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MEMORANDUM

TO: Ed Bowles, ODFW

FROM: Michele DeHart

DATE: October 7, 2013

SUBJECT: Review Comments, 2013 Draft FCRPS Supplemental Biological Opinion

In response to your request the Fish Passage Center (FPC) staff have reviewed the hydro systems operations portion of the NOAA 2013 Draft FCRPS Supplemental Biological Opinion (herein referred to as Draft BIOP) and the three supporting documents: (1) "Federal Columbia River Power System Improvements and Operations under the Endangered Species Act — A Progress Report" by the Bonneville Power Administration based upon analyses by BioAnalysts Incorporated and Anchor QEA LLC, (2) "Limitations of Correlative Investigations of Identifying Causal Factors in Freshwater and Marine Survival of Columbia River Salmonids" by Skalski et al. (2013), and (3) "Review of *Assessing Freshwater and Marine Environmental Influences on Life-Stage-Specific Survival Rates of Snake River Spring–Summer Chinook Salmon and Steelhead*" by Manly (2012).

Although Skalski et al. (2013) and Manly (2012) are presented by NOAA as the primary foundation for elements of the Draft BIOP, or at least retaining the status quo, these documents were not available for public review until September 16, 2013, after half of the public review period for the Draft BIOP had passed, although previous requests for those documents had been submitted to NOAA

Our overall conclusion is that the hydro systems operations sections of the Draft BIOP reduces fish passage protections and does not incorporate new data, analyses, and knowledge that have been gained since the 2008 version of the Biological Opinion was completed. In this way the Draft BIOP provides less than the previous Biological Opinion in fish protection. The

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Action Agencies and NOAA have contracted with consultants, Skalski et al. 2013, Manly 2012, BioAnalysts, Inc., and Anchor QED, 2013, for analyses intended to support their decision to discount and exclude new data and analyses from incorporation into the Draft BIOP, therefore maintaining, or in some cases reducing, the present status quo in fish protections in the Draft BIOP. Our review of these specific documents concludes that they do not provide a reasonable or technically sound basis for excluding new data and analyses from the Draft BIOP. These recent data and analyses clearly indicate that some of the fundamental components of the 2008 and Draft BIOP should be reconsidered, specifically the at-dam performance standards and spill for fish passage.

In the following we have organized our comments according to key issues regarding the Draft BIOP and our summary conclusions, followed by detailed discussion of each. We also provide specific comments on each of the above listed documents which NOAA has provided to support the Draft BIOP. We offer the following review comments for your consideration.

Spill for Fish Passage

- The Draft BIOP reduces spill for fish passage, reducing the time period that spill is provided by ending spill prior to August 31, and by starting lower summer spill levels at an earlier date.
- The Draft BIOP does not provide any scientific biological rationale for providing lower spill, below gas cap levels, for fall Chinook summer migrants.

Performance Standards Evaluation and Accomplishment

- The Draft BIOP does not address significant serious technical concerns that have been raised over the past several years regarding the concept and approach of performance standards. NOAA has failed to address or consider recent data and analyses that raise serious issues regarding the validity of the performance standard concept and approach, specifically that route of dam passage affects survival at later life stages and adult return rates.
- The present performance standard testing is likely generating estimates that are biased high and do not represent the run-at-large.
- Recent data and analysis indicate that freshwater passage experience affects later life stages and adult returns, which are not considered in performance standard implementation in this Draft BIOP therefore underestimating the impact of dam passage. Recent data indicate that a smolt-to-adult return rate would provide a more appropriate performance standard.
- NOAA does not offer any rationale for lower performance survival standards for fall Chinook compared to standards for spring/summer Chinook and steelhead.

Smolt Transportation

- The Draft BIOP increases the proportion of smolts transported by implementing an earlier date for the start of transportation.

- NOAA does not provide a biological scientific rationale for this action, but recognizes that this will provide no benefit to spring Chinook, which migrate earlier in the spring and will receive the majority of the impact from this action.

- Recent data and analyses indicate that overall transport SARs have improved with later transport dates and transportation of later migrating fish. Recent data and analyses indicate that powerhouse bypass passage should be avoided, indicating that increased spill at collector projects such as Lower Granite would result in higher SARs, rather than transporting earlier and increasing transportation.

- Recent data and analyses have shown that increasing transportation will increase straying and increase the negative impact of straying on other listed populations of salmon and steelhead.

Benefits of Spill for Fish Passage/Experimental Spill Management

- NOAA's rejection of consideration of Experimental Spill Management on the basis of the spring Chinook returns from the 2011 outmigration year is unfounded.

- NOAA fails to consider the high fall Chinook return from the 2011 outmigration year, which also experienced high spill and flow.

- NOAA fails to recognize, address or propose mitigation measures for hydrosystem operations under the present FCRPS configuration that took place in the 2011 outmigration year that were adverse for spring migrants.

The Draft BIOP Excludes Recent Data and Analyses and Maintains the Status Quo

- NOAA excludes recent data and analyses from consideration in the Draft BIOP on the basis of three documents: Skalski et al. 2013, Manly 2012, and BioAnalysts Incorporated and Anchor QEA 2013. These documents do not provide valid technical justification or rationale for excluding consideration of recent scientific findings from development and modification of RPAs in the Draft BIOP.

- Technical and analytical issues and methodology contained in Skalski et al. 2013, indicate that conclusions are not supportable and do not provide a valid rationale for rejecting recent data and analyses.

- Specific comments on these documents are provided in subsequent discussion sections of this review.

Detailed Discussions

Spill for Fish Passage

Reductions in Spill under RPA 29 in the Draft BIOP

Through the Draft 2014–2018 Implementation Plan (herein referred to as the Draft IP), the Draft BIOP proposes to change the start date for summer spill to begin earlier than presently implemented. At many projects summer spill volumes are less than those in the spring.

- The action agencies do not provide any scientific basis for the provision of lower spill for fish passage levels for summer migrating fall Chinook. At some projects spill could be increased to the gas cap for fall Chinook summer migrants to increase juvenile survival.
- Earlier summer spill will result in reductions in June spill volumes at Lower Granite (LGR), Lower Monumental (LMN), and Bonneville (BON) in medium and low flow years. June spill reductions at these sites are less likely in high flow years.
- Earlier summer spill will result in additional spill in June at McNary (MCN) because summer spill levels are higher than spring spill levels at MCN.
- Earlier summer spill will result in no change in June spill at LGS, Ice Harbor (IHR), John Day (JDA), and The Dalles (TDA) because spill levels do not change between seasons.

The Draft IP proposes to terminate spill at Snake River sites in August, based on subyearling Chinook collection counts.

- This change reduces spill at these sites. On average, over the last 9 years, summer spill at LGR, LGS, LMN, and IHR would have been terminated on August 7th, 16th, 18th, and 20th, respectively.
- Since flow conditions in August vary little among years and are usually below hydraulic capacity, the amount of reduced spill in August is not likely to be impacted by the flow conditions.

Spill could potentially decrease in spring and summer at IHR and JDA and in summer at BON if performance standards testing shows performance standards are met at the lower spill levels. Performance standard testing and implementation should be reconsidered based upon significant technical concerns and new scientific data.

Despite repeated objections from most of the salmon managers over the years, the Draft IP proposes to continue bulk spill at LMN during the spring, which effectively limits spill at LMN due to the fact that bulk spill generates high levels of total dissolved gas (TDG).

The Draft BIOP references RPA 29 of the Draft IP for proposed spring and summer spill operations at FCRPS projects. The Draft IP reduces spill from the current Court Order in at least two ways. In addition, there is language in the Draft IP that has the potential to further reduce spill at some sites from what is being provided under the current Court Order. Below is a

detailed explanation of these known reductions, as well as the language that allows for potential further reductions.

