Independent Scientific Advisory Board
for the Northwest Power and Conservation Council,
Columbia River Basin Indian Tribes,
and National Marine Fisheries Service
851 SW 6th Avenue, Suite 1100
Portland, Oregon 97204

Review of the Comparative Survival Study's Draft 2012 Annual Report

Richard Alldredge
James Congleton
Kurt Fausch
Colin Levings
Katherine Myers
Robert Naiman
Bruce Rieman
Greg Ruggerone
Laurel Saito
Dennis Scarnecchia
Chris Wood
Carl Schwarz, Ad Hoc

ISAB 2012-7
October 15, 2012

ISAB Review of the Draft 2012 CSS Annual Report

Contents

ISAB Review of the Draft 2012 CSS Annual Report

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. This regular system of reviews includes evaluations of the Comparative Survival Study's draft annual reports. These ISAB reviews began two years ago with the evaluation of the CSS's draft 2010 Annual Report (ISAB 2010-5), followed by a review of the draft 2011 Annual Report (ISAB 2011-5). This ISAB review of the draft 2012 CSS Annual Report is the ISAB's third review of CSS annual reports in response to the Council's 2009 Program.

Overview

The draft 2012 CSS Annual Report is well organized and well written. The ISAB acknowledges the continued progress by the CSS in addressing key questions and reporting results. As the dataset includes more years and a wider range of environmental conditions, the ability to address how the river environment affects juvenile salmon migration rates and survival continues to improve. The long time series in survival rates by species, hatchery and wild stocks, and watersheds are valuable in this regard. The CSS authors should continue to produce yearly updates.

The ISAB members who attended the CSS Annual Meeting April 12, 2012 would like to acknowledge the very useful exchange of information that took place.

This ISAB review begins by suggesting topics for consideration by the region as a whole. These overarching issues, presented in bulleted format, are related to material presented in the draft 2012 CSS Annual Report, but the topics are not specifically limited to consideration by the CSS team. Next the ISAB provides general comments to be considered in revising the draft 2012 CSS report or, if time does not allow, for possible inclusion in subsequent CSS Annual Reports. These comments for the CSS team are presented in a numbered list for ease of discussion, but the list is not in any priority order. Finally, the ISAB review provides specific editorial comments and suggestions to aid in preparation of the final 2012 report.

Topics for Consideration by the Region

- An evaluation is needed for whether the NPCC's 2-6% SAR goals and objectives are sufficient to meet salmonid species conservation, restoration, and harvest goals. Chapter 4 describes SAR Program goals as being for spring/summer Chinook and thus not tailored for other species, races, and age of smolts. These SAR goals should be broken out by species, race, and age at smoltification rather than one goal across all species. Coho, fall Chinook, and steelhead have different juvenile life histories, and it is likely inappropriate to generalize SAR objectives for viability across these species. The analyses in Chapter 5 lead to the important conclusions that overall SARs for Snake River subyearling fall Chinook are "well short of the NPCC goal of 4% SAR needed for recovery" and that there is little or no benefit to transport. But given that fall Chinook migrate as subyearling smolts (whereas spring/summer Chinook migrate as larger, yearling smolts), the NPCC's 2-6% SAR objectives may be higher than needed to meet conservation, restoration, and harvest goals for fall Chinook. As with other species, the NPCC (2009) SAR objectives should be updated to specify the critical points in the life cycle where smolt and adult numbers should be estimated and to identify ESU-specific SARs necessary for survival and recovery.

- Development of technology to improve PIT-tag recovery in the estuary is needed. PIT-tags detected on bird colonies in the estuary are used to augment NOAA Trawl detections below Bonneville. The problems with trawl detections indicate this is a difficult area for PIT-tag recovery contributing to uncertainties concerning smolt migration and survival.

- In response to last year's ISAB advice to discuss and compare CSS results with other studies using different methods (e.g., McComas et al. 2008, also more recently Harnish et al. 2012) the CSS stated (CSS 2011, Appendix F): "*Response: The CSS-OC concludes that it is not currently possible to estimate smolt survival for PIT-tagged fish below BON through the Columbia River estuary. The CSS-OC is aware of the McComas et al. (2008) study; however, the results are not robust enough for application of acoustic tag survival estimates through the estuary to CSS PIT-tag groups, or to the retrospective estimates of S.oa and S.o1.*" The CSS-OC has made an important conclusion. If PIT-tags cannot be used and acoustic tag results are not "robust enough" to estimate estuarine survival, a thorough review of this issue is needed, especially given the increasing scientific evidence of survival bottlenecks in the Columbia River estuary and extensive efforts to restore estuarine habitats to improve salmonid survival. A review is needed of estimation methods for smolt survival below Bonneville Dam through the Columbia River estuary using PIT-tags, acoustic tags, and other methods. If necessary, existing methods should be improved or new methods developed to estimate estuarine survival of salmonid smolts (see ISAB 2012-6; Levings et al. 1989;

Macdonald et al. 1988).

- Measurement error in SAR estimates associated with PIT-tags needs comprehensive examination and description in a report dedicated to this issue.

General Comments for CSS Consideration

1. A topic of much interest that is related to the 2012 CSS Annual Report is the relationship between proportion of spill and juvenile survival. The topic may be framed by considering a conclusion from the FPC's History of Spill Report,[1] *"Increasing proportion of spill provided for fish passage at hydroelectric projects has resulted in higher juvenile spring/summer Chinook, fall Chinook, sockeye and steelhead survival and faster juvenile fish travel time through the FCRPS."* The ISAB considers this conclusion to be a strong hypothesis worthy of further investigation in the context of reviewing the draft 2012 CSS Annual Report. In Chapter 3 the authors provide some evidence that spill has an impact on fish travel time and instantaneous mortality while Figure 3.5 shows that spill had relatively little effect on instantaneous survival (Z) of sockeye and steelhead below McNary Dam. Furthermore, a variety of other factors also had similar importance as spill for species where spill was found to be a key variable. A correlation matrix and scatter plots showing relationships among independent variables would be informative. Additional effort is needed in using models to quantify the effect of spill and water travel time (WTT) on each species, while holding other variables in the model constant. An analysis of competing hypotheses is needed with particular attention to the possibility of model selection bias. Clarification is needed concerning how the results translate to management guidelines. This type of analysis would inform managers as to how much benefit can be expected by altering spill levels and reservoir elevations.

