COLUMBIA BASIN FISHERY AGENCIES AND TRIBES

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MEMORANDUM

TO: Dean Holecek, USACE
Walla Walla District

Michele DeHart

FROM: Michele DeHart

DATE: February 25, 2015

RE: Review Comments on the USACE-funded research report entitled *Evaluating the Responses of Snake and Columbia River Basin Fall Chinook Salmon to Dam Passage Strategies and Experiences, 2006 and 2008,* (Smith, Marsh, Connor 2014)

In response to your request the Fish Passage Center staff and individual members of the Comparative Survival Study Oversight Committee reviewed the subject research report and provide the following comments for your consideration. The report's Executive Summary describes the approach and objective of this report as an evaluation of hydroelectric project management strategies for fall Chinook juveniles. Our review followed the same approach, concentrating on the applicability of these research results to prevailing fish passage management strategies for juvenile fall Chinook in the Snake and Columbia rivers.

Our overall conclusion, from our review, is that these research analyses and report do not provide a robust basis to support fish passage management decisions for juvenile fall Chinook migrants, either on the very narrow focus of routing fish which pass through the powerhouse or the larger question of hydro system passage management for fall Chinook survival. The following list summarizes our review conclusion points. Following the summarized conclusions is a specific discussion of each point.

I. **The Consensus Study Design does not provide a robust basis to support fish passage management decisions for juvenile fall Chinook migrants. This research is too narrow both in terms of management alternatives considered and the number of years evaluated (only two years of the five available years of research data are included).**

II. The conclusions regarding benefits of transportation seem to be primarily based upon the "surrogate" fish test groups. The assumption that these "surrogate" test groups represent wild/natural groups is doubtful. The "surrogate" test groups are a unique hatchery group. These surrogate hatchery groups are no longer produced and are no longer present in the system. The results from these groups are not applicable to the fall Chinook run-at-large.

III. Significant study design implementation issues raise concern about the applicability of the results to management questions, since the approach does not incorporate or address current knowledge regarding other life-history implications of transportation, including delayed mortality, adult straying, and adult migration success when compared to fall Chinook that migrate in-river (i.e., avoiding powerhouse passage).

IV. The benefit associated with the transportation over bypass subsequent to July 15 is more likely due to the reduction of in-river migration conditions associated with the Biological Opinion. This reduction in protection measures for summer migrants occurs after June 20th and consequently the conclusion that transportation provides a benefit is confounded by the changing in-river conditions and is not supportable. The data indicate the need to continue springtime flow and spill conditions through the fall Chinook migration period.

V. The narrow focus of this research does not provide an adequate basis for passage management decisions because it does not consider potential differential impact on specific parts of the passage distribution. Also, regional production programs are not considered, which could affect passage distribution and timing. Present programs to increase natural/wild production of fall Chinook could increase the number of fish migrating during later parts of the passage distribution more similar to natural/wild timing. Existing later migrating groups, such as Clearwater River groups, could benefit from higher flow and spill, and increased probability of avoiding powerhouse passage. Because the actual benefit of transportation is highly uncertain for later migrants, the survival of the non-powerhouse passage group should be considered in future decisions regarding fall Chinook.

VI. The models that are used to recommend a management strategy of transporting fish after a certain date have too many shortcomings and uncertainties to provide a basis for reliable decision making. These models did not utilize all the available data and instead omitted daily count data below an arbitrary threshold. Also, the uncertainty reported from these models is likely underrepresented due greater variability in the data than what would be expected based on the model. The models, in their current form, cannot provide a reliable management recommendation regarding the benefits of transportation. Concerns were also found with other technical approaches that were used to produce the results of this research.

I. The Consensus Study Design does not provide a robust basis to support fish passage management decisions for juvenile fall Chinook migrants.

The authors suggest that the Consensus Study Design avoids many of the problems associated with other methods that have been used to evaluate transportation benefits, specifically, ones that estimate a Lower Granite equivalent population. The consensus study compares only the survival of bypass collected fish that are released below the project, and bypass collected fish that are transported. In limiting the program to the use of this study design the only management question that can be addressed is whether to change the presently implemented management action that requires all collected fish to be transported. However, even when addressing this question it is important to consider the historical context of hydro system operations for fall Chinook passage. From 1988 through 2005, fall Chinook juvenile transportation was maximized with only turbine passage and transportation available. Maximization of transportation was not effective in mitigating for the development and operation of the hydro system.

- **Significant issues are discussed by the authors that resulted in failure to implement the study design as planned. As a result, analytical "adjustments" were required after the data were collected. This raises serious questions about validity and applicability of results**.

The Consensus Study Design has been shown to have serious implementation issues requiring post-implementation "adjustments." For instance, the authors describe in Appendix A the analytical approach that was used to adjust adult counts in each TWS and BWS category for juveniles that were mistakenly given the wrong treatment (e.g., a BWS fish that was transported). This adjustment is made because the authors recognized the potential bias introduced in the estimate of the TWS:BWS ratio. Thus, while we agree that the problems arising in implementation of this study design potentially bias the TWS:BWS ratio, we are concerned about post-hoc adjustments to the data as included in this report. It is also unclear whether the uncertainty due to these adjustments was incorporated into the bootstrap confidence intervals. The necessity to "adjust" data post-hoc suggests that the study design may not be adequately robust to provide a basis for fish passage management decisions.

