FISH PASSAGE CENTER

1827 NE 44th Ave., Suite 240, Portland, OR 97213
Phone: (503) 230-4099 Fax: (503) 230-7559
http://www.fpc.org/
e-mail us at fpcstaff@fpc.org

MEMORANDUM

TO: Rick Kruger, ODFW

FROM: Michele DeHart

DATE: March 24, 2011

RE: Review 2011 Acoustic Tag study design for John Day Dam

You have requested that the FPC review the draft 2011 Acoustic Tag study design planned for 2011 at John Day Dam prior to a discussion scheduled for March 24. As of March 23, 2011, the complete study design for the 2011 test at John Day was not available for review. The COE has advised us that the 2011 study will essentially be the same as the 2010 study design posted on the COE website, with some modifications to evaluate the performance standard at John Day at the 30% spill level and at the 40% spill level. However, the COE has advised that they will not test or compare the 30% spill level with the 40% spill level. Because the John Day study design will be discussed tomorrow, March 24, and the complete design is not available for review, the FPC staff reviewed the 2010 Acoustic Tag study design and the outline of changes provided by the COE.

The study design for performance standard testing involves extremely complicated logistics, extensive fish handling and transportation. The design is fraught with a myriad of assumptions, which introduce potential biases. Quoting page 43, of the August 2010 study design, stage 3, covering capture histories to survival estimates, the proposal states, ***"However, before the analysis can proceed, an analyst must first select fish releases and survival arrays to be used. This will require an evaluation of differences in survival among release groups and sets of arrays to identify a set that does not violate, violates the fewest, or minimizes the extent of violations of model assumptions."*** As has been demonstrated in the FPC analysis of the 2008 results, the process by which the analyst "selects" fish releases and survival arrays can have a significant impact on the results (see FPC memo from February 16, 2011). Our overall

conclusion is that the study design incorporates such a wide range of assumptions and subjectivity and is based upon so few fish that it cannot, alone, support the magnitude of the management decision, which is to determine future project operations for fish passage. The summary conclusions from our review are listed, followed by a detailed discussion of each point. We have attached our previous comments regarding this study design for reference.

- The study design is dependent upon a high degree of subjectivity on the part of the "analyst". This raises a high level of concern since there is no public acoustic tag data base and the criteria for determinations by the "analyst" are not identified or documented and cannot be reviewed. These undocumented subjective decisions by the "analyst" can determine the results of whether or not the performance standard is met. In the following discussion we explore the 2008 John Day data and results as an example of this concern.
- The study and results are presented without a Decision Framework, so there is no context provided for management application of results that accounts for study assumptions. There is no context provided for consideration of non-acoustic data and other results. The performance standard consideration should include the evidence for delayed mortality associated with project powerhouse and juvenile bypass passage.
- Acoustic tag data may have the most appropriate application in basic telemetry, however the myriad of assumptions incorporated into this design raises concern regarding estimation of survival as proposed. The study design requires a large degree of subjectivity raising doubt about the researcher's contention in the study design, that the survival data are "bullet proof".

The study design describes a process in which study results are based upon a high degree of subjectivity by the analyst, by the process of including or excluding data from the survival estimation.

The Fish Passage Center provided comments on the John Day acoustic studies provided to date,(FPC.org, memo 2/16/2011). In preparation of those comments the FPC reviewed the 2008 acoustic study report at John Day Dam. Utilizing the data in the report and data provided by the Corps of Engineers, the FPC was unable to reproduce the results reported by PNNL, that juvenile fish survival was higher for the 30% spill level than the 40% spill level. In fact utilizing the data in the report, the FPC results were opposite of the PNNL conclusions, and concluded that the 40% spill level resulted in higher juvenile survival. In response PNNL provided an analysis which included 2009 and 2010 data. PNNL reported that they could reproduce the FPC results. In reviewing the PNNL response, the difference between the PNNL results and the FPC results was due to the elimination of some data, and inclusion of other data in the analyses by a subjective, good, bad, mostly good determination of data by PNNL. The 2008 report did not include a description of criteria for including or excluding data from the analyses. This is an example of how subjective determinations by the "analyst" can determine results. The elimination and/or inclusion of good, bad, and mostly good data in 2008 was due to the difficulty experienced in maintaining the study conditions, due to high flows during the study period. Given predicted flow and runoff conditions predicted for 2011, it is likely that similar problems may occur when attempting to maintain the study conditions that are outlined for a performance test at 40% spill and a performance test at 30% spill. This will introduce another opportunity for subjective determinations of inclusion or exclusion of data from survival estimation which will affect study results.

