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MEMORANDUM

TO: Matthew Eppard, USACE
Erick van Dyke, ODFW

Michele DeHart

FROM: Michele DeHart

DATE: June 8, 2015

RE: Comments on *Systematic review of JSATS passage and survival data at Bonneville and The Dalles Dams during alternative turbine and spillbay operations from 2008–2012*.

At your request, we have reviewed the report *Systematic review of JSATS passage and survival data at Bonneville and The Dalles Dams during alternative turbine and spillbay operations from 2008–2012*. This report uses acoustic tag data collected during various performance standard testing from 2008 through 2012. Our primary conclusions after reviewing the subject report are summarized below followed by specific discussion of each point.

- The stated result of this study, "that different turbine operations at Bonneville Dam Powerhouse 2 have no effect on juvenile survival," are unfounded and cannot be supported by the data or the analyses.
- The fundamental premise in this report, that data collected over a range of years from different studies can be collapsed into one data set, requires the assumption that year-to-year variation does not exist, or that it can be ignored. This assumption is obviously not supportable.
- Shortcomings of the scientific study design, study implementation, and data analyses problems have been documented regarding the individual performance standard studies results have been documented. These technical issues are serious and countermand the authors' assumption that combination of results of these studies over several years into one data set is valid.

- The statistical analysis is weak. Throughout the report, the authors assert whether or not a statistically significant difference in survival rates between any of the treatment operations was found. These statements need to be supported with p-values. This is a common procedure ubiquitous to all rigorous scientific reports, particularly if these conclusions are to be used to inform management decisions.
- The authors ignore documented delayed mortality, injury, and mortality from powerhouse passage caused by turbine operations.

In this study, juvenile survivals through the turbines are compared at different turbine operations. This does not address the issue of high gatewell turbulence at high turbine flows, which has caused high mortality and descaling in the juvenile bypass facility and instigated the formation of the FPOM subcommittee on Bonneville operations, change forms to the Fish Passage Plan, System Operational Requests from the Fish Passage Advisory Committee, and memos from the Fish Passage Center.

In this report, data are grouped across years without accounting for differences in flow, operations, and general inter-annual variation that one would expect when analyzing data over this large of a time-frame. It is not clear why the others chose to do this. One can surmise that the sample sizes for each annual operation were too small to detect meaningful differences in survival between different operations, which in turn suggests that the results grouped across years are also inconclusive and cannot inform management decisions.

Additionally, these studies involved different study objectives, collection sites, tagging criteria, and operations, and differ so greatly that results from combining these studies should be undertaken with serious reservations. FPC has reviewed many of these studies and has found numerous problems that have not been addressed, including the high-grading of samples for both size and condition, the inflation of survival estimates, unrepresentative use of flows and operations, the exclusion of the effects of delayed mortality, and the lack of needed data included in reports. Data used in this synthesis should be available, but not all the relevant citations are provided. We request that this data be made available for review, followed by an extended period for comments, so all information can be adequately reviewed.

In summary, the results of *Systematic review of JSATS passage and survival data at Bonneville and The Dalles Dams during alternative turbine and spillbay operations from 2008–2012*" (herein referred to as 2015 PNNL Report) cannot be used to inform managers about the effects of turbine operations on smolt survival at Bonneville Dam. Please see a summary of our findings below, followed by a detailed discussion of each point.

- The methods used in this report do not address the juvenile injury and mortality issues raised in numerous FPC memos, SORs from FPAC, and the formation of the FPOM "Bonneville Turbine Operations" subgroup.
    - Issues with elevated mortality and descaling at BON-PH2 are related to increased turbulence in the gatewells when units in BON-PH2 are operated at the upper end of the 1% efficiency range. The analyses presented in the 2015 PNNL Report completely ignore fish passing through the bypass system that are subject to

    increased turbulence in the gatewells. Therefore, results from these analyses should not be used to assess BON-PH2 operations and the efficacy of operating at the upper end of the 1% efficiency curve.

  - FPC analyses conducted in 2012 (FPC Memo December 17, 2012) focused on fish passing through the BON-PH2 bypass system. These analyses found that operating units at BON-PH2 at the upper end of the 1% efficiency range had a significantly increased effect on sample mortalities for subyearling Chinook, sockeye, and yearling Chinook. As the proportion of PH2 units operating at the upper end of 1% increased, sample mortalities also increased.
  - The above-mentioned memo included smaller subyearling fall Chinook from Spring Creek NFH and sockeye, which have routinely demonstrated increased sensitivity to BON-PH2 operations since 2008. The 2015 PNNL Report included only late season subyearling Chinook which were much larger than Spring Creek subyearlings and, therefore, not comparable. The 2015 PNNL Report analyses did not include sockeye. Therefore, results from these analyses are not applicable to all species passing through turbines during the spring outmigration.