- **NOAA should include analyses of data from more recent years, including migration years 2009 through 2011, in order provide adequate information to make better informed management decisions regarding transporting fall Chinook. Conclusions regarding the overall TWS:BWS ratios, (0.96 for production releases in 2006 and 2008) would likely be even lower than reported if more recent years were included. Adult returns are nearly complete for migration years 2010 and 2011 and ratios of transport to in-river adults are unlikely to be affected by the addition of the few remaining adult returns expected in the next few years.**

NOAA states in the concluding sentence of their executive summary that "*These analyses will become more comprehensive and informative as data accumulate from additional study years.*" We agree that it is critical that more years of data be presented prior to using these results to inform management decisions. Based on past patterns in adult return rates, it is reasonable to include results for migration years 2009, 2010 and 2011 in the final report. However, NOAA presents analyses from only two outmigration years (2006 and 2008) of the fall Chinook transportation study. Based on analyses presented in the CSS 2014 Annual Report (McCann et al. 2014), 2006 and 2008 had juvenile survivals that were relatively low and also showed some of the highest transport benefit (see Figure 1). The CSS showed that as in-river survival increased the benefits of transportation decreased. From Figure 1, it is evident that the 2006 and 2008 cohort data points were mostly clustered in the upper left hand quadrant of the plot. Those data points generally had low reach survival estimates (at or below 0.60), and relatively high log TIR values (above zero indicates positive transport benefit). In contrast, the later years of 2010 and 2011, which NOAA has not presented in their report, had the highest reach survivals and many groups had log TIRs below zero. The CSS reported 7 out of 19 fall Chinook cohorts in the years 2010 and 2011 had TIRs significantly below 1 — equivalent to a log TIR less than zero in Figure 1 (McCann et al. 2014). Based on the CSS analysis it appears that because NOAA included only 2006 and 2008, their analysis would likely overestimate the benefit of transportation. It is important that more recent years be added to the analyses in order to account for those years when in-river survivals were higher.

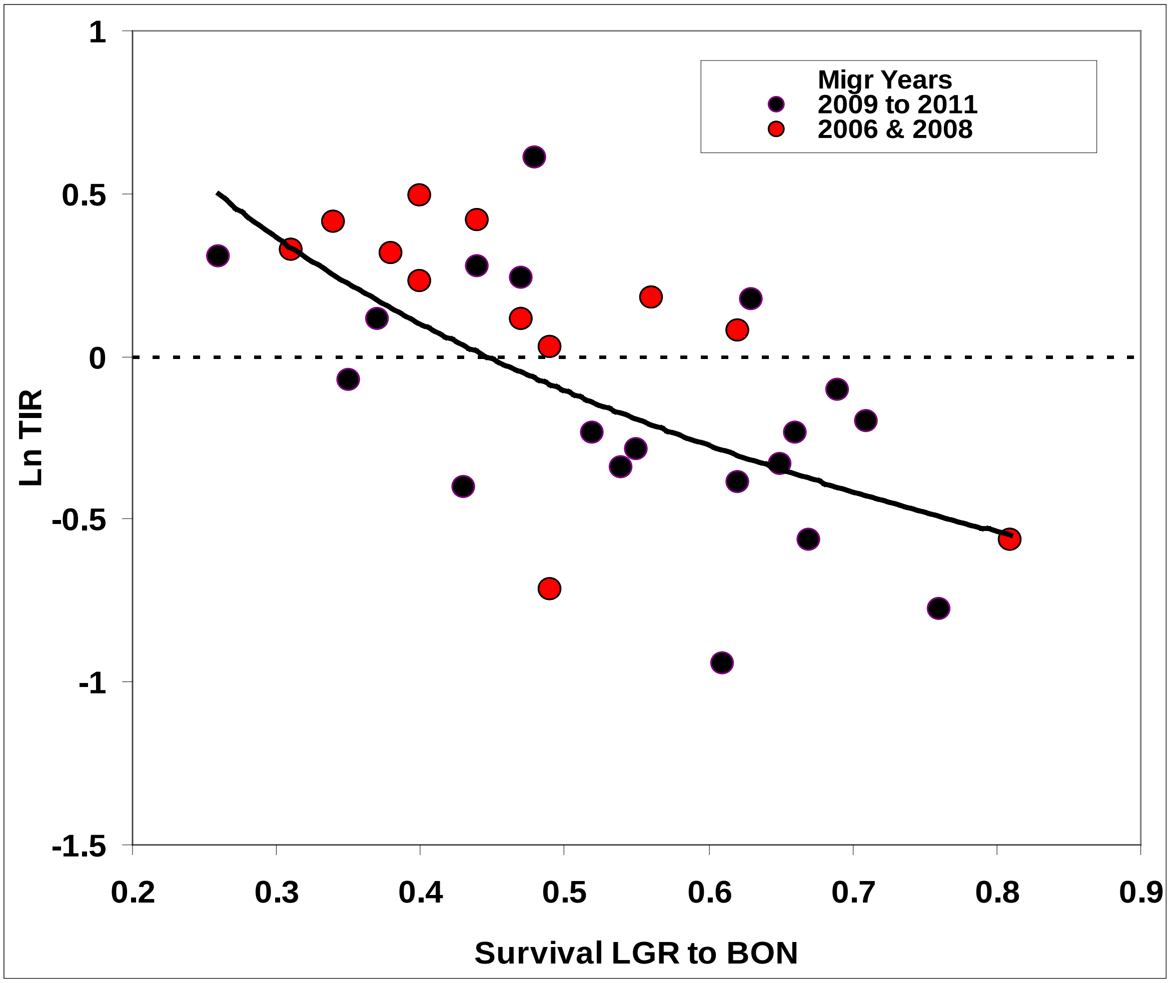

Figure 1. Log Transport In-River ratio plotted versus in-river survival from LGR to BON for the years 2006 to 2011. Plotted line shows the regression Log TIR versus Survival as reported in McCann et al. (2014). Figure based on fall Chinook SAR data from McCann et al. (2014).

Based on a review of ocean adult return data from the transport fall Chinook PIT-tag data, it is likely that over 99% of production fall Chinook adults have returned from the 2011 migration year. Similarly, over 96% of surrogate adults have returned from 2011 as well as at least 89% of wild fall Chinook adults. There appears to be no reason why preliminary analyses of 2011 data could not be included as well as the nearly complete data for 2010. Our analyses of the fall Chinook PIT-tag data showed very low numbers of 4-salt adult returns and only two 5-salt returns. It is likely transport return ratios will not change much with the addition of further adult returns for 2010 and 2011 migration years.

- **The conclusions regarding benefits of transportation by a particular date are not supportable due to very small data sets for the time periods that the report concludes that transportation benefits occur**.

