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Dr Usha Varanasi, Ph.D.
Northwest Fisheries Science Center
NOAA Fisheries
Seattle, Washington

Dear Dr. Varanasi:

Thank you for your review of the Draft, Ten-year Retrospective Summary Report. The following response was developed by the Comparative Survival Study Oversight Committee, (Committee) comprised of, the Columbia River Inter-tribal Fish Commission, the Washington Department of Fish and Wildlife, the Oregon Department of Fish and Wildlife, the Idaho Department of Fish and Game, and the US Fish and Wildlife Service. As you are aware the Comparative Survival Study is a joint project of the agencies and tribes. The study design, the implementation of the study and the analysis are carried out collaboratively among the sponsoring fish and wildlife management agencies and tribes. The Committee has developed the following response to your general comments, which are followed by the response to each specific comment.

The CSS study uses regionally accepted analytical methodologies, and innovative approaches based upon peer-reviewed scientific literature. The methods and analysis are well within the methods and analytical approaches utilized by the Northwest Fisheries Science Center (NWFSC) in the 2005 Technical Memorandum available to the region. By working collaboratively on study implementation, design development and analysis, the experience and skills of the state, federal and tribal fishery managers have been a valuable asset for this study. We have addressed the NWFSC comments on the CSS report in the attached (attachment 1) document.

The CSS Oversight Committee is grateful for the significant investment by NOAA in the review and preparation of comments on the draft report. The report has been improved as a result of addressing and incorporating comments. We look forward to future positive collaboration with NOAA on future CSS monitoring and evaluation.

Sincerely

Michele DeHart

Project Leader, Comparative Survival Study

Attachment 1

Reviewer Comment :At the request of Paul Wagner and Ritchie Graves, we reviewed the DRAFT “Comparative Survival Study (CSS) of PIT-tagged Spring/Summer Chinook and Steelhead in the Columbia River Basin Ten-Year Retrospective Analyses Report.” The report is extraordinarily long (377 pages); too long to read, digest and provide finely detailed commentary in the review time available. The following paragraphs summarize our major concerns with the report. Please call John Williams (206.860.3277) if you have any questions regarding these comments.

Response: The main report is actually 212 pages (plus appendices), similar in length to the 2006 annual report. The NWFSC provided comments on previous annual reports. The ten-year report deadlines and the review schedule were determined by the NPCC, with little input from the authors. While we sympathize with the tight review schedule, we also note that the NPCC required schedule for report preparation was extremely tight for a ten-year report with this breadth and depth of analysis – November 2006 to June 2007.

Reviewer Comment:1. Most strikingly, despite its title and the fact that the CSS study group has PIT-tagged hundreds of thousands of juvenile Chinook salmon and steelhead, the CSS retrospective report does not contain a holistic analysis of this 10 –year effort or an integration of the results across all species that considers different migration conditions.

Response: The Merriam-Webster dictionary defines holistic as “relating to or concerned with wholes or with complete systems rather than with the analysis of, treatment of, or dissection into parts”. The CSS Oversight Committee believes that we have presented and integrated the various components and analyses to present a holistic depiction of SARs and factors affecting SARs for the target species and study period, as requested by the ISAB. Certainly, with a large, robust data base such as provided by CSS, other analyses are possible and desirable.

This comment missed the substantial work that was done and presented throughout this report to holistically analyze the results that have been obtained to date through the CSS. Chapter 2 contains an extensive, holistic synthesis of observed fish travel time, survival and instantaneous mortality rates, along with an explicit evaluation of the effects of different migration conditions on these rates. The study covers a number of years for both species that reflect quite varied migration conditions (e.g., drought year 2001 versus high-flow year 1998). Further, within-season variation in SARs of both transported and in-river fish is explored in Chapter 4. In addition, we evaluated the influence of in-river, climatic and ocean conditions on Snake River SARs in Chapter 5.

Reviewer Comment: 2. The data presented and the discussion and conclusions’ sections all seem focused through the lens of specific positions favored by the authors; hydropower system-related latent mortality is large in magnitude, transportation is not beneficial, management actions directed at the hydropower systems have generally failed, and consequently SARs have been low in recent years and drastic actions are needed to recover the wild Chinook salmon populations, as PIT-tagged wild fish fail to meet a minimum 2% SAR. Results that do not support desired

positions are usually discounted by carefully placed language. For example, from the conclusions in Chapter 8 (all italics are ours):

*“Variation in [survival] in the MCN-BON reach was explained by temperature and Julian day. However, there was substantial uncertainty in the lower reach due to reduced numbers of PIT-tagged fish available, which may have affected the ability to identify **the important factors**”.*

*“In general, transportation provided benefits most years to Snake River hatchery spring/summer Chinook 1997-2004, **however** benefits **varied** among hatcheries.”*

*“Migration year 2001 had very high **but imprecise** TIRs, for both wild and hatchery steelhead.”*

“Overall SARs for wild spring/summer Chinook fell short of the NPCC SAR objectives. Overall SARs of wild steelhead also fell short of NPCC SAR objectives although they exceeded those of wild Chinook. Based on these CSS SAR results relative to the NPCC SAR objectives, it appears that collecting juvenile fish at dams and transporting them downstream in barges and trucks and releasing them downstream of Bonneville Dam did not compensate for the effect of the FCRPS on survival of wild Snake Basin spring/summer Chinook and steelhead migrating through the hydrosystem.”