Known Reductions in Spill

Earlier Start to Summer Spill

At many FCRPS sites, summer spill volumes are lower than spring spill volumes. The technical, biological basis for providing less spill for summer migrating fall Chinook is not explained by NOAA fisheries and should be evaluated. RPA #29 of the Draft IP proposes to implement summer spill at an earlier time than the current Court Order. Under the current Court Order, summer spill begins on June 21st at Snake River projects and July 1st at Lower Columbia sites. There have been some exceptions to these start dates to accommodate research/performance standard studies over the years. However, in the absence of performance standards testing, June 21st and July 1st would be the summer spill start dates under the Court Order.

According to Table 2 of the Draft IP, the initiation of summer spill at Snake River sites will be based on collections of subyearling Chinook, and may occur as early as June 4th. Using the last ten years (2004–2013) of Smolt Monitoring Program (SMP) data at LGR, LGS, and LMN dams we estimated when summer spill would have begun, had the Draft BIOP been in place. Over the last 10 years, the average summer spill start dates would have been June 4th at LGR, June 6th at LGS, and June 7th at LMN (Table 1). Furthermore, all three Snake River sites had at least one year in the last ten that had a summer spill initiation date of June 4th (Table 1). Under the Draft IP summer spill at Lower Columbia sites will begin on June 16th, rather than July 1st under the current Court Order.

NOAA indicates that these earlier summer spill start dates are at least partly due to earlier run timing of subyearling fall Chinook. While it is true that subyearling Chinook timing has been earlier over the years, run timing of subyearling Chinook at SMP sites is largely influenced by the timing of hatchery releases. On average, approximately 80% (range 65%–100%) of hatchery subyearling Chinook released above LGR were released prior to the estimated summer spill initiation date over the last 10 years (2004–2013) (Table 1).

Table 1. Estimated summer spill initiation dates at Snake River sites, based on the 50% subyearling Chinook collection criteria outlined in RPA 29 of the Draft 2014–2018 Implementation Plan. Hatchery release data are only for subyearling Chinook releases above LGR.

Migration Year	LGR	LGS	LMN	Hatchery Release Total Above LGR	Percent Released Prior to LGR Summer Spill Initiation Date
2004	9-Jun	12-Jun	11-Jun	1,517,006	100%
2005	4-Jun	4-Jun	6-Jun	3,964,117	99%
2006	4-Jun	4-Jun	4-Jun	3,615,216	65%
2007	5-Jun	9-Jun	10-Jun	2,251,450	77%
2008	5-Jun	7-Jun	9-Jun	4,117,657	80%
2009	4-Jun	4-Jun	4-Jun	5,108,127	78%
2010	4-Jun	12-Jun	11-Jun	4,613,429	77%
2011	4-Jun	4-Jun	4-Jun	4,919,891	73%
2012	4-Jun	6-Jun	7-Jun	4,889,575	78%
2013	4-Jun	4-Jun	6-Jun	4,456,543	75%
Avg. ('04-'13)	**4-Jun**	**6-Jun**	**7-Jun**		**80%**

Earlier End Date for Summer Spill at Snake River Sites

Under the current Court Order, summer spill at Snake River sites occurs through August 31st. However, the Draft IP indicates that summer spill may be terminated as early as August 1st at LGR, August 4th at LGS, August 7th at LMN, and August 9th at IHR. According to Table 2 of RPA 29, the actual end date for summer spill will be based on subyearling Chinook collection counts in late July and into August. Using the criteria outlined in Table 2, we reviewed the last nine years (2005–2013) of Smolt Monitoring Program data at LGR, LGS, and LMN dams to estimate when summer spill would have ended had the Draft IP been in place (note: 2004 was not included since it was before the Court Order and there was no summer spill).

Over the last 9 years, the average summer spill end dates were August 7th at LGR, August 16th at LGS, August 18th at LMN, and August 20th at IHR (Table 2). In addition, over the last nine years, spill would have been terminated prior to August 31st in all 9 years at LGR, in 8 of the 9 years at LGS, and in 7 of the 9 years at LMN and IHR. Finally, August spill would have been terminated completely at LGR in 3 of the last 9 years (2005, 2006, and 2007).

Table 2. Estimated end dates of summer spill at Snake River projects
based on criteria outlined in RPA 29 of the Draft 2014–2018
Implementation Plan over the last 9 years (2005–2013).

Migration Year	LGR	LGS	LMN	IHR
2005	1-Aug	5-Aug	8-Aug	10-Aug
2006	1-Aug	4-Aug	7-Aug	9-Aug
2007	1-Aug	6-Aug	9-Aug	11-Aug
2008	31-Aug	29-Aug	1-Sep	1-Sep
2009	3-Aug	12-Aug	15-Aug	17-Aug
2010	11-Aug	24-Aug	27-Aug	29-Aug
2011	6-Aug	15-Aug	18-Aug	20-Aug
2012	7-Aug	18-Aug	21-Aug	23-Aug
2013	10-Aug	1-Sep	1-Sep	1-Sep
Average ('05-'13)	**7-Aug**	**16-Aug**	**18-Aug**	**20-Aug**

The Draft IP also specifies a criterion for resuming spill at the Snake River sites if subyearling
Chinook collections exceed 500 for two consecutive days. However, it is difficult to determine
if this criterion would have been met over the last 9 years because we do not know what
collections would have been, had spill not been provided.

***Estimated Impact of Earlier Summer Spill Start Dates and Earlier Termination of Summer
Spill***
To investigate the impact of these date changes, the Court Order and Draft IP schedules were
modeled using hourly flow data for 3 flow years: a high flow year (2011), a medium flow year
(2009), and a low flow year (2013). Instantaneous spill volumes under both scenarios were
based on Table 2 of the Draft IP (RPA 29). For the Court Order schedule, we assumed the
spring spill dates of April 3 to June 20 and summer spill dates of June 21 to August 31 at Snake
River sites. For Lower Columbia sites, the Court Order schedule assumed spring spill dates of
April 10 to June 30 and summer spill dates of July 1 to August 31. For the Draft IP schedule, we
assumed the estimated dates for the initiation of summer spill and the termination of summer
spill under the Draft IP (Tables 1 and 2) for Snake River sites. The Draft IP schedule for the
Lower Columbia sites included spring spill dates of April 10 to June 15 and summer spill dates
of June 16 to August 31. In addition, when spill volumes alternated at IHR, JDA, and BON, we
assumed the spill schedules used in 2013. Finally, under both scenarios excess spill due to lack
of market was not included, as this is not predictable from year to year. However, excess spill
due to hydraulic capacity was included in both scenarios.

Results from this modeling exercise can be found in Tables 3 and 4. Spill volumes for April,
May, or July are not provided in these tables because these 3 months are not affected by the
proposed changes in the Draft BIOP spill schedule. Furthermore, since spring and summer spill
percentages are the same at JDA and TDA, there were no differences in spill volumes between
the two different schedules at these sites (Table 3). Spring and summer spill operations are also
the same at LGS and IHR. This is why there were no differences in spill volumes at these two
sites for the month of June (Table 3). However, since the Draft IP proposes to terminate summer

spill in August, based on subyearling Chinook collections, there were reductions in the August spill volumes at both LGS and IHR in 2 of the 3 years modeled (Table 4).

Table 3. Estimated June spill volumes (MAF) at FCRPS sites under Court Order schedule versus the Draft 2014–2018 Implementation Plan schedule.