- The data in this report were not initially collected to compare turbine operations at Bonneville Dam. This has led to a number of problems, including:

  - Flows and operations are combined between years, meaning that inter-annual variation is not incorporated into the analysis.
  - The sample sizes at many of the operations do not provide enough statistical power to reliably make conclusions and hence are prone to false-discovery rates.
  - The rationale for particular comparisons between operations is not stated and it appears that all possible combinations were tested. Justification and hypotheses for comparisons need to be provided in order for the results to be more meaningful. .

- The methodologies used in the studies that comprise this report are not directly comparable to each other due to the differences in study objectives and design. The rejection rates and average tag size for tagged fish vary widely and are not reported in some cases. Collection sites, release locations, and acoustic arrays vary between years and species. The data collected for different studies across years should be combined only after serious consideration.

- The performance tests that comprise the data in this report have come under repeated scrutiny. Concerns include:

  - Tags require that study fish be of a certain size and in relatively good condition. These restrictions mean that studies often do not represent the run-at-large, but only the survival of the largest and healthiest smolts. Rejection rates are not reported for several years of data in this report, so the representation cannot be evaluated.

    - Fish that are tagged, handled, and released upstream must migrate to the first detection site for inclusion in this study. Mortalities between the release and first detection are not included, meaning that only smolts with a high survival probability make it into the study group. This may artificially inflate the survival estimates generated by this study.

- The methodology used in these studies measures only turbine survival. It does not include the injury rate due to turbine passage, and it does not include any measure of delayed mortality. The short reach of these survival studies cannot provide a representative metric for actual mortality due to turbine passage.

- The results of this report cannot be fully evaluated due to missing data. These data should be provided, followed by an extended period for additional comments:

    - The collection, tagging information, and release location for yearling Chinook and steelhead used for Bonneville turbine survivals in 2012 have not been reported. Additionally, the purpose of these tags and reports for the resulting studies should be included. These data are not contained in any of the cited references.
    - The presence of any fish tagged for other studies used from 2008–2011 should be included, if not already cited.
    - The rejection rates for 2008 and 2009 tagging are not reported with the same methodology as in performance testing conducted between 2010 and 2014. Comparable information should be provided. Rejection rates are not included at all for 2010 tagging and should also be included.
    - The average size of tagged fish of each species is not included for data from 2009. This information should be included.

Bonneville Operations and Juvenile Mortality Concerns

Some level of mortality and descaling occurs at every hydro-electric project bypass system. However, since 2008 mortality and descaling rates at the Bonneville Dam second powerhouse (BON-PH2) have been elevated, particularly during periods immediately following releases of subyearling tule fall Chinook from Spring Creek NFH and during peak sockeye passage. These elevated mortality and descaling rates were first noted in 2008 after changes were made to the juvenile bypass system at BON-PH2 to improve the proportion of fish passing through the system. A study conducted by Hughes et al. (2011) obtained information on velocity measurements near the screens. The study revealed approach velocities exceeding recommended criteria intended to improve fish passage conditions. The authors concluded that the turbulence in the gatewell region in proximity to the Vertical Barrier Screen when BON-PH2 was operated at the upper 1% efficiency range could be expected to result in suboptimal fish passage conditions. The high velocities and turbulent conditions could cause impingement, impact, or descaling of juvenile salmonids before they exit through the orifice into the juvenile fish bypass channel. In addition, the powerhouse turbine unit discharge rate directly affected the velocity distribution as well as the turbulence conditions in the gatewell. Both the velocity and the turbulence increase as the operation within the 1% efficiency range increases.

The issue of operating BON-PH2 at the high end of the 1% efficiency range was raised again during the 2012 juvenile salmon migration. In April, approximately 8.0 million subyearling tule fall Chinook were released from Spring Creek NFH about 35 kilometers above BON. Subsequent to their arrival at BON, sample mortality rates of subyearling Chinook began to increase. Due to the elevated mortality rates, members of the Fish Passage Advisory Committee (FPAC) submitted a System Operational Request (SOR 2012-1) for a special operation at BON. Under this SOR, all units at BON-PH2 would operate at the mid-point of the 1% efficiency range, establishing a lower hydraulic capacity for the project, and any additional water above this hydraulic capacity would pass as spill. The U.S. Army Corps of Engineers proposed an alternative operation, which called for no additional spill and included the operation of the first powerhouse (BON-PH1) in open geometry (also referred to as BOP) to minimize the time that units at BON-PH2 would operate at the higher end of the 1% efficiency range. The Action Agencies alternative operation was implemented on the afternoon of April 13th and continued off and on until the majority of the juvenile Snake River sockeye run had passed in mid-June.