*And finally the tacit assumption exists that differential post-Bonneville mortality between transported and in-river fish is “delayed mortality”, i.e. an actual mortality event separated in time from its cause (once stated in the text specifically as “delayed mortality **from** transport”)*

We point out that : 1) whether or not the observed SAR in these years fell short of NPCC objectives provides no evidence one way or the other about compensating for the effects of the FCRPS; 2) the authors of the report have no knowledge of what the SAR would have been in these years if the FCRPS had not been in place; and 3) data now clearly provide the evidence that post-Bonneville mortality of transported fish is higher than for in-river migrants, but the reasons for this difference are still hypothetical.

Response: This NWFSC criticism is not well justified. The qualifying language (italicized by NWFSC) for the first three quotes accurately described our findings (identifying where transportation was beneficial, contrary to the NWFSC comment). For example, transportation did provide benefits most years to hatchery spring/summer Chinook, and benefits did vary among hatcheries. Also, TIR estimates for steelhead were imprecise in 2001. We have used neutral terms to describe results and implications of the CSS. Overall SARs from wild Snake River spring/summer Chinook and steelhead clearly have been less than NPCC objectives (minimum 2%, average 4%) across a wide range of ocean and migration conditions; whereas wild stream-type Chinook from downriver populations passing fewer dams have fared much better (see Figure 5.15). Post-Bonneville differential mortality between transported and in-river migrants is differential delayed mortality because it takes place after fish have transited the FCRPS. Moreover, our conclusion that transportation did not fully compensate for FCRPS effects is completely consistent with the NWFSC “Effects memo” (Williams et al. 2005) conclusion (p. xvi) that “transportation is not a panacea for negative effects of dams on fish stocks.”

3. The authors repeatedly state that wild Chinook salmon do not meet the minimum 2% return rate goals of the region. Granted the CSS study uses only PIT-tagged fish, but in all cases where the comments on the 2% SAR goal are stated, no caveat exists that this represents data for PIT-tagged fish returns. The ISAB (2006) specifically indicated in comments on the 2005 CSS report that CSS participants needed to look into the potential disparity between PIT-tag returns and the unmarked population. Yet, in this report the ISAB comments are treated by a short discussion indicating that it was not clear how many actual wild spring-summer Chinook salmon passed Lower Granite Dam because some fish without ad-clips (ostensibly wild) were actually hatchery fish. Nonetheless, Copeland et al (2007) provided analyses of SARs for run-at-large nonad-clipped fish from the Snake River basin. In 3 of 5 years included in the CSS study (migration years 1998-2002, Figure 5.11), Copeland et al (2007) found that SARs exceeded 2% and more than 3.1% in 2 of them. They did not adjust for non-clipped hatchery fish in either the smolt or the adult life stages, so some bias in SARs may occur if differential survival existed between unmarked hatchery smolts and wild returns. Some unpublished analyses by NWFSC staff estimated the number of non-clipped hatchery smolts in the outmigration and used that to adjust adult returns to estimate numbers of wild fish (Figure 1). These analyses derived slightly different SARs than Copeland et al(2007) but they were similar.

Response: The introduction to Chapter 5 (p. 105) cites the ISAB (2006) issue that more attention should be given to whether PIT-tagged fish survive as well as untagged fish. Chapter 5 contains a section (p. 147) titled: “Do PIT-tag SARs represent SARs of the run-at-large?” with further discussion on p. 150-151. We agree with the ISAB (2006) conclusion that more attention should be given by CSS *and the Region as a whole* (emphasis added) to the discrepancy of SARs between PIT-tagged and untagged fish. However, the extremely tight reporting requirements did not allow for an examination of all the assumptions and data adjustments currently necessary to estimate SARs of the untagged component. Because the issue involves potential bias of both run-reconstruction and PIT-tag methodologies, resolution will require a collaborative effort among several technical groups in addition to the CSS project.

Contrary to the NWFSC comment that no caveat exists that PIT-tagged SARs may have a bias relative to the NPCC goal, the draft report explicitly stated (p. 147) “[t]he primary concern of negative bias from PIT-tag SARs would be in evaluating whether SARs are meeting NPCC biological objectives (2% minimum, 4% average).” Also, “[i]mplications of bias (if present) would be negligible for relative comparisons of the CSS PIT-tag SAR data, such as between Snake River migrants with different hydrosystem experiences, or between Snake River and downriver populations.” We also point to future monitoring and evaluation tasks to help resolve this issue in the future. We note that the 2 to 4 % goal itself was based on analyzes involving tagged fish that presumably experienced some handling mortality relative to the unmarked population.

Reviewer Comment: 4. Despite the ISAB recommendation to do so, this report does not include analyses of return rates of PIT-tagged and unmarked fish based on data in the CSS 2005 report (Berggren et al 2005). This seems most surprising given that the first four conclusions of this retrospective report laud the ability of the CSS group to PIT-tag over 2 million hatchery fish and analyze data from them. The absence of these analyses begs the question as to why and implies

the analyses may have wakened the reports statements about wild fish SARs. When NWFSC staff analyzed the CSS data we found that unmarked hatchery Chinook salmon returned at higher rates than PIT-tagged fish (Figure 2) which is similar to results from the analyses of wild Chinook Salmon and steelhead (Figure 1).