There is a disparity between total sample sizes of bypassed and transported groups early in the outmigration season compared to later in the outmigration season, which potentially confounds interpretation of the results. For example, we compiled total samples of bypassed and transported fish at Lower Granite Dam for 2006 and 2008 surrogate and production Chinook (Figure 2). Generally, by mid-July, daily counts of bypassed and transported fish start to decline and are often below minimum sample size thresholds (i.e., daily count of 5 or fewer fish) for modeling temporal patterns in SARs identified in the Objective 3 section. When counts get below this number, the data are omitted from the analysis. It is problematic to make a recommendation regarding the benefit of transportation during the time of the outmigration when the uncertainty in that recommendation, due to small sample sizes, is the highest.

Biweekly total counts of transported and bypassed surrogate and production fish in 2006 and 2008 at Lower Granite (and other projects) are provided in Appendix C of the report. These tables also show a declining pattern in total sample size later in the migration season. For production fish in both 2006 and 2008, biweekly counts are nearly always less than 10 after the beginning of August. Sample sizes for surrogate subyearlings do not look as limiting. However, these are biweekly counts and the modeling carried out in the Objective 3 section was conducted on daily count data. In actuality, the sample sizes that informed the transportation benefit were much smaller.

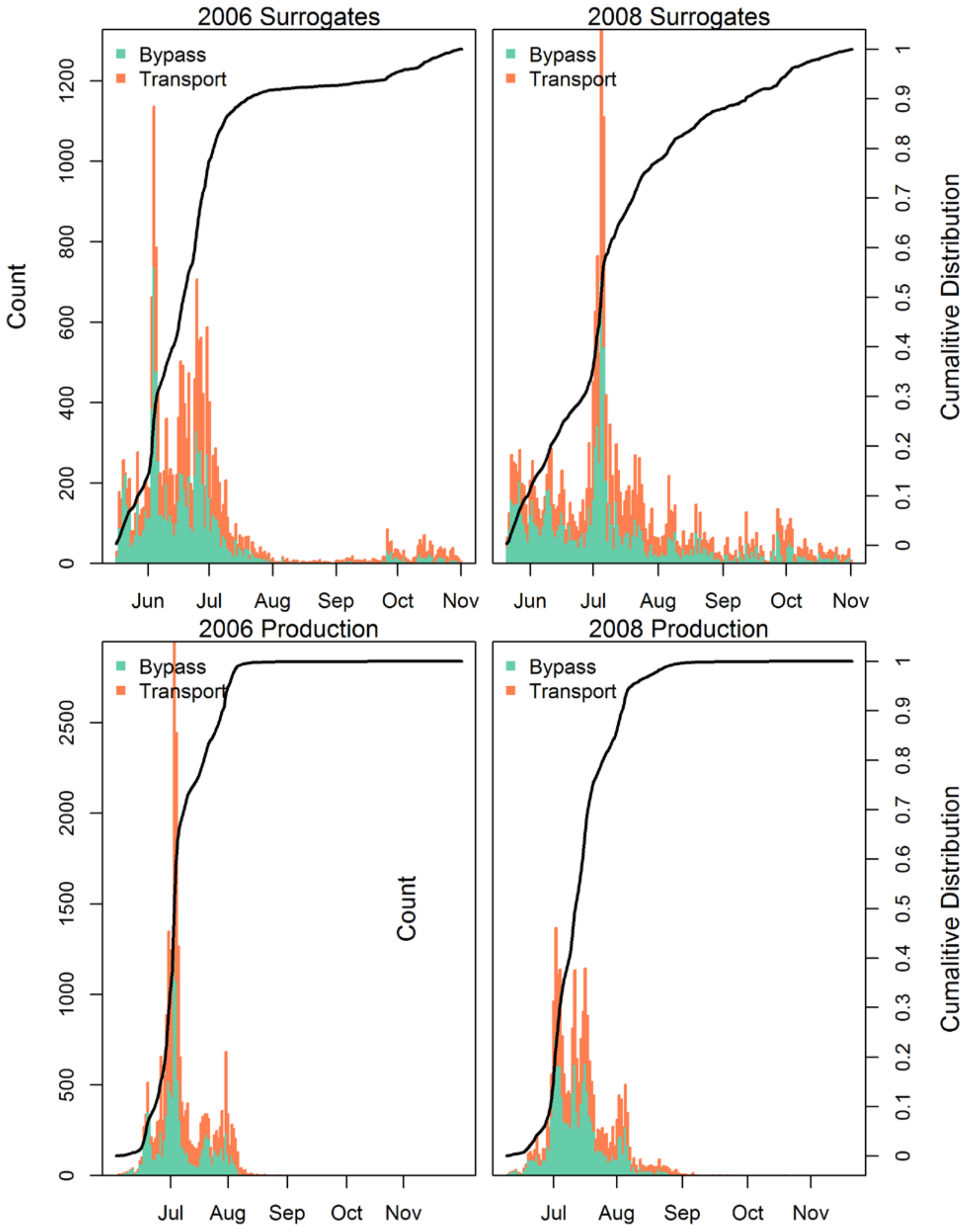

Figure 2. Daily counts of bypassed and transported subyearling and production surrogate fall Chinook at Lower Granite Dam in 2006 and 2008. The cumulative distribution of the arrival time is shown in black.

II. The conclusions regarding benefits of transportation seem to be primarily based upon the "surrogate" fish test groups. The assumption that these "surrogate" test groups represent wild/natural groups is doubtful. The "surrogate" test groups are a unique hatchery group. These surrogate hatchery groups are no longer produced and are no longer present in the system. The results from these groups are not applicable to the fall Chinook run-at-large.