The issue with operating BON-PH1 at open geometry is that there were no empirical data to suggest that forcing more fish through BON-PH1 presents the better fish operation. In addition, there is no condition monitoring at BON-PH1 and, therefore, if there was additional mortality imposed on these fish, there is no way of knowing or measuring the impact. Justification for the open geometry operation was based on limited data, including observational studies of turbine models and a balloon tag study that was conducted in 2000. In a July 30, 2012 memo (FPC Memo July 30, 2012), the FPC provided a review of these studies and concluded that rigorous testing of juvenile survival, and particularly its relation to subsequent adult survival, would be necessary before open geometry is utilized as a permanent operation.

During the May 16, 2012, TMT discussion, it was determined that a more permanent solution was needed for these types of incidences. It was decided that discussions of a permanent solution should be held in various regional forums, including the Fish Passage Operations Managers (FPOM), Fish Facility Design Review Work Group (FFDRWG), and Scientific Review Work Group (SRWG). An FPOM Task Group was convened in July 2012 to addresses these concerns, which led to two data requests for the Fish Passage Center to conduct analyses to: (1) investigate whether there was evidence that operating above the mid-range of the 1% efficiency curve at BON-PH2 resulted in increased levels of juvenile sample mortalities, and (2) investigate whether there was evidence of increased adult fallback with increased spill and/or increased discharge from BON-PH1. On December 17, 2012, the FPC provided two memoranda to the FPOM Task Group in response to these requests (FPC Memos from December 17, 2012). Findings from these two memoranda are summarized below.

Juvenile Mortality and BON-PH2 Operations (FPC Memo July 30, 2012)

- The percent of units operating above the mid-range often had a significant effect on sample mortalities, particularly for subyearling Chinook, sockeye, and yearling Chinook.
    - In general, as the percent of units operating above the mid-range increased, average sample mortality also increased. In fact, sample mortality was often

significantly higher when >95% of BON-PH2 units operated above the mid-range than when <5% of BON-PH2 units operated above the mid-range.

Adult Fallback (i.e., re-ascension) in Response to BON-PH1 and Spill Operations in 2012 (FPC Memo December 17, 2012)

- Based on multivariate regression modeling of re-ascension rates versus spill and BON-PH1 discharge, at flows in the range of 320 to 350 Kcfs, it appeared that decreasing spill and increasing BON-PH1 discharge led to higher re-ascension rates in 2012 for both steelhead and spring/summer Chinook adults.
- Re-ascension rates were highest for all species of PIT-tagged adult salmon exiting the ladder into BON-PH1 forebay at 11.2%, while re-ascensions for adults exiting into BON-PH2 forebay were only 1.5%.
- Adult steelhead had the highest re-ascension rate at Bonneville Dam from April 1 to July 1, at 9%, while 6% of adult spring/summer Chinook re-ascended, followed by only 0.5% of sockeye adults.
- Between 40% and 46% of PIT-tagged adult salmon exited ladders into the forebay of BON-PH1 compared to 54% to 60% exiting into the forebay of BON-PH2.
- Patterns of discharge in BON-PH1 and spill were not significant in explaining the variability in proportion of adults entering the ladder entrances that exit into the BON-PH1 forebay (BO1 exit).

Results from the FPC analyses suggest that the open geometry operation is not a good alternative for limiting BON-PH2 operations to the mid-range of the 1% efficiency curve, as increasing discharge from BON-PH1 resulted in higher re-ascension rates than increasing spill. Therefore, increasing spill is likely a better alternative, which is what was originally proposed by FPAC in SOR 2012-1. Based on these analyses and results, the FPC recommended to FPAC that hydraulic capacity for BON-PH2 should be capped at the mid-range of the 1% efficiency curve, particularly when subyearling Chinook, sockeye, and yearling Chinook are actively migrating past the project (FPC Memo December 18, 2012). Under current Total Dissolved Gas waivers, capping hydraulic capacity of BON-PH2 at this level would allow for additional spill from BON without violating water quality standards, as spill in excess of hydraulic capacity is not subject to these water quality standards.