Response: We addressed this issue in detail in Chapter 5. In addition, we also addressed this issue, in part, in Chapter 6, where we identify potential ways to address the question of PIT-tag detection and recovery at the hatchery weirs. Figure 2 of the NWFSC comments does not accurately represent hatchery-to-hatchery SARs of the PIT-tagged releases; the reviewers included a known negative SAR bias by including the bypassed group (C_1) as part of the PIT-tagged population, and by not weighting the C_0 and T_0 groups according to their actual proportions for the run at large. SARs of the C_1 category are substantially lower than those of C_0 (e.g., Figure 4.22), and the C_1 group is overrepresented in the NWFSC figure 2 analysis.

Reviewer Comment: 4. The reported SARs in this report are biased downward compared to standard SARs (eg Petrosky et al (2001)) because the authors base their SARs for Chinook salmon on adult returns only, not including jacks. This is important because the oft stated goal of reaching SARs of 2% is based on SARs that include jacks.

Response: The NPCC SAR goal was adapted from the 1998 PATH report (Marmorek et al. 1998). Comparison of model-generated median SARs and jeopardy probabilities (based on the NMFS interim standard for the 2000 BiOp) suggested median SARs must exceed 4% for the 48-year (interim) recovery standard, and 2% for the 100-year (interim) survival standard (Marmorek et al. 1998).

SARs may be calculated with or without jacks as recruits; there is no “standard” SAR. For most purposes, CSS has excluded jacks from the SAR calculations. However, a review of the 1998 PATH analysis indicates that jacks were included as recruits in the SARs, as noted by the reviewers. Therefore the CSS draft report contains a slight negative bias from this factor relative to the NPCC objective for spring/summer Chinook. Wild stream-type Chinook returns averaged only 4.2% jacks during the study period (Appendix D-39). Our initial comparison had the (quantitatively minor) inconsistency that we included jacks in the run-reconstruction estimates, which we have addressed. The run reconstruction SARs in the draft report inadvertently included jacks. This has been corrected to exclude jacks for consistency with the CSS SARs, and text has been modified. Methods and statistical assumptions for the CSS SARs are covered in Appendix B (and elsewhere) in the report. The inclusion of jacks in the SAR estimates would not change conclusions of the ten-year report regarding NPCC objectives because SARs missed the 2% NPCC minimum by such a wide margin.

Reviewer Comment: 5. The chapter deals extensively with within-season estimates of the following 4 quantities: water travel time (WTT), fish travel time (FTT), fish (cohort) survival (S), and “instantaneous mortality rate” (Z), which is derived as $S = \exp(-Z \cdot WTT)$ or equivalently, $\log(S) = -Z \cdot FTT$.

Response: This comment mischaracterizes our work on several levels. First, the comment reflects a fundamental misunderstanding of the differences between the dependent and

independent variables that were analyzed. We analyzed three demographic rates as dependent variables: fish travel time, survival, and instantaneous mortality rates. We evaluated the degree of association between these dependent variables and seven independent variables: temperature, turbidity, flow, flow⁻¹, water travel time, average percent spill, and Julian day. Second, we

defined the instantaneous mortality rate (Z) as $\hat{Z}_i = \frac{-\log_e(\hat{S}_i)}{\hat{FTT}_i}$, which is the maximum

likelihood estimate for Z (Seber 1982:216). We did not equivocate WTT and FTT, as this commenter suggests, and this is a mischaracterization of our work. We found that FTT is a function of WTT, average percent spill, and Julian day, not just WTT as suggested by the commenter.

Reviewer Comment: 5. This formulation posits that a given cohort (as used here, weekly groups of fish arriving at Lower Granite Dam) has a particular instantaneous mortality rate and that direct survival through the hydropower system is directly related to fish travel time.

Response: First, the cohorts were defined as PIT-tagged fish detected and released into the Lower Granite Dam tailrace over a weekly time period, not weekly groups of fish arriving at Lower Granite Dam. Second, we estimated instantaneous mortality rates for weekly release cohorts through the equation defining the maximum likelihood estimate for Z, which is simply a transformation of the observed survival and median fish travel time rates. Third, we did not posit that weekly groups of fish have a particular instantaneous mortality rate upon arrival at Lower Granite Dam. Rather, that instantaneous mortality rates in each reach reflect the environmental or seasonal conditions experienced during migration through each reach. Predicted survival rates were then a function of the predicted instantaneous mortality rates and predicted fish travel times, both being functions of the environmental or seasonal conditions experienced during migration through each reach (termed “variable Z survival approach”). As an alternative analysis, we compared an approach where instantaneous mortality rates were at fixed levels within- and across-years, and that observed survival rates were primarily a function of changes in fish travel time (termed “constant Z survival approach”). We compare these two approaches, along with an approach that simply modeled survival rates as a function of environmental and seasonal conditions experienced during migration through the reach.

Reviewer Comment: 5. This formulation ignores that a substantial portion of the mortality occurs at the dams and is unrelated to fish travel time.

Response: The formulation used for instantaneous mortality rates accounts for differences in mortality rates that may occur during different periods during the migration. It reflects these differences as representing the arithmetic average mortality rates in cases where mortality rates may change over time (Keyfitz 1985:18-19).

Reviewer Comment: 5. As the authors note, FTT generally decreases within a season, and S (and log(S)) generally remains constant.

Response: While we found that FTT generally decreases over the migration season, there was substantial variation in survival rates over the migration season. There were examples of increasing survival trends, decreasing survival trends, and parabolic survival trends. Within-year

survival rates could differ by up to 39 percentage points for both wild Chinook and steelhead, and by up to 32 percentage points for hatchery Chinook. We would not characterize survival rates as remaining constant within a season for either yearling Chinook or steelhead.