2. Attention needs to be given concerning the interplay and effects of spill and surface bypass structures. The report should more completely describe surface bypass structures and consider their influence on smolt survival.

3. A tremendous amount of SAR data have been collected including survival estimates after smolts leave Bonneville Dam. It would be useful to associate these data with estuarine and oceanographic conditions to further evaluate factors affecting salmon survival and

[1] Historical Spill Summary 1981 to 2011. FPC 46-12; April 18, 2012. http://www.fpc.org/documents/memos/46-12.pdf

abundance. The ISAB recommends additional collaboration between CSS researchers and ocean researchers on this issue. (See Pyper et al. 2005; Mueter et al. 2005.)

4. The relationship between annual variation in stock composition and survival is difficult to see from the material presented in the report. Explicit evaluation of the effect of annual variation in stock composition of PIT-tagged aggregate samples of wild and hatchery fish on annual variation in survival estimates could provide useful insights.

5. As noted in the ISAB review last year (ISAB 2011-5), there is a need to investigate PIT-tag related mortality and shedding of PIT-tags. The CSS agrees this is an issue that needs to be examined.

6. An overarching comment is that connections of the migration and survival with larger ecological concerns should be emphasized more. It would be beneficial to increase collaboration with researchers working on other species, food webs, habitat, physiology, contaminants, and disease. Such combined studies might give added insights into mechanisms causing the observed temporal patterns in migration and survival. With respect to ocean ecosystem issues, the CSS staff are commended for publication of two recent peer-review journal papers (Petrosky and Schaller 2010; Haeseker et al. 2012) that relate climate, river, and ocean conditions to annual trends in smolt-to-adult survival estimates. Nevertheless, the ISAB continues to emphasize the need to improve scientific collaboration between CSS staff and estuary-ocean experts working on BPA-funded programs to address migration and survival of Columbia River salmon and steelhead in the estuary and ocean.

7. Age terminology should be clearly defined and the convention used should be consistent across species. The age convention used for fall Chinook (in Chapter 5) appears to differ from that typically used for other Pacific salmon. For example, sub-yearling (age 0+) smolts migrating downstream in 2006 are said (page 106) to have returned as 5-year olds in 2011, but under the usual conventions for determining age in salmon, if they hatched from eggs deposited late in 2005, then would reach age 5 at spawning time late in 2010.

8. The limitations of the dataset used to estimate TIRs and the preponderance of higher point estimates for survival of transported fish suggest that conclusions concerning benefits of transportation are premature. Caution in interpreting such results is encouraged.

9. The potential for ocean harvest to be a source of bias in SAR estimates should be quantified.

Chapter Specific Comments

Throughout the document, many useful tables and figures are provided that enhance understanding of complex results. The detailed Table of Contents is useful for guiding the reviewers and readers through the manuscript. Inclusion of a Glossary of Terms, which includes acronyms, is appreciated.

Chapter 1. Introduction

As in 2011, the Introduction provides an excellent description of the history and objectives of the CSS program and of the methodologies used. This section provides a useful orientation to coordination and collaboration with other PIT-tagging projects, a summary of historical in-river conditions and transportation, and the organization and content of the report.

The Introduction is informative, but some additional information is needed.

- The addition of a table with an historical timeline of key objectives and results from past years of CSS work would be useful.
- The inclusion of detailed description of some methods but not others is somewhat confusing. In the introduction, a section containing a brief review of primary methods would be sufficient. To avoid redundancy, detailed methods would be better placed in the methods sections of the appropriate chapters that use these methods.
- When describing the juvenile and adult PIT detector systems, it would be informative to indicate what proportion of total juveniles and adults are detected by the detectors at each dam.
- A brief (1-page) abstract at the beginning of the report that summarizes major conclusions in the 2012 report would be useful.

The CSS examined the holdover issue involving fall Chinook in Chapter 5, which makes sense because fall Chinook migrate as subyearlings and yearlings. However, there was no discussion of steelhead holdovers. After release most hatchery steelhead will migrate to the ocean as yearlings, but some might delay emigration to age 2 or residualize in the watershed. In contrast, it appears that most Chinook mini-jacks are excluded from PIT-tag analyses by only using juveniles that are detected at the first detection site.

Fallback and straying of salmon and steelhead is an important issue that can confound SAR estimates, and it would be worthwhile for the CSS report to address the issue so the reader knows the issue has been properly considered. PIT-tagged fallback salmon may be detected again if they re-ascend the dam; are these fish correctly accounted for in the estimates? Some fallback salmon may not re-ascend the dam; how are these fish treated in the SAR calculations, especially given that transported fish reportedly stray more than in-river fish?

Chapter 2. Using PIT-tag detections probabilities to estimate route-of-passage proportions at hydropower dams

The authors propose a method to estimate route selection from PIT tags without having to monitor each of the routes past the dam. The successful use of the method depends on monitoring a group of fish that passes a dam when no water is being spilled, as was the case in 2001. If there is no such data, then this method will not work. It is also making the strong assumption that the rate in 2001 is applicable to all years, but no assessment of the implication of this assumption was made. For example, no water was spilled because it was a low water year which in turn may have affected the choice of passage route. It also may be optimistic to think that that data from 2001 may still be applicable in higher water years.

The precision estimates are likely too optimistic because of the assumption that the different groups of fish all had the same fish guidance efficiency (FGE). The authors used the "average" rate over the different cohort. However, Figure 2.1 indicating the range of passage probabilities for hatchery and wild steelhead in 2001 appears too wide to be due to sampling variation. This excess variation should be accounted for in the estimation procedure. It would be useful to present a table specifying which species, year, group, and individual estimates were used to estimate the average overall FGE.