In 2014, the Fish Passage Plan included modified guidelines for the operation of BON-PH1 and BON-PH2 for the period of April 1 to October 31, with the goal of minimizing gatewell turbulence for bypassed juvenile salmonids in BON-PH2. When flows are sufficiently high, these modified operations included increasing BON-PH1 operations to open geometry. From April 10 to June 15, increased spill may also be provided (after open geometry is implemented) in order to limit BON-PH2 to the 1% mid-range. However, the modified operation also established an "adult trigger" that eliminates additional spill if spring Chinook adults out-number juvenile yearling Chinook collections for two consecutive days. During this time, operations at BON-PH2 would be increased to the upper end of the 1% range.

Statistical Design and Analysis Considerations

Lack of Proper Study Design

The data utilized in this study were not intended for the use of testing differences between turbine treatment operations. As stated by the authors, "*This analysis was conducted by PNNL…using data acquired during Biological Opinion compliance and preliminary studies conducted from 2008 through 2012.*" However, the authors then assert that, "*These large data sets can be analyzed to answer questions beyond those asked by the compliance studies.*" We strongly disagree with this statement and hence the data that forms the basis for the conclusions reached by the authors in this study.

One must be careful when using data to answer questions that the data were not meant to inform. If this opportunistic data truly has utility, then no manipulation or aggregation of this data should be required. However in every analysis in this study, data spanning all five years of the BiOp studies (2008–2012) were collapsed and analyzed under the implicit assumption that there was no year-to-year variation in the data. Ignoring year-to-year variation in the data is the antithesis of conventional statistical techniques that try to account for and explain as much variation in the data as possible. Also, ignoring this temporal dependency (i.e., observations from within the same year are expected to be more similar compared to observations between other years) is a form of pseudoreplication (Hurlbert 1984) which can result in unreliable and overly optimistic estimates of standard errors.

Weak Statistical Hypothesis and Inference Testing

Throughout the report, the authors assert whether or not a statistically significant difference in survival rates between any of the treatment operations was found. These statements need to be supported with p-values. This is a common procedure ubiquitous to all rigorous scientific reports especially if these conclusions are to be used to inform management decisions. Without p-values, it is impossible for the reader to determine the strength of the authors' conclusions regarding differences between treatments. For instance, if no significant statistical difference (at a significance level of 0.05) between treatments were found, the conclusion of this finding would be much different if the p-value were 0.06 compared to 0.9. The former p-value indicates that there may be some difference, but not at the chosen significance level, where the latter p-value strongly supports the conclusion of no statistical difference.

Instead of using p-values (the standard method), the authors chose to statistically compare treatments by comparing 95% confidence intervals. Although not stated by the authors, when these intervals overlap, the hypothesis that the two treatments are the same cannot be rejected. First, these 95% confidence intervals need to be provided in a table, as visually comparing the widths of confidence intervals in bar graphs is cumbersome and imprecise. Second, while the method of examining overlapping confidence intervals is simple and convenient, it is not equivalent to the standard method. As Schenker and Gentleman (2001) show, the method of examining overlapping confidence intervals is more conservative (i.e., rejects the null hypothesis that the two treatments are the same when in fact they are the same less often than the standard method), and it mistakenly fails to reject the null hypothesis that the two treatments are the same when in fact they are different more frequently than occurs with the standard method. It should not be used for formal significance testing.

In a mark-recapture setting, likelihood ratio tests are most often used to statistically compare whether two parameter estimates (or in the case of this study, two survival estimates under different turbine operations) are statistically different (Burnham and Anderson 2002). Without the raw data, we were not able to conduct a likelihood ratio test. However, when testing two-sided hypotheses, a likelihood ratio test should closely approximate the more familiar t-test (Casella and Berger 2002). A t-test can easily be conducted with the point estimates, standard deviations (converted from the standard errors), and sample sizes provided in the Appendices. To illustrate, we compared the six non-overlapping treatments for yearling Chinook at Bonneville Power House 1 (Table 1). As asserted by the authors, there were significant differences between Q1 and Q2, and Q4. However, the overlapping confidence interval approach failed to identify significant differences between Q1 and Q2, and Q3, in addition to Q1 and Q2, and ABOP that were found using the standard approach. That is, the authors failed to correctly report significant differences in treatment operations that indicated higher survival in Q1 and Q2, compared to Q3 and ABOP. This example is merely illustrative, and we have not checked whether the authors failed to report statistical differences for the remainder of the species, dam routes, and grouped treatments.

Table 1. Statistical comparison of turbine treatment operations for yearling Chinook at Bonneville Powerhouse 1 determined by the overlapping confidence interval approach (significant results indicated by "X") and the p-value approach (standard approach). P-values less than 0.05 significance level are indicated in bold.

