



# FISH PASSAGE CENTER

847 NE 19<sup>th</sup> Avenue, #250, Portland, OR 97232

Phone: (503) 833-3900 Fax: (503) 232-1259

<http://www.fpc.org/>

e-mail us at [fpcstaff@fpc.org](mailto:fpcstaff@fpc.org)

## MEMORANDUM

TO: Rich Alldredge, Chair, ISAB  
ISAB Administrative Oversight Panel  
Bill Bradbury, Chair, NPCC  
Paul Lumley, Executive Director, CRITFC  
John Stein, Science Director, NOAA Fisheries Science Center

FROM: Michele DeHart

DATE: November 30, 2013

RE: Response to ISAB comments on the Draft 2013 Comparative Survival Study Annual Report

Attached, please find the Comparative Survival Study (CSS) Oversight Committee responses to ISAB comments on the draft 2013 Comparative Survival Study Annual Report. As in past years the ISAB comments are insightful and have improved the report overall. The response to each of the ISAB's comments is presented in *Italic font* following the original comment.



**Independent Scientific Advisory Board**

*for the Northwest Power and Conservation Council,  
Columbia River Basin Indian Tribes,  
and National Marine Fisheries Service  
851 SW 6<sup>th</sup> Avenue, Suite 1100  
Portland, Oregon 97204*

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

Richard Alldredge

Kurt Fausch

Colin Levings

Katherine Myers

Robert Naiman

Bruce Rieman

Greg Ruggerone

Laurel Saito

Dennis Scarnecchia

Carl Schwarz

Chris Wood

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# ISAB Review of the Draft 2013 CSS Annual Report

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# ISAB Review of the Draft 2013 CSS Annual Report

## I. Background

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

## II. Summary

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

The ISAB suggests five topics for further CSS review:

1. hypotheses on mechanisms regulating smolt-to-adult survivals (SARs)
2. life-cycle modeling questions and Fish and Wildlife Program SAR objectives
3. data gaps
4. rationalization of CSS's Passive Integrated Transponder (PIT)-tagging, and
5. publication of a synthesis and critical review of CSS results

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

A detailed reevaluation of SAR objectives (2-6%) is warranted. These objectives should be reevaluated for each species and Evolutionarily Significant Unit (ESU) of salmon and steelhead based on realistic values needed to support robust viable populations. Discrepancies in SARs between PIT-tagged and non-PIT-tagged fish reported in other publications raise two important issues that could be addressed now: (1) what are the implications of correcting biased SAR

estimates from PIT tags with respect to performance against recovery and Fish and Wildlife Program objectives, and (2) what proportion of US Endangered Species Act (ESA)-listed populations are being PIT-tagged and what are the implications for imposing this additional mortality? Further work is needed to analyze the relationship between the ratio of transport/in-river SARs and in-river survival. With many years of experience now, the CSS needs to identify critical data gaps. What crucial pieces of information are not addressed by the CSS, and what improvements can be made to provide them? Some examples provided by the ISAB include the lack of habitat-specific estimates of smolt survival in the estuary, information on how age at maturation affects SARs, the contribution of mini-jacks to SARs, and the relationship between SARs and biomass of adult returns of hatchery and wild salmon.

**Response:** *Many of the questions raised in this discussion are beyond the scope of the CSS. There is a limited budget of funds, staff and mark groups in the study. The CSS Oversight Committee expends considerable effort in generating the data and analyses that are presented to the region. CSS provides all of the data and analysis to the region so that managers may apply these data to these prevailing management questions. Although the ISAB questions are valid, the reality is that the CSS Oversight Committee cannot address all of the prevailing biological and management questions in the region.*

*Some of these questions are the purview of NOAA Fisheries in their role as ESA managers. For example, the question regarding the proportion of ESA listed populations that are marked or PIT tagged, is within the purview and under the oversight of NOAA Fisheries. They have an established process of reviewing, reporting and issuing take determinations under ESA. The CSS marking program annually requests permits for determination of take for research purposes from NOAA and follows the guidelines set forth by that process. This question clearly falls under the authority and mandate of NOAA Fisheries, and is not an appropriate undertaking for the CSS Oversight Committee.*

*Where possible, a number of the above questions have been addressed in these responses to comments on the 2013 CSS draft report. Additional pertinent information on age-at-maturity and more information on the mini-jack question has been supplied in the chapter 4 (overall SARs) responses below. The issue of whether or not there is a bias in PIT tag SARs has been continually addressed in Chapter 4 each year and as more information becomes available from ongoing studies it will be incorporated here. Finally, questions on the TIR vs.  $S_R$  relationship were addressed in the Appendix A section below.*

*A few of the above questions were within the reach or scope of the CSS and, where possible, have been broached in CSS analyses. Age at maturation has been a focal point of a chapter in the last two CSS annual reports (see chapter 7 of CSS 2011 and chapter 6 of CSS 2012 annual reports). One goal of the CSS 2011 workshop, which included 27 attendees from state, federal,*

*tribal, and academic entities, was to hold collaborative discussions and evaluations of knowledge gaps, uncertainties, and futures steps in regard to the CSS. In regard to delayed mortality, several hypotheses were discussed. One key comment regarding delayed mortality was, "The evidence presented for the impacts of the hydrosystem on survival and for delayed mortality arising from earlier experience in the hydrosystem is strong and convincing . . . it is difficult to imagine how any of these [other factors] would align so well, both in time and space, with the establishment of the hydrosystem." The results from that workshop were included as Appendix G of the CSS 2011 report.*

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

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

An alternate but similar approach would be through the use of state-space models in a Bayesian context. This would allow the incorporation of natural variation in the transition between life stages that is currently ignored in this approach and the ability to incorporate prior information on some parameters in a more natural way rather than, for example, assuming the

second year of ocean survival is exactly 0.6. Applying a hierarchical approach to the spawning parameters across the populations would also allow some sharing of information across population when data are sparse. Finally, it would also provide probabilistic forecasts of future population trajectories.

### **III. Suggested Topics for Further CSS Review**

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

**Response:** *We agree that there are some stocks of salmon and steelhead in the Basin that have relatively high SARs. CSS has analyzed and compared these stocks where the mark group data has allowed comparisons. There are limited PIT tags mark groups on Hanford Reach Fall Chinook which restricts the analysis options. However, John Day River wild steelhead and Chinook, and Yakima River wild steelhead and Chinook and hatchery Chinook PIT tag mark groups have been utilized to generate SARs and have been included in CSS Annual Reports.*

*Discussion of these groups is included in Chapter 4. There are fall Chinook groups that are not represented in the Comparative Survival Study to date. The fall Chinook analyses that are included in the CSS are a utilization of large mark groups that were generated by other studies. In future reports, we fully expect to include analyses and discussion for the 2013 fall Chinook return where there is available data. The Snake River fall Chinook groups that are included in the CSS stopped being PIT tagged in 2012 but those marked smolts will include coverage for 2013 returning adults.*

*Regarding fall Chinook PIT tagging for the future, the CSS Oversight Committee has discussed the possibility of collaboration with state, federal and tribal fall Chinook marking programs but has not yet been successful. The ISAB has identified many prevailing key questions that relate to many aspects of salmon management and recovery including harvest management and hatchery management. We agree that these are important questions for the region, but are beyond the scope of the CSS study. Many of the questions raised have been addressed in previous annual reports, such as age at maturity for spring/summer Chinook; these analyses were presented at past CSS Annual Review meetings. In these past analyses, it was clear that age at maturity was strongly stock specific for hatchery stocks, and that hydrosystem operations and passage did not appear to affect age at maturity.*