Project	High Flow Year (2011)		Medium Flow Year (2009)		Low Flow Year (2013)	
	Court Order	Draft IP	Court Order	Draft IP	Court Order	Draft IP
LGR	3.85	3.85	1.60	**1.56**	1.15	**1.08**
LGS	3.37	3.37	1.92	1.92	0.98	0.98
LMN	3.26	3.25	1.50	**1.32**	1.29	**1.10**
IHR	5.61	5.61	3.16	3.16	1.74	1.74
MCN	17.18	*17.18[†]*	7.43	*8.17*	5.96	*6.69*
JDA	10.25	10.25	5.81	5.81	5.15	5.15
TDA	11.44	11.44	6.43	6.43	5.44	5.44
BON	13.69	13.69	6.01	**5.89**	5.95	**5.82**

Bold = spill reduction under Draft IP schedule, ***Bold Italics*** = additional spill under Draft IP schedule
[†] There was a slight increase in the June spill volume under the Draft IP schedule, although not shown here due to rounding.

Table 4. Estimated August spill volume (MAF) at Snake River sites under Court Order schedule versus Draft 2014–2018 Implementation Plan schedule.

Project	High Flow Year (2011)		Medium Flow Year (2009)		Low Flow Year (2013)	
	Court Order	Draft IP	Court Order	Draft IP	Court Order	Draft IP
LGR	1.11	**0.19**	1.06	**0.08**	0.61	**0.23**
LGS	0.75	**0.37**	0.58	**0.23**	0.42	0.42
LMN	1.04	**0.58**	0.91	**0.45**	0.54	0.54
IHR	1.92	**1.28**	1.32	**0.72**	0.79	0.79

Bold = spill reduction under Draft IP schedule

The summer spill percentage volume at MCN is actually higher than the spring spill percentage. Therefore, the earlier start date for summer spill under the Draft IP results in higher June spill volumes, when compared to the Court Order schedule (Table 3). August spill volumes at MCN were not different between the two scenarios because both schedules call for 50% spill through August 31st.

For the remaining sites (LGR, LMN, and BON), the Draft IP schedule resulted in reductions in June spill volumes in medium and low flow years (Table 3). However, there were no reductions in June spill volumes in the high flow year (Table 3). This is because flows tend to peak in June and, in a high flow year, it is possible that flows will be above hydraulic capacity for the entire month of June, making it virtually impossible to implement the lower summer spill volumes until flows decrease. This was the case in the high flow year modeled (2011), as flows remained above hydraulic capacity through all of June and into July at these three sites. This general pattern may not be true for all high flow years, as it depends on how long flows exceed hydraulic capacity.

The changes in August spill dates pertain only to the Snake River projects. August spill volumes at the Snake River sites were not affected by the magnitude of the flow year (i.e., high, medium,

or low), as August flows are generally below hydraulic capacity by this time. However, as is illustrated in Table4, reductions in August spill volumes were dependent on the collection counts of subyearling Chinook. This is why reductions in August spill volumes occurred in all 3 of the modeled years at LGR but only 2 of the 3 years at LGS, LMN, and IHR. Due to high collection counts at LGS throughout August of 2013, spill at LGS, LMN, and IHR would have run through August 31st in this year. However, it is important to note that spill at LGR, LGS, LMN, and IHR would have been terminated early in 78%–100% of the last nine years, depending on the site (Table 2). Therefore, the Draft IP schedule will likely result in reductions in August spill volumes, regardless of the flow year.

Potential Reductions in Spill

Selection of a Single Spill Operation at IHR, JDA, and/or BON
According to RPA 29 of the Draft IP, "…changes in spill or the selection of a single spill operation at a project where two operations are currently being implemented may occur either for testing purposes or after performance standard testing confirms that the performance standards are being achieved." This language would apply to spring and summer spill at IHR and JDA and summer spill at BON and could lead to reduced spill at these projects if the lower of the two spill operations is implemented.

Other Comments

Bulk Spill Pattern at LMN
According to RPA 29 of the Draft IP, spring spill at LMN will be to the Gas Cap, under a bulk spill pattern. While this spill pattern is a continuation of the current Court Order, the fish managers have routinely contended that the use of a bulk spill pattern above flows of 60 Kcfs is not ideal. On June 26, 2012, many members of the Fish Passage Advisory Committee issued a Joint Technical Staff Memorandum (http://www.fpc.org/documents/joint_technical/79-12.pdf) that outlined their concerns with the bulk pattern at LMN. Among the concerns with the bulk spill pattern is the increased TDG production and consequent spill curtailments that occur, just as large numbers of smolts arrive at the project. The Salmon Managers have made repeated requests to change this spill pattern (SOR 2011-02 and FPP Change Form 12LMN007) to one that better meets the 2008 BIOP objectives of RPAs 15 and 29, to provide spill to improve juvenile fish passage while avoiding high levels of TDG.

In past years, the Fish Operations Plans have included a statement, as justification for the bulk pattern, that, "Based on a previous year's study results, dam survival is higher under the 'bulk' spill pattern compared to a 'uniform' pattern." The above mentioned Joint Technical Staff Memo pointed out that this statement is statistically inaccurate and misleading. In fact, the authors of the 2009 study at LMN (Hockersmith et al., 2010) clearly state that the results of comparing bulk and uniform patterns indicated no significant difference in direct concrete survival. The point estimates for concrete survival for yearling Chinook under the bulk and uniform patterns were 0.975 and 0.973, respectively (Hockersmith et al., 2010). Further review of Hockersmith et al. (2010) revealed that the point estimate for the bulk pattern was only higher than that for the uniform pattern because the turbine survival estimate under the bulk pattern was significantly greater than 100%, which is clearly an overestimation. In addition, sole consideration of concrete survivals ignored the fact that the uniform pattern passed more fish

over surface routes with less delay (Hockersmith et al., 2010), while also leading to less TDG production.

Literature Cited:
Hockersmith EE, Axel GA, Absolon RF, Burke BJ, Kinsey EF, Lamb JJ, Nesbit MG, Dumdei ND, and Sandford BP. 2010. Passage Behavior and Survival for Radio-Tagged Yearling Chinook Salmon and Juvenile Steelhead at Lower Monumental Dam, 2009. Fish Ecology Division, Northwest Fisheries Science Center, National Marine Fisheries Service, Seattle, WA.

Performance Standards

- Performance tests have utilized radio and acoustic tags, which cannot represent the run-at-large. Smolts are rejected from studies due to size and condition, and therefore represent survival only for the healthiest smolts in the run. Those already affected by disease or injury are those mostly likely to have reduced survival due to dam passage, and their exclusion inflates survival estimates. Recorded rejection rates have ranged from 3.2% to 16.4% of collected fish. Rejection rates are unknown for 2009 studies, which were not conducted for performance standards testing but have been included as such, so their representativeness of the run-at-large cannot be estimated.

- Smolts included in the dam-passage treatment group are released at multiple locations upstream, and some pass through several projects before being included in the test group. This process may eliminate from the sample weaker fish more susceptible to mortality due to tag burden; so only tagged fish most likely to survive dam passage are included in the test group. The inclusion of multiple control groups for each performance test raises concerns that dam passage survival estimates may be artificially inflated. This inflation can be caused by random effects or the unequal mortality between groups from factors such as predation in the tailrace.

- The majority of performance standards tests conducted to date occurred during 2011 and 2012. Both of these years were above-average water flow years. Although the required standards may have been met, it does not address the issue that survival may not be as high when flow levels are lower.

- During periods of high flow, spill levels were not consistent with planned operations during much of the testing in 2011 and 2012. Although required standards may have been met, they do not reflect actual survival estimates that would be expected when spill levels conform to the Fish Operation Plan. Additionally, operations that may affect juvenile survival, such as "open geometry" turbine operations at Bonneville, were not incorporated into performance testing.