Reviewer Comment: 5. Thus, if two different groups of fish take a different amount of time to travel through a reach but their probability of surviving is the same, then per-day mortality of the two groups must be different.

Response: We would not disagree with this statement, as it follows from the inter-relationships between instantaneous mortality rates, survival rates, and time. However, this statement appears to imply that the instantaneous mortality rate is somehow a response variable, rather than the correct interpretation that it characterizes the average proportional mortality rate over time, essentially a transformation of observed survival rates and migration rates.

Reviewer Comment: 5. To conclude that decreasing FTT by managing the river to decrease WTT will result in increasing S (survival) requires the assumption that the quantity Z is an intrinsic characteristic of a group of fish; i.e., that the instantaneous mortality rate of the group is fixed at the time they leave Lower Granite Dam and that if we could only decrease their travel time to McNary Dam, then less mortality would occur.

Response: Again, this comment reflects some fundamental misunderstandings about our analyses. We did not assume that instantaneous mortality rates were fixed at the time they leave Lower Granite Dam. Rather, we assumed that instantaneous mortality rates reflected the environmental and/or seasonal conditions experienced *during migration* through the reach. Actions which may affect instantaneous mortality rates and/or actions which may affect fish travel times, both could affect resulting survival rates (under the variable Z survival approach). We also examined two other approaches (standard survival approach and constant Z survival approach) for predicting survival rates.

Reviewer Comment: 5. At least equally plausible and supported by observed data using the exact same relationship is a conclusion that management actions to decrease fish travel time would increase instantaneous mortality and that survival would remain the same.

Response: We have added a section to the discussion that examines this NWFSC hypothesis. To examine this hypothesis, we plotted the LGR-MCN instantaneous mortality rate estimates against observed median fish travel times for the early, mid, and late migration periods (Figure 2.23). We grouped the data by the early, mid, and late migration periods to account for potential seasonal differences in instantaneous mortality rates. An increase in instantaneous mortality rates as median fish travel times decrease would lend support to the NWFSC hypothesis. However, the data do not indicate that instantaneous mortality rates increase as median fish travel times decline (Figure 2.23). Based on the simple plots presented in Williams et al. (2005), which did not account for potential seasonal differences in instantaneous mortality, we understand how one might surmise that instantaneous mortality increases with decreasing fish travel times. However, we believe this is an incorrect interpretation of the data brought about by not accounting for the seasonal increases in instantaneous mortality that we frequently observed.

Reviewer Comment: 5. Therefore, the conclusion by the authors that decreasing FTT by half a day in the lower river would decrease steelhead mortality by 5.6% is highly questionable. Furthermore, the authors have incorrectly interpreted their result to derive this estimate. A Z of 0.112 does not imply a mortality of 11.2% per day. The correct interpretation is that the daily mortality is $1.0 - \exp(-0.112)$, or 10.6%. Note that this discrepancy grows larger as FTT increases.

Response: The conclusion that decreasing FTT by half a day in the lower river would decrease steelhead mortality by 5.6% simply follows from the law of exponential population decline and the mean instantaneous mortality rates that were observed. Furthermore, for values of $Z \leq 0.1$, mortality rates and Z estimates are approximately equivalent (Ricker 1975). However, to clear up any confusion on the trivial differences between the two, we have provided both daily percent mortality estimates and Z estimates (Tables 2.1, 2.2).

Reviewer Comment: 5. When the authors relate Z to a variety of factors, an additional problem is encountered. WTT and FTT are correlated with each other and relatively stable within seasons, and as stated above, S (and log(S)) has repeatedly remained relatively constant within seasons, especially for spring-summer Chinook salmon. The final quantity (Z) is derived by dividing the relatively constant quantity log(S) by the relatively variable FTT. It is no surprise, then, that Z and WTT are correlated. In fact, this is inevitable because of the relationships described above and is a classic example of a “spurious correlation.”

Response: First, consistent with Williams et al. (2005), we examined the relationship between instantaneous (daily) mortality rates and water travel time (along with five other independent variables). Criticisms levied the NWFSC for our examination of the relationship between instantaneous mortality rates and WTT, when the NWFSC has conducted similar analyses (Williams et al. 2005), are hypocritical. Second, with the correlation between WTT and FTT, one must remember which is considered a response variable (FTT) and which is considered an independent variable (WTT). FTT cannot influence WTT, whereas WTT may or may not influence FTT. We found that several other independent variables (average percent spill and Julian day), not just WTT, influenced FTT. Third, we observed some fairly dramatic increasing, decreasing, and parabolic seasonal trends in within-season estimates of survival. Within-season survival rates could differ by up to 39 percentage points for both wild Chinook and steelhead, and by up to 32 percentage points for hatchery Chinook. We would not characterize survival rates as remaining constant within a season for either yearling Chinook or steelhead. The instantaneous mortality rates (Z) largely reflected these changes in survival rates, with most of the variation in instantaneous mortality rates associated with variation in survival (49% for Chinook and 58% for steelhead), followed by Julian day (35-36% for Chinook and steelhead) (Table 2.11).

Reviewer Comment 6. Comments regarding attention on wild vs. hatchery fish, use of C₀ vs. C₁ fish, and evidence indicates only that there is no benefit to transporting wild Chinook, not that it is harmful.