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

**Response:** *Upstream migration success is addressed in past CSS Annual Reports (for example chapter 5 in the 2011 report). These analyses indicated that upstream migration success is affected by juvenile downstream passage history. Transported smolts had a lower upstream migration success, and steelhead transported as juveniles had a higher stray rate than non-transported steelhead. The time series of estimates for adult success for previously transported*



*smolts, in-river outmigrants, and the run-at-large is updated in each annual final report (Appendix H of the 2013 final report) so that managers may apply these data to these prevailing management questions. The CSS provides these data to fishery managers that are responsible for harvest and fishery management for their consideration and analyses. The broad range of management questions and issues raised by the ISAB goes beyond the scope and assignment of the CSS. Although the ISAB raises some important questions, they are beyond the reach of the CSS and may be addressed in other studies. However, we agree that CSS data and analyses could contribute to addressing these questions.*

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

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

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

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

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

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

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

**Response:** *The above comments are reiterated in the chapter 2 comments. A detailed response is provided there.*

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

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

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

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

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

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

**Response:** *We do not disagree with the ISAB discussion of data gaps. However, the CSS has a limited scope, limited work statement, limited staff and limited funding. Many of the issues identified by the ISAB address, hatchery management, hatchery evaluation, harvest management, CSS data and analyses are made available to the region so that managers responsible for addressing these issues can utilize these data.*

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

**Response:** *The CSS collaborates with numerous projects and agencies to address multiple objectives, and prioritize tagging efforts for program efficiency and cost savings. We will continue to pursue further efficiencies in coordination of objectives and tagging where possible, however, full integration of tagging programs may be very challenging and reaches far beyond the scope of the Comparative Survival Study Oversight Committee. Recognizing the limitations of the influence of the CSS Oversight Committee, the Committee does expend considerable effort to coordinate with and utilize PIT tag groups from other studies to maximize the efficiency and application of PIT tag groups. Chapter 1, Tables 1.1, 1.2 and 1.3 show the mark group coordination that took place in 2013. Overall the CSS strives to maintain and balance mark groups to generate SARs and passage survivals over the range of wild and hatchery production. Development of stock specific estimates requires adequate mark groups, for some populations this could result in marking a large proportion of the juvenile migration which could result in disproportionate marking impacts. The CSS Oversight Committee works to minimize marking proportion and marking impacts while generating robust time-series of SAR and passage survival data for the region.*

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

**Response:** *We appreciate the ISAB suggestion to continue publishing our most significant findings. The CSS and individual participants have published a number of reports and papers addressing aspects of the question of efficacy of transportation and related hypotheses. Because the TIR and D metrics are only relative measures, the issue of transportation efficacy is*

*closely tied to the issue of delayed mortality on in-river migrants (i.e., the life-cycle effect of the FCRPS).*

*Specifically, in addition to CSS Ten-year and annual reports, we would point to the 2004, 2011 and 2013 CSS Workshops (Marmorek et al. 2004, 2007; Hall and Marmorek 2013). These workshops retrospectively evaluated the weight of evidence for and against the delayed mortality hypotheses, and developed prospective models to explore spill management experiments. CSS-related journal publications addressing these topics include Budy et al. (2002), Wilson (2003), Schaller and Petrosky (2007), Petrosky and Schaller (2010), Haeseker et al. (2012) and Schaller et al. (in press).*

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

### **Chapter 1. Introduction**

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

#### **Specific Comments and Questions**

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

**Response:** *The CSS has not considered that a potential PIT-tag loss would be expressed at a different rate for transported vs. in-river outmigrants. The CSS oversight committee is not aware of research that suggests that this is happening or of a reasonable causal explanation for such a mechanism. Any potential losses are assumed to be equal for each outmigration type.*

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

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

**Response:** *The figure has been modified following the ISAB 2012 and 2013 comments and should be more quickly and easily digested by the reader. However, the reader is assumed to have some level of familiarity with salmon life history.*

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

**Response:** *Although the CSS makes use of all pertinent and available PIT-tag information through the PTAGIS database, the installation of PIT-tag detectors in the Federal Columbia River Power System falls under the purview of the Army Corps of Engineers. State, Tribal, and Federal management agencies are consulted on the location of various PIT-tag detectors but the Comparative Survival Study is not. Ostensibly the decisions to install or not install PIT-tag detection incorporates logistic concerns, needs of the basin, and cost.*

*Marking does occur at the John Day rotary screw traps. The figure is in need of updating to reflect the current marking sites used in CSS and we plan to incorporate an updated map into the 2014 CSS report.*

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

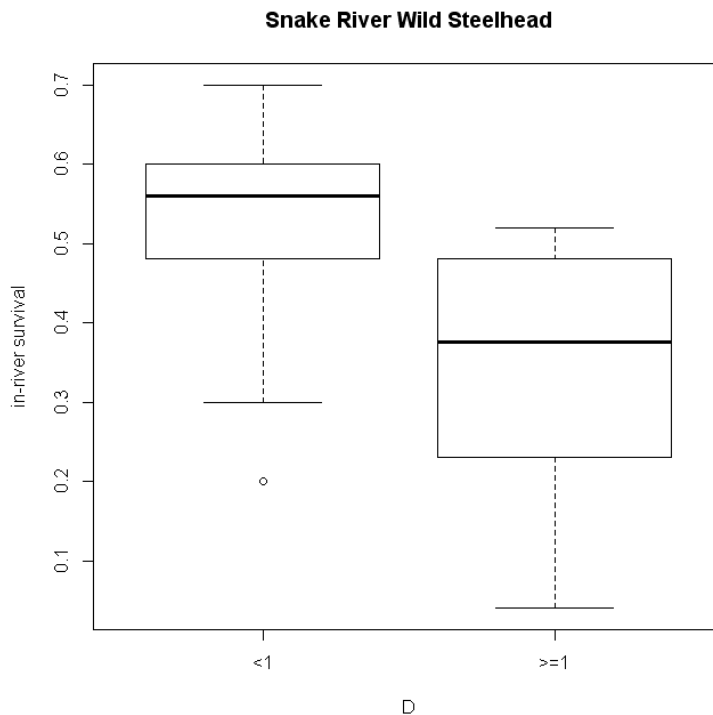
**Response:** *To provide the reader with some feeling for the range of detection probabilities we've added a sentence to indicate that detection probabilities can range from 5-90% depending on interactions between species, site, and environmental conditions and facility operations while smolts are passing. Understanding how these factors influence fish route of passage is a complex question. This was a key point of the CSS 2013 Workshop and presented in the workshop report which was Appendix B of this draft CSS report (see page 314 of the draft report). Also, this subject was covered in Chapter 2 of the CSS 2012 report.*

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

**Response:** *It is not considered in either the analysis or interpretation of the results. If such a mechanism is occurring, it is inseparable from the in-river experience. The CSS is an observational study where  $D$  is used to compare the survival of two outmigration experiences that occur every year in order to assess the transportation program. Hypothetical outmigration experiences (e.g., in-river outmigration without the suggested culling of “less fit” fish) are not compared.*

*If the hypothesis is true, then it implies that a lower hydrosystem survival produces a “more fit” smolt at Bonneville. If such a mechanism were occurring then as in-river survival decreases a lower value for  $D$  would be expected because culling is increased and post BON in-river smolts would be “more fit” than their transported counterparts (i.e.  $D < 1$ ). Conversely, under higher in-river survival conditions a reasonable expectation is that  $D$  would be higher ( $D > 1$ ) because of the increase in “less fit” in-river smolts.*

