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MEMORANDUM

TO: Andrew Murdoch

Michele DeHart

FROM: Michele DeHart

DATE: September 19, 2018

RE: Review of Peven et al. (2005) “Guidelines and suggested protocols for conducting, analyzing, and reporting juvenile Salmonid survival studies in the Columbia River Basin.”

At your request, the Fish Passage Center staff has reviewed Peven et al. (2005) “Guidelines and suggested protocols for conducting, analyzing, and reporting juvenile Salmonid survival studies in the Columbia River Basin.” This report was intended to provide guidelines to researchers conducing survival estimates of out-migrating juvenile salmonids through hydropower projects. It includes an outline of various experimental designs, sampling techniques, and suggestions for standardizing across studies.

Survival studies are conducted to examine the impact of hydropower and changing operations on juvenile survival. Minimum survival estimates are determined by Habitat Conservation Plans (HCP) in Douglas and Chelan Counties, the Salmon and Steelhead Settlement Agreement in Grant County, and the Biological Opinion in the Federal Columbia River Power System (FCRPS). In the FCRPS, juvenile survivals of 96% are required for yearling Chinook and steelhead and 93% for subyearling Chinook. The experimental designs described in Peven et al. (2005) are intended to measure survival through only the dam ("concrete survival") or for a reach that may include the dam forebay or some distance upstream or downstream ("reach survival").

Despite being used for management decisions regarding dam operations, these types of survival studies cannot measure the impact of dam passage on juvenile populations in their entirety. Passage through turbines or juvenile bypass systems during the freshwater outmigration, in contrast to passage through the spillways, has been shown to reduce survival through the estuary and in the ocean. This mortality and impact to populations cannot be measured using dam survival estimates over short reaches. When determining the impacts of hydropower and changes in operations on salmonid populations, impacts on the entire life cycle should be considered.

We have previously made comments on the experimental designs in Peven et al. (2005) (Appendix A). Acoustic tagging studies are inadequate to represent the run-at-large, meaning that results rarely represent the outmigration experience of the whole population. Control groups released in the tailrace of the dam artificially inflate survival estimates of the dam passage groups. The power of studies attempting to compare two different operations has often been insufficient to provide a statistical comparison. Below is an overview of our concerns, followed by more detail.

- Dam-specific survival studies are designed to measure mortality that occurs at the dam, and cannot be used to assess mortality due to passage that occurs downstream of the project, in the estuary, or in the ocean. However, passage through turbines or juvenile bypass systems during the freshwater outmigration significantly reduces smolt-to-adult returns (SARs), while smolts that pass through the spillway have higher SARs. Survival estimates generated by these tests are misleading because they do not incorporate total mortality due to dam passage and do not include all data regarding salmon life-cycle survival.

- Acoustic tags, the method most commonly used from Peven et al. (2005), cannot represent the run-at-large due to tagging limitations.
    - The tag burden imposed by acoustic tags limits the minimum size of tagged fish and eliminates juveniles with conditions such as descaling. In some studies, this has caused significant portions of the run-at-large to be eliminated from the study.

    Estimated survivals from these studies represent survivals only of the largest and healthiest portions of the run.

  - The sample sizes required by acoustic tag studies mean that the tail ends of the run do not provide sufficient numbers of juveniles to be included in the study. This means that early or late migrating runs, often representing unique characteristics, are not included in survival studies.

- Paired release and virtual-paired release survival studies have the potential to significantly inflate the survival estimate of dam passage groups. If predation or other sources of mortality are higher in the tailrace than other locations, the dam survival estimate will be increased far beyond the single-release estimate.

- Peven et al. (2005) provides guidelines on assigning surrogate species to tag when the species of interest is not available due to concern about impact to species with low numbers. However, changes have been made to operations without testing species of interest and without justification using the guidelines provided. One example is changes to operations at Wanapum and Priest Rapids Dams, although survival goals had not been met for steelhead and not tested for subyearling Chinook.

- Use of Peven et al. (2005) to design survival studies is intended to standardize survival testing between sites and years. However, the document does not include guidelines for the inclusion of varying water years or an explanation of why survivals measured under extreme conditions or operations should not inform general operations. An example of this is the use of survival results from Wanapum Dam in 2014, a year when structural damage to the dam face resulted in a significant draw-down of the reservoir. Passage routes and survivals during these conditions will not reflect those measured in an average years.

- Numerous survival tests have attempted to compare routes of passage and survivals under different operations within a season. These comparisons require large samples sizes to achieve an appropriate study power to test for differences at a biologically significant level. Without an established level of significance established prior to the start of the study and adequate sample size, the study will incorrectly determine that the operations being compared have no impact on survival. Peven et al. (2005) does not provide any guidelines for planning for these kinds of comparisons.