	Overlapping confidence interval method					
	Q1	Q2	Q3	Q4	BOR	ABOP
Q1	-			**X**		
Q2	-	-		**X**		
Q3	-	-	-			
Q4	-	-	-	-		
BOR	-	-	-	-	-	
ABOP	-	-	-	-	-	-

	P-value (standard approach)					
	Q1	Q2	Q3	Q4	BOR	ABOP
Q1	-	0.78	**0.04**	**<0.01**	0.10	**0.02**
Q2	-	-	**0.03**	**<0.01**	0.09	**0.02**
Q3	-	-	-	0.98	0.54	0.58
Q4	-	-	-	-	0.42	0.58
BOR	-	-	-	-	-	0.85
ABOP	-	-	-	-	-	-

Finally we note that it would be useful if the authors more explicitly stated which treatment operations they statistically compare and a motivating reason for the comparison, followed with their hypothesis of why the treatments may be different. This is suggested because it appears that the authors blindly test every combination of the six non-overlapping treatment operations (Q1–Q4, BOR and ABOP), a total of 15 comparisons, and every combination of the grouped treatment operations (LL to UL, LL to BOP, BOR and ABOP), a total of 4 comparisons. These tests are followed by statements of whether or not statistically different comparisons were found. However, these statements are uninformative without a hypothesis or explanation of why there should or should not be a difference. For instance, not finding a statistical difference between LL to UL, and LL to BOP for yearling Chinook is not surprising as it can be inferred from the provided sample sizes that these two treatment groups share over 80% of the same data. Why should there be a statistical difference between treatments when the data forming the two treatments largely overlap?

Lack of Statistical Significance Cannot be Used to Support Management Operations

Throughout this report, the authors test a myriad of statistical null hypotheses that two treatment operations are equal and report which treatment operations had statistically different survival rates. In a strict statistical sense, statistically significant results imply that the probability of the observed data is extremely unlikely if in fact the null hypothesis were true. However, due to this interpretation, null hypotheses can never be proven. Failing to reject a null hypothesis does not mean that it is true and, especially with small sample sizes, one must be careful not to accept the null hypothesis simply because it was not rejected (Johnson 1999).

When small sample sizes are used to test statistical hypotheses, statistically insignificant results do not imply a lack of effect, but instead may result from an insufficient amount of data relative to the effect size that is being tested. Statistical power is the probability of correctly rejecting the null hypothesis when in fact it is not true (i.e., the probability of making the correct conclusion). Thus, studies with low power produce more false negatives (i.e., erroneously conclude that there isn't a difference between two turbine treatment operations) than high-powered studies. In order to avoid making erroneous conclusions, studies typically make sure that there is sufficient statistical power to detect the hypothesized effects or safeguard against wrongly concluding that there isn't one.

Because the data used in this study were not intended for the use of testing for differences between turbine operations, and instead were intended for the BiOp compliance standard testing, no statistical power calculations were conducted. This is troublesome, particularly because the authors repeatedly conclude that they could not detect statistically significant effects between turbine operations, but did not provide a prior expectation to detect a difference if indeed there was one.

Since the authors did not provide any power calculations, below we provide an example of a power calculation in a CJS model setting for illustrative purposes. Specifically we tested the power to detect a statistical difference between two treatment operations resulting in 95% and 98% survival over a range of sample sizes for both treatment operations. Since the report did not provide estimates of survival below the tailrace, nor detection probability at downstream arrays, we assumed 95% survival downstream of the tailrace for both treatment groups, and 90% detection probability at downstream arrays for all treatment groups. These values were chosen to represent typical survival and detection probabilities observed across the five years of performance standards testing at Bonneville Dam. A simulation approach was used to calculate statistical power wherein random capture histories over a range of sample sizes were generated under the assumptions stated above. This process was repeated 1,000 times and statistical power was calculated as the proportion of the 1,000 iterations for which a statistically significant difference (α=0.05) was obtained.

The target power of a statistical hypothesis is an arbitrary choice (e.g., as is the significance level of a hypothesis test), but most researchers try to typically achieve at least 80% power. For the example described above where we tested for statistical differences between two turbine operation treatments characterized by 95% and 98% survival, 80% power was never achieved even for the largest sample sizes of 1,500 individuals in each treatment group

(Figure 1). Interestingly, regardless of sample size for the treatment group with 95% survival, statistical power was always less than 10% whenever the same size of the treatment with 98% survival was less than 500 individuals.