*An examination of the in-river survival for wild steelhead plotted vs.  $D$  (both are presented in Appendix A of the draft report) provides some evidence that the hypothesis in the question is not true. The plot below shows in-river survival in two blocks: (i)  $D$  is less than one; and (ii)  $D$  is greater than or equal to one. When  $D$  is less than 1 (i.e., the in-river group had relatively less post BON mortality than the transport group) in-river survival was relatively high. When  $D$  is greater than 1 (i.e., the in-river post BON mortality for in-river group greater than transported) in-river survival was relatively low. These results are the opposite of what would be expected under the stated hypothesis.*



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

**Response:** *This subject is covered extensively in the method sections of the report for Chapter 4 (overall SARs) and Appendix A (TIR, D, and SARs by outmigration type). It is difficult to broach this subject satisfactorily in the introduction chapter without covering much of the methods that are covered in detail later in the report. To prevent confusion we've reworded this section but left the more complete methods in their respective chapters where they are near the results and discussions pertinent to those methods.*

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

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

The CSS's objective in this chapter is to develop a multiple-stock model that links freshwater spawning and rearing (FSR) production and survival to mainstem passage survival and ocean survival. The ultimate goal is to use the model to assess important management scenarios to recover spring Chinook salmon. Three different versions of the model (BH, LC, and LCX) with



different levels of complexity were evaluated using data for the Grande Ronde Major Population Group (GRMPG) of spring/summer Chinook salmon. The investigation uses a long time series of SAR data and smolt per spawner data for up to six populations within the GRMPG. The analytical approach is characterized as a first step, indicating the investigators plan additional work with the model. The ISAB encourages the investigators to continue with model development and to explore ideas presented in the report. Nevertheless, there are numerous difficulties in the description of the model that make it uncertain if the three model versions have been implemented correctly in this initial step. Below, the ISAB provides a number of detailed comments on the analysis.

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

**Response:** *The model was implemented initially as a process error model in the early stage of development specifically to focus attention on the mechanisms proposed, i.e., environmental forcing of survival, and not to conflate interpretation with observation uncertainty. The advice to follow a state-space approach is the next logical step and will be pursued in the 2014 implementation.*

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

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

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

**Response:** *These questions were the motivation for developing a life-cycle analytical framework because they so clearly point to two aspects of the system that can be affected by management intervention and result in a higher number of recruits per spawner, one by increasing the number of smolts entering the main stem of the Columbia, and the other by increasing the*

*survival of smolts passing through the hydrosystem. Before proceeding with a comprehensive analysis to answer the questions, the CSS will engage in further model validation so that that analysis performed will be based on a more rigorously tested model foundation. At this stage of development of the life-cycle model, it appears feasible to proceed with an analysis of question 2 in the 2014 implementation of the model.*

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

**Response:** *Chapter 2 in the final report will include more statistical model fitting results, including AIC table, and standard errors of parameter estimates.*

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

**Response:** *Plots of survival against variables included in final revised chapter. Magnitude of variation explained by each variable plotted.*

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

**Response:** *Correlation plot of recruits per spawner for each population included in revised chapter 2. There are no missing survival values, only missing smolt or adult abundance data for a population. This would only mean that the predicted survival would be influenced by the other populations for the missing years.*

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

**Response to comments on assumptions of life-cycle models:** *(1) The fact that the MPG has historically been classified as a population group, which implies a common pool in both fresh and ocean stages is the rationale for continuing to treat the population as a common ocean stage for the early stage of development of this model framework. (2) The populations are at low historic densities, and density dependence (through competition) is not likely driven by just the GRMPG, but rather an aggregate of salmon populations that goes far beyond the GRMPG. Looking for sensitivity to density in the ocean would need to include abundances from many other populations. Setting the density dependent parameter to infinity (actually 1.0E+12) merely states that the GRMPG data is not uniquely determining its own density limits. (3) The significance of PDO, UPW and NPH is a mechanism being implemented with this framework. The rationale was provided by Petrosky and Schaller 2010. (4) Survival in year of ocean entry affects all ages of returns. Subsequent years are less influential on overall survival of smolts because the rates affect a smaller portion of all returns. The 2014 implementation will treat the 2<sup>nd</sup> and 3<sup>rd</sup> ocean-year survival rates as variable in the state-space version of the model.*

**Response to growth comment:** *It is true that growth can be treated as a surrogate for survival, and that predicting growth could improve the prediction of survival. However, the life-cycle model is not currently implementing a growth prediction. It is possible that, once the model has been more thoroughly vetted and integrated with more temporal resolution in the mainstem, the analysis will progress toward estimating growth. Until mainstem modeling improvements are made in 2014, growth modeling will not be implemented.*

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

**Response:** *The CSS oversight committee is aware of modeling efforts by NMFS scientists, but has elected to develop a life-cycle model that can be used to investigate mechanisms of survival through the hydro-system, in addition to those in the ocean and the tributaries. This is necessary to have a framework capable of addressing question (2). The members of the CSS committee are in regular communication with NMFS and ODFW scientists in related modeling efforts.*

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

**Response:** *CSS oversight committee members have brought these findings to the attention of other investigators familiar with the Grande Ronde basin, and confirmed similar findings in other modeling analyses, indicating no major conflicting results. The CSS makes no claim that the life-cycle model in its current form has addressed the potential for explaining variability in tributary productivity or capacity, but rather that the model structure lends itself well to explaining freshwater variability in survival in the same way as it did for survival from LGR to end of first year in the ocean, i.e., to relate productivity to environmental conditions. In 2014, the life-cycle model will incorporate environmental conditions in the tributaries.*

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

**Response:** *Values in Table 2.3 are in smolts per spawner. Values in Table 2.4 are in spawners. The relative estimates are empirically fit to smolt production for 4 of the 6 populations. The estimates for the 2 populations without empirical smolt data are inferred, and reflect that these fish would likely have experienced the same conditions as the other 4 populations. If that assumption does not hold, there could be a bias.*

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

**Response:** *Fecundity does indeed vary with age, but if the relative proportions of returning age classes is not systematically changing, then accounting for age specific fecundity is unnecessary. The average fecundity of a mixed age escapement would be the proportion-weighted average of each age fecundity. The CSS is aware that this assumption may not hold, i.e., that age at maturity is showing a temporal trend, but we think this mechanism needs to be investigated independently of the life cycle model, otherwise the maturation rates become highly confounded with survival rates. Comment: surely survival and maturation rates would be confounded, but this is chiefly related to the ISAB's point here, which remains valid.*

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

**Response:** *This analysis makes no empirical estimates of the effect of habitat conditions on spawner to smolt productivity. We can only infer a decline in productivity for some populations from the fact that the model is under-predicting production in early years for those populations. In other words, the estimated average productivity over the time period is lower than early*

*productivity, indicating a decline. Model improvement for 2014 will include environmental conditions in tributaries.*

### **Specific Comments and Questions**

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

**Response:** *The CSS is concerned that including brood table data for six populations won't do much to clarify the model description. The data can be downloaded from the link provided. The process error does not capture uncertainty in observed data. This will be implemented in 2014 with a state-space model.*

P 23. Figure 2.1 indicates that smolts are subject to two survival rates ( $s_0$  and  $s_1$ ), but equation (2) only uses  $s_1$ . This needs to be clarified. The ISAB does not understand the figure. Why are there three spawner boxes with the first two for populations 1 and 2? What are populations 1 and 2, and why are they special?