- Radio tag studies from 2009 at LMN and LGS should not be included in performance testing requirements. These studies do not report rejection rates, do not include appropriate control groups, have significant tag burdens beyond those of the current JSATS tags, and have utilized different release locations from 2012 studies and therefore are not comparable.

- There is no biological reason given why subyearling Chinook have a dam passage standard of 93% survival, while yearling Chinook and steelhead have a dam passage standard of 96% survival.

- A public database for raw and processed data, when available, with detailed descriptions of post-hoc inclusions and exclusions of data, would make outside evaluation more straightforward. Despite numerous requests and commitments by the Army Corps of Engineers to provide these data, a database of this type has not been made available.

- Dam passage survival, as measured by performance tests, is currently the only required metric of juvenile passage through the hydrosystem. For yearling Chinook and steelhead passing at LGR and all downstream dams, the overall probability of survival as estimated by performance testing must be at least 72% (0.96^8). For subyearling Chinook, this standard is 56% (0.93^8). However, these standards do not include other juvenile metrics which are known to impact adult returns, such as passage route. Smolts that pass through bypass systems are known to return at lower rates than those that pass through spill. However, this has not been incorporated into the Draft BIOP. Performance standards should be a single component of many in determining hydrosystem operations.

Study Fish May Not Represent Run at Large

Juvenile Salmon Acoustic Telemetry System (JSATS) tagging protocols require rejection of fish based on multiple criteria, including size and condition. These rejection rates have ranged from 3.2% to 16%, depending on the year and species. These rejection rates mean that only the healthiest portions of the population are used in performance testing, and survival estimates are inflated to reflect this bias. Actual survival rates are likely much lower than those estimated by performance testing.

Selection on Dam Passage Group May Inflate Survival Estimates

In the Virtual-Paired Release design, fish are released upstream of the dam so they achieve a distribution through passage routes that reflects the run at large. Fish that die between tagging and the forebay of the dam are not included in the study. However, this means that fish that have lower survival through the reaches will not be included in the study. Mortality between tagging and detection was as high as 12.5% in yearling Chinook in 2012. Similar to the effects of tagging only healthy fish, this means that only the healthiest of tagged fish are included in the dam survival estimates.

Performance testing utilizes fish collected at the juvenile bypass unit at JDA (Lower Columbia testing) or LMN (Snake River testing). These fish have successfully survived a minimum of one bypass, as fish that die in the bypass are not included in the study. Therefore, these fish may not represent the survival probabilities of previously undetected fish not included by the study design. Survival estimates greater than 100% for juvenile bypass systems may be due to this aspect of the study design.

Inflated Survival Estimates Due to Experimental Design

The virtual/paired-release design used in most of the performance tests utilizes two control groups: one released in the tailrace of the dam (R_2) and one released further downstream (R_3). The R_3 group is intended to account for any handling mortality experienced by the R_2 group, which could inflate survival estimates.

Under this experimental design, however, upward biasing of survival estimates could be caused by high mortality in the R_2 group. It is unlikely that tagged fish in both stretches of river encounter the same environmental conditions, especially since predation rates at many projects are higher in the forebay and tailrace than mid-reservoir (Petersen 1994, Ward et al. 1995). If survival in the R_2 group is lower than survival in the R_3 group, the ratio of survivals (S_2/S_3) will be biased low and will artificially increase estimates of dam survival. Please see Beeman et al. (2011) and FPC Memos (March 24, 2011; February 15, 2012; March 23, 2012; see http://www.fpc.org/documents/FPC_memos.html) for detailed descriptions regarding upward biases inherent in this study design.

A further cause of differential mortality may be the fact that fish in the R_2 and R_3 groups will not have the vertical or horizontal distribution of fish that are naturally migrating through the hydrosystem. In contrast, fish that pass through the dam are not included in the study group until they have migrated through and survived some distance from their initial release point. At TDA, release of the R_2 group occurs near islands downriver of the dam. At the Studies Review Work Group (SRWG) meeting on February 6, 2012, concern was expressed that this release occurs in an equal distribution across the river, rather than attempting to mimic natural migration patterns. Therefore, it is unlikely that (1) mortality will be equal between release groups and (2) that these releases represent mortality of the run-at-large.

Survival estimates generated with this multiple-release design may further increase dam survival estimates due to random sampling effects, in some cases moving survival estimates upward enough to meet performance standards when they would not have with only one control group. If there is limited handling and transportation mortality, the use of the R_3 group will introduce additional variation to the study. Beeman et al. (2011) concluded that this result is "contrary to the goal of adjusting a paired-release estimate downward to account for handling mortality."

An example of inflation due to experimental design is the performance testing results from MCN in 2012. The single-release survival estimates were 0.9136 (steelhead), 0.9171 (yearling Chinook), and 0.9149 (subyearling Chinook). None of these survival estimates met the performance testing criteria. After corrections with R_2 and R_3, these survival estimates were inflated to 0.9908 (steelhead), 0.9616 (yearling Chinook), and 0.9747 (subyearling Chinook). In this case, the experimental design of the performance test has clearly artificially inflated dam survival estimates. Another example of this inflation is reflected in survival estimates greater than 100%. Survivals of greater than 100% were reported for 3 route-specific estimates in 2011 and 5 estimates in 2012.

High Flows in 2011 and 2012 Limit Applicability of Results

In both 2011 and 2012, spill levels during performance testing were much higher than those outlined in the Fish Operation Plan. Although survival estimates may meet the minimum requirements of performance testing, they reflect survivals only during periods of high flow and high spill, and have limited applicability to normal or low-flow water years. This issue has previous been outlined in an FPC Memo dated February 15, 2012 (see http://www.fpc.org/documents/FPC_memos.html).

Spill and Operations During Testing Have Not Been Consistent

For much of 2011 and 2012, spill levels did not conform to the Fish Operations Plan due to high flows. Although performance tests were met when spill levels were much higher than planned, that does not reflect the survival estimates that would be obtained under planned operations.

In addition, many operations are not included in survival estimates although they may affect juvenile survival. One example is the use of "Open Geometry" turbine operations at Bonneville Powerhouse 1. This operation is assumed to have equal or greater survival than other operations, but no actual tests of juvenile survival have been conducted to test this operation.

Studies Not Conducted As Performance Tests Are Not Comparable and Therefore Do Not Meet Standards

Radio-tag studies from 2009 should not be included as performance tests. These studies were not conducted with the intent to measure performance testing, and differ significantly from later tests. Rejection rates are not available from these studies, so it is impossible to assess how representative they are of the run-at-large. Given that the smaller JSATS tags used in 2010 required rejection rates of 12.6%, it can be expected that rejection rates in 2009 were higher.

The study design for 2009 radio-tag studies were conducted with a single release group, a design that has been since abandoned in favor of the Virtual-Paired Release design to avoid excessive inflation of survival estimates. Although the Virtual-Paired Release design also has the potential to inflate survival estimates, the adjusted survival is calculated using different methods than tests in 2009. Consequently, adjusted survival estimates from 2009 are not comparable to adjusted estimates from 2010–2013.

The significance of where control groups are released has been a topic of discussion in numerous SRWG meetings. The control group must be released in a distribution that is representative of the distribution treatment fish will assume upon passage through the dam. If not, the survival estimates of the control group used to adjust the dam survival are being misused. The release points in 2009 studies are not the same points used for the 2012 studies, and there is no experimental or biological justification for this difference. This may have a significant impact on control group survival rates, and therefore on the adjusted survival rates from 2009. Please see the FPC Memo from March 19, 2013 (http://www.fpc.org/documents/FPC_memos.html) for more detail on this topic.