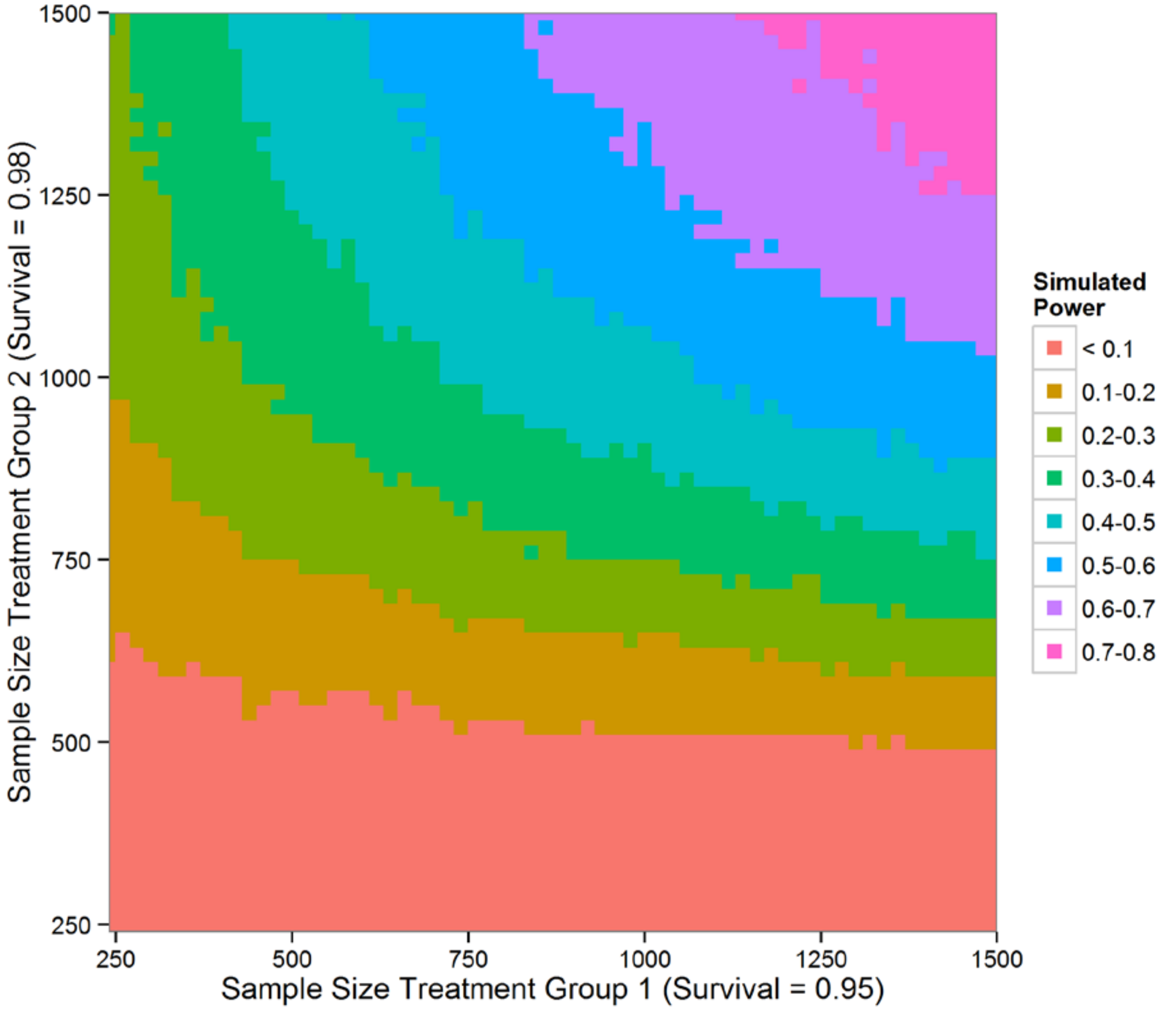

Figure 1. Statistical power as a function of sample size of two treatments resulting in 95 and 98% turbine survival. Statistical power was determined via simulation and for this reason the boundaries of the power bins used in the plot are not smooth. Smoother bins could have been achieved with more iterations of the simulation (e.g. 10,000 instead of 1,000).

The effect sizes (i.e., differences in survival between two treatment operations) and sample sizes of treatments varied considerably for the various combinations of species, passage routes and treatment operations examined in this study. In addition to this, we arbitrarily assumed fixed values for detection probability and survival downstream of the tailrace for our power calculations. Therefore, Figure 1 is only illustrative in nature, but nonetheless, because of the reasonableness of the parameters assumed in this illustration relative to those observed in the authors' study, these calculations suggest that many of the statistical comparisons that the authors made had low statistical power to begin with, and hence a low probability of discovering a statistically significant result even if a true turbine treatment effect existed.