**Response:**  *$s_{01}$  is the product of  $s_0$  and  $s_1$ . This is clarified in methods and figure of revised chapter.*

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

**Response:** *Model prediction is deterministic statistically. Variation is assumed in the prediction of smolts and adult returns for minimizing the deviates of predicted minus observed, but not each individual stage transition. The variability in productivity comes from being forced by PDO, UPW, and NPH, which vary in time. Notation in equations and figure are consistent in final Chapter 2.*

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

**Response:** *The run of fish of age  $a$  in year  $t+a$  are recruits for brood year  $t$ . The indexing is always for brood year  $t$ , and therefore not all recruits return in the same year. Fish spawn in different years, but they are still recruits to the same brood year.*

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

**Response:** *Symbol typos noted. Notation remedied to be consistent in Tables, Figures, and Equations.*

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

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

**Response:** *AIC results presented in revision. Deviates are also included.*

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

**Response:** *Prediction of future trajectories requires inclusion of future values for PDO, UPW, and NPH. 2014 chapter will use projected environmental conditions from University of*

*Washington Climate Impact Group's climate model scenarios as the basis for predicting salmon population trends.*

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

**Response:** *As stated in the report, the values are not actually infinite, just very high, so effectively producing very little evidence of density dependence within the range of data used to fit the model for those populations. At least for the WEN and IMN, it is also a product of the fact that there is no smolt data, and says less about a failure of the LC model to effectively disentangle the sources of variability in survival than it does about how the LCX model overcomes this shortcoming. Sampling correlation, if the comment has been properly understood, implies a Bayesian posterior. These will be included in 2014. The estimated covariance is included in the revised 2013 Chapter 2.*

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

**Response:** *Standard errors included.*

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

**Response:** *The improvement in the fits from BH to LCX provide evidence that main stem/estuary/ocean variability in survival may be affected by environmental conditions from LGR to the ocean. If one population in the MPG responds differently than others, it suggests that either it does not behave like the others from main stem to the ocean, or the difference is in the tributary. The fact that CC and GR populations fail to respond as well to the inclusion of those factors suggest that some variability in survival might be explained by conditions in those tributaries.*



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

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

#### **Specific Comments and Questions**

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

**Response:** *Fall Chinook salmon were included in the chapter, and are referred to as “subyearling Chinook salmon.”*

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

**Response:** *A figure (3.1) is now provided describing the reaches analyzed.*

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

**Response:** *We have reduced the description of the mortality rate and its interpretation. However, our application is somewhat unique in that the mortality rate is not based on a fixed time interval, as is the case in most applications. We have added language in an effort to better describe this.*

P. 44, l. 24. Equation 3.3 needs some care. The estimated value of Z applies to all fish that start passage at the start of a reach, including those that do not survive to the end of the reach. However, the mean FTT is based only those fish that survive the entire reach. Consequently, the mean FTT is based on fish that tend to swim faster (because they did survive) and so will “underestimate” the exposure time of the entire cohort of fish. It should be possible to modify the CJS model to incorporate both the travel time and Z directly in the estimation process, or

perhaps small simulation in an appendix will show that equation (3) works well enough that any bias is negligible. [Again, the responses to the 2012 report dealt with this issue; perhaps adding a paragraph to future reports is justified.] The modified CJS model would also give the SE of Z directly.

**Response:** *It is true that the FTT estimate is based only on those fish that survive and therefore may show some degree of negative bias (i.e., imply a faster migration than the full release cohort which includes both the observed and unobserved individuals), especially in cases where travel times are slow and mortality rates are high. We have conducted a simulation exercise (see Appendix) to evaluate our approach for estimating FTT and Z. Based on these simulations, however, the degree of bias appears to be small (<10% for FTT and <5% for Z).*

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

**Response:** *It would be possible to estimate the variance of Z using a bootstrapping approach, however it is unlikely that the variances would change. We will attempt to verify that this is the case by next year.*

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

**Response:** *We have revised the chapter to now include model-averaging for all FTT and Z model predictions, along with presenting figures with the model-averaged coefficients and relative variable importance values. We feel that presenting figures with the model-averaged coefficients along with their unconditional confidence intervals, along with figures of the relative variable importance values is the most effective manner of conveying the results of the multi-model inference results.*

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

**Response:** *The most recent Box-Cox analyses indicated that a square root transformation was most appropriate for Z, and the equations now reflect this.*

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

**Response:** *The interpretation is supported by the model-averaged coefficients and relative variable importance values, which indicate that spill is an important factor for reducing FTT of subyearling Chinook salmon.*

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

**Response:** *We have revised the methods and have adopted a multi-model inference approach for predictions and now present figures containing the model-averaged coefficients along with their unconditional confidence intervals and relative variable importance. We have revised the text to reflect the signs and importance of each of the variables for each species-reach presented.*

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

**Response:** *We have revised the methods and have adopted a multi-model inference approach for predictions and now present figures containing the model-averaged coefficients along with their unconditional confidence intervals and relative variable importance values. We have revised the text to reflect the signs and importance of each of the variables for each species-reach presented.*

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

**Response:** *This is an excellent comment. In response, we have evaluated the standard linear regression (LM) model (which treats each estimate as an independent observation) along with two forms of mixed-effects models. Following the suggestion, one mixed-effect model (ME) treats the release cohorts as a random effect within each year. We also fit a mixed-effect model that allows the within-year random errors to vary according to a lag-1 autoregressive process*

(AR1). Results generally showed that the AR1 mixed-effect model was appropriate for the FTT data, while the standard linear regression model was appropriate for the Z data (Table 3.3).

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

**Response:** *While it is true that the model-averaged predictions for Z are somewhat low, the model-averaged predictions for FTT are very high. Our approach to modeling survival is to use the model-averaged predictions for Z and FTT in order to make predictions about survival. The  $R^2$  values for those survival predictions indicate that our approach does have predictive power, with the average  $R^2$  for survival being 49%.*

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

**Response:** *We have revised the methods and have adopted a multi-model inference approach for predictions and now present figures containing the model-averaged coefficients along with their unconditional confidence intervals and relative variable importance values. We have revised the text to reflect the signs and importance of each of the variables for each species-reach presented.*

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

**Response:** *We have revised the methods and have adopted a multi-model inference approach for predictions and now present figures containing the model-averaged coefficients along with their unconditional confidence intervals and relative variable importance values. We have revised the text to reflect the signs and importance of each of the variables for each species-reach presented.*

## **Chapter 4. Patterns in Annual Overall SARs**

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

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

**Response:** *The CSS intends to begin exploring this issue during 2014.*

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

**Response:** *We added a citation (Brignon and Haeseker 2011) to the text regarding the Carson PIT-tag study. Final results from the study are not yet available.*