Acoustic tag data are most appropriately applied to basic telemetry, however the myriad of assumptions incorporated into this design raises concern regarding the estimation of survival based upon acoustic tagging as proposed.

The Skalski 3-dam design (Figure 5) is intended to provide unbiased estimates provided the model assumptions are met, and that is the difficult part of this design considering the extensive assumptions that must be met. There could be an interaction between handling effects and release locations whereby if the upper release site of a paired release (e.g., release sites R2, R4 and R6) has a greater decrease in survival due to handling impacts relative to the lower release site (e.g., release sites R3, R5, and R7) then the resulting estimated dam survival rate would be biased high. On the other hand, if the lower release site of a pair had the greater decrease in survival due to handling, then the resulting estimated dam survival rate would be biased low. The researchers attempt to control for this potential impact by implanting acoustic tags randomly across release groups and establishing a constant transportation time of 0.85 hrs for the R2-R3 pair and R4-R5 pair, and 2.25 hours for the R6-R7 pair due the longer distance to those two sites below Bonneville Dam. However, by implementing a constant transportation time within a pair, the researchers sacrifice the ability to get complete mixing of tagged fish between the two releases of a given pair (Table 4 shows the harmonic mean travel times of fish from JDA to each release site). The researchers claim on page 43 that "minor violations of assumptions like mixing may be inconsequential to the validity of estimates." This is because they are pooling all release over time, both day and night, to create a single seasonal estimated dam passage survival rate. If that were the extent of the analysis, then their claim may be justified. However, if there is a randomized block design with two different spill percentages superimposed over this study, as occurred in the past 3 years, then a lack of adequate mixing should not be viewed as inconsequential to the estimation of separate dam passage survival rates, performance standards, for the two spill levels.

The section of the proposal describing the handling of the raw acoustic data and the filtering process to distinguish fish from ambient noise highlighted that it is not a straightforward task to create a data set of "true" fish detections. Even with the researchers detailed QA/QC process, aimed at making the data "bullet-proof" as they claim, it appears that there remains a fair amount of subjectivity determining what is a valid fish detection. In particular, the following description of what needs to be done before survival estimation can take place should raise doubts regarding the reliability of estimates generated with these acoustic data. Quoting page 43 stage 3 covering capture histories to survival estimates, the proposal states "However, before the analysis can proceed, an analyst must first select fish releases and survival arrays to be used. This will require an evaluation of differences in survival among release groups and sets of arrays to identify a set that does not violate, violates the fewest, or minimizes the extent of violations of model assumptions." This is a post-hoc way of setting up the groups of study fish. The researchers go on to give an example, but state their example incorrectly. They state that "if the survival of the most upstream releases of fish was lower than that of fish release further downstream, the upstream release probably should not be used for the analysis." However, the most upstream releases need to have a lower survival rate than releases further downstream, otherwise $S_{Dam} = S_1/(S_2/S_3)$ would result in estimates >1.

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R_2 release may be biased low if predator density in tailrace is high relative to release location R_3 and post release disorientation leads to increased predation vulnerability relative to V_1 and/or R_3.

The dual release of control fish (R_3) at a point well below the tailrace of the dam is not likely to control for all bias related to handling. In fact the point of release for R_2 at the end of the tailrace is likely an area of high predator density. Both the R_3 and R_2 release groups would have some level of post-release disorientation. However, if predator density is higher in the tailrace than mid-reservoir and R_2 has higher post-release mortality due to handling/disorientation and subsequent predation than R_3, survivals for virtual release groups would be overestimated at the project. Since John Day Dam survival is calculated as:

$$S_{dam} = \frac{S_1}{\frac{S_2}{S_3}} = \frac{S_1 \bullet S_3}{S_2}.$$

See proposal entitled "Integrated Survival Studies at John Day, The Dalles, and Bonneville Dams" for details. It is critical that S_2 not be underestimated due to post-release mortality for R_2 that other groups do not experience in the reach (i.e. releases V_1 and R_3). If post-release morality for R_2 is different from V_1 and R_3, the estimate for S_1 survival would be bias upward.