Different Methodologies Mean Studies are Not Directly Comparable

The techniques used in performance testing have evolved since their first usage at Bonneville Dam in 2008. These differences create differences in how the test represents the run-at-large, the tag burden on individuals, the amount of handling for each group, the release location, and the distance travelled between release and detection. All of these factors can affect the swimming ability, survival, and effects of differing operations. Additionally, because not all operations were tested in all years, methodological differences may confound the results of comparisons between operations.

Tag size ranged between 0.304g to 0.485g over the studies combined in this report. The minimum tagging size of 95 mm did not change, so the tag burden varied between 2008 and 2012 studies. Tag burden can affect behavior, swimming ability, and passage route (see FPC Memos June 24, 2009; March 24, 2011; February 15, 2012; and March 19 2013). While the 2015 PNNL Report does include subyearling Chinook, these subyearlings were much larger than the subyearling fall Chinook that are released from Spring Creek NFH, which have routinely had high injury rates associated with BON-PH2 operations. For example, over the past five years, subyearling Chinook from Spring Creek NFH have averaged 76 – 78 mm for the April release and 83 – 86 mm for the May release (Table 2). Both of these release sizes do not meet the minimum size requirements for the acoustic tag studies, so the 2015 PNNL Report does not include one of the populations most affected by turbine operations.

Table 2. Average size of subyearling Chinook releases from Spring Creek NFH. The minimum tag size for acoustic tagging in the 2015 PNNL Report is 95 mm.

Month	2010	2011	2012	2013	2014
April	76.20 mm	76.20 mm	76.20 mm	78.74 mm	76.20 mm
May	86.36 mm	83.82 mm	86.36 mm	86.36 mm	83.82 mm

Acoustic tagging imposes a significant burden on fish, and therefore not all collected smolts are suitable for tagging. The percentage of fish rejected for tagging, due to size or condition, indicates how well the study represents the population of migrating smolts as a whole. In 2012, managers and researchers developed a more inclusive tagging criteria than had been previously used. However, the 2008 and 2009 studies at Bonneville report rejection rates of 0.8% to 1.9%, while studies in 2011 and 2012 had rates between 3.2% and 16.4%. This difference in rejection rates indicates that in earlier studies, rejection rates were likely calculated *after* unsuitable fish had already been removed from the samples, and do not reflect the rejection rate of smolts from the run-at-large. Without the correct rejection rates, these studies cannot be considered to reflect the actual migration conditions for smolt populations and cannot be used in the kind of comparative analysis generated in the 2015 PNNL Report.

According to the citations provided in the 2015 PNNL Report, all tagged smolts in this analysis were collected and tagged at John Day Dam. However, in 2012, no yearling Chinook or steelhead smolts were collected at John Day for tagging, but data from these two groups are included in the analysis. Presumably these fish were tagged and released upstream for different studies. The differences in collection and release sites means that the fish experienced different handling protocols than yearling Chinook and steelhead smolts included in data from 2008–

2011. They travelled a greater distance to detection in the Bonneville forebay with tags, which may affect passage route and survival probability. If these fish came from tagging studies even further upstream than John Day, tag life may also affect the results from these fish. This is not addressed in any of the analyses, and the potential impact on the results cannot be addressed since no information is provided on the tags and methodologies used for these fish.

Performance Testing Does Not Reflect Mortality Due to Hydrosystem

Performance testing, as used to measure survival through individual projects, has raised numerous concerns during their usage (see Appendix A). Particularly relevant to this study are tagging criteria that require the rejection of a large percentage of the run-at-large (see above). Another source of potential high-grading sampled populations in comparison to the run-at-large is the distance travelled between the release site and the first detection in the Bonneville forebay. If smaller and weaker fish are more susceptible to mortality due to the tag burden, these fish will not be included in the dam-passage group. These fish are also those least likely to survive turbine passage, so the sample is artificially biased upward.

Study Does Not Include Delayed Mortality and Other Effects of Turbine Passage

The authors of this study failed to comment on previous studies that found evidence for delayed mortality associated with powerhouse passage. That is, direct mortality (mortality to the tailrace of the dam) is only one component of the cumulative effect of powerhouse passage on subsequent survival. Ferguson et al. (2006) showed that fish passing through turbines have a lower survival rate when survival was measured over a longer reach than when measured over a short reach. Fish released into turbines had relatively high survival to the tailrace of McNary Dam (0.93 to 0.946) as measured by balloon tags. Survival to arrays located 45 km downstream was between 0.814 and 0.858 and was found to be significantly lower. Ferguson et al. (2006) concluded that direct mortality (mortality to the tailrace of the dam) made up 30% to 54% of total mortality. In this case delayed mortality was up to 70% of total mortality estimated in this study. Since adult returns are available for nearly all of the outmigration years analyzed in this study, the authors should investigate post-Bonneville Dam survival for fish passing through the different turbine treatment operation compared to other routes of passage during similar time frames.