Survival studies do not incorporate delayed mortality associated with powerhouse passage

Although dam-specific survival estimates generate a series of easily testable checkmarks for compliance with regards to survival from one side of a dam to the other, they do not measure the myriad of impacts that dam operations have on juvenile salmonid survival. Although some bypass systems have route-specific survivals similar to spillways, juveniles that pass through juvenile bypass systems are less likely to survive in the estuary and the first ocean year, and are less likely to return as adults, than those that pass undetected through the hydrosystem (Haeseker et al. 2012, Petrosky and Schaller 2010, Tuomikoski et al. 2010, FPC Memos Oct 6 2010, Jan 19 2011, July 14 2011). None of the experimental designs provided in Peven et al. (2005) (single release-recapture, paired release-recapture, and variations) can incorporate dam-related mortality beyond the tailrace.

To determine the impacts of dam passage and operations on salmonid populations, more than survival estimates generated by the experimental designs outlined in Peven et al. (2005) will be necessary. Management decisions should be made with a decision making framework that incorporates the impacts through the entire life-cycle (FPC memos June 24 2009, July 29 2010, March 24 2011, Feb 15 2012, March 16 2012, March 23 2012, Jan 4 2013, May 22 2013, July 11 2013, Aug 2 2013, Dec 3 2013, May 2 2014, Nov 9 2016).

Acoustic tag studies do not represent the run-at-large

A key requirement of tagging studies is that tagged individuals should be representative of the untagged population of interest. If significant differences exist between the tagged and untagged populations, then inferences must be limited to tagged individuals themselves and are not applicable to the run-at-large. Acoustic tag studies impose a relatively large tag burden when compared to PIT-tags, and juveniles are rejected from tagging due to small size or injury. When these rejection rates are high, the study will represent survivals only for the largest, healthiest fish and the results must be interpreted with extreme caution (FPC Memos June 24 2009, March 24 2011, Feb 15 2012, March 23 2012, Jan 4 2013, Feb 11 2013, March 19 2013, May 22 2013, July 11 2013, Aug 2 2013, Dec 3 2013, Jan 14 2014, May 2 2014, Feb 3 2015, Nov 9 2016). In acoustic studies conducted within the FCRPS, rejection rates have ranged from 3.7% to 18.0%. Unfortunately, survival studies conducted by Grant and Chelan PUDs in the Upper Columbia have not reported the tagging rejection rates, so the applicability of survival estimates are not informative with regards to population-level metrics of dam passage (FPC Memos July 11 2013, Aug 2 2013).

Project survival estimates are often artificially inflated due to controls

Peven et al. (2005) includes outlines of two study designs, the single-release estimate and the paired-release recapture estimate, which includes several variations. Paired estimates utilize one or two control groups, released in the tailrace and further downstream. With one dam-passage group (R1) and one control group released into the tailrace (R2), the survival of R1/R2 is used as the adjusted survival estimate for dam passage. This design requires all mortality not associated with the dam, from sources such as handling, tagging, or predation, to be equal between the two groups. However, because tailrace mortality is often higher than in other river reaches due to predation (Petersen 1994, Ward et al. 1995), survival in the R2 is depressed, increasing the ratio of R1/R2 and giving the appearance of higher dam passage survival than observed in the single-release estimate (FPC Memos July 11 2013, Aug 2 2013).

In a variation on the paired-release experimental design, some studies have used a second control group below the tailrace (R3). This release group is intended to control for handling effects expressed in the R2 group, and a ratio of survivals of R2/R3 is used as the control group survival. Dam passage survival is calculated as R1/(R2/R3). However, as discussed above, the tailrace group can experience higher predation, further inflating dam survival estimates (Beeman et al. 2011, FPC Memos March 24 2011, Feb 15 2012, March 23 2012, Jan 4 2013, Feb 11 2013, March 22 2013, May 22 2013, Dec 3 2013, Feb 3 2015). This was most clearly shown in survival studies conducted at McNary Dam in 2012. As seen in Table 1, survival estimates generated using control groups were significantly higher than the single-release estimates, and were unlikely to reflect true dam passage conditions.

Table 1: Survival estimates at McNary Dam in 2012, using the single release and virtual paired-release methods (Hughes et al. 2013).

	Single-Release Survival	Paired-Release Survival
Yearling Chinook	91.7%	96.2%
Steelhead	91.4%	100.1%
Subyearling Chinook	91.5%	97.5%

To study survival through a single project, we recommend the use of single-release estimates paired with testing for dead fish detections, as described in Peven et al. (2005). A potential confounding factor in single-release estimates is that fish die during dam passage but are detected in downstream arrays, thereby inflating survival estimates. By tagging dead fish and testing for detections in the tailrace, this factor can be controlled for. In contrast, paired-release estimates have significant potential to artificially inflate dam-survival estimates.

Non-representative water years and operations do not provide information for other passage conditions

Survival studies are used to test operations and inform management decisions. However, the results can only be applicable to years and operations that are similar to test conditions. If studies are conducted during abnormal water years, or during operations not used under normal conditions, the results cannot be applied to decision for conditions differing from test conditions. With changing spill proportions due to operations or extreme flows, the proportion of juveniles moving through each route of passage will vary, changing overall dam survivals (FPC Memos Feb 15 2012, Jan 4 2013, Feb 11 2013, March 22 2013, Dec 3 2013, May 2 2014, Feb 3 2015, June 22 2015, Nov 9 2016).