Response: In the report, we did look at temporal (within-season) variation in SARs in Chapter 4, using C₁ fish as surrogates. Further, annual estimates can be useful in comparing seasonal

transportation modification strategies, under an adaptive management regime (i.e. change strategy, monitor how annual SARs, TIRs, *D*s change from the “baseline”).

In a sense, CSS C_0 fish are not represented by reach survival rate estimates of tagged fish, due to different disposition at dams. However, the CJS model requires downstream recaptures (detections) in order to estimate detection probability and survival rates. Therefore, the assumption that detection history doesn't affect significantly affect short reach survival rates is necessary for survival rate estimation. If violation of this assumption is influential, all reach survival estimates (including NOAA's) are affected.

Chapter 3 provides extensive results for SARs, TIRs, and *D* estimates for hatchery Chinook and steelhead. Absolute values and trends in these quantities are compared between wild and hatchery fish. Chapter 4 suggests that transportation, as currently implemented, is detrimental to wild Chinook, since a majority of the TIR distribution at each project falls below one.

6. Chapter 3 NOAA Comment (Part A): The chapter focuses mostly on wild Chinook salmon, and therefore does a poor job of comparing the results of analyses among wild and hatchery Chinook salmon, and wild and hatchery steelhead. Without these comparisons, managers have little ability to determine the best strategies that will lead to the optimum return for the different species and type (wild or hatchery).

Response: Based on all comments from all reviewers of Chapter 3, a major rewrite of the results and discussion section of this Chapter has rectified those concerns.

Chapter 3 NOAA Comment (Part B): Another shortcoming of the analysis derives from the authors' insistence on only using C_0 fish as “true controls.” They argue that because these fish are not seen at transport dams, no temporal analyses are possible. Thus, the analyses presented in this chapter will provide little guidance on the important management questions for each transport dam related to when to begin transportation within a season, and when and how much spill should occur. The emphasis on “true controls” in the CSS study seems misplaced. A better foundation for analyses would use data similar to what is presented in Table 5.16. Here, data comparing C_0 to C_1 fish (for fish observed at Bonneville Dam) indicate that in the preponderance of comparison, C_1 fish have equivalent SARs of the C_0 fish (point estimates in most years for bi-weekly comparisons are higher). These are the fish that make it successfully to Bonneville Dam from the different categories. Thus, it appears that use of C_1 fish would provide some useful insight into temporal changes in return rates of transported and non-transported fish. Analyses along this line would significantly improve this chapter.

Response: The wording “true controls” for C_0 fish has been removed from the text. The C_0 group is the closest representation of the untagged run-at-large fish that are not transported from the three Snake River collector dams during the years analyzed in this report. With the exception of 1997 when a management operation of bypassing most untagged steelhead at LGS and LMN throughout the season was attempted, the other years analyzed in this report (1994-1996 and 1998-2004) were periods when the management operation was to transport all collected untagged run-at-large fish. In the estimation of TIR, we are evaluating the operational condition whereby untagged run-at-large fish are transported if collected relative to those untagged run-at-

large fish not collected. Therefore using the PIT-tagged groups that closest reflect those two groups are proper choices for the TIR estimation. If the question had been what to do with the collected fish, then using SAR(T_0) and SAR(C_1) in the TIR estimation would have been proper. The question of temporal changes in SARs was not covered in Chapter 3, but is covered for wild Chinook and wild steelhead in Chapter 4 using dam-specific estimates of transported and bypassed PIT-tagged fish. Whether one uses C_0 or C_1 fish in a particular evaluation must be determined by the question at hand though, and not by whether post-BON SAR estimates for groups C_0 and C_1 are similar, as inferred by NOAA in the latter part of their comment regarding data from Table 5.16. PIT-tagged fish in Table 5.16 are fish that survived to the lower river, whereas the PIT-tagged fish used in the CSS estimations of TIR and D are based on estimated numbers of T_0 and C_0 fish beginning their passage through the hydrosystem.

Chapter 3 NOAA Comment (Part C): Additionally, nearly all the analyses discussed presume that survival estimates for non-transported fish (the “true controls”) are the same as those of the marked population used to make juvenile survival estimates. However, using the CSS argument, the PIT-tagged fish returned to the river do not represent “true controls” and do not measure the survival of fish not detected at transport dams because they are based on the combined population of detected and non-detected fish. A disconnect thus occurs. Since non-detected fish mostly pass through spill, one might reasonably assume they have a higher survival than the combined population.

Response: In the estimation of in-river reach survival rates between the dams with detectors, all users (including NOAA) of PIT-tagged data in the Columbia River basin have had to rely on the assumption that prior detection history is not influencing subsequent detection probabilities and reach survival rates when using the CJS model to estimate those reach survival rates. NOAA is trying to paint the picture that since we do not use C_1 fish as “true controls,” then we should not be using C_1 fish in the estimation of reach survival rates. As stated earlier, the term “true controls” is misleading since the proper in-river group to use in any comparison will be determined by the question being answered. There is no such thing as a “true control” for every analysis. That said, NOAA raises a legitimate concern that has ramification for all users of PIT-tag data (including NOAA themselves) within the Columbia River basin for reach survival estimation. It is generally accepted based on years of COE funded evaluations of survival through spillways, bypasses, and turbines, that the spillway route gives a higher survival than bypass route. Therefore, when using the CJS model to estimate a common parameter of survival for a particular reach, all researchers (including NOAA) need to realize that each inter-dam reach survival rate estimate encompasses the unmeasured components of reservoir survival rate times weighted average of route-specific survival rate across the routes of spillway, bypass, and turbine, where the weights are the proportion of the population of PIT-tagged fish utilizing each of these three routes through a project. But in using the CJS model, we, NOAA, and others accept the assumption that all PIT-tagged fish used in estimating a particular reach survival rate are independently and identically distributed about a common reach survival rate for that particular reach. If a “disconnect” exists as stated by NOAA, then they too are part of that disconnect.