No Biological Reason for Lower Survival Standard For Fall Chinook

There is no biological reason for fall Chinook to have lower survival standards (93%) than yearling Chinook and steelhead (96%). This reduced standard decreases the probability of strong adult returns, and NOAA provides no rationale for this reduction.

JSATS Data Should Be in a Public Database

Currently, the data collected during performance testing, both raw and filtered, is available only through request to the Army Corps of Engineers. However, this method is time consuming and can lead to confusion regarding analyses (see FPC Memos July 29, 2010; February 16, 2011; March 24, 2011; June 21, 2011 posted at http://www.fpc.org/documents/FPC_memos.html). A publicly accessible database, such as that used for PIT-tag data, would minimize these types of discussions and could potentially make results available sooner than the current speed of receiving final reports.

Management Decisions Should Not Be Based on Single-Dam Performance Standards

Past FPC memos have reviewed performance standards testing throughout the hydrosystem (June 24, 2009; July 29, 2010; March 24, 2011; February 15, 2012; March 16, 2012; March 23, 2012; January 4, 2013; February 11, 2013; March 22, 2013; see http://www.fpc.org/documents/FPC_memos.html). Repeatedly, these memos have raised concerns regarding the usage of these studies for project management decisions. Management decisions should reflect the entire life cycle of the fish, rather than survival at projects considered in isolation. All available data should be utilized, rather than ignored in favor of simplistic performance standards that do not reflect the current understanding of salmonid survival factors.

The long-term effects of passage routes for juvenile fish have been well documented in recent years. Fish that survive juvenile bypass systems or powerhouse passage are less likely to survive the first ocean year, and 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 October 6, 2010; January 19, 2011; July 14, 2011; see http://www.fpc.org/documents/FPC_memos.html). These effects of project operations on these metrics are not included in the current performance testing requirements and therefore underestimate the full effects of dam passage.

Acoustic tag studies provide only short-term survivals for specific projects, and current performance standards do not include important metrics like forebay residence time, travel time, or latent mortality. Performance testing cannot fully inform policy makers about methods for improving adult returns. FPC recommends a decision-making framework for the Columbia Basin that will incorporate the strengths and limitations of each data type as part of a straightforward guide to the results of project operations.

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Smolt Transportation

Impacts of Earlier Start of Transportation

- The Draft BIOP proposes to change the start date for juvenile transportation at LGR to a fixed date of April 21st, which is earlier than what has been implemented under the Court Order since 2007.
- Justification provided for the earlier transport start date focuses on maintaining a 50/50 split in transported and in-river migrants, per the recommendation for a spread-the-risk strategy by the ISAB in 2010. However, the interpretation of the ISAB's recommendation is incorrect. The ISAB never included a specific transport to in-river migration ratio in their recommendations in 2010. They simply stated that transportation should occur with spill, as opposed to turning spill off while maximizing transportation.
- The Draft BIOP acknowledges that an earlier start date for transportation has no benefit for Chinook, yet still proposes an earlier start date.
- Analyses of PIT-tags found that moving the transportation start date to April 21st resulted in an increase in the proportion of fish transported for all groups of hatchery and wild yearling Chinook and steelhead, with the largest impact on wild yearling Chinook. Historically, wild yearling Chinook have shown the least benefit from transportation, particularly in years when transportation began in early April.

- Snake River adults that out-migrated in-river have a higher survival from BON to LGR vs. adults that were transported. The in-river group represents only a portion of the run at large. However, the Draft BIOP uses only adults returning from the in-river juvenile migration group (the known higher survival rate to Lower Granite Dam) to compare to the 2008 Adult Performance Standards.

- Increasing the proportion of transported Snake River fish will lower the overall adult conversion rates which, in some cases, are already below the 2008 BIOP Adult Performance Standards.

- Increasing the proportion of transported fish will increase the population of Snake River hatchery adults that stray above BON. On average, the returning adult population of Snake River hatchery steelhead is more than ten times the combined naturally spawning steelhead population for the Deschutes and John Day river basins, and therefore small increases in straying can potentially have large impacts on downriver populations.. NOAA has identified out-of-DPS (distinct population segment) hatchery strays as a limiting factor to the recovery of the Deschutes and John Day River steelhead populations (NMFS 2009; Appendix A).

Impacts of Earlier Start of Transportation

Background and Justification to Earlier Start of Transportation
The Draft BIOP cites RPA 30 of the Draft IP which specifies that juvenile transport will begin at LGR on April 21st, with transport from LGS and LMN beginning 4 and 7 days after LGR, respectively. Since 2007, the Fish Operations Plan has specified a transportation start date beginning no earlier than April 21st but no later than May 1st at LGR. In each of these years, the actual start date has been determined through coordination with the Technical Management Team. Since 2007, transportation at LGR has generally begun on May 1st, with only one exception (in 2010 when transportation from LGR began on April 25th). Transportation in 2006 began on April 21st and prior to 2006, transportation at LGR generally began in early April.

Section 3.3.3.4 of the Draft BIOP provides background and justification for this change from the current Court Order. One justification provided for the earlier fixed transportation start date is the ISAB's recommendation in 2010 to continue a spread-the-risk strategy. However, the Draft BIOP misinterprets this spread the risk recommendation to mean the maintenance of a specific 50/50 split between transport and in-river migration. The ISAB was convened in February of 2010 to determine whether the cessation of spill from May 7–21, as outlined in the 2008 BIOP, was warranted, given that flows were predicted to be low in 2010. Data from out-migration years 2005 (no spring spill) versus 2007 (spring spill provided) were presented as a justification for providing spill in May, even in a low flow year. In their 2010 report, the ISAB concluded: "…using combinations of transport and in-river migration with spill spreads the risk across species, stocks, and the ecosystem, while offering an approach that can shed light on uncertainties in the longer-term dataset" (ISAB 2010). This statement clearly indicates what the ISAB meant by a spread-the-risk strategy, one that involves a combination of spill and in-river migration with spill. Nowhere in the ISAB's conclusions is there any mention of a specific transport to in-river migration ratio that is needed to meet a spread-the-risk strategy.

A second justification provided for the earlier fixed transportation start date is the maximization of transport/in-river ratios (TIRs). As an example, the Draft BIOP points out that 2006 is the only year among recent years where the TIRs for steelhead and Chinook were below 1.0, which indicates no benefit of transport (transportation in 2006 began on April 21st). The Draft BIOP goes further to highlight that there is a documented seasonal benefit from transport for Chinook, where no benefit is seen prior to May 1st. Given that the data presented by the Draft BIOP seem to support a May 1st start of transportation, there is no biological basis for the April 21st start date that is proposed. In fact, an April 21st start date is contradictory to the point that the Draft BIOP makes regarding the earlier start date in 2006. It appears that the only reason the Draft BIOP proposes an earlier start date is to better meet the target of a 50/50 split between transport and in-river migration. However, as stated above, the desire for a 50/50 split is a misinterpretation of the ISAB's recommendation for a spread-the-risk strategy.

On July 1, 2013, the FPC issued a memo in response to a data request to estimate the impact of moving the start date of transportation to April 21st, over the last five years (2008–2012). These analyses found that moving the transportation start date to April 21st resulted in an increase in the proportion of fish transported for all groups of hatchery and wild yearling Chinook and steelhead, with the largest impact on wild yearling Chinook. Historically, wild yearling Chinook have shown the least benefit from transportation, particularly in years when transportation began in early April (Tuomikoski et. al. 2013). Finally, a transportation start date of April 21st resulted in an estimated proportion destined for transport of greater than 50% in 3 of 5 years for hatchery yearling Chinook, 4 of 5 years for wild Chinook, 3 of 5 years for hatchery steelhead, and 4 of 5 years for wild steelhead.