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

This is a well written and informative chapter. The synthesis at the end of the chapter in which some management actions are recommended based on the findings is particularly appreciated.

The effects of environmental variables on fish travel times (FTT), instantaneous mortality rates (Z) and survival rates (S) were modeled by using multi-model inference techniques to reduce model selection uncertainty. Models were ranked according to calculated Akaike information criterion (AIC) values, weights were calculated for each model, and model-averaged predictions were made. This methodology should minimize model-selection error, although it would be desirable to make more detailed information on the individual models accessible in an appendix or on the web.

The charts show that instantaneous mortality of most species increased rapidly with “day” of migration each year. A discussion about factors contributing to the increasing instantaneous mortality rate of each species as time progresses each season would be worthwhile. What is causing this increase in mortality? Is it related to physiology of the fish, increased predation, disease, or reduced feeding and growth? Do earlier releases of hatchery fish lead to greater in-river survival? To what extent is increased FTT over time within a given year related to water travel times (WTT), fish behavior and size? Are salmon smoltification indices consistent with expected levels at upper, mid, and lower river locations? The CSS analysis provides a good time series of key metrics, but it would be worthwhile to further explore key questions about factors affecting salmon migration and survival.

The authors present relative variable importance histograms for FTT and Z, but not survival (S). This would be worthwhile even if S is related to FTT and Z. These charts indicate the overall weighted contribution of the variable to the suite of key models based on AIC.

The relative variable importance charts are a good approach for summarizing the results and for highlighting the key variables in the models. Still, it would be worthwhile to show the top models in a table, either in Chapter 3 or an appendix. For example, how many models were within two or three AIC points, and therefore indicating little difference in the suitability of the top models? A potential drawback to the relative variable importance charts is that they did not appear to show whether the coefficient sign for a variable switched among the models; in complex multivariate models the coefficient can change, which is an indicator of collinearity. However, change in the sign of the variable could be incorporated into the metric, leading to a lower score when the sign is changed.

The report states that reduced FTT of subyearling Chinook was related to the court ordered spill beginning in 2005. A discussion concerning why the court ordered spill seemed to have a greater effect on subyearling Chinook (e.g., Fig. 3.1, Table 3.2) compared with other species would be useful. Is this effect related to migration timing, or fish size, or something else?

The discussion addresses management actions that might lead to greater survival or faster travel time, for example, increase spill or reduce WTT. The discussion could have benefitted from using the quantitative models to predict the benefits of changing spill percentage and/or WTT while holding other variables constant. In that way managers could more fully appreciate the impact of altering these variables.

The discussion and some analyses in the results indicated that spill had a relatively large impact on subyearling Chinook salmon. However, the modeling effort (Fig. 3.5) revealed that the effect of spill on subyearling instantaneous mortality was similar to that of day, WTT, temperature, and surface spillways, indicating that a variety of factors affected subyearling Chinook salmon. See General Comments above for suggestions.

The 2012 report states that improving detection probabilities at each dam would also be a good way to reduce uncertainty in the RIS-MCN and MCN-BON models. The ISAB concurs. It would be interesting to see how detection probability for both juveniles and adults varies by species.

The discussion concludes that FTT and Z are reduced when WTT is lower and spill levels are higher and therefore that "*improvements to...survival are possible through management actions that reduce WTT and increase spill...*." These are strong hypotheses on the basis of data collected to date and modeling results summarized in Chapter 3, particularly for juvenile fish passage through the Snake River reaches of the hydropower system. See General Comments above for suggestions.

The evidence is less supportive of the specific suggestion that "*there is opportunity to reduce fish travel time and increase survival through [the McNary pool to Bonneville reach] if these four projects were to operate at their minimum operating pools...adaptive management experiments, such as reducing WTT in the MCN-BON reach...could reveal...dramatic improvements for yearling and subyearling Chinook, steelhead, and sockeye*." Data sets are smaller and models incorporate more uncertainty for juvenile passage through the MCN-BON reach than for passage through the Snake River. Moreover, the models presented in Chapter 3 do not assign high relative importance values to the effects of WTT on Z of either steelhead or Chinook in the MCN-BON reach; temperature is the only highly ranked variable in the model for instantaneous mortality of steelhead in this reach. Water travel time is, however, a highly ranked variable (along with 4 to 5 other important variables) in FTT models for Columbia River passage. Currently available data are therefore at best weakly supportive of the notion that survival would be increased by drawdown of lower Columbia reservoirs. Adaptive management experiments are most easily justified for testing of strong hypotheses. Further consideration and justification is needed for the suggestion concerning adaptive management experiments to reduce WTT in the MCN-BON reach.

Modeling results indicate that the number of surface passage structures is correlated with decreased FTT and decreased Z for several of the study groups. Little is said about this observation in the Chapter 3 Discussion, but further elaboration would be welcome. A table summarizing the location and date of installation of surface passage structures would be useful in this chapter. Is the benefit derived from these structures likely to be fully realized from the existing installations and their operation, or is there scope for further improvement?

Highly ranked variables in the FTT and Z models could have widely differing effects quantitatively. If and when does the CSS expect that it will be possible, assuming that the data set continues to grow for additional years, to quantitatively model the effects of alternative modes of hydropower system operation and of surface passage structures on FTT, Z, and survival? This could be a powerful tool to guide cost-effective expenditure of funds for passage improvements and adaptive management.

How comparable are the arithmetic mean mortality rate, estimated by Equation 3.3, and the instantaneous mortality rate predicted by Equation 3.7? Are there any issues with comparing these two variables against each other as shown in Figure 3.2, 3.5, and Table 3.3?

It is customary to show the number of observations that are used in calculations. If not too confusing, showing the number of observations might make it easier to see why there is more variability in some reaches as compared to others.

Some explanation is needed for the upper left figure in Figure 3.4 in which it appears that all of the variables have strong importance for characterizing FTT. The validity and reason for this result should be discussed.