Chapter 3 NOAA Comment (Part D): Finally, even the data presented in the CSS study, when considered on an annual basis, do not indicate that transportation harms wild Chinook salmon;

just that it provides no benefit. The annual data for hatchery Chinook and steelhead all show a substantial benefit that would potentially translate into thousand of additional adult returns if spilling or collecting and transporting fish were optimized for all species at each dam. Caution on potential benefits for hatchery Chinook is warranted, however, as the CSS associated hatcheries and numbers of PIT-tagged fish released from each do not mirror the total hatchery production released in the basin.

Response: We report that the SAR data from 1994 to 2004 does not appear to show a benefit of transportation except in drought years such as 2001. The CSS did show and acknowledge transportation benefits to four of the five hatcheries used in the CSS (Rapid River, McCall, Imnaha, and Catherine Ck, but not Dworshak), and for wild and hatchery steelhead. However, delayed differential mortality of transported fish compared to the in-river migrants dampens the potential that may be achieved by transportation alone as a management tool aimed at recovering listed fish. We do not claim that the five hatcheries above LGR used in the CSS reflect all of hatchery production. Since we see differences in response to transportation among the five hatcheries used in the CSS, which currently account for approximately half of production of spring/summer Chinook from hatcheries above LGR, it is likely differences in response to transportation will also occur across those remaining hatcheries.

Reviewer Comment 7. The graphs in Chapter 4 always indicate the 2% SAR line when the majority of estimates fall below the line, but often do not include the 2% SAR line when the majority of estimates fall above it.

Response: The 2-6% desired range of SARs adopted by the NPCC was originally developed for Chinook, rather than steelhead. At the time of some of the analyses, the author of Chapter 4 was uncertain whether the target had been adopted for steelhead as well, so these weren't included in some of the steelhead figures (though the 2-6% target range was included in the aggregate steelhead SAR figure). In the rush to meet the deadline for posting the draft report, standardization of all figures was not a priority. In Chapter 4 of the revised report, the 2-6% range is indicated on all SAR figures, with the exception of the within –season figures (to avoid clutter).

Reviewer Comment: 8. The continued emphasis by CSS to compare upstream/downstream population productivity appears misplaced and has limited utility for estimating overall hydropower system impacts. We concur with the conclusion of the ISAB latent Mortality Report (2007) which stated “The ISAB concludes that the hydrosystem causes some fish to experience latent mortality, but strongly advises against continuing to try to measure absolute latent mortality. Latent mortality relative to a damless reference is not measurable. Instead, the focus should be on the total mortality of the in-river migrants and transported fish, which is the critical issue for recovery of listed salmonids. Efforts would be better expended on estimation of processes such as in-river versus transport mortality that can be measured directly.”

In addition the ISABs comments and flaws of the upstream/downstream approach that have been identified previously (Zabel and Williams 2000; Williams et al 2005), we provide two additional comments;

- *Weak scientific methodology. The standard scientific method operates by stating a null and alternative hypotheses and considering all available information in an effort to reject the null hypotheses. Science does not work by laying out a hypothesis then saying it is correct unless positive proof exists to show that it is wrong. Yet, this is what has occurred here.*
- *Ignores data from other systems. Data on natural sockeye salmon populations in Bristol Bay have shown similar trends in overall productivity as have the upstream/downstream comparisons used by CSS. Overall productivity of the Bristol Bay populations increased and decreased over a period of decades, concomitant with major changes in ocean conditions. However, some of these eight closely related populations demonstrated strikingly divergent temporal patterns (Hilborn et al, 2003; Peterman et al. 2003). Yet the analyses comparing Snake River and John Day River Chinook salmon populations assume that changes in temporal patterns do not exist. The Bristol Bay data suggest a lack of foundation for this assumption.*

Response: One major objective of the CSS study was to “begin a time series of SARs for use in hypothesis testing and in the regional long-term monitoring and evaluation program”. The intent was not to limit analyses to one particular statistical model. CSS did lay out several null hypotheses and the study was designed to address these, e.g., through estimating number of marked fish in each group to achieve target confidence levels that $TIR > 1$. The hypotheses were framed as in the 1996-98 CSS status report (CSS 2000): “Test if the annual ratio of transport survival rate to in-river survival rate (measured at Lower Granite Dam) is greater than 1.5 with sufficient power to provide a high probability that the ratio is greater than 1.0.” The “standard scientific method” with null and alternative hypotheses is hardly the only way that applied science is conducted. CSS has tested particular hypotheses under the null/alternative hypothesis formulation (e.g. see below), but has also performed parameter estimation, especially confidence interval estimation, and model selection. There is much applied science done outside of the traditional null/alternative hypothesis formulation in other ways, too; e.g. model selection, estimation of Bayesian credibility intervals, formal decision analysis, etc.