Assumption 8 that all releases, V_1, R_2, and R_3 experience the same survival probabilities in the lower river segments they share in common could easily be violated. This is because the V_1 release is not likely to have the same survival probability in the tailrace of the dam as R_2 because the virtual release group will not be recently released. Two control releases are supposed to account for handling effects by dividing S_3 into S_2 (see above). However any mortality related to increased predation vulnerability of newly released fish at R_2 will not be accounted for and will make survival for the control fish in R_2 lower thus biasing the V_1 survival higher.

Research on the distribution and abundance of predators in the reservoirs demonstrated that the area where releases occur at John Day Dam and The Dalles Dam are likely to contain greater numbers of predators than the reservoir (Poe and Rieman 1988). Given the presence of greater numbers of predators, it is possible that releases in the tailwater may be subject to increased predation during a brief period after release than those released in mid-reservoir.

Tagging/handling effects

A primary goal of the Lower Columbia River Survival Study for 2011 is to measure the overall survival of fish that pass in several routes of passage through Columbia River dams (Carlson 2010). This approach has several explicit assumptions as outlined in Skalski 2009. Two implicit assumptions not mentioned in model assumptions A1-A10 (Skalski 2009) is that tagging/handling effects are expected to cancel out or be expressed before survival is measured.

These two assumptions are: (i) the handling/tagging effects for release R_1 is expected to be zero by the time these fish are re-counted as virtual release 1 (V_1); (ii) the handling/tagging effects on R_2 and R_3 are expected to be equal and cancel out in the calculation of dam survival (S_{dam}).