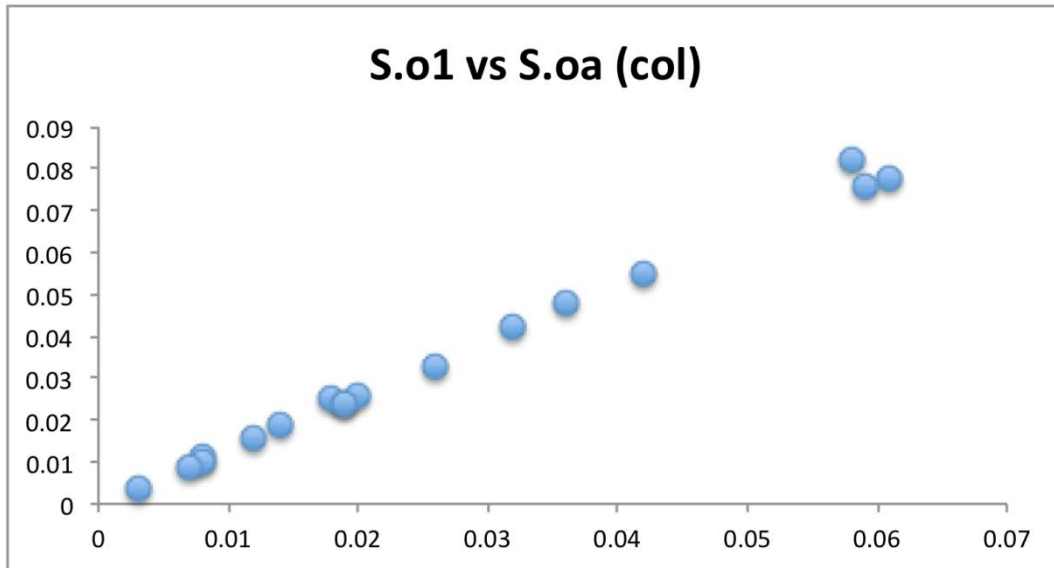
### **Specific Comments and Questions**

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

**Response:** *The  $t_i$  are not equivalent to the  $X_i$  in Appendix A. The  $t_i$  are [total detected \* percent transported], with percent transported coming from the SMP run-at-large collection and transportation data from the Snake River collector dams in the weighting (see text on p. 60).*

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

On page 85, the investigators note that S.o1 and S.oa are perfectly correlated, so why include S.o1?



**Response:** We agree that S.o1 and S.oa provide essentially the same information for Snake River spring/summer Chinook and steelhead, as well as other ESUs/DPSs that are largely unexploited in ocean fisheries. However, we suggest that there are two rationales for continuing to report both metrics:

- 1) S.o1 has been used in the literature and will likely continue to be used in life-cycle models (e.g., matrix models) for these populations. Reporting of both metrics also provides a scalar (albeit, assumption dependent) between expected first year and overall marine survival for these species.
- 2) For populations that are exploited in ocean fisheries, marine survival calculations need to account for fishing mortality at different ages, incorporating assumptions for natural mortality similar to our assumptions in the S.o1 calculation. As noted in the methods, our approach is consistent with other cohort-based Chinook modeling studies (e.g., Pacific Salmon Commission 1998) for exploited stocks.

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

**Response:** *The proportions of jacks in the return to Bonneville Dam for CSS spring/summer and spring Chinook groups are shown below (source: Chapter 4 tables). Both stock and year differences are apparent. Migration years 2008 and 2010 had higher than average jack proportions, while the 2009 jack proportions were about average. Jack proportions for Snake River wild Chinook were lower than for most Snake River hatchery Chinook (except DNFH). In the mid and upper Columbia, Cle Elum Hatchery (CLEE) had noticeably higher jack proportions than the other CSS groups. The 2012 CSS report (Chapter 6) suggested that “significant, across-stock variation in age at maturity and jack proportions by outmigration year indicates that there is some factor or factors that are influencing age at maturity across Columbia River Basin stocks.”*

Proportion of jacks in return at BOA, smolt migration years 2000-2010.											
Migration year	Snake wild	DNFH	RAPH	CATH	MCCA	IMNA	John Day wild	Yakima wild	CARS	CLEE	LEAV
2000	0.03	0.01	0.01		0.10	0.23	0.02	0.08	0.01	0.09	0.01
2001	0.09	0.14	0.07	0.14	0.17	0.31	0.06	0.20	0.02	0.04	0.00
2002	0.12	0.14	0.16	0.19	0.19	0.25	0.05	0.02	0.03	0.21	0.04
2003	0.00	0.03	0.20	0.21	0.28	0.46	0.04	0.15	0.00	0.31	0.05
2004	0.01	0.00	0.02	0.08	0.16	0.16	0.05	0.02		0.17	0.02
2005	0.03	0.00	0.06	0.12	0.08	0.19	0.10	0.00	0.03	0.16	0.22
2006	0.07	0.18	0.13	0.13	0.15	0.30	0.04	0.10	0.05	0.23	0.09
2007	0.09	0.21	0.23	0.42	0.34	0.48	0.14	0.00		0.33	0.13
2008	0.14	0.24	0.28	0.26	0.49	0.55	0.12	0.26	0.12	0.37	0.10
2009	0.08	0.10	0.08	0.12	0.25	0.35	0.05	0.11	0.04	0.21	0.09
2010	0.20	0.39	0.34	0.39	0.44	0.48	0.27		0.13	0.40	0.34
mean	0.08	0.13	0.14	0.21	0.24	0.34	0.09	0.09	0.05	0.23	0.10
min	0.00	0.00	0.01	0.08	0.08	0.16	0.02	0.00	0.00	0.04	0.00
max	0.20	0.39	0.34	0.42	0.49	0.55	0.27	0.26	0.13	0.40	0.34

*CSS does not count mini-jacks in the SARs. The 2012 CSS report (Chapter 6) examined challenges and potential methods to enumerate mini-jacks with PIT-tag data:*

*“The utilization of PIT-tags to monitor mini-jack returns introduces different challenges. During their outmigration, spring and summer Chinook juveniles sometimes enter adult ladders and are recorded as an adult observation in the PTAGIS database. The dataset housed at the Fish Passage Center currently includes only first and last detections within the adult ladder. Separating juvenile outmigrants in the ladder and true mini-jack returns requires all of the coil data from the PTAGIS database to establish a direction of movement within the adult ladder (i.e. upstream or downstream) in order to separate the juvenile outmigrants from true mini-jacks. Potential candidates for the mini-jack category can be chosen but using these candidates in any calculations would bias the mini-jack contribution higher than it actually was and confound any results. For these reasons, mini-jacks counts are not included in this update of the adult age data.”*

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

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

**Response:** *To our knowledge, this is the first report identifying that Clearwater wild spring/summer Chinook SARs were lower than SARs in other Snake River MPGs (smolt migration years 2006–2011); thus it’s unlikely that the question is being explored by other projects at this time.*

*There was no consistent correlation in survival from release to LGR (S1) and survival through the hydrosystem (S.r) or SARs for Snake River hatchery spring/summer Chinook. The table below shows the within-year correlations between S1 and the other metrics for Snake River hatchery stocks. In addition, the correlation between S1 and S.r across all 86 year/hatchery combinations was 0.04.*

Smolt migration year	count	S1 v S.r correlation	S1 v SAR(CO) correlation	S1 v SAR correlation
1997	4	0.08	-0.02	0.13
1998	4	-0.71	-0.04	-0.87
1999	4	0.36	-0.48	-0.94
2000	4	-0.93	-0.93	-0.95
2001	5	0.44	0.48	0.34
2002	5	0.04	-0.06	-0.32
2003	5	0.75	0.11	0.19
2004	5	-0.09	0.80	-0.50
2005	5	0.04	-0.60	-0.58
2006	6	0.25	-0.56	-0.13
2007	7	-0.35	-0.29	-0.45
2008	8	-0.24	-0.21	0.02
2009	8	-0.13	-0.09	0.04
2010	8	0.65	0.28	0.06
2011	8	-0.11	-0.24	-0.20
average		0.00	-0.12	-0.28

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

**Response:** *The SAR of the PIT-tag study group that year (2010) would not reflect the SAR of the run-at-large without re-weighting study group SARs. Most untagged “run-at-large” sockeye were transported from Snake River dams if collected, whereas PIT-tagged sockeye were*



bypassed that year, because sockeye PIT-tag releases were not placed in “monitor mode” as intended by investigators. Therefore, SARs for the different outmigration routes needed to be weighted to construct a “run-at-large” sockeye SAR in 2010. Few PIT-tagged sockeye were transported (Sawtooth  $n = 29$ ) and no adults returned to GRA from the transport group that year (see response to question on p. 71).