We are confused by the reviewers’ characterization of the CSS analysis in this comment. Contrary to NWFSC comment, we clearly stated that the purpose (p. 106) of the upriver/downriver SAR comparison was to determine if the difference in mortality estimated from spawner-recruit (SR) analyses was also apparent in the SARs (i.e., H_0 : differential mortality from SARs equals differential mortality from SR). Contrasts of the point estimates and 90% CI from the two types of data (p. 131-133) indicated SAR-based estimates of differential mortality agreed well with published SR-based estimates of differential mortality. We characterized the upriver-downriver comparison as a “natural experiment”, which therefore has some design limitations (p. 150). Further, we investigated and tested hypotheses regarding possible non-hydrosystem causes (including alternative hypotheses previously suggested by NWFSC) of differential mortality between upriver and downriver wild stream-type Chinook (p. 136-143).

Based on 5 years of PIT-tag SAR comparisons between wild Snake River and John Day smolts, we have seen a consistent pattern of differential mortality across poor and favorable ocean conditions. Combined with estimates of in-river survival and relative survival of transported smolts, this is one line of (indirect) evidence that the magnitude of delayed hydrosystem

mortality is large (e.g., Peters and Marmorek 2001; Schaller and Petrosky 2007). However, actual estimation of delayed or latent mortality (of in-river migrants) was not an objective of CSS, and we did not attempt to estimate it in the CSS draft 10-year report, contrary to the reviewers' comments.

In addition to the upriver-downriver comparison, we investigated the influence of ocean/climatic and migration conditions on SARs of wild spring/summer Chinook in Chapter 5. Water travel time (WTT), a measure of water velocity through a fixed reach, was influential in all top multiple regression models (p. 128-131); May or September PDO were also typically incorporated in top models. The coefficients for WTT vs. $\ln(\text{SAR})$ were consistent across models, ranging from -0.053 to -0.076. That is, for each day increase in WTT, the SAR would be expected to decrease 5% - 8%, or 65%-78% for a 20 day increase in WTT. This result is generally consistent with the differential mortality estimated from upriver-downriver comparison of wild Chinook, and was an important independent estimate that did not rely on the use of downriver reference populations.

Contrary to the NWFSC reviewers' comments, we have previously examined data from other systems, including the Bristol Bay dataset, which the reviewers claim invalidates comparing performance of different populations from the same region. We don't agree. Pyper et al. (2005) incorporated this stock group in their analysis, and found correlations in survival rate patterns up to 500 km from the ocean point of entry (upriver and downriver stocks in our analysis have the same point of ocean entry). Schaller and Petrosky (2007) found that variation of survival rates (SR residuals) of Snake River stream-type Chinook were more variable than those from than most other stock groups used in Pyper et al. (2005). Specifically, Snake River populations showed significantly greater variability in survival rate indices than the Bristol Bay group ($F=3.42$, $p<0.0001$). We plotted the mean and range of the SR residuals for the Bristol Bay sockeye stock group in Figure 1 below (data from R. Peterman and B. Pyper, personal communication). Even within the diverse complex of Bristol Bay sockeye salmon, there are discernable annual survival rate patterns (Figure 1); correlations between sockeye stocks within the Bristol Bay stock group ranged from 0.23 to 0.75 (geometric mean 0.44).

Further, the reviewers' reference to Hilborn et al. (2003) failed to identify that many of the differences within the Bristol Bay sockeye salmon complex were attributed to varying challenges imposed by the different freshwater spawning and rearing environments (e.g., lakes, rivers, and streams). The upriver and downriver stream-type Chinook compared in CSS (and previous SR contrasts) have more similar freshwater life-history characteristics than the Bristol Bay sockeye. The situation in the Columbia River stream-type Chinook SR analyses is that these papers (Schaller et al. 1999; Deriso et al. 2001; Schaller and Petrosky 2007) explicitly compared populations from stream spawning and rearing fish, where we specifically accounted for differences in freshwater carrying capacity and productivity in the SR analysis (given that we have stream specific spawner, age structure, and recruit information). In any case, Bristol Bay sockeye data do not support the implied criticism that variability in ocean survival among groups could create the false impression of systematic differences between groups of sockeye.

The present CSS comparison extends the SR analyses (and provides an independent estimate of differential mortality that does rely on assumptions for a particular recruit/spawner function) by estimating differential mortality based on PIT-tag SARs, and also by examining specific life-

history characteristics which might support alternative hypotheses regarding causes of differential mortality. Our approach is consistent with the recommendations of Hilborn et al. (2003) in that analysis should be applied on a scale where one can estimate stream-specific recruit/spawner ratios and survival rates.