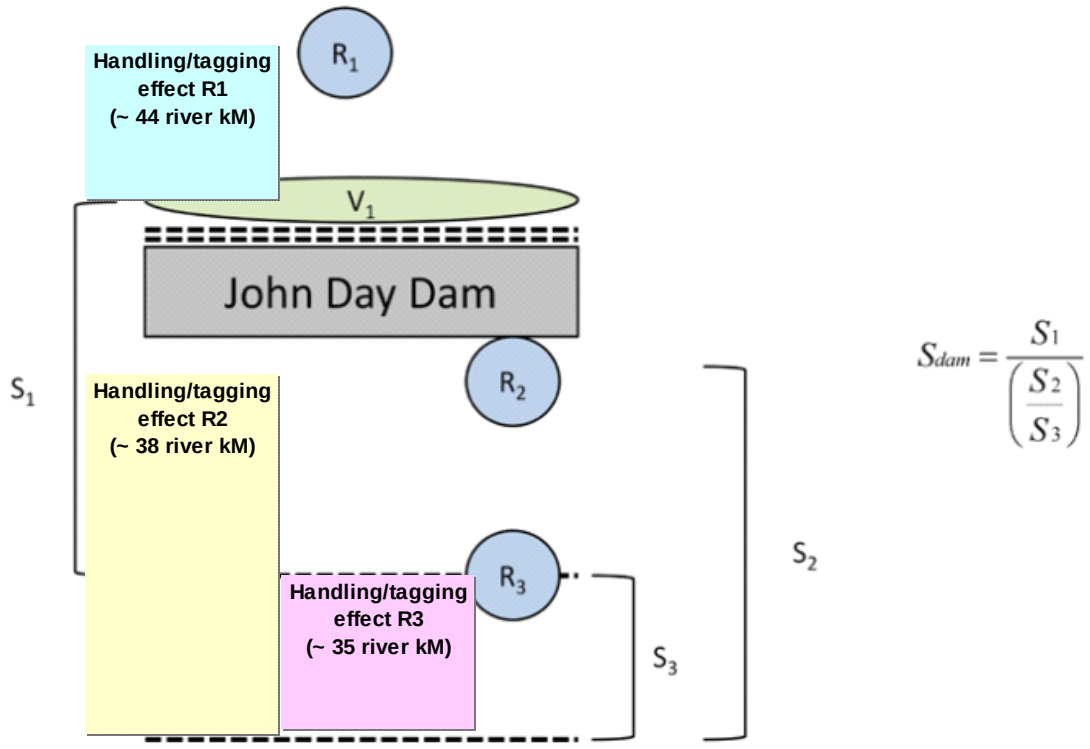

Figure 1. Modified from Figure 2 (Anon. 2011). Schematic of the Virtual-Paired Release model for a single dam. Shaded areas of potential handling/tagging effect added by Fish Passage Center.

The handling/tagging effects on R_2 may be more than those of R_3 for two reasons. First, the approximate river kilometers where handling/tagging effects could be expressed are greater for R_2 than R_3. The amount of handling/tagging bias, and duration over which it is expressed is unknown. However, since the unadjusted survival of R_2 is measured over a longer reach than for R_3, this could bias the survival for R_2 low. Second, there is no accounting for any interaction between tagging/handling effect and where the release takes place. One working hypothesis could be that handling/tagging effects have a more negative effect on the subsequent survival in an area that ostensibly contains high predator numbers (e.g. tailrace of a dam) than for downstream releases. Both of the above potential negative biases of S_2 would cause the measurement of S_{dam} to be biased high. This relationship can be shown with a simplification of the equation shown in Figure 1:

$$\uparrow S_{dam} = \frac{S_1 * S_3}{\downarrow S_2}$$

Selection of Marked Individuals

Statistical study designs often employ some sort of randomized selection; in this way the sampled population is assumed to be representative of the population of interest. This is a principle of experimental design and avoids bias (Williams et al. 2002; assumption A1 in Skalski 2009). In this case, the tagged fish are expected to represent the run-at-large. According to Carlson 2010, fish can be rejected for the study for several reasons including: inhibitory malformations, descaling, physical injuries to impede performance, evidence of parasites, evidence of BKD, evidence of cold water disease, or trematodes. This approach does not measure survival for the run-at-large but measures survival for the portion of the run-at-large that does not show evidence of these various maladies. Although tagging a fish that appears unhealthy and may not survive through the length of the study seems wasteful, if that fish is a representative of the run at large then it must be tagged. Otherwise, inferences made from the sampled population are false. Removing unhealthy fish from the study effectively "high-grades" the sampled population and biases survival estimates high.

Processing the Data for Individual Treatment Blocks

According to (Carlson 2010) the number of fish released for each treatment block will be approximately 50 fish (R_1, R_2, and R_3 for each block). The process for filtering and managing data for the datasets used in this study is also roughly outlined in (Carlson 2010). Often detection probabilities for these JSAT tagged fish are near 100%. So, with 50 fish released within each treatment block, a miscounted fish (either one extra or one less) would bias that block's unadjusted survival estimate by +/- 2%. Because each fish accounts for a 2% magnitude in survival within each treatment block, detailed examples are needed to clearly explain the data processing steps.

Representation of the run-at-large by acoustic tagged mark groups
Although the management objective of the acoustic tag study evaluation of project performance standards is determining the project operation and spill level for the run-at-large, there is no discussion of comparison of acoustic tag reach survivals with PIT tag reach survivals. The degree to which the acoustic tagged fish in this study accurately represent the experience of the run-at-large is a key assumption in the study design but the validity of the assumption is not addressed or tested. This is a key concern in determining the management application of the results.

The transition period between adjacent blocks of 40% and 30% spill could confound the determination of whether or not performance standards are met at each spill level.
Tagged fish passing through the forebay, project and tailrace during periods of transition between spill blocks will be affected by the spill condition in the adjacent block and this could affect the evaluation of survival, because the number of fish in each block is small. The transition period is not addressed in the study design, potentially affecting the ability to evaluate the performance standard separately at the two separate spill levels, accurately.

References

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