower proportion of the variability (43%). Why does the Snake model perform so much better than the Lower Columbia model? Does this affect how the models should be used? For example, is it possible that this is a result of analytical decisions? Examining Figure 9.6, this reach seems to be missing extreme temperatures, and the model fits the few high temperatures pretty well (as opposed to the Snake model, which fell apart a bit at extreme temperatures). Is it possible the averaging of the two datasets might be a source of error or at least variability? Please discuss this difference in performance and offer possible reasons why this occurs.

Minor comments

The goals and objectives of the chapter, and why they are important to address, should be clearly stated in the Introduction.

A short explanation, with appropriate references cited, of how WTT is derived and is related to water level and reservoir outflow would help some readers. When WTT is defined, please add a brief description about how temperature increases are manifest across the entire period of travel through a reach, such that longer WTT values not only increase the temperature of that reach but also the time period of exposure.

A more intuitive description of the within-year and between-year models would help the reader.

Please clearly state the reasons for including environmental co-variates in Analysis 3. The objective of this can be inferred but should be explicitly stated in the text. Seeing the plots of the covariates and the final model equations and their fits would help.

p. 265. Is the use of the nearest data at Priest Rapids dam representative of the McNary reach? It may be, but not enough information is presented to understand how they may be different.

p. 265. How different are WRNO and CCIW? The approach makes sense if they are very similar, but as presented, it is not clear if any important information is lost by averaging them.

p. 266. For the fixed effects model, is the assumption of normally distributed residuals appropriate? Was it tested? There are reasons why they wouldn't be, but it would be better to know.

Figure 9.1 is difficult to interpret; it could be divided into multiple plots (perhaps by year). Where is ΔT in the figure? It is referred to in the text. This relates to the more general suggestion that the description of Analysis 2 needs expansion. Maybe a table with the equations would help?

Figure 9.2 shows that the estimated lag was reasonable. Is this the correct interpretation?

Figure 9.4: The performance of the model decreases at higher temperatures. Is this related to the issue noted in Figure 9.2? Figure 9.4 is important, as it shows the performance of the prediction of ΔT . Is the ΔT shown the same as the response variable in equation 9.6?

Perhaps explain in the beginning of the Discussion why the results mostly pertain to adult migration. The discussion of management implications and effects of the dams in the Discussion section is compelling; Please consider expanding this discussion.

p. 274. This final paragraph could be moved to the Summary Conclusions. This would ensure the management implications are widely read.

Editorial comments

Top of page 265. prior to and ending is missing the “to”

p. 265. TDGS and REML should be defined.

p. 267. Two lines under the top bullet points. β_3 and β_4 's subscript needs fixing.

Table 9.1: Too many decimal places reported for the format. Presumably $p < .000$ should read $p < .001$.

Figure 9.1: Ice Harbor tailrace acronym in the y axis should be defined in the figure caption.

Figure 9.2: What are the multiple lines overlaid on the plot? This should be described in the legend.

Figure 9.3: The black dots should be identified in the figure caption. Ideally, all of the relevant dam locations that establish reaches (e.g. McNary, Priest Rapids, etc.) would be labeled in this figure.

Equation 9.5: Use proper subscripts in the equations.

Equation 9.6: Delta T has an R subscript in Equation 9.6 but not in Equation 9.5. Was this intended?

V. ISAB Appendix: Suggested Topics for Further Review 2011-2025

[ISAB 2025-3](#), pages 5-6

1. CSS and/or others, develop a focused assessment of the influence of climate-related and density-dependent factors on the marine survival of Columbia River salmon.
2. Conduct a brief review of the methods currently and previously used by others and potential new methods that could be used to better understand smolt and adult survival in the estuary below Bonneville Dam, in the North Pacific, and throughout the Basin.
3. Further analyze the effects of hydrosystem operations on overshoot, fallback, and straying and include other stocks and species, such as hatchery steelhead and spring Chinook salmon, and at finer resolution within basins or around specific dams and river confluences.
4. Develop a “multi-model guide” to help standardize the approach and documentation so that readers can better understand analyses that use multiple models and properly interpret differences and similarities in predictions.

[ISAB 2024-3](#), pages 4-7

5. Consider including coho salmon (e.g., in the PIT tagging effort and reporting) to help understand the dynamics of success of the large reintroduction effort in the mid and upper Columbia River, and the potential for inter-specific effects on other salmonid populations as coho salmon become more abundant.
6. As recommended in the ISAB’s 2023, consider how to incorporate the influence of climate-related and density-dependent factors on the marine survival of Columbia River salmon in future reports.
7. Revisit the ISAB’s suggestion from the review of the 2023 CSS report on breaching by examining lessons learned from Klamath dam removals, which were removed this year [2024], and other dam removals in the region.
8. If analyses are preformed that involve *a priori* selection of specific and limited explanatory variables, the description of the methods should clearly describe why those variables were selected and the implications of their use for the results and interpretation.
9. Consider expanding certain analyses (e.g., SARs and benefits of flow augmentation) to sockeye salmon.

10. Some analyses would benefit from further resolution of the ocean phase (SARs), analyses of flows to include the lower estuary (WTTs), and decomposing results into year-types to more fully understand the robustness of general patterns.
11. When information is available on the details of the Columbia River Treaty, analyze the likely effects of the revised treaty on passage and survival metrics based on observed responses in the system to date.

[ISAB 2023-2](#), pages 7-9

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 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.
3. 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.

[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, condition factor, and date of ocean entry—to survival. The current literature is

confusing (e.g., Faulkner et al. 2019 vs 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.

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