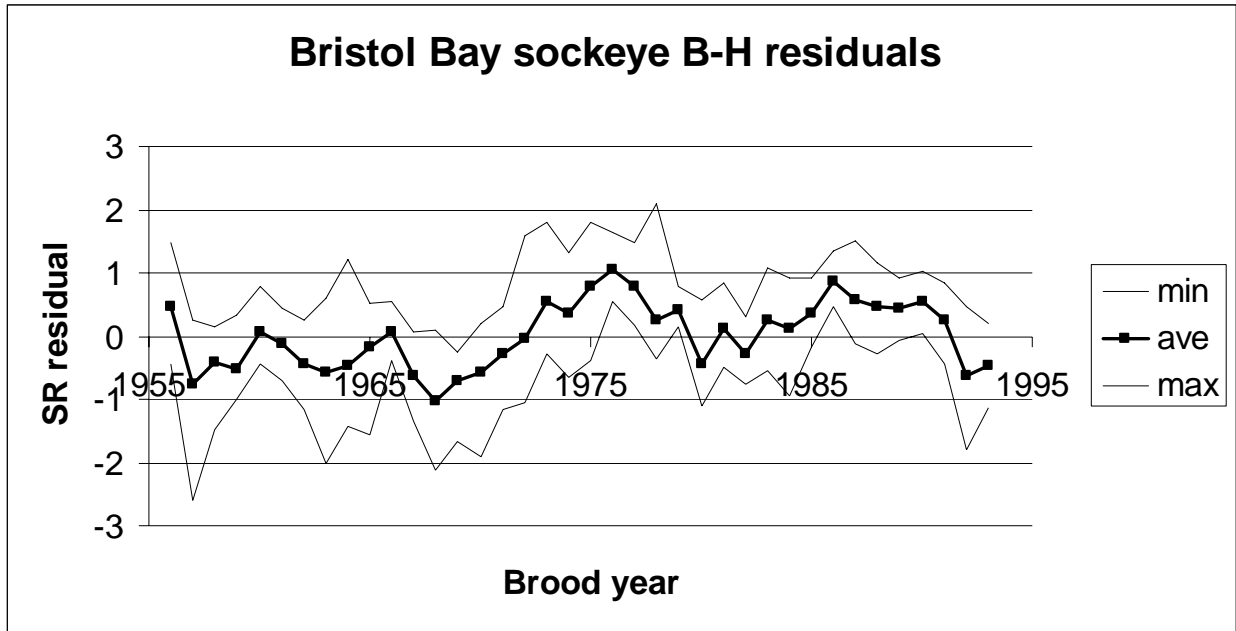


Figure 1. Minimum, mean and maximum annual spawner-recruit residuals for Bristol Bay populations from Pyper et al. 2005 (R. Peterman and B. Pyper, pers. comm.).

Reviewer Comment 9: No clear direction exists to argue for continuing the large releases of hatchery fish for the purposes of ‘comparative’ survival. This is based on: 1) It does not appear that hatchery Chinook salmon provide any useful information related to wild Chinook salmon other than when SARs for hatchery Chinook salmon go way up or way down, proportionately, so do SARs for wild Chinook salmon. This could be determined from a much smaller number of PIT-tagged fish or from adult returns by comparing the clipped to unclipped population. 2) The CSS results indicate that on an annual basis, transportation would benefit hatchery Chinook salmon but not wild Chinook salmon. Since the distribution of hatchery Chinook salmon past lower Granite Dam is much more compressed than that of wild Chinook salmon, it is not clear that even analyses on a temporal basis with hatchery Chinook salmon would provide information on how best to operate the system for wild Chinook salmon. 3) Hatchery Chinook salmon have a wide range in return rates. McCall fish do particularly well, and have a different distribution than Dworshak fish. Which hatchery fish then represent wild fish?

Response: Hatchery Chinook salmon and wild Chinook salmon responded nearly identically to environmental and/or seasonal conditions in terms of their fish travel time, instantaneous mortality rates, and survival rates in the LGR-MCN reach. Thus, hatchery Chinook salmon

provide valuable information on the response of wild Chinook salmon to conditions experienced in the hydrosystem.

Differential mortality between upriver and downriver stream-type Chinook populations has been estimated for wild populations from both spawner-recruit (Schaller et al. 1999; Deriso et al. 2001; Schaller and Petrosky 2007) and PIT-tag SAR (CSS study) data sources. The CSS also investigated whether a similar level of differential mortality was present between PIT-tag SARs for five upriver and one downriver hatchery Chinook populations. Because biological characteristics of a population could differentially influence survival to adult return (see above), we also summarized hatchery pre-smolt FL at the time of tagging, and hatchery smolt arrival timing distributions entering the hydrosystem (LGR or BON) and arriving at the estuary (BON).

Upriver and downriver hatchery spring/summer Chinook SARs did not show the same level of differential mortality as was apparent from the wild populations. Survival of hatchery fish is subject to additional fitness and rearing factors that may not affect wild populations. CSS currently has the ability to compare SARs from a single downriver hatchery (Carson NFH) with those from five Snake River hatcheries. Additional candidate populations relevant to these SAR comparisons from downriver hatcheries of the Interior Columbia include Klickitat, Warm Springs, and Round Butte (depending on fish health constraints). Future monitoring should also consider incorporating PIT-tag SARs from the upper Columbia region to expand these regional comparisons.

Although Snake River hatchery Chinook exhibited a generally more positive response to transportation and relatively lower levels of differential mortality than wild populations, annual SARs of wild and hatchery Snake River Chinook were highly correlated. In view of this high correlation, continuing the CSS time series of hatchery SARs will be important to augment wild Chinook SAR information following future years of low escapements, in addition to providing valuable management information for the specific hatcheries. One advantage of the CSS study is that tagging takes place at the hatcheries and in the tributaries for wild populations. This approach allows for detecting different responses to management actions for different components of the wild and hatchery aggregate groups, unlike approaches that only tag at the upper most dam. Finally, it is of interest to the region of how the specific hatchery groups respond to the hydrosystem management actions. The reviewers suggest a much smaller number of PIT-tagged hatchery fish could be used. We believe that the sample sizes should be periodically reviewed based on updated survival estimates, and regional monitoring and evaluation needs.