Impacts on Adult Conversion Rates and Straying Effects
The Draft BIOP notes that estimates of adult survival are above what was expected for Snake River fall Chinook, and Upper Columbia spring Chinook and steelhead. In addition it notes that although adult survival for Snake River Chinook, steelhead, and sockeye are below expectations, this is not considered an RPA implementation deficiency. The Draft BIOP goes on to state that there is no obvious explanation for low adult survival for these Snake River stocks and that a variety of factors could be affecting adult passage including the river environment, structural modifications, errors in the harvest or stray rate estimates (which as presented in the BIOP are added back in to adult survival thereby removing their effects), run timing, or concurrent effects of several factors at once.

Many studies have concluded that the transportation of out-migrating juvenile salmon and steelhead negatively affects the adult's return migration (Quinn et al. 1989; Johnson et al. 1990; Solazzi et al. 1991; Mundy et al. 1994; Chapman et al. 1997; Keefer et al. 2008; Tuomikoski et al. 2012). This phenomenon is one component of delayed mortality due to transportation at FCRPS collector dams and has been measured with both radio tags and PIT tags relatively recently (Keefer et al. 2008; Tuomikoski et al. 2012). The decreased survival for adults with a transport history can also be seen by comparing the adult survival of transported and in-river out-migrants as estimated with PIT tags in the Supplemental Comprehensive Analysis (NMFS 2008 SCA, Adult Survival Estimates Appendix). Curiously, the Draft BIOP uses only adult survival for in-river out-migrants to evaluate the RPA which does not represent adult survival of the run as a whole or include detrimental effects of transportation on Snake River stocks (Figure 3.3-1 in

the Draft BIOP). Given all the available evidence, increasing transportation for Snake River stocks will likely decrease the overall adult survival.

The decrease in adult survival for the portion of the run that is transported could be due to impaired homing, straying, mortality, longer travel times/greater exposure to harvest or a combination of factors. Transported Snake River steelhead stray more often than their in-river counterparts and tend to enter the Deschutes and John Day river systems (Keefer et al. 2008; Tuomikoski et al. 2012; Keefer and Caudill 2012). Keefer and Caudill (2012) noted that natural straying occurs geographically near the natal site whereas transported fish stray into much more distant rivers. This unintended consequence of the transportation program has the potential to negatively affect much smaller downriver stocks. Using a modeling exercise, Keefer and Caudill (2012) found that strays from large donor populations can numerically overwhelm native fish in small recipient populations, even at low (~1%) stray rates. The size of the combined Deschutes and John Day River spawning populations (~7131; NOAA 2009 Appendix A) is more than an order of magnitude smaller than the numbers of returning Snake River hatchery steelhead adults that pass these basins (~134,145; geomean of hatchery steelhead count at LGR from 2000–2012). Finally, transported Snake River steelhead stray 2–11 times more often than their counterparts that out-migrated in-river (Keefer et al. 2008; Tuomikoski et al. 2010).

The Middle Columbia River Steelhead ESA Recovery Plan (NMFS 2009; Appendix A) concluded that a significant portion of spawners in the Deschutes and John Day River populations were out-of-DPS strays and identified out of basin hatchery steelhead strays as a limiting factor for the recovery of these subbasin populations. Increasing the numbers of transported hatchery Snake River steelhead increases the potential impact of a large Snake River hatchery steelhead population on smaller subbasin populations (Figure 1).
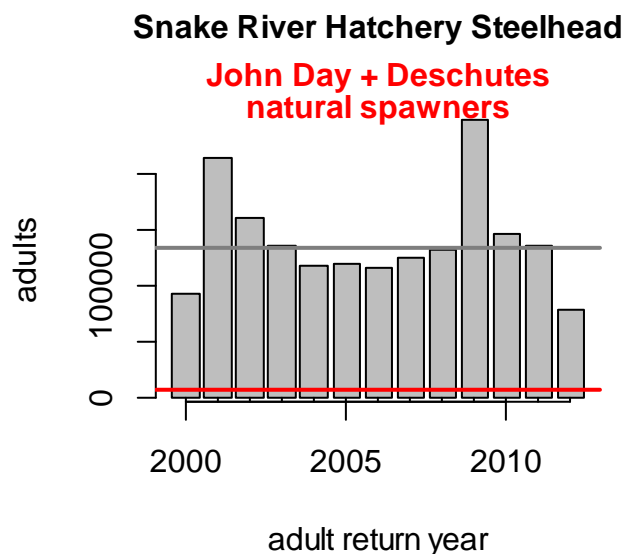

Figure 1. The grey bars are the adult counts at Lower Granite Dam for hatchery steelhead from 2000 through 2012; the grey line is the geometric mean. The red line is the combined geometric mean of spawning populations for the John Day and Deschutes River basins from NMFS (2009 Appendix A).

General Editorial Comments

- The Action Description section of RPA 30 (Draft IP) references a Table 3 and Table 4. These tables no longer exist and, thus, these references should be removed.
- The Action Description section of RPA 30 (Draft IP) mentions that the Corps and BPA will continue to collect and transport juvenile fish at MCN. However, the Adaptive Management section states that transportation for MCN will not occur in 2014–2018.

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Benefits of Spill for Fish Passage – Experimental Spill Management

- NOAA's use of the spring Chinook returns from 2011 as the basis for rejecting experimental spill management is unfounded because they fail to consider other hydrosystem operations that were adverse for 2011 spring outmigrants. NOAA also fails to recognize the record fall Chinook adult return from the 2011 outmigration, which also experienced high flow and spill throughout the migration period.
- NOAA fails to account for the uncontrolled spill levels due to uncontrolled flows and flood control operations, which were exacerbated by unit outages at projects. The adverse hydrosystem operations resulted in TDG levels often exceeding 130%. In addition, the federal action agencies were unable to; manage debris build-up in the forebays of projects, complete powerhouse outages, and the removal of fish screens sending more fish through turbine units. NOAA failed to address the role these factors played in spring Chinook survival during the 2011 outmigration.
- NOAA fails to recognize or address the adverse outmigration conditions that occurred in 2011 that were a result of the present FCRPS system configuration and operation. NOAA is inappropriately comparing the conditions that occurred in 2011 to a voluntary and controlled spill program as considered under Experimental Spill Management, where 125% TDG is considered as a maximum implementation level.

The Draft BIOP contains the following statement: "We note the adult returns from the year 2011, a year which had high levels of spill and flow, has produced below average adult return rates. Results such as this reinforce our current management approach to hydrosystem operations. Substantial progress has been made in improving survival of juvenile anadromous fish in the hydrosystem."

It is true that 2011 was a year with high flow and high spill, and thus far the spring Chinook adults that have returned in 2013 appear to be below average. Spring Chinook are the only stock with complete adult counts for 2013 as of this date, with 83,345 adult spring Chinook returned to

Bonneville Dam. The 10-year average count is 141,713. Steelhead returns also appear to be less than the 10-year average, but the B run steelhead that primarily migrated as juveniles in 2011 are still migrating and counting continues.

NOAA's subsequent conclusion that 2011 returns of spring/summer Chinook are a reason to dismiss the experimental spill approach in favor of the current management approach ignores what could likely be the effect of other conditions that occurred in 2011. Furthermore 2011 clearly indicates that the current management approach is flawed, because the current management approach did not provide adequate protection for downstream migrants under the current FCRPS configuration. In 2011 debris could not be managed, screens were removed, and operations were implemented that were adverse to fish.