- **Surrogates appear to benefit more from transportation than production fish based on the NOAA analysis. However surrogates have not been demonstrated to be a useful representation of wild fish. A comparison of the percentage of 1-salt adult returns found that surrogates are more similar to production fish than wild fish. This finding differs from previous analyses that focused mostly on juvenile migration characteristics that suggested surrogates were more similar to wild fish.**

NOAA states that "*…performance metrics of tagged surrogate subyearling Chinook released at a particular time and location provided an index of performance for naturally produced subyearling Chinook present at that time and location.*" However, they did not provide sufficient justification for, or explanation of, the usefulness of surrogates as an "index" of wild fish. The use of surrogates to represent wild fish is problematic. Much of the focus comparing surrogates to wild and production fall Chinook has focused on juvenile migration characteristics (Connor et al. 2008, FPC 2008). For this review, we relied on the same similarity index outlined in Connor et al. (2008) to investigate the similarity between surrogates and wild and production fish, in terms of age of adult returns. For migration years 2006 to 2010 (not including 2007), the average percent 1-salt returns were 34% for production fish, 25% for surrogates, and 11% for wild fish. The similarity indices were 1.39 for the production to surrogate comparison and 2.19 for the surrogate to wild comparison. In this case, the higher number indicated less similarity, thus illustrating that surrogates are more similar to production fish with regard to age of adult returns than they are to wild fish.

While Connor et al. (2008) concluded that surrogates are more similar to wild fish than hatchery when comparing juvenile migration characteristics, it does not necessarily mean they are useful as a representative of wild fish. Our analyses of age of adult returns found that surrogates were more similar to production fish in terms of age of adult returns. As such, surrogates neither represent production fish nor wild fish and inferences made from analyses of surrogates should only pertain to this unique group of fish, which are no longer produced.

Regarding juvenile migration characteristics, the FPC provided an analysis in 2008 examining the representativeness of 2005 and 2006 surrogate subyearling fall Chinook to their wild/natural counterparts per the request of the Fish Passage Advisory Committee (FPC 2008). In this memo, the FPC examined a suite of factors characterizing the life-history of the surrogate and wild stocks. Key findings from this memo include:

- Wild fall Chinook populations were captured and marked with PIT tags in the Snake and Clearwater River over 58–92 day periods in 2005 and 2006, whereas surrogate fish were released from hatcheries over periods of 11–20 days in the same years.
- The mean fork length of surrogate fish was usually 4–10 mm (5–15%) larger than their wild counterparts in the same year.

- The mean weight for wild fish was always 0.1 to 1.7 grams (3% to 34%) greater than surrogate fish of similar length (i.e., wild fish had better “condition factors”).
- For Snake River surrogates, the median detection dates at Lower Granite Dam were 4–13 days earlier for wild fish in 2005. In 2006, surrogate and wild fish had a similar arrival times at Lower Granite. In 2005 and 2006, surrogate and wild fish from the Clearwater River also had similar arrival times at Lower Granite Dam.
- Outmigration survival from Lower Granite to Little Goose and from Little Goose to Lower Monument dams was always higher for wild fish compared to surrogate fish. Survival was not estimated for Clearwater stocks due their higher propensity to overwinter.

Details surroundings all of the above findings are in the referenced memo. Before surrogate subyearling Chinook are used to make management decisions, their utility as a tool to represent the wild population needs to be carefully considered.

III. Significant study design implementation issues raise concern about the applicability of the results to management questions since the approach does not incorporate or address current knowledge regarding juvenile passage history, subsequent survival and adult return. The approach does not address the smolt-to-adult survivals of juvenile fall Chinook that migrate in-river (i.e., avoiding powerhouse passage), nor does it include the impact on other life stages. Because the actual benefit of transportation is highly uncertain for later migrants, the survival of the non-powerhouse passage group should be considered in future decisions regarding fall Chinook.

By excluding in-river migrants that avoided powerhouse passage, this study did not incorporate any delayed mortality associated with powerhouse passage, which may artificially inflate the benefits of transportation. Both spring/summer Chinook and steelhead exhibit significant delayed mortality associated with powerhouse passage (Schaller and Petrosky 2007, Petrosky and Schaller 2010, Tuomikoski et al. 2010, Haeseker et al. 2012, FPC Memos May 21, 2009; February 3, 2010; October 5, 2010; October 6, 2010; January 19, 2011; May 22, 2013). To fully determine the impacts of transportation, undetected in-river migrants must be included in the study.

The transportation of juveniles additionally has impacts throughout the life cycle of fall Chinook. As adults, fall Chinook that were transported as juveniles are 10 to 21 times more likely to stray into non-natal territory than fish that migrated in-river (Bond et al. 2015). Straying behavior has a significant negative impact on adult populations, and the exclusion of straying rates in this analysis limits its applicability to any potential management decision. The data presented in this report clearly indicate that fall Chinook juvenile migrants have higher survival and better adult returns with higher flow and spill and that smolt transportation does not mitigate for the adverse impact of hydro system development and operation on fall Chinook.

IV. The reduction in in-river survival shown for bypassed migrants subsequent to July 15 is more likely reflecting the reduction in in-river migration conditions associated with the Biological Opinion after June 20th, rather than a supposed benefit to transportation.

In river juvenile survival is a function of the flow and spill levels that occur in any given year. The Biological Opinion specifies a flow level for spring and summer flows, where the summer flow objective is considerably less than the spring flow objective. For the two years studied (2006 and 2008) the spring flow was at, or considerably greater, than required by the Biological Opinion. This resulted in better than average in-river migration conditions (Table 1).

While there is no biological justification for providing less protection for subyearling fall Chinook migrants, the Biological Opinion allows for a reduction in flow after June 20th. Table 1 shows that average summer flow conditions were considerably less than those experienced prior to June 15th, and in most cases were considerably below the already reduced Biological Opinion flow objectives.

Table 1. Biological Opinion spring and summer flow targets versus actual flows.