Literature Cited

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Appendix A
Further Documentation

FPC Memos relating to Bonneville turbine operations and the effects on juvenile survival:

April 24, 2012: Overview of Bonneville Dam powerhouse operations and Spring Creek subyearling Chinook passage and mortality.
May 4, 2012: Bonneville Dam Passage.
May 10, 2012: Sockeye juvenile mortality and descaling at Bonneville Dam, May 3-9, 2012.
June 7, 2012: Juvenile fish mortality estimates for Bonneville Second Powerhouse Bypass.
July 30, 2012: Open Geometry Operations.
December 17, 2012: Effects of operations at Bonneville Dam second powerhouse and juvenile sample mortalities, 2008-2012.
December 17, 2012: Re-ascension rates at Bonneville Dam in response to operations in 2012.
December 18, 2012: Recommendation for future operations at Bonneville Dam with regard to open geometry.
May 7, 2013: BON Operations Task Group- Comments on Proposed Fish Passage Plan Change Form and Supporting Memorandum.

System Operational Requests relating to Bonneville turbine operations and the effects on juvenile survival:

2012-1: Bonneville operations over next five days to facilitate Spring Creek release; To improve the survival of subyearling Chinook Salmon at Bonneville Dam over the next five-day period.
2012-2: Bonneville operations; to reduce descaling of Sockeye at Bonneville Dam. May 29, 2012
2013-3: Bonneville operations; Due to the high rate of descaling of juvenile sockeye salmon passing through the Bonneville Dam juvenile bypass facility, implement the Fish Passage Plan Request Change Form written and recommended by the FPOM sub-group concerning Bonneville turbine operations.

FPC Memos relating to performance testing:

July 24, 2009: Review of "Statistical Design for the Lower Columbia River Acoustic-Tag Investigations of Dam Passage Survival and Associated Metrics".
July 29, 2010: Review of Acoustic Telemetry Evaluation of Juvenile Salmonid Passage at John Day Dam, 2009 Draft Final Report
February 16, 2011: Review Performance Standard Testing/John Day 2010
March 24, 2011: Review 2011 Acoustic Tag study design for John Day Dam
June 21, 2011: John Day acoustic tagging compliance monitoring
February 15, 2012: Acoustic tagging studies in the Lower Columbia, performance standards, and review of "Analysis of Dam-Passage Survival of Yearling and Subyearling Chinook Salmon and Juvenile Steelhead at The Dalles Dam, Oregon, 2010".

March 16, 2012: FCRPS Juvenile performance standard and metrics
March 23, 2012: Comments on BON and TDA Performance Testing in 2010
January 4, 2013: Summary of comments on performance testing
February 11, 2013: 2012 performance testing at Lower Monumental and Little Goose Dams
March 19, 2013: Performance testing at Lower Monumental and Little Goose Dams, 2009 and 2012
March 22, 2013: 2012 Performance testing at Lower Columbia Dams
October 7, 2013: Review comments, 2013 Draft FCRPS Supplemental Biological Opinions
December 3, 2013: Response to request- Review BPA SMART Spill PowerPoint Presentation
January 14, 2014: Performance testing at LGS and LMN Dams for subyearling Chinook in 2013
May 2, 2014: Performance testing at LGS and LMN Dams for subyearling Chinook in 2013 and additional memorandum.
February 3, 2015: Comments on draft of *Compliance monitoring of yearling and subyearling Chinook salmon and juvenile steelhead survival and passage at McNary Dam, 2014*
February 20, 2015: Comments on draft of *Compliance monitoring of yearling and subyearling Chinook salmon and juvenile steelhead survival and passage at John Day Dam, 2014*

Joint Technical Staff Memorandums relating to performance testing:

August 17, 2012: Lower Columbia Survival Studies, SRWG meeting April 13, 2012.
January 21, 2014: Comments on draft report "BiOp Performance Testing: Passage and Survival of Subyearling Chinook Salmon at Little Goose and Lower Monumental Dams, 2013".
January 27, 2015: Comments on draft report *Compliance monitoring of yearling and subyearling Chinook salmon and juvenile steelhead survival and passage at McNary Dam, 2014*
February 20, 2015: Comments on draft report *Compliance monitoring of yearling and subyearling Chinook salmon and juvenile steelhead survival and passage at John Day Dam, 2014*