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

**Response:** *Standardizing the mid-Columbia Chinook SARs for juvenile survival through the FCRPS still results in much greater return rates than for Snake River Chinook. The geometric mean SARs for 2000–2010 were 4.27% and 3.11% for John Day and Yakima Chinook, respectively (Tables 4.34 and 4.35). Assuming a juvenile survival rate of 0.80 for the mid-Columbia populations, and 2% fishery impact rate of (wild) adults in Zones 1–5 below Bonneville Dam, the corresponding marine survival  $S_{oa(col)}$  would be 5.45% [ $0.0427/(0.80*0.98)$ ] and 3.97%, respectively. This compares to a geometric mean marine survival rate ( $S_{oa(col)}$ ) for Snake River wild Chinook of 1.75% during 2000–2010 (Table 4.52). In other words, the marine survival rate for John Day and Yakima Chinook would be 3.1 times and 2.3 times, respectively, that of Snake River Chinook. Assuming juvenile survival for mid-Columbia stocks was 0.70, the marine survival rate for John Day and Yakima Chinook would be 3.6 times and 2.6 times, respectively, that of Snake River Chinook.*

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

**Response:** *In 2010 Snake River hatchery sockeye were routed in-river with only a few incidentally transported PIT-tagged fish in the samples. Lacking this pre-assignment of PIT tags we had to use the “old methods” where each route of outmigration has an estimated SAR, which is summed using a weighted method to represent the run at large.*

*Because there was no pre-assignment, no PIT-tag groups exist to explicitly represent transported smolts. There were only 29 incidentally transported PIT tagged smolts for the Sawtooth group with one returning adult to BOA and zero returning adults to GRA. We did estimate an overall SAR from LGR to BOA with very high degree of uncertainty and a 90% CI ranging from 0.38 to 3.14. Because zero adults returned to GRA from the transport group, we were unable to estimate the uncertainty around the overall SAR using bootstrap method and therefore did not report it.*

P. 79, l. 14-17. Fig. 4.11 does not show in-river survival as described in the text – it is Fig. 4.13 that contains these data. But the authors make a good point about the high mortality in the RIS-

MCN reach. Survival in the upper Columbia needs to be monitored. The effect of this high mortality on Wenatchee steelhead is noted in the text, but the effect seems to be even greater for Leavenworth Chinook where MCN-BOA SAR is often 1% or less (Fig. 4.11).

**Response:** *Fixed the figure reference.*

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

**Response:** *Reworded according to suggestion.*

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

**Response:** *A high degree of correlation should be expected between  $S_{o1}$  and  $S_{oa}$  (within species) when a constant survival assumption is applied, if ocean exploitation is negligible (or at least constant) and age composition of recruits is relatively stable. As we responded above, for ESUs/DPSs that are exploited in ocean fisheries, some partitioning by age of fishing and natural mortality must be made; the  $S_{o1}$  metric arguably would be more reflective of marine survival rates for exploited stocks. We added a qualifier to the concluding sentence in the paragraph to indicate the possible advantage of  $S_{oa}$  applies to Snake River spring/summer Chinook and steelhead.*

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

**Response:** *Differences in freshwater life history characteristics between wild and hatchery steelhead (e.g., smolt age and size, hatchery vs. stream rearing conditions, migration timing and conditions through the FCRPS) may contribute to the observed differences in SARs, as well as the opportunistic manner in which the hatchery aggregate was constructed prior to 2008. However, we have only 3 years of SAR estimates since systematic tagging commenced for Snake River hatchery steelhead, so it is a bit early to hypothesize on causes for differences (if real).*

*Additional years at sea likely contribute some to SAR differences between A- and B-run steelhead, but annual marine mortality rates would have to be large for this to be a dominant factor.*

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

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

**Response:** *See above responses to this issue. We don't intend to imply the estimates of  $S_{o1}$  and  $S_{oa}$  are independent from each other. In fact, we explicitly stated in the methods: " $S_{oa}$  is calculated as the survival rate of in-river migrants below Bonneville Dam to adult return (including jacks) to both Lower Granite Dam and the Columbia River mouth.  $S_{o1}$  is back-calculated from the age-structured recruits to the Columbia River mouth, assuming 80% annual survival of sub-adults". To (hopefully) reduce confusion we added the following sentence: "A high degree of correlation between  $S_{oa}$  and  $S_{o1}$  metrics within species would be expected for Snake River spring/summer Chinook and steelhead, which are exploited very lightly in ocean fisheries".*

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

**Response:** *Yes. Installation of juvenile PIT-detection equipment in upper Columbia dams would address some critical data gaps regarding juvenile survival and SARs for upper Columbia populations.*

## **Chapter 5. Estimation of SARs, TIRs and D for Snake River Subyearling Fall Chinook**

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

Unless one is familiar with how the CSS estimates the SARs, it is not clear how holdovers cause problems. It would be helpful to give an explanation of how holdovers cause problems in the CSS methodology by looking at several scenarios. For example, suppose holdovers did not

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

**Response:** *We appreciate this comment. We do assume some level of familiarity. It's hard to determine if too much detail is overkill. The chapters in each annual report often build on work presented in previous reports and attempt to properly reference those previous works in order to clarify, support, or give context to current work. The 2011 CSS Annual Report (Tuomikoski et al. 2011) has a considerable discussion of the topic noted in the comment. That was the initial year of fall Chinook inclusion in the CSS. We thought it was appropriate then to provide such a level of detail especially as we developed the predictive model for trying to determine holdover detection probability. We added a reference to that document in order to provide those unfamiliar with the holdover issue a more detailed discussion.*

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

**Response:** *We are including a flow chart in the chapter methods section to describe the calculations involved in determining the potential bias from holdovers.*

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

**Response:** *We agree that adding a movement model would be helpful. And CSS attempted to incorporate movement into the initial modeling of holdover probability, using a multistate model, but it proved untenable for several reasons. Foremost was the lack of data on fish that had not migrated (i.e., no observations in the reservoir) made modeling movement difficult. Second, using a multistate model required the estimation of 30 or more parameters which was not desirable. Also, the period of shutdown of the PIT-tag detection system made estimation of some parameters impossible over the winter period. The low number of observations available for individual fish made the estimation of many parameters very imprecise and often the maximum likelihood estimates did not converge unless a number of parameters were fixed—essentially defeating the purpose of the model in the first place. As to estimating the bias, without movement, or a multistate model, we see no way to estimate bias with any rigor. Instead, we wish to answer the question: is there evidence of enough bias to cause concern about the accuracy of the CJS and subsequent SAR estimates? For some groups it is clear there is enough bias that estimation of juvenile survival, using CJS is not warranted and we have not estimated various parameters for those groups. However, for many groups the CSS methodology seems tenable because all of the PIT-tag evidence supports the notion that holdovers are near zero in those groups.*

### **Specific Comments and Questions**

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

**Response:** *We modified the sentence to read: Fish passing during the winter shutdown are not represented in estimates of survival and detection probability within the hydrosystem but may introduce bias into SARs, particularly for the C<sub>0</sub> group, which relies on estimated survival to Lower Granite Dam as well as detection probabilities at downstream dams.*

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

**Response:** *We inserted the following sentences to clarify: “Had we been able to remove individual fish with high holdover probability, it may have been possible to utilize groups such as Clearwater River surrogates that had relatively large numbers of holdovers. But individual fish within releases were relatively uniform in size and timing of release (variables used to predict holdover detection probability) and that similarity within release groups, made separating out individual fish from within the groups impossible.”*