Survival studies conducted in the FCRPS follow outlines from the Federal Columbia River Power System Juvenile Dam Passage Performance Standards and Metrics (2012) document, recognizing the importance of testing during representative water years when interpreting survival results, states:

> "the AAs with NOAA Fisheries concurrence, may elect to accept the highest actual spill level minus 5 percent . . . or an average of the two actual spill levels

> under which the two successful tests were conducted as the new target spill level to attain juvenile performance standards. Alternatively, to be determined on a case by case basis, the AAs with NOAA Fisheries concurrence may elect to either conduct additional testing at the original target spill level or adjust future target spill levels by the amount exceeding the acceptable variance as stated above."

However, Peven et al. (2005) does not provide any guidelines on using the results of survival studies conducted in years when operations and/or flows do not represent the general conditions of dam passage. In 2014, a survival study was conducted at Wanapum Dam as a "continued assessment of juvenile steelhead" and a re-evaluation of yearling Chinook, which had met survival standards in 2003 – 2005 (Hatch et al. 2014). However, in 2014 a fracture in the spillway of Wanapum Dam forced a drawdown of the reservoir of 28'. This significantly changed the distribution of juveniles through the various routes of passage, reduced forebay residence time, and impacted overall survival. Because the drawdown of Wanapum reservoir will not be intentionally repeated in the future, the results of the survival test clearly should not be used to determine if survival objectives are being met in other years.

Power Analyses

To compare two operations within a single study, the power of the test must be determined prior to the start of sampling, given the available sample size and effect size of interest. Statistical power is defined as the probability of correctly rejecting the null hypothesis (i.e., there is no difference in survival under two different spill proportions) when in fact it is not true. More generally, statistical power can loosely be thought of as the probability of correctly finding effects that are genuinely true. Thus, studies with low power produce more false negatives (e.g., erroneously conclude that there isn't a difference between the two spill proportions) than studies with high statistical power. In order to avoid making erroneous conclusions, studies generally make sure that there is sufficient statistical power to detect the hypothesized effects or to safeguard against wrongly concluding that there isn't one. Statistical power is evaluated under a range of sample and effect sizes. Peven et al. (2005) does not include a discussion on methods of power analysis. This process should be required for survival studies intended to compare operations. The FPC has commented on comparisons of 30% and 40% spill at John Day (Feb 16 2011, March 24 2011, June 21 2011) and in comparisons of turbine operations at Wanapum Dam (July 22 2015, June 22 2015).

Appendix A

FPC Memos and Joint Technical Staff Memos Regarding Performance Testing

June 24, 2009 - http://www.fpc.org/documents/memos/91-09.pdf
July 29, 2010 - http://www.fpc.org/documents/memos/93-10.pdf
February 16, 2011 - http://www.fpc.org/documents/memos/20-11.pdf
March 24, 2011 - http://www.fpc.org/documents/memos/37-11.pdf
June 21, 2011 - http://www.fpc.org/documents/memos/91-11.pdf
February 15, 2012 - http://www.fpc.org/documents/memos/11-12.pdf
March 16, 2012 - http://www.fpc.org/documents/memos/25-12.pdf
March 23, 2012 - http://www.fpc.org/documents/memos/31-12.pdf
January 4, 2013 - http://www.fpc.org/documents/memos/02-13.pdf
February 11, 2013 - http://www.fpc.org/documents/memos/15-13.pdf
March 19, 2013 - http://www.fpc.org/documents/memos/32-13.pdf
March 22, 2013 - http://www.fpc.org/documents/memos/44-13.pdf
July 11, 2013 - http://www.fpc.org/documents/memos/86-13.pdf
August 2, 2013 - http://www.fpc.org/documents/memos/101-13.pdf
October 7, 2013 - http://www.fpc.org/documents/memos/120-13.pdf
December 3, 2013 - http://www.fpc.org/documents/memos/138-13.pdf
January 14, 2014 - http://www.fpc.org/documents/memos/05-14.pdf
January 21, 2014 - http://www.fpc.org/documents/joint_technical/07-14.pdf
January 27, 2014 - http://www.fpc.org/documents/memos/10-14.pdf
January 27, 2014 - http://www.fpc.org/documents/joint_technical/21-15.pdf
May 2, 2014 - http://www.fpc.org/documents/memos/60-14.pdf
February 3, 2015 - http://www.fpc.org/documents/memos/25-15.pdf
February 20, 2015 - http://www.fpc.org/documents/memos/33-15.pdf
February 20, 2015 - http://www.fpc.org/documents/joint_technical/34-15.pdf
June 8, 2015 - http://www.fpc.org/documents/memos/92-15_rev1.pdf
June 8, 2015 - http://www.fpc.org/documents/joint_technical/96-15.pdf
June 22, 2015 - http://www.fpc.org/documents/memos/104-15.pdf
July 22, 2015 - http://www.fpc.org/documents/memos/120-15.pdf
November 9, 2016 - http://www.fpc.org/documents/memos/58-16.pdf

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