The development of the estimates uses a “statistics on statistics” approach whereby estimates from the mark-recapture model are further analyzed outside of the model. Incorporating individual travel times into the Cormack-Jolly-Seber (CJS) model is quite difficult because of the need to model the travel time between dams for fish not detected. For example, suppose that a fish is released at day 0 at dam 1, not detected at dam 2, and then detected at dam 3 on day 10. All that is known is that the fish must have passed dam 2 somewhere between day 0 and day 10. Presumably the travel time of other fish released at dam 1 and detected at dam 2 tells something about the travel time between dams 1 and 2, but this turns into a formidable estimation problem because of the need to integrate over the possible travel times when a fish is not detected at a dam. This approach would then allow direct estimation of the effect of travel time and other covariates on the survival probabilities. Muthukumarana et al. (2008) started work on this, but much more work is needed.

One of the dangers of the “statistics-on-statistics” approach is the ecological fallacy where relationships between averages (the average survival rate for a cohort of fish versus the average travel time for a cohort of fish) may not hold for individual fish. There could be confounding variables with the cohort, for example flow in the system, which spreads the averages out and gives rise to an apparent relationship, but this does not hold for individual fish within each cohort. A discussion of this potential problem should be presented in the chapter.

Chapter 4. Patterns in Annual Overall SARs

Chapter 4 explains that the SAR objectives of 2-6% are based on the original PATH analyses for Snake River spring/summer Chinook (Marmorek et al. 1998). These SAR objectives appear to have been applied to steelhead and fall Chinook without further modeling despite obvious differences in juvenile life history, especially the age and size at smolting. To achieve equivalent population viability, other factors being equal, SAR targets would need to be relatively higher for older, larger smolts (steelhead) and relatively lower for younger, smaller smolts (fall Chinook). A caveat to this effect is needed so that readers will not be misled about the relative status of fall Chinook and steelhead populations based on the magnitude of deviation from the SAR objectives.

The analyses in Chapter 4 lead to the important conclusion that overall SARs for Snake River wild spring/summer Chinook and wild steelhead fell well short of the NPCC SAR objectives of a 4% average and 2% minimum for recovery. On page 44 the authors state, "*The NPCC (2009) SAR objectives did not specify the points in the life cycle where Chinook smolt and adult numbers should be estimated … PATH analyses also did not identify specific SARs necessary for steelhead survival and recovery.*" This information should be specified and agreed upon in the region, perhaps in future Fish and Wildlife Program amendments. On page 45, lines 23-25 the authors also state, "*We have made preliminary comparisons of the overall SAR estimates to the NPCC 2-6% SAR objectives, recognizing additional accounting for harvest, straying and other upstream passage losses may be needed in the future as NPCC and other SAR objectives are clarified*." This is an important programmatic issue, as above, NPCC and other SAR objectives for the CSS are apparently not clear. See Topics for Consideration by the Region above.

The discussion concerning environmental correlates of annual patterns in survival in the 2012 CSS report relies heavily on methods and results reported in Petrosky and Shaller (2010) and Haeseker et al. (2011). The CSS report would be improved by including more detailed information on the methods and results of these two studies, as well as any updated analyses using new data.

Recent studies (e.g., Knudsen et al. 2009) and older studies indicate PIT-tagged salmon may shed tags or experience higher mortality. The run reconstruction (RR) analysis presented by CSS supports the concern that SARs based on PIT-tags may be biased low as evidenced by survival based on RR being consistently higher than that based on PIT-tags. The CSS also notes that RR may also have its own bias, for example RR estimates are not corrected for fallbacks. The issue of fallbacks is well-known, so it is not clear why RR estimates did not correct for fallback salmon. In the discussion, as recommended by the ISAB, the 2012 CSS report describes ongoing efforts to further evaluate potential tag effects of PIT-tags on salmon SAR estimates. CSS considers these studies to be a "high priority." The ISAB agrees. The CSS report states that SARs based on PIT-tags and RR are highly correlated, perhaps providing some level of comfort in the results. However, closer examination of the data would reveal that percent differences in the annual SAR estimates based on the two approaches and variation in this difference from year to year can be quite high. Measurement error in SAR estimates associated with PIT-tags needs comprehensive examination and description in a report dedicated to this issue. PIT-tags are an extremely valuable tool, but investigators need to know the magnitude of introduced measurement error and what factors influence the error.

The SAR estimates appear to adequately exclude potential effects of mini-jack production in hatcheries. The text on page 49 states that the initiation point for counting PIT-tagged smolts is the first detection system (dam) below the release location (apparently the number of tagged fish is not used). Most mini-jacks probably do not emigrate downstream to the dam where they could be counted, and those mini-jacks that migrated through a dam might be detected going back upstream, and excluded from the analysis. Please clarify the approach to reduce potential bias caused by mini-jacks on SARs. Also identify the survival fraction of the PIT-tag release population from release to the first detection site (dam) and how it compares with other periods. This estimate would reflect mini-jacks and in-river mortality. The blood bioassay developed by NMFS to identify mini-jacks prior to release should be used to identify the proportion of mini-jacks each year and compared with mini-jack levels calculated from PIT-tag data.

The correlation between SARs for wild steelhead and wild spring/summer Chinook may be influenced by a few data points. It would be helpful to see scatterplots of the estimates with the brood year attached to each point to see if this is occurring.

For example, the correlation of 0.71 reported on page 54 appears to be based on the similar pattern of Figures 4.1 and 4.4. The correlation may be due to the high estimates of SAR in the early 1960s, but there may be less of a relationship in later years. Some thought should be given to how to present correlations in SARs among several runs other than pairwise scatterplots.

The discussion of potential factors causing biases in estimates of SAR from PIT-tags (page 68) could be improved by presenting some rough estimates of effects. A goal would be to identify the most sensitive factors influencing bias. For example, assessing the impact of various levels of PIT-tag loss on the estimates and bias could be useful. If this has been done in the past please identify where.