2006				
Dam	**Spring Flow Objective**	**Spring Average**	**Summer Flow Objective**	**Summer Average**
Lower Granite	100	125.3	54.5	37.6
McNary	260	325.4	200.0	166.5
2008				
Lower Granite	100	98.7	52.5	57.0
McNary	260	286.7	200.0	172.8

In addition to flow, the following figures (Figure 3 and Figure 4) show that during the study, spring spill was provided at levels in excess of that required by the Biological Opinion. Like flow, the Biological Opinion provides lower spill levels during the summer period at many of the projects in the FCRPS

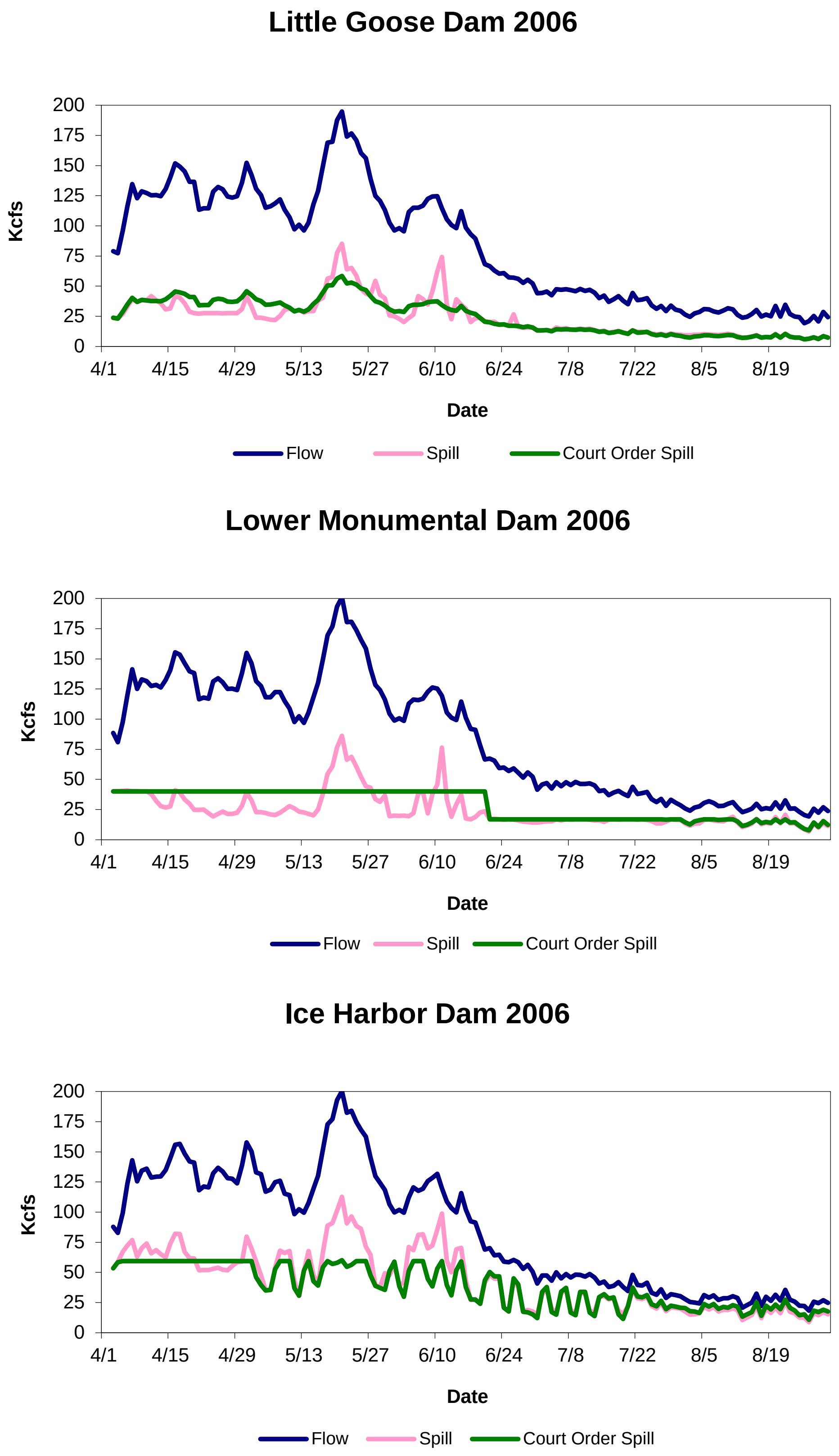

Figure 3. 2006 Snake River flow and spill experienced by transportation study bypass groups.