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

**Response:** *We deleted an unnecessary phrase from the sentence that should clarify the remaining sentence. It now reads as follows: “The presence of winter migrants would cause an underestimate of C<sub>0</sub> juvenile population using CJS survival estimates inherent in the CSS method.”*

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

**Response:** *This comment appears to be related to the general comment we addressed above. We added a reference to the 2011 CSS Annual Report, in which we introduced the addition of fall Chinook in the CSS and addressed the issue of holdover fish in more detail.*

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

**Response:** *We changed the statement to read: “Simulations were developed to determine the potential amount of bias to juvenile population estimates that might be expected from including various numbers of holdover fish in the SAR estimation.”*

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

**Response:** *We changed the sentence to read, “...some fish could have passed LGR after LGR was shutdown December 5, 2009...”*

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

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

**Response:** *We added a flow chart as suggested in the general comments that we think will clarify these calculations.*

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

**Response:** *Because the % bias is a best approximation given very limited data of the maximum potential bias. Therefore it is not a real representation of actual bias but relies on a series of very conservative calculations and assumptions to arrive at this number. It is meant to answer the question: Is there evidence that there is a large amount of bias due to holdovers? If so we should not use the data. The % bias calculation is used to put the “maximum potential bias” calculation in perspective, to show that there is generally very little evidence of bias despite our very conservative approach to calculating it.*

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

**Response:** *That is correct. Those fish passing undetected during operation of detectors would be represented by detected fish and the detection probability (downstream ratio of previous detects to undetected). However, those passing after detector shutdown are not part of this ratio (or could bias the ratio). We added the word “estimated” to the sentence to clarify the meaning.*

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

**Response:** *A joint probability was modeled. As stated earlier, we were not successful in developing a multistate model that could differentiate survival, holdovers and detection. So we stuck with the joint probability of survival, holding-over and detection. Multiple assumptions were embedded in the detection probability, but it avoided the nearly impossible step of modeling separate components of survival, holding-over and detection, while still providing a useful indication of holdover rates. We made the assumption that higher holdover detection counts indicate higher holdover rates for a group. And in the logistic regression framework this was relatively easy to model. There is no indication that groups of fish with high holdover probability, pass entirely undetected, so we can use detections to get at the larger question of holdover probability assuming detected fish represent all holdovers, albeit without the ability to directly estimate holdover rates.*

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

**Response:** *We added the following sentence: “In addition low survival to LGR likely exacerbated the lower sample size issue, making LGR starting populations smaller and subsequent adult return numbers quite low.”*

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

**Response:** *We added a discussion section to the TIR portion of the results in order to seek a broader context to the patterns observed in TIRs than just reporting the patterns in the tables. We added juvenile survival data and figures depicting the relationship between in-river survival and TIR similar to what had been done for yearling Chinook and steelhead. We believe this will provide greater depth for understanding these patterns.*

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

**Response:** *We added the sentence: “As described above these detections were used to gauge the potential for undetected holdovers.”*

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

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

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

**Response:** *A figure was added to indicate where some primary statistics and parameters are located.*

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



**Response:** *The TIR parameter is a comparison of SAR for two treatment groups and is used by the region to evaluate the effectiveness of transportation. However, using TIR without regard to in-river survival and its effects on TIR may lead to a misleading conclusion. What is often overlooked is that the in-river treatment can be influenced by variables that are within the bounds of management control (see Chapter 3 of this report) and that these occur simultaneous with transportation. The reason for this section is to show that in-river survival is not constant and to show the point at which transportation is viable or not viable. Although a relationship between TIR and  $S_R$  is expected mathematically because TIR includes  $S_R$ , in practice this can be easily overlooked thus, the reason for the graph.*

*Much work has been done on the delayed effects of the hydrosystem and this subject was covered extensively at the CSS 2013 workshop, the report of which is Appendix B of the draft CSS 2013 report.*

*A parameter consisting of  $\log(TIR) - \log(S_R)$  vs. time is nearly the same, but perhaps harder to interpret, as the D parameter. D is presented in 15 tables and plotted in log scale vs. time in four figures within this appendix.*

### **Specific Comments and Questions**

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

**Response:** *The last sentence in the paragraph was confusing because survival estimates are not selectively omitted. Since that sentence does not add to the discussion, we’ve removed it.*

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

**Response:** *We’ve changed  $d_0$  to  $d_{C0}$  and  $d_1$  to  $d_{C1}$  for clarity.*

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

**Response:** *We’ve reworded this section to reflect the comment.*

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

**Response:** *The “noted bias” in this section refers to discussion in the previous paragraph where we explain the bias associated with increased mortality of detected fish at LMN. For clarification, we have edited this section to read “because of remarkably low detection probabilities at LMN that were probably a result of the above noted bias.”*

P. 11. In Equation A.10, SAR(Tx<sub>t</sub>) is shown to be calculated by summing adult counts at three dams (AT<sub>LGR<sub>t</sub></sub> + AT<sub>LGS<sub>t</sub></sub> + AT<sub>LMN<sub>t</sub></sub>). However the AT<sub>i</sub> are not defined anywhere; is there a procedure to obtain a cumulative count over the dams that prevents counting the same adult fish more than once?

**Response:** *All Adults counts in this appendix are from the adult PIT-tag detection system at LGR. An advantage of PIT tags over more traditional counting methods is that, since PIT tags are individual fish marks, no adult is counted more than once. The LGR, LGS, LMN subscripts refer to which dam the smolts were transported from 2–3 years prior to the adult return. We’ve added explicit definitions of the adult terms to the appendix (e.g., AT<sub>LGR<sub>t</sub></sub>, AT<sub>LGS<sub>t</sub></sub>, AT<sub>LMN<sub>t</sub></sub>, etc.) and reworded parts of this section for clarity.*

Is it statistically reasonable to conclude from S<sub>R</sub> values for Snake River Chinook in figures A.2 - A.3 (i.e., data in tables A.1 - A.3) versus corresponding SAR values for C<sub>0</sub> fish in figures A.7 and A.8 that SAR values are typically higher for wild than hatchery Chinook whereas S<sub>R</sub> values are typically the same for wild and hatchery Chinook (or perhaps slightly higher in hatchery Chinook)? This conclusion (if supported by the data) would indicate that wild Chinook typically survive better than hatchery fish below Bonneville and would be worth reporting here.

**Response:** *These survival estimates are provided to the region so that managers may apply these data to these prevailing management questions. Generally, the report has avoided “graphical” analyses and instead leaned on quantitative comparisons to reach conclusions. The hypothesis that wild Chinook typically survive better than hatchery fish, given similar outmigration conditions, once they pass Bonneville, is interesting but cannot be assessed well only using these estimates. This question would be best served by an analysis specifically directed at testing this hypothesis.*

P.44-45. Characterizing the relationship between log<sub>e</sub>(TIR) and in-river survival (S<sub>R</sub>): The sentence on lines 36-37 states that “the effectiveness of transportation as measured with TIR should be partly dependent on in-river survival.” More generally, TIR must depend on the trade-off between (i.e., the relative magnitudes of) S<sub>R</sub> and D. For Figure A.18, it might be more informative to include a reference line that corresponds to the expected relationship between log<sub>e</sub>(TIR) and log<sub>e</sub>S<sub>R</sub> when D=0. The plot would then show the amount of variation in TIR that could be attributed to the null hypothesis (S<sub>R</sub>) alone and how much remained to be attributed to D (that statistic could also be calculated). A key point, suggested in the text but not the

existing figure, is that a linear relationship between  $\log_e(\text{TIR})$  and  $\log_e S_R$  (not  $S_R$ ) is expected by definition, and need not be estimated empirically by regression. From equation A.15 (i.e., by definition),  $\text{TIR} = D S_T/S_R$ , so  $\log(\text{TIR}) = \log D + \log S_T - \log S_R$  where the  $\log S_T$  might reasonably be assumed to be constant at  $\log(0.98)$ . Note that the expected linear relationship requires taking the log of  $S_R$  (which is not done in Figure A.18).