Chapter 5. Estimation of SARS, TIRS, and D for Snake River Subyearling Fall Chinook

The mini-jack issue is an important topic, and the information provided in Chapter 5 is appreciated. Clarification is needed concerning what is meant by targeting mini-jacks and excluding Snake River stocks. It is not clear why detection of juveniles in the system and their direction of movement would lead to a high bias of mini-jacks. Regardless, the discussion and explanation about why mini-jacks were excluded from age at maturation estimates is appreciated. Another possible confounding factor would be that most mini-jacks probably do not migrate down to a dam where they might be detected. Another method for estimating mini-jacks would be to use the bioassay developed by NMFS and applied to smolts prior to release.

The CSS puts considerable effort into investigating and estimating holdover probabilities for age-0 fall Chinook. Much of this discussion is obscure for readers not already familiar with the topic. It might help to (1) begin by explaining that the major concern is possible bias of estimates for the number of undetected juvenile migrants passing Lower Granite Dam (C_0) when calculating SARs. This explanation is presented, but it is not identified as the primary concern, (2) explain how C_0 estimates are made in this section of the report, and (3) explain how C_0 estimates may be biased, or not, by different types of holdover behavior, that is whether detected as subyearling migrant, as a subyearling migrant and again the following spring as a yearling migrant, only as a yearling migrant, or never detected. This information could be summarized in a simple table.

The CSS report concludes (p. 115, lines 15-17) that "*based on TIRs (transport to in-river survival ratios) there appears to be no benefit to transport evident in the 2006 returns. Returns of more recent years are not complete but the pattern of little or no transport benefit appears to be holding.*" The limitations of the dataset and the preponderance of higher point estimates for survival of transported fish suggest that this conclusion is premature. Using data for the six study groups of age-0 Chinook released in 2006, in-river fish returned at a significantly higher rate for one group and transported fish at a higher rate for another group. For the seven groups released in 2008, transported fish returned at a higher rate for two groups. Although wide confidence limits preclude significant differences in most comparisons in these years, point estimates of survival are higher for transported than for in-river fish in 11 of the 13 comparisons. In addition, transportation has been thought of as a strategy for off-setting disastrously low in-river survival in extreme low-flow years, so particular attention should be given to the performance of transported fish in those years.

A discussion of how TIRs would be changed if holdover SARs were also considered could be informative. Some studies have reported high return rates for holdover fish.

Chapter 6. Patterns in age at maturity for PIT-tagged spring/summer Chinook salmon and sockeye

Chapter 6 analyzes age-at-maturity data for 16 stocks of spring/summer Chinook salmon for juvenile migration years 2000-2009. Consistent differences are reported between stocks, accounting of 50% of overall variation. The conclusion that forecasting of adult returns from prior-year jack returns should be done on a stock-specific basis follows logically from this result. The examination of age at maturation is very useful and informative.

In addition to inter-stock differences in age-at-maturity, synchronized year-to-year changes over multiple-year periods are reported for some stocks, accounting for 32% of overall variation. This is a biologically interesting correlation, suggesting a strong influence of cyclic environmental factors most likely during the marine phase of the life cycle. Two papers are cited that hypothesize an effect of ocean conditions on age-at-maturity. If so, increased understanding of the ocean ecology of salmon could help to improve run-forecasting methodology.

It is unclear how missing data are dealt with in the statistical analyses and visual presentation: ten years of data are available for some hatcheries, but five or fewer years of data were available for five of the hatcheries (Figure 6.2, p. 120). Because age-at-maturity changed non-randomly over time, missing data for some years could bias estimates of mean age-at-maturity (Figure 6.3) and jack proportions (Figure 6.4). Although it is stated that "*the data and analyses ...in this chapter follow last year's report...We updated the analysis of variance...the logistic model was updated as well,*" more description of the statistical methods used in this chapter is needed.

The large proportions of jacks returning for some stocks raise many interesting questions that may be outside the scope of the CSS study. It would appear that the great majority of males return as either mini-jacks or jacks in some populations. What percentages of adult fish are males? Does a high percentage of prematurely maturing males (mini-jacks) result in a decreased percentage of jack returns?

In Table 6.1, please identify the race of Chinook salmon (e.g., spring, summer, fall). Are all PIT-tagged Chinook and sockeye emigrating as yearlings?

Typically, age at maturation of Chinook varies with gender—females are older on average. There is some evidence that females have a lower survival rate, at least in Alaska, possibly due to older age and associated mortality risks. Is it possible to identify gender on at least a portion of these adults so that age at maturation can be compared by gender?

It is logical to expect that mean age at maturation will vary between stocks and between hatchery and wild Chinook. This variation and changes in jack abundance between stocks can confound forecasts that do not use stock-specific data. The report notes that an unknown factor(s) is causing age in maturation to vary synchronously among the stocks. There has been a tendency for age at maturation to decline over time, as shown in Fig. 6.2, indicating that the reproductive potential of individual Chinook is declining. The decline in age at maturation of Chinook salmon has been also observed in Alaska Chinook salmon and Atlantic salmon. Growth is known to be an important factor influencing salmon age at maturation, as well as age at smoltification, such that faster growth often leads to earlier maturation. Therefore, the investigators should consider length at age data as a factor that might explain earlier maturation as indicated by more jacks in recent years. However, it is possible that seasonal growth patterns, rather than cumulative growth quantified by length at age, may be a key factor. Another consideration is size-selective ocean harvest, which was an important factor in Atlantic salmon.

Appendix A: Survivals (Sr), SAR, TIR, and D for Snake River hatchery and wild spring/summer Chinook salmon, steelhead, and sockeye

This appendix contains interesting information in long list of figures and tables with essentially no text. The legibility of the main document has been improved by placing the lengthy section of figures and tables in the appendix.

Please clarify that mini-jacks were excluded by counting the smolt population at the first detector rather than at release.

It would be helpful to include a full "flow" diagram of the route of the fish, for example expand Figure A.1 to show the dams along the route, with the survival and detection parameters overlaid so that the rationale of the equations can be more readily seen. For example, different actions occur at different dams at the collector/detection stations and it is difficult to piece these together from the various equations.