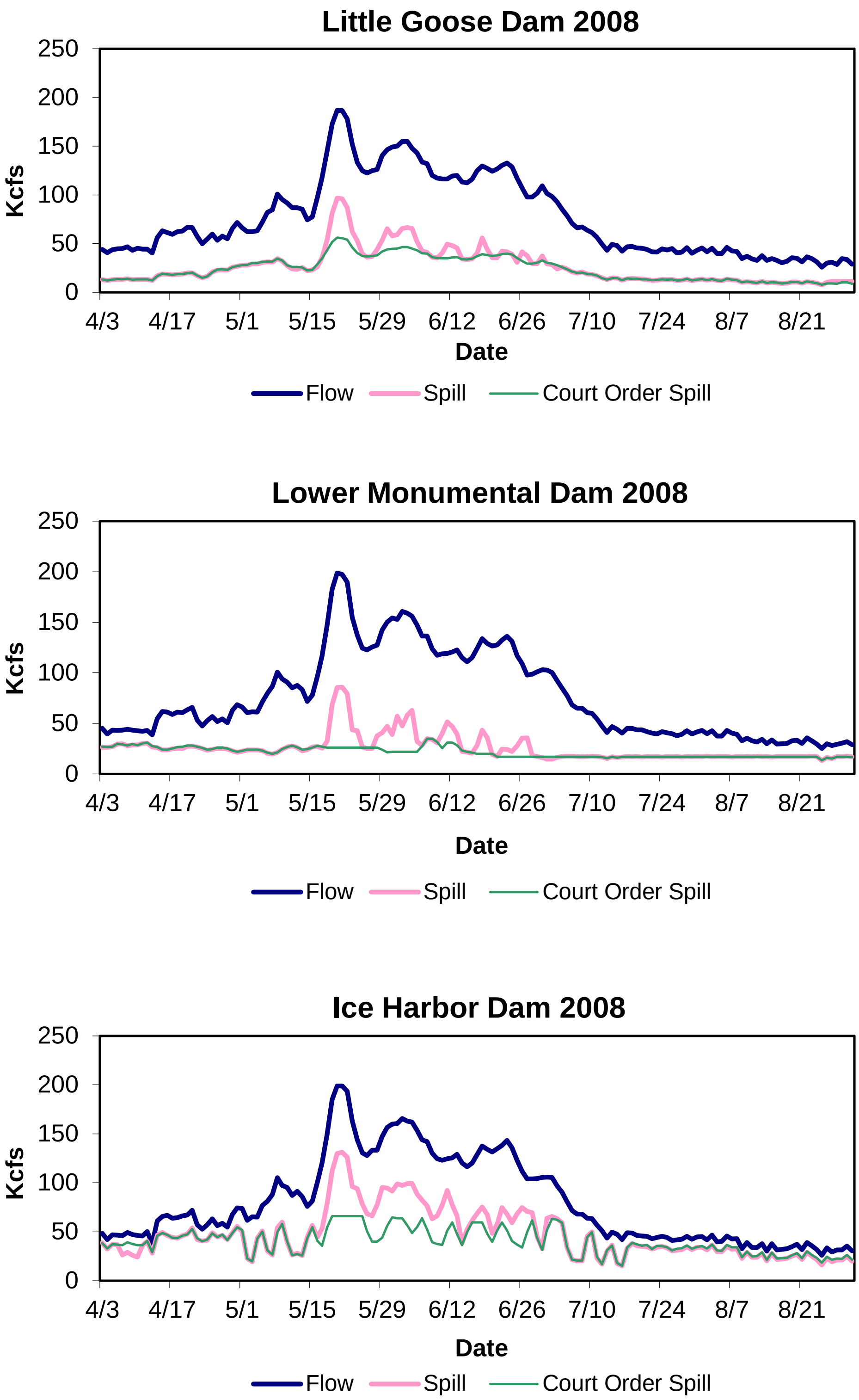

Figure 4. 2008 Snake River flow and spill experienced by transportation study bypass groups.

In addition to the concerns regarding sample size discussed above in Section II, the conclusions regarding the benefits of transportation by a particular date are not supportable due to the comparison of survival with reduced migration conditions. The data are more likely supporting the conclusion that reductions in protection measures based on season and migrating populations are not defensible.

V. The narrow focus of this research does not provide an adequate basis for passage management decisions because it does not consider potential differential impact on specific parts the passage distribution. Also, regional production programs are not considered, which could affect passage distribution and timing. Present programs to increase natural/wild production of fall Chinook could increase the number of fish migrating during later parts of the passage distribution more similar to natural/wild timing. Existing later migrating groups, such as Clearwater River groups, could benefit from higher flow and spill, and increased probability of avoiding powerhouse passage.

Annual run reconstructions on adult fall Chinook returns at Lower Granite Dam have demonstrated that the proportion of natural fall Chinook in the returning adult population at Lower Granite Dam is increasing (Figure 4).

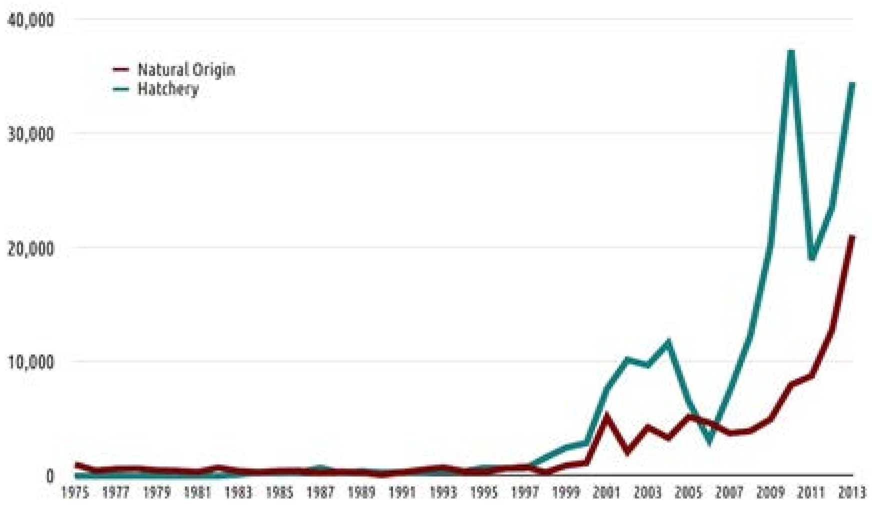

Figure 5. Natural and hatchery returns of Snake River fall Chinook to Lower Granite Dam based on run reconstruction. Figure was taken from http://plan.critfc.org/2013/spirit-of-the-salmon-plan/about-spirit-of-the-salmon/abundance-trends/

The present juvenile timing of naturally migrating juvenile fall Chinook is difficult to assess. However, prior to the introduction of large releases of hatchery fish, the 95% juvenile passage date at Lower Granite Dam, based on the run-at-large, was estimated to be in mid- to late September.

The large releases of hatchery juveniles has since confounded the juvenile migration timing results, as these hatchery fish were released early and in large numbers, overwhelming the natural population, and causing the 95% passage date to be estimated as occurring earlier. The timing of large hatchery releases masks the timing of smaller groups of wild/natural fish.

The timing of natural migrants is a function of temperature units associated with growth and environmental cues. It is very likely that the juvenile timing of fall Chinook could shift later as natural spawners increase.

VI. The models that are used to recommend a management strategy of transporting fish after a certain date have too many shortcomings and uncertainties to provide reliable decision making. These models did not utilize all the available data and instead omitted daily count data below an arbitrary threshold. Also, the uncertainty reported from these models is likely underrepresented due to greater variability in the data than what would be expected based on the model. The models, in their current form, cannot provide a reliable management recommendation regarding the benefits of transportation.

We reviewed the technical aspects of the modeling approach in Objectives 1–3. Of primary concern is the data and modeling approaches utilized in Objective 3, but we also provide specific comments from Objectives 1 and 2. Regarding the Objective 3 section, several choices regarding how the data were organized and what models were applied to those data, had to be made in order to investigate temporal trends in SARs for the T and B groups. Ultimately, these models are used to suggest a date at which the transport to bypass SAR ratio exceeds one. However, before such conclusions are made, the authors should conduct a more thorough examination of how this date might change under different aggregations of the data and different formulations of the models.