The Draft BIOP alludes that this is a reason for not addressing Experimental Spill Management, but makes no attempt to address the several distinctions that must be made between the conditions that occurred in 2011 and those under a voluntary spill program as considered in the Experimental Spill Management analyses. NOAA neglects to point out that at least 50% of the juvenile yearling Chinook were past MCN and BON by mid-May. Spill and TDG were generally less than 120% through this period and would have had only positive benefits for fish survival.

It is also important to note that implying the low adult returns from 2011 were a function of high flow and spill is not based on any analysis of the data. Figure 2 shows the juvenile passage dates at LMN for juvenile PIT-tagged Snake River yearling Chinook that were detected as adults at BON this year. While over 60% of the returning PIT-tagged adults that were detected at BON this year passed through the Snake River as juveniles when TDG was below the current States' standards of 120% in the tailrace, the other 40% were migrating in the Snake River when TDG levels exceeded 125% and, at some projects, exceeded 130%. This suggests that NOAA has made the statement in the Draft BIOP prematurely and their dismissal of the Experimental Spill Management on the basis of the low returns from 2011 is unfounded.
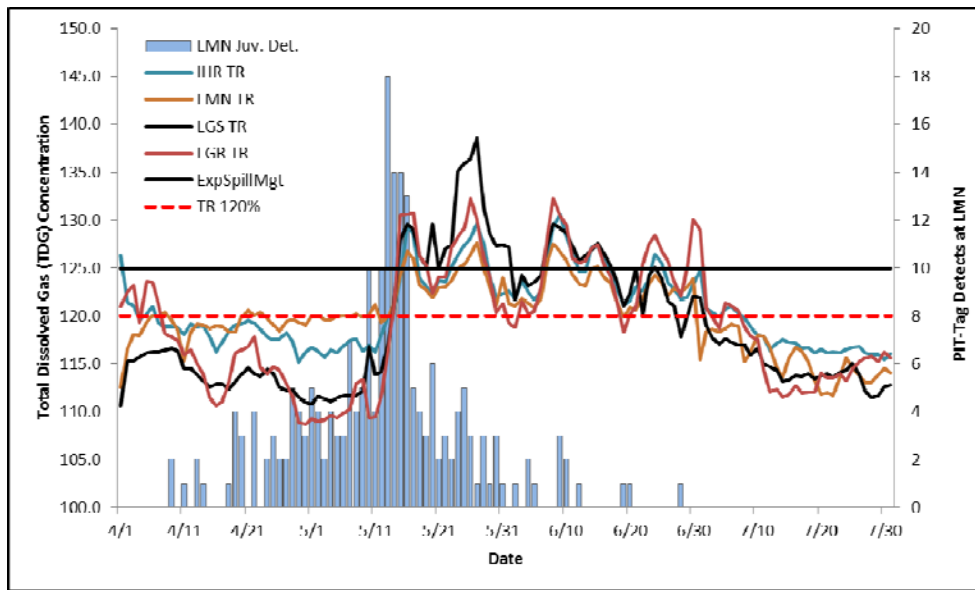
Figure 2. Juvenile passage date (2011) at Lower Monumental Dam of Snake River spring Chinook detected at Bonneville Dam in 2013.

Additionally, the record returns of fall Chinook to the Snake River were predominantly (about 60% based on PIT-tagged adult fish at BON) from 2011 juvenile outmigrants returning as adults. The juvenile passage timing for subyearling migrants was earlier than average and reflected the high flow conditions (Figure 3). More than half of subyearling migrants passed through the hydrosystem during June and the first half of July, and were subjected to the high levels of TDG.
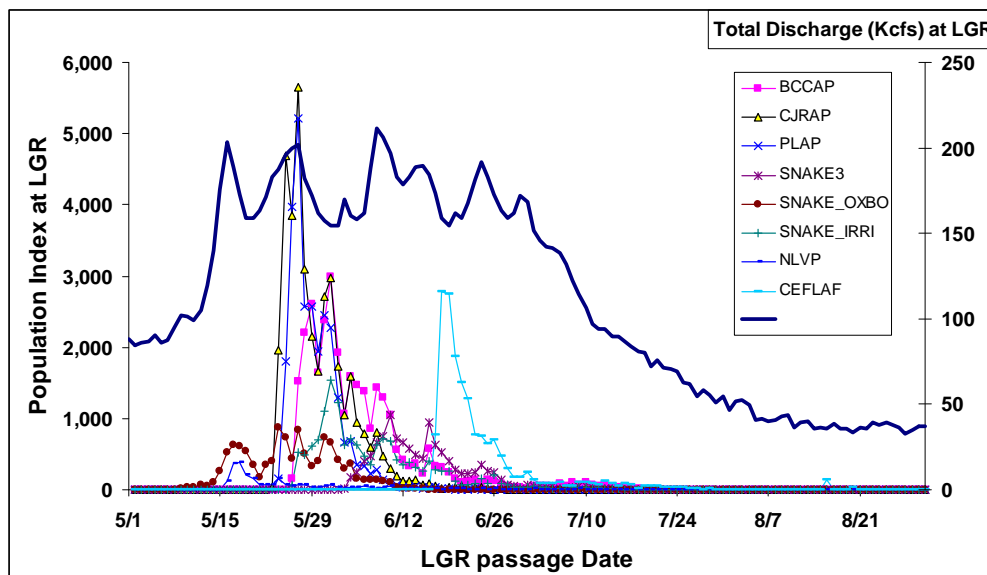

Figure 3. Passage timing of Snake River Fall Chinook PIT-tagged above Lower Granite Dam in 2011 in comparison to high flows that year. Pit-tag release groups include: BCCAP – Big Canyon Creek Acclimation Pond; CJRAP – Captain Johns Rapids Acclimation Pond; PLAP-Pittsburgh Landing Acclimation Pond; SNAKE3-Snake River Releases of Production Fish; SNAKE_OXBO – Snake River releases of fish reared at Oxbow Hatchery; SNAKE_IRRI- Snake River releases of fish reared at Irrigon Hatchery; NLVP-North Lapwai Valley Acclimation Ponds; CEFLAF-Cedar Flats Acclimation Ponds.

Data are too preliminary to determine what the overall SARs for Snake River fall Chinook PIT-tag groups will be, but it is likely the SARs will be relatively high compared to other recent years. In-river fish comprise a large portion of the return. Preliminary SAR analyses of six PIT-tag release groups from migration year 2011 showed five of six groups had transport/in-river-ratios less than one indicating higher return rates for in-river migrants than for transported fish. While it is too early to make any statements about conditions and the return of fall Chinook, the information is introduced here to suggest that NOAA's reliance on 2011 spring Chinook returns to reject Experimental Spill Management is unfounded particularly since fall Chinook, migrating during the peak of the high flow and spill event in 2011, are returning at notably high rates.

High flow conditions are advantageous for fish survival. However, the present configuration and operation of the FCRPS presents issues in a high flow year. High flows often exceed the hydraulic capacity of projects and the excess water must be passed via the spillway. The high spill levels are uncontrolled and cannot be addressed even when criteria for fish condition monitoring for by gas bubble trauma (GBT) are exceeded, as happened in 2011. In fact in 2011 the total dissolved gas levels exceeded the 120% tailrace criteria for extended periods of time during the spring migration period, and, from mid-May until the end of June, the TDG was well in excess of the 125% level, often exceeding 130%. Biological criteria were also exceeded at some projects during this period (FPC Annual Report 2011).