The definition of C_0 should be more carefully stated. On page 126, C_0 is defined as the "PIT-tagged smolts that migrate through the hydrosystem without being bypassed at any of the Snake River collector dams." The phrase "that migrate through the hydrosystem" seems to imply that they fish actually survive to the mouth of the Columbia, but in fact, C_0 includes those smolt that die before reaching the mouth whose "virtual" bodies would not have been detected at subsequent dams, that is assuming that the dead fish still move through the system. This is why equation A.5 has the form it does, because it uses conditional detection probabilities, that is p_i is the probability of detecting a fish given that it is alive at dam i, in an unconditional fashion.

The rational for using the expected counts starting on page 152 requires justification. Additional variability is introduced when the raw data is used, but the bootstrap confidence interval is supposed to capture this variability. The bootstrap confidence intervals are likely too narrow because of the use of the expected counts. This may make the test for equal SAR in equation A.13 too liberal.

Similarly, clarification is needed to understand if bootstrapping accounted for the adjustment on a per mile basis for those groups where the missing portion of the total in-river survival rates was imputed.

More explanation is needed of how bird predation at the bypass outfall of LMN (presumably after PIT-tag detection at LMN) would bias the estimate of survival for the LGS-LMN reach, and how this bias relates to the remarkably low detection probabilities at LMN.

Specific Editorial Comments

Many of the tables and figures in this report use acronyms, abbreviations, and symbols that are not defined in the table heading or footnotes. Although many are defined in the glossary at the end of the report, for clarity it would be helpful to include definitions in the table heading, as well.

p. 4-5: What is meant by "complete return data"? Do complete return data include all ocean age groups or just the dominant age groups mentioned in the text for each species? There is no explanation of the age designation method, i.e., "1-salt, 2-salt, 3-salt" or Chinook salmon life history types, i.e., "ocean-type" and "stream-type." Perhaps these could be added to the glossary of terms. There is no mention of steelhead life history types (stream-maturing or summer-run vs. ocean-maturing or winter-run). How are PIT-tag detections of steelhead kelts (repeat-spawners) treated in the SAR estimates?

p. 5, 1st paragraph: "The number of individuals detected from a population of tagged fish decreases over time, allowing estimation of survival rates." The critical assumption seems to be that if a tagged fish is not detected then the tagged fish is dead, but this assumption is not stated. Perhaps this is a good place for a brief review and discussion of critical assumptions and uncertainties in survival estimates using PIT-tag data.

p. 5: Figure 1.1 is an informative figure, but it does not completely clarify the classifications and nomenclature used in the report. The use of different colors on the figure is more confusing than helpful. The “D” almost looks like a typo and the caption definition does not clarify. One needs to read much farther into the report to determine what “D” is. This is a key figure that could more effectively educate the reader about the different ways in which fish movement through the system is evaluated. One suggestion is to include the site abbreviations on the figure (e.g., Lower Granite (LGR), Lower Monumental (LMN), etc.). Further, could colors and dashed lines be used to distinguish different calculation mechanisms or to distinguish between calculations for wild versus hatchery fish? The fish show as two colors on the top of the figure, but are not distinguished by these colors on the figure. Could the path for transport fish be shown with a dashed line, for example, through the entire figure (all the way to return)?

p. 8, paragraph 1: *“Therefore we measure SARs against the regional management goal to maintain SARs between 2 and 6%, where....”* Please add that Lower Granite Dam is the reference site for enumeration of migrating juveniles and returning adults. This was done in the caption for Figure 1.1, but not in the text.

p.10, line 34: Indicates that the probability of detecting PIT-tagged adults at LGR is nearly 100%. It would be useful to also summarize the typical range in detection probabilities for juveniles in this paragraph.

p. 11, lines 1-2: Consider clarifying the statement *“Because TIR compares SARs starting from collector projects, it does not by itself provide a direct estimate of delayed mortality specific to transported fish”* by adding the parenthetical clause “(see below for description of use of the factor “D” as an estimate of transportation-related delayed mortality”).

p. 11, Line 39-40: Are these tagged fish assigned to treatment groups at random? If not, how are they assigned?

p. 11, Line 43-45: Are these fish tagged but treated the same as untagged fish? Are they barged?

p. 12, bottom of paragraph 4: When describing the assumptions regarding in-river survival and survival in barges, it would be informative to show in parentheses the range in assumed survival values. Given that delayed mortality estimates (D) are based in part on these assumed downstream migration/transport survival values (as a means to estimate survival beyond

Bonneville), it is important that the bootstrapped confidence intervals incorporate this additional uncertainty.

p. 12: Clarification is needed in the first sentence of the last paragraph of the section on data generation. The last phrase of this sentence: *"…first from passage at LGR as smolts to their return as adults to LGR (TIR)"* was confusing. Consider rewording as "first using TIR for passage at LGR as smolts to these smolts' return as adults to LGR."

p. 13, lines 29-31: "*Wild Chinook from each tributary (plus fish tagged at the Snake River trap near Lewiston) were represented in the PIT-tag aggregates for migration years 1994 to 2011. The sample sizes for each group with tags provided by the CSS from 1994-2011 are presented in the appendices at the end of this report.*" These appendices were not provided in the 2012 CSS draft report, and would have aided the ISAB's review. How does interannual variation in aggregate stock composition, for example in the Snake River spring/summer ESU that includes 28 independent populations and 5 major population groups, influence annual variation in survival estimates?

p. 15: "*Based on past estimates of SARs, sufficient numbers of smolts were tagged to ensure enough returning adults to compute statistically rigorous SAR estimates. Required samples sizes for SAR estimates are discussed in Appendix B of the CSS 2008 annual report*." A brief summary of sample size requirements would be useful.

p. 16-18, Tables 1.1 to 1.3: It would be useful to indicate grand totals for the number of fish tagged in the previous year (2011) or indicate % change.

p. 22, Line 18-19: Evidence should be presented or cited that the detection probability through bypass systems is nearly 100%.

p. 23, Line 27-28: Here the one best fitting model was used, but in Chapter 3 the estimates were model averaged. Usually the best fitting model is used if the Akaike weight is high (e.g., >0.8-0.9). Is this the case? If not, is there the possibility of model averaging over the multiple top models? Burnham and Anderson (2002) give guidelines for how many of the top models to average.