- **Technical Comments on Objective 1 Section**

The authors need to further justify the use of the weighting scheme that was utilized in the weighted geometric mean estimates of TWS:BWS ratios across drainages. We suggest assessing how sensitive these estimates are to other weighting schemes which presumably will reflect uncertainty in previous and future abundances.

Page 33. *"To calculate estimated TWS:BWS ratios across drainages within a study year, we used the weighted geometric mean of the individual estimates. For surrogate subyearlings, weights were 70% on the Snake River estimate and 30% on the Clearwater River. These weights were based on the estimated proportion of total natural redds in each drainage."*

Basing the weights for the TWS:BWS estimates across drainages from a single estimate of production based on redd counts, which are prone to be variable and unreliable due to sampling inefficiency, may be problematic. To address this, the authors should, at the very least, examine the sensitivity of the estimate to the 70:30 ratio that was used in the analyses. How would the conclusions change if, instead, the ratio were 60:40, 80:20, 50:50, etc.?

Assuming a 70:30 abundance ratio for the weights used in the geometric mean also assumes that this ratio will be the same in the future. Ultimately, these data are being used to make prospective management recommendations, not retrospective ones and so the uncertainty in future conditions needs to be reflected in this estimate.

Page 33. "*To calculate average estimates of the same quantity across years, we used a simple geometric mean.*"

A weighted geometric mean was used for the drainage-wide estimates, but an unweighted geometric mean was used here. Were the sample sizes roughly equal in 2006 and 2008? If not, is a weighted geometric mean based on the total sample sizes in each year more appropriate? The choice to use an unweighted estimate needs to be justified here or else it seems as if statistics are being picked and chosen opportunistically to demonstrate certain effects.

Page 37. *Table 8*

The number of adult returns in this table and Table 10 are presumably based on the adjustments described in Appendix A. It was never stated, however, if the uncertainty in the TWS:BWS ratio (i.e., 90% bootstrap approach) accounted for this adjustment. The estimate of the misrouted fish (described in Appendix A) is also a statistic with uncertainty, and this uncertainty needs to be incorporated into the confidence interval for the TWS:BWS ratio. Presumably it was, as this would be relatively straightforward to accomplish in a bootstrap routine, but this should be clarified.

- **Technical Comments on Objective 2 Section**

The technical methodology used in Objective 2 is relatively straightforward and so few technical comments are provided. In summary, we suggest reporting survival estimates based on the mark/recapture model of Lowther and Skalski (1998). From this model one can estimate apparent survival as was done in this section, but also the amount of bias in true survival that is due to overwintering fish that survive through the reach the following year. Finally we note that some patterns in SARs are counterintuitive wherein the benefit of transportation appears to increase, rather than decrease, at transportation projects further downstream. An explanation of this effect should be provided.

Page 45. "*For apparent downstream survival estimates, we excluded detections that occurred the year following release (i.e., detections of fish that overwintered in reservoirs), because combining detection data from multiple juvenile migration seasons can cause considerable bias.*"

Utilizing detection data from multiple years will cause considerable bias only if the detection probability the year following release is considerably different from the detection probability the year of release. This changes the interpretation of survival, however. It is correct that only utilizing detection data from the year of release requires that the interpretation of survival be a joint probability of surviving and migrating during the year of release.

It would be informative to report survival estimates based on the multistate model described by Lowther and Skalski (1998). This model is capable of estimating the apparent survival parameter (i.e., the joint probability of surviving and migrating in the year of release), but also the joint probability of overwintering and surviving the reach of interest the following year. The latter parameter is very informative because it indicates the degree of bias in the Cormack-Jolly-Seber reach survival estimate. In addition to reporting apparent survival, it would also be informative to have an indication of the degree of bias due to overwintering fish that survive to the following year.

Page 50. "*For subyearlings that survived and were transported from Lower Monumental or McNary Dam, SAR estimates were generally imprecise. . . .Sample sizes were larger and estimates more precise for fish transported from Lower Granite or Little Goose Dam.*"

The above statement needs to be qualified, especially the latter statement that estimates were more precise for fish transported from Lower Granite or Little Goose dams. For transportation sites further downstream, SARs should increase as this rate then reflects a shorter portion of the life-cycle, but this was not always the case. For example, for 2006 production subyearlings, three of the six SARs reported for individual release groups had smaller SARs for fish transported at Little Goose compared to Lower Granite dam. This seems counterintuitive based on the premise above. While the estimates of SARs from Lower Granite and Little Goose Dam may have been more precise compared to sites further downstream, logically, many of the estimates do not make sense which deserves further explanation.

- **Technical Comments on Objective 3 Section**

Models are fit to data that represent the SAR rate for fish detected in the bypass system on a single day. Using daily SAR rates in the modeling approach is problematic for several reasons, but in particular, because: (1) an unknown amount of data was excluded on days when juvenile counts were less than 5, and (2) modeling daily SAR rates results in too many observations with zero adult counts which in turn can result in biased and unreliable estimates of model parameters. Both of these problems could be avoided by instead modeling weekly or biweekly SARs. Further, the notion of a daily SAR rate in and of itself is obscure as an SAR rate summarizes the cumulative effects that occurred during a population's life-cycle from smolt to adulthood, but those cumulative effects typically operate on a scale greater than a single day.

A candidate set of Poisson log-linear models were chosen to model daily SAR rates across the time in which transportation occurred. This model may be problematic, however, due the excessive number of occurrences when zero adults from a daily cohort of fish return. In short, the features of the data do not match the assumptions of the model (e.g., overdispersion) and consequently parameter estimates and standard errors can be biased. An attempt was made to adjust standard errors for overdispersion, but this attempt is an indirect fix to a problem due to an inadequate model. If daily SAR rates are going to be modeled, then models that directly account for this source of overdispersion, such as zero-inflated models, should be used instead of indirect approaches.

Below we expand on the themes mentioned in the previous two paragraphs and use sections from the text to direct our comments.