**Response:** *Please refer to the response to the previous question on the model.*

*We have added a version of the model that includes  $\log_e(S_R)$ . This appears to give a better fit to the data than the version with the untransformed  $S_R$ .*

*Much work has been done on the delayed effects of the hydrosystem and this subject was covered extensively at the CSS 2013 workshop, the report of which is in Appendix B of the draft CSS 2013 report.*

*The part of the comment above that notes “. . . how much remained to be attributed to D (that statistic could also be calculated)” implies that D is not calculated. Because the Appendix presented D in Tables 23–37 and figures 14–17 this is confusing.*

## V. Editorial Suggestions

### Glossary

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

**Response:** *We've updated the glossary.*

### Missing Section

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

## Chapter 1

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

**Response:** *Last year's report had a typographical error. This sentence refers to the number of migration years where the CSS has had a PIT-tagging program which is 1997 through 2013; stating the 17<sup>th</sup> year is correct.*

## Chapter 2

**Response:** *Where not specifically responded below, revised Chapter 2 addresses all minor editorial comments.*

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

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

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

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

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

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

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

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

p.26, l. 22. Why is there is an additional  $a_1$  parameter when  $ap$  parameters have already been defined?

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

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

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

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

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

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

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

p.39, l. 41. The authors claim that there are only 13 parameters for the LC model. But there are  $6 \times 2 = 12$  parameters alone for the recruit/spawner relationship, plus the ocean survival, maturation, and variance parameters for more than 13 parameters. Please explain how this count was obtained.

**Response:** *Typo corrected. Table 2 shows that there are 15 parameters in the LC model, 12 in freshwater, 1 common  $s_{01}$  productivity, and two maturation rates. The variance parameters for the process errors (one for each age of return and one for smolts) were treated as a nuisance parameters, and analytically integrated into the likelihood, so they not estimated and do not add to the count of parameters. There are no estimated “variance parameters” for each estimated parameter. The variances of the estimates of 15 parameters (now reported in revised Chapter 2) are derived from the hessian matrix, which is an integral part of getting the maximum likelihood estimates. They are byproducts of getting estimates of parameters, not inherently estimates in themselves.*

### Chapter 3

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

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

**Response:** *We have replaced “survival rate” with “survival probability.”*

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

**Response:** *The text has been modified to reflect juvenile migration years 1998–2012. The description of Table 3.1 has also been modified to reflect years 1998–2012.*

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

**Response:** *We use program RELEASE which is embedded in program MARK to perform goodness of fit (GOF) testing and to generate an estimate of  $C_{\hat{}}$ . This method utilizes a chi-squared approach and is one of the recommendations in the MARK software manual (“Program MARK – a ‘gentle introduction’”).*

*We consider this a reasonable and valid approach to estimate the  $C_{\hat{}}$  parameter. There is precedence for using the chi-squared method in the scientifically reviewed literature (Anthony et al. 2006, Glen et al. 2010, Mulders et al. 2007, Franklin et al. 2002). Also, the MARK help file page referenced by the ISAB explains that the median  $C_{\hat{}}$  procedure was developed to ameliorate the problem where many of the model types available in MARK do not have a useful GOF procedure. However, for CJS models the help file says, “Program RELEASE provides useful goodness-of-fit (GOF) tests and estimates of  $c$  [hat] for the CJS data type.”*

*The primary criticism of using this technique instead of various other methods is that when few of all the potential encounter histories are present, the deviance is not well approximated by the asymptotic distribution (White et al. 2001). However due to large sample sizes in the FPC data sets, nearly all encounter histories are typically present and there are multiple observations per encounter history. Therefore we consider this a valid technique for estimating goodness-of-fit and  $C_{\hat{}}$ .*

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

**Response:** *Text changed to 41%.*

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

**Response:** *The caption now includes the years over which the survival estimates and their CV was calculated.*

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

**Response:** *Equation 3.3 (now equation 3.2) correctly has a minus sign, as can be seen by rearranging equation 3.1 to express Z in terms of S and t.*

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

**Response:** *Text revised to reflect 64 possible model combinations using six environmental factors.*

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

**Response:** *Text and equations now clarify that  $\log_e$  transformations were used.*

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

**Response:** *Text changed to “response variable”.*

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

**Response:** *The equation now shows that a square-root transformation was used.*

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

**Response:** *Table now numbered correctly. A section has been added to describe how the  $R^2$  values were calculated for fish travel time, instantaneous mortality, and survival.*

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

**Response:** *Text has been modified to state that improvements in precision could help separate process variability from measurement error. We have also qualified that improved precision “could be useful” to improve understanding of the effects of environmental factors rather than the previous “would be useful.”*

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

**Response:** *In this context, our use of the word precision was in reference to the level of measurement error associated with the survival estimates. The relatively high levels of measurement error in the RIS-MCN and MCN-BON reaches make it difficult to separate and identify the factors contributing to process variation from measurement error. We have added language to better describe these points.*

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

**Response:** *Although we did not specify that the “all subsets” model selection procedure avoid models with only Day<sup>2</sup>, inspection of the model selection output revealed that models with only Day<sup>2</sup> fit poorly (i.e., high AICc) with nearly zero AICc weights, and therefore had little impact on the model-averaging results. Models that had high relative variable importance for Day<sup>2</sup> also included Day in all of the top fitting models. The original analyses that examined the Day\*WTT interaction did avoid fitting models without the component main effects. However, the revised analyses have dropped the Day\*WTT interaction. The processes that we are attempting to characterize appear to be captured by using either a Day<sup>2</sup> or a Day\*WTT interaction. Given that the model selection procedure is able to properly handle the Day\*WTT interaction by only fitting models with the component main effects, we will likely use that approach in the future.*

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

**Response:** *Tables 3.4–3.7 showed the ranges for WTT and spill levels that had been observed historically in the LGR-MCN and MCN-BON reaches and the ranges that could be tested using adaptive management experiments, along with the predicted survival rates for each*



*combination of WTT and spill level. Adding the 2012 data did not change the range of observed conditions. The point of the tables was to show the value of deliberate, adaptive management experiments where WTT and spill levels are extended beyond the historical range. This research has shifted towards the development of models that are examining the effects of these changes in terms of full reach survival (i.e., LGR-BON), ocean survival, and SARs, which was the subject of the 2013 CSS Workshop. The summary report from the Workshop was included as Appendix B.*

## **Chapter 4**

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

**Response:** *We have added an explanation for 'salt' in the glossary.*

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

**Response:** *We changed the caption to indicate the RR data series is 1964-1984 and 1993 and extended the data series to 2011.*

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

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

**Response:** *We extended the data series to 2010.*

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

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

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

**Response:** *We have fixed the three preceding editorial comments.*

## Chapter 5

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

**Response:** *We changed the sentence to read "for three of four years"*

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

**Response:** *We changed this as suggested.*

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

**Response:** *We changed this as suggested.*

## Appendix A

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

**Response:** *We have added a reference in Chapter 4 to this figure.*

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

**Response:** *Word "parameters" was changed to "statistics."*

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

**Response:** *We changed this as suggested.*

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

**Response:** *Figure caption has been updated.*

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

**Response:** *90% confidence interval for 2011 TIR is now bold-italics (upper limit values <1.0).*

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