The methods used seem appropriate, but more information about the probabilities of detection should be given. In the Appendix, the methods used to estimate the detection probabilities are given, and Figure 2.1 shows the detection probabilities calculated and predicted, but more information on the linkage between these values and how they are applied in the equations in the Appendix is needed. For example, are all the detection probabilities shown in Figure 2.1 used or are the values aggregated for some calculations? Also, it seems that separate probabilities should be calculated for each of the detection facilities. Is the discussion on pages 23-24 of how detection probabilities were calculated at MCN an example and was the same method used for all other detection facilities? It is not clear if the values in Figure 2.1 are values only for MCN.

p. 29: Consider changing the wording of lines 33-34 to "We calculated fish travel time as the number of days between release of a cohort at LGR until detection at MCN for each fish subsequently detected at MCN."

p. 32: It is mentioned that "average spill percentage" was calculated. More details are needed on how this is calculated. On page 33, it is mentioned that spill is one of the seven environmental factors evaluated. Is this "spill" the same as the "average spill percentage" mentioned on the previous page? Is this variable considered a continuous variable (i.e., if it's a percentage, it varies between 0 and 100%) or a binary variable (yes there is spill/no there is not). What variable is used here is important in terms of the management implications.

p. 35: what is meant by "high degree of contrast" in the first sentence? Would "high degree of variability" be appropriate?

p. 35: The reference to Figures 3.4 and 3.5 in the first paragraph on page 35 is in error and is referring to other figures.

p. 45: The NPCC 2-6% SAR objective for Chinook addresses the total adult return including jacks (i.e., 1-salt male Chinook). Although the CSS draft does indicate the complementary value of reporting SARs both including and excluding jacks, this reporting has not been consistent throughout the document.

p. 48: It is not clear if the bootstrapping captured all of the uncertainty. Did the bootstrapping also apply to estimating the survival rates between dams based on the mark-recapture model that was used to obtain the T_0^* on page 48?

p. 49: "*The method of deconstructing SARs into first year ocean survival rates used here is described in Petrosky and Schaller (2010) (Appendix D), and is similar to approaches used in STUFA (2000), Wilson (2003), and Zabel et al. (2006)*." A brief summary and justification of the method and a discussion of any differences in methods in the cited references would be useful.

p. 47: "*point estimates are calculated from the population*…" Estimates are computed from samples. It is not clear if the use of the "population" refers to a specific stock.

Figure 4.1: Why are there no SAR estimates from RR for wild Snake River spring/summer Chinook in years 1985-1993 (as suggested in caption and shown in Figure 4.4 for steelhead)?

Figure 4.4: The 1999 LGR-Columbia River SAR for wild Snake River steelhead appears to be much lower than the corresponding LGR-GRA SAR in Figure 4.5 and Table 4.19. LGR-BOA SARs for 1997-1999 are missing from Table 4.19, so some explanation is needed for where the points plotted in Figure 4.4 came from.

p. 54: "*Estimated overall SARs (LGR-GRA) were higher for A-run hatchery steelhead than for B-run hatchery steelhead in 2008 and 2009 (Table 4.20)*." Is this result influenced by differences in ocean age composition of adult returns of the two runs (i.e., A-run fish are typically younger than B-run fish)? Does smolt outmigration timing differ between A- and B-run stocks?

p. 68: "*To date, a definitive control group has been lacking to quantify the potential post-marking mortality or tag shedding bias in PIT-tag SARs*." What are the criteria for a "definitive control group"?

p. 71, lines 1-2: It would be useful to suggest why the adjusted window counts of hatchery Chinook were 12-23% higher than the expanded PIT-tag estimates and to describe the implications of this discrepancy.

Editorial: Chart axis labels "Migration year" should be relabeled as "Smolt Migration Year" to avoid confusion with adult migration year.

In general, the smolt migration years with highest SARS correspond to negative Pacific Decadal Oscillation (PDO) values, i.e., cool sea surface temperature (SST) periods in the Northeastern Pacific, Gulf of Alaska, and southeastern Bering Sea. Could the PDO index be used as a tool to estimate SARs or to better manage the hydrosystem to increase SARs?

Does the CSS have any hypotheses as to why the 2008 smolt outmigration year was the best survival since 2000 for many Chinook and steelhead stocks?

p. 98, line 25: error in first variable in last expression? Presumably FN/N should be TN/N.

p. 100, lines 22-23: The procedure is not clear from this sentence.

p. 100, lines 33-36: The last part of this sentence is confusing; "*or passed LGR the following spring after the bypass and detection systems restarted.*" Does the restarting refer to BON or LGR?

p. 102, lines 7-8: The definition of HO_u needs clarification, and it would also help to explain what the first term in the equation on line 4, $HO_u * (HO_{bon} / p_{bon})$, refers to in contrast to the second term which refers to the holdovers estimated from detections at BON after detection was restarted.

p. 106, lines 27-28: Does the age convention used here for fall Chinook differ from other Pacific salmon? If not, sub-yearling (age 0+) smolts migrating downstream in 2006 would have hatched from eggs deposited in late 2005, and would reach age 5 in late 2010, rather than 2011 as seems to be indicated here. Same comment applies elsewhere in this chapter, for example page 110, lines 14-15 where the age convention appears to have been used inconsistently.