Page 60. "*Based on date of juvenile passage, we estimated daily and biweekly SARs for each group. We used biweekly estimates as descriptive statistics, and used the statistical techniques described below to model patterns in daily SARs for T and B groups.*"

It is misleading to use one model to examine trends in the data, and then descriptive statistics based on a courser aggregation of the data to present the uncertainty in that trend. The figures in Appendix C that show the relationship between SAR and date of passage separately for transported and bypassed fish, show a fitted relationship based on the daily SAR data without any confidence intervals for that relationship. Instead biweekly estimates of SARs, along with their associated standard errors, are overlaid on top of the fitted relationship to the daily data. These biweekly standard errors are an inadequate representation of the uncertainty in the fitted relationship to the daily data. The confidence intervals for the fitted relationship should be shown, and if points are overlaid on any graph (i.e., all figures in Objective 3 Section), they should be the data that the model was fit to.

Page 60. "*It is important to emphasize that we determined membership in the T or B group at a particular dam solely by experience at that dam, without regard to subsequent downstream experience.*"

Since the benefits of transportation (or lack thereof) tend to vary from year to year, within the migration season, and for surrogate and production subyearlings, it is unclear whether fish that are bypassed at an upstream dam and subsequently transported at another dam will negatively or positively bias the results. At the very least, we recommend reporting these rates for a better understanding of the magnitude of this potential effect.

Page 62. "*To be relevant to the question of whether or not to transport a fish once it is collected, we needed an unbiased comparison of transport versus bypass SARs for fish that migrated as juveniles only during the period in which the transportation program operated. . . . At Lower Granite Dam, juvenile fish were collected until December 16th in 2006 and December 13th in 2008, but no smolt transportation occurred after November 1st in either year.*"

Based on Figure C1, C3, and C13, it does not appear that fish bypassed after November 1st were removed from the analysis. For instance, the upper panel of Figure C1 shows a fitted relationship between SAR and date for bypassed fish up until December 1st. The fitted relationship between T:B ratio and date (lower panel of Figure C1), shows data only until November 1st as no comparison is possible after this date. However, because the upper panel shows a fit beyond November 1st, it is unclear whether data beyond November 1st was used to fit the relationship for the T:B ratio. Were two separate models, fit with different data, used to produce the upper and lower panels in Figure C1? This would be the correct approach, but to avoid any confusion, we recommend not using any data beyond the cessation of transportation, for both of the fits in the upper and lower panel, to avoid any confusion.

Page 62. “*Regression Analyses of Daily SAR Estimates*”

We would strongly encourage that the Poisson log-linear regression models be fit either to weekly or bi-weekly SAR data, instead of daily SAR data. Fitting these models to daily SAR data will result in an excess number of days with zero adult returns. Serious errors can result in Poisson regression models when there are far more zeros than what would be expected under the assumption of a Poisson distribution. In general, when this occurs, there are typically two consequences. First, the estimated parameters and standard errors may be biased. Second, the excess number of zeros can cause overdispersion. The latter effect was accounted for by fitting quasi-Poisson models which inflate the standard errors of parameter estimates for overdispersion, but this is a post-hoc fix to the problem rather than initially addressing the issue of overdispersion.

For example, using the 2006 subyearling surrogates bypassed or transported at Lower Granite Dam, we found that over 65% of the daily observations had zero adult returns. This larger than expected number (under the assumption of the Poisson distribution), would likely create a large degree of overdispersion. Analyzing the data at a more coarse resolution, such as weekly SARs, would likely help alleviate this problem.

In addition to fitting models to weekly or biweekly SAR data, there are other models which could be explored to alleviate problems associated with excess number of zeros. These include negative binomial linear models, zero-inflated Poisson models and zero-inflated negative binomial models. These models would provide a more direct way to deal with overdispersion and are recommended if models continue to be fit to daily SAR data.

Page 62. “*We omitted from the regression analyses any daily treatment group that had 5 or fewer juveniles (B or T).*”

The choice to omit treatment groups that had 5 or fewer juveniles needs to be justified or explained further. Rather than omitting these data entirely, a more traditional approach would be to pool those treatment groups with the treatment groups occurring the day before or after. This approach was taken in order to calculate descriptive statistics for the bi-weekly periods. Why was it not taken for the analysis of the daily data? Furthermore, more detail needs to be provided on what data was omitted. Were transport treatment groups just as likely to be omitted as bypass treatment groups? Was there a higher tendency to omit treatment groups earlier and later in the migration season?

Page 65. “*For each model we calculated small-sample corrected AIC (AICc) adjusted for extra-Poisson variation (overdispersion) where necessary (QAICc).*”

It would be informative in the results to present the values of c-hat from the global model (i.e., model 8) that were used to correct the models for overdispersion as this would also provide some indication of the goodness-of-fit of the model. It would also be informative to show the QAICc tables in order to gauge which models are more parsimonious than others, or consistently outrank other models.

Page 65. "*An overall average estimated T:B ratio for each date at each dam was calculated as the geometric mean of the available model-average estimate for the date.*"

It appears that the techniques described here are used to recommend a date at which the transportation SAR exceeds the bypass SAR for all years and hatchery types combined (i.e., the conclusions drawn from Figures 3, 5, 7 and 9). If this is the case, then a much more thorough description of the methodology needs to be provided in order to understand the robustness of the conclusions drawn from these figures. If understood correctly, it is statistically unorthodox, and technically incorrect, to take the average over several already model-averaged estimates of 1 to 4 T:B ratio estimates. This is problematic for many reasons, two of the biggest being: (1) the uncertainty in the average response is underrepresented because the uncertainty in the model-average estimate is not accounted for, and (2) certain data points might not be weighted correctly if the amount of data that formed one model average estimates was much greater than another. If the goal in this part of the analysis is to suggest a date at which the T:B ratio at a particular dam from all 4 datasets (i.e., 2006 and 2008 surrogate and production datasets) differs from 1, then a single model, or a candidate set of models which will be model averaged, should be fit to all of the four datasets combined. Averaging over already model-averaged estimates is not the correct approach.

It is somewhat surprising that the lines of the geometric mean of model-averaged T:B ratio estimates are so smooth in Figures 3, 5, 7 and 9. However, lines should not be drawn in this case because they imply a mathematical relationship which is not presented and, therefore, is be misleading. Since an average of 1 to 4 points is being taken here, points should be drawn instead of lines to make this clearer.

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