Tables 5.11 to 5.17: Why are SARs, TIRs, and Ds reported only without jacks, whereas previous tables report values with and without jacks, and all previous figures in Chapter 4 show results including jacks?

p. 114: "*The method CSS developed for differentially identifying subyearling fall Chinook holdover probability worked well on a population level but did not work well for identifying individual fish within release groups*." Is the CSS exploring other methods that might succeed in identifying individual fish as holdovers? Can a method be developed to estimate SARS for groups with high percentages of holdovers?

p. 115, last bullet of Conclusions: Perhaps it is more accurate to say that significant benefits from transport were evident in 2008, but no consistent benefits from transport were evident in 2006 and 2009.

p. 116, lines 12 and 16: The words "former and "latter" are switched.

p. 117, lines 12-16: The first sentence in this paragraph states that age-at-maturity was determined for sixteen stocks, but the third sentence makes reference to seventeen stocks. The reason for this (Table 6.1) is that two wild stocks were aggregated for analysis.

p. 117: According to an aging scheme in common use, a S/S Chinook juvenile produced by, for example, the 2006 spawning run would migrate to sea in its second spring (2008) and be designated a yearling, with one freshwater annulus on the otolith (winter of '07-'08). If it returned in the spring of the following year (2009) it would be designated a "jack" (one-salt, with one freshwater annulus and one annulus from the winter at sea) and an age-3 spawner. The description on page 117 (lines 22 to 23) seems to use this scheme: "Chinook adult returns consisted of age-3, age-4, and age-5 fish. Age-3 fish are predominantly male and are termed jacks." However, the following paragraph (lines 28-29) then states: "jacks, 1-salts, and 2-salts are presented as age-4, age-5, and age-6, respectively." This terminology appears to be incorrect and does not match the earlier terminology. Age terminology should be clearly defined and used consistently.

p. 154, line 1: appears to be an error in the coding as codes 102 and 1002 appear twice.

Table A.35: The TIR for Oxbow sockeye in 2009 appears to be significantly >1. Lower CI should be in bold font.

References

Burnham, K. P., and Anderson, D.R. (2002). Model Selection and Multimodel Inference: A Practical Information-Theoretic Approach, 2nd ed. Springer-Verlag.

Haeseker, S.L., McCann, J.S., Tuomikoski, J., and Chockley, B. (2012) Assessing freshwater and marine environmental influences on life-stage-specific survival rates of Snake River spring–summer Chinook salmon and steelhead. *Transactions of the American Fisheries Society* 141:121-138.

Harnish, R.A., Johnson, G.E., McMichael, G.A., Hughes, M.S., and Ebberts, B.D. (2012) Effect of Migration Pathway on Travel Time and Survival of Acoustic-Tagged Juvenile Salmonids in the Columbia River Estuary, Transactions of the American Fisheries Society, 141:2, 507-519

Knudsen, C.M., Johnston, M.V., Schroder, S.L., Bosch, W.J., Fast, D.E., and Strom, C.R. (2009) Effects of Passive Integrated Transponder Tags on Smolt-to-Adult Recruit Survival, Growth, and Behavior of Hatchery Spring Chinook Salmon. *North American Journal of Fisheries Management* 29:658–669.

Levings, C.D., C.D. McAllister, J.S. Macdonald, T.J. Brown, M.S. Kotyk, and B. Kask. (1989) Chinook salmon and estuarine habitat: A transfer experiment can help evaluate estuary dependency. p. 116-122. Can. Spec. Publ. Fish. Aquat. Sci. 105. 199 p.

Macdonald, J.S., C.D.Levings, C.D. McAllister, U.H.M. Fagerlund, and J.R. McBride. (1988) A field experiment to test the importance of estuaries for Chinook salmon (Oncorhynchus tshawytscha) survival: short term results. Can. J. Fish. Aquat. Sci. 45: 1366-1377.

Marmorek, D.R., Peters, C.N., and Parnell, I. (eds.). (1998) PATH final report for fiscal year 1998. Compiled and edited by ESSA Technologies, Ltd., Vancouver, B.C. Available from Bonneville Power Administration, Portland, Oregon (http://www.efw.bpa.gov/Environment/PATH/reports/ISRP1999CD/PATH%20Reports/WOE_Report/).

McComas, R. L., McMichael, G.A., Vucelick, J.A., Gilbreath, L., Everett, J.P., Smith, S.G., Carlson, T., Matthews, G., and Ferguson, J.W. (2008) A study to estimate salmonid survival through the Columbia River estuary using acoustic tags, 2006. Report to the U.S. Army Corps of Engineers, Portland District. Available: http://jsats.pnl.gov/Results/Publications.aspx (September 2009).

Mueter, F.J., Pyper, B.J., and Peterman, R.M. (2005) Relationships between coastal ocean conditions and survival rates of salmon in the Northeast Pacific Ocean at multiple lags. *Transactions of the American Fisheries Society,* 134:105-119.

Muthukumarana S., Schwarz C.J., and Swartz T.B. (2008) Bayesian analysis of mark-recapture data with travel-time-dependent survival probabilities. *Canadian Journal of Statistics* 36:5-28.

Petrosky, C. E., and Schaller, H.A. (2010) Influence of river conditions during seaward migration and ocean conditions on survival rates of Snake River Chinook salmon and steelhead. *Ecology of Freshwater Fish* 19:520–536.

Pyper, B. J., F. J. Mueter, F.J., and R. M. Peterman, R.M. (2005) Across species comparisons of spatial scales of environmental effects on survival rates of Northeast Pacific salmon. Transactions of the American Fisheries Society 134(1):86-104.

STUFA (State, Tribal and U.S. Fishery Agencies) (2000) A technical review of the National Marine Fisheries Service's Leslie matrix model of Snake River spring and summer chinook populations. April 28, 2000. Prepared by State, Tribal and U.S. Fisheries Agencies. Submitted to NMFS for ESA record. http://www.fws.gov/columbiariver/publications.html

Wilson, P.H. (2003) Using population projection matrices to evaluate recovery strategies for Snake River spring and summer Chinook salmon. *Conservation Biology* 17(3): 782-794.

Zabel, R.W., Scheuerell, M.D., McClure, M.M., and Williams, J.G. (2006) The interplay between climate variability and density dependence in the population viability of Chinook salmon. *Conservation Biology* 20:190-200.