The following graph (Figure 4) shows the tailrace TDG levels that occurred through June 2011 and uses the 125% Experimental Spill Management level for reference. The 125% is the highest level considered in the Experimental Spill Management analyses for fish survival in a risk-based spill program. Consequently, comparing 2011 to what happens under a planned, voluntary spill program in Experimental Spill Management is not appropriate.
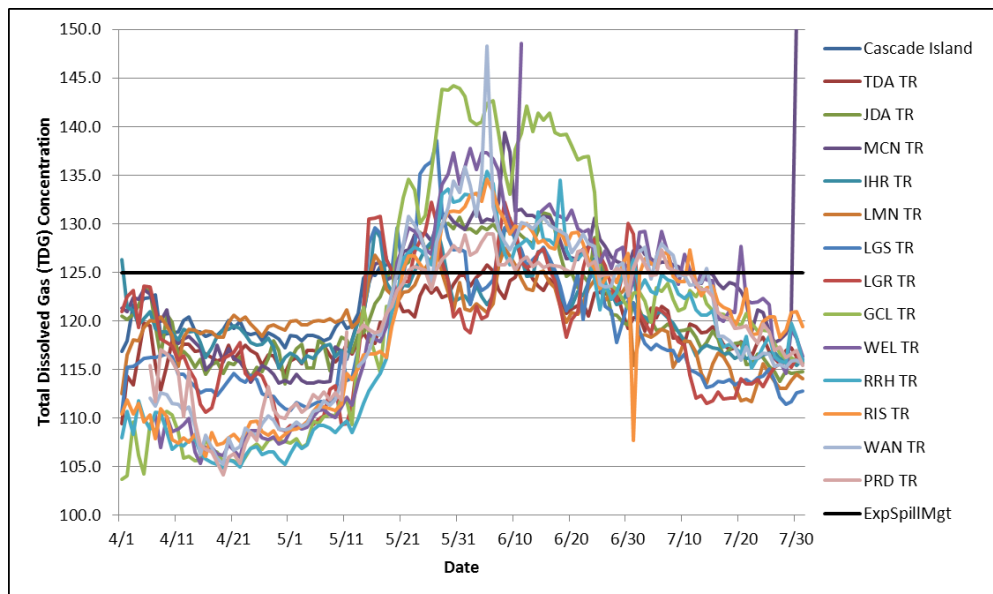

Figure 4. Total dissolved gas (TDG) concentration at each of the TDG tailrace monitors in the Columbia River Hydrosystem, and the experimental spill management level of 125%.

In addition, there were several specific hydropower project configuration operation-related issues that occurred in 2011 that likely contributed to adverse passage conditions for fish, including:

1. Grand Coulee Dam was operated for flood control during most of the spring and early summer season. This resulted in high levels of TDG entering the Upper Columbia River below Chief Joseph Dam, and the high levels of spill and TDG continued through the Upper Columbia projects. TDG levels in the tailraces of projects upstream of Rock Island Dam (where GBT samples are obtained for the Smolt Monitoring Program) exceeded 135% on several days during this period. These high levels of TDG are reflected by the increased GBT incidence in several samples collected between May 31st and June 23rd.

2. There was a very limited hydraulic capacity at LGR due to two out of six units being out of service during the majority of the fish migration season. This resulted in high levels of spill throughout the migration season, with tailrace TDG levels near or greater than 130% for over a month.

3. The Little Goose powerhouse experienced two separate problems that affected all six generating units. This meant that the entire flow in the river was spilled at this project from May 24th at 0600 hours to 1200 hours on June 1, 2011. The TDG below Little Goose exceeded 130% for a 5-day period and reached a 12-hour average high of 138.6%. TDG levels greater than 130% were also observed at the LMN forebay gage during this time period. Both the incidence and severity of the signs of GBT (up to Rank 3 signs) increased during this period. The COE originally expected the repairs to be completed in just a few days, however, during repairs the transformer core was exposed to the atmosphere under wet weather conditions and additional work requiring several additional days was needed to remove all moisture from the transformer core insulation. The biological criteria for spill management were exceeded at LMN as a result of this hydro operation. In a controlled spill program the spill levels would have been reduced, whereas in this operation there was no ability to address these issues.

4. At MCN there were turbine outages throughout the spring and summer, limiting powerhouse capacity. The high river flows that occurred for flood control operations and the limited hydraulic capacity of the project, resulted in uncontrolled spill in excess of hydraulic capacity during early April and from early May until later in July. Tailwater TDG levels often exceeded 130% during this time.

5. There were other factors that occurred in 2011 that are not associated with TDG production that likely contributed to the less than average survival observed by NOAA. High flows are associated with a high debris load from terrestrial runoff. In an undeveloped system the debris would pass through the system, whereas in the developed system debris accumulates at the upstream end of a project and interferes with the safe operation of the hydroproject for fish passage and survival.

6. Considerable debris was collected on fish screens at BON and the COE did not have the resources to remove the debris in a timely manner so as to not affect the juvenile fish

migration. Consequently, due to the increased debris accumulation, the fish screens at BON were removed beginning on May 19th until July 12th. During this period the Bonneville first powerhouse was operated in an overload situation (best geometry), which has unknown impacts for juvenile migrants since no data has been collected on passage through the project under these operations. In addition, the removal of fish screens and its impact are unknown because adequate sampling below BON does not occur in such a way that allows survival estimation.

To summarize, NOAA makes definitive but unfounded conclusions regarding the below average returns of spring Chinook and the attribution of the low returns to high flow and spill that occurred in 2011. Several hydrosystem-related issues occurred during that year that likely contributed to the high TDG levels observed. In addition the potential for project-related mortality from dam operations was high that year. Moreover, the ongoing record return of fall Chinook, with a preponderance of the 2011 cohort in the population, confounds NOAA's statement.

Most importantly, however, is that a high uncontrolled flow and spill year (such as 2011) with the present project configuration and operation is not at all comparable to a voluntary spill program. Experimental Spill Management would be implemented to limit TDG levels, would not exceed 125%, and would be implemented without the configuration and management issues that occurred in 2011.

The Draft BIOP excludes consideration of recent data and analyses and maintains the status quo

The hydrosystem portions of the Draft BIOP rely on three separate documents prepared by consultants for the Action Agencies. They focus unsuccessfully on discounting analyses and conclusions in Haeseker et al (2012). These are:

> Manly, 2012. Review of *Assessing Freshwater and Marine Environmental Influences on Life-Stage-Specific Survival Rates of Snake River Spring–Summer Chinook Salmon and Steelhead*.

> Skalski et al., 2013. Limitations of correlative investigations of identifying causal factors in freshwater and marine survival of Columbia River salmonids.

> BioAnalysts, Inc. and Anchor OEA, LLC, 2013. Federal Columbia River Power System Improvements and Operations Under the Endangered Species Act – A Progress Report, 2013 (Progress Report).

FPC has reviewed each document and provide the following detailed comments on each document. Our overall summary conclusions are listed below followed by specific comments on each document.

- None of these documents, either considered in total or separately, provide a valid, scientific basis for excluding recent data and analyses from the Draft BIOP.

- Manly 2012 is supportive of recent data and analyses and primarily comments that additional data used in the analyses would be useful. These data were presented graphically, but journal policies did not allow tabular presentation of the same data. The Skalski et al. (2013) document contains several problematic analytical issues that raise serious concerns about the validity of its conclusions.

- The Progress Report is not accurate. It inappropriately relies on Skalski et al. (2013) to discount or exclude new data and analyses that raise serious questions regarding the validity of fundamental components of the Draft BIOP. A significant body of technical review comments and concerns have been raised since 2008 which are not addressed in the progress report, in particular those regarding the application of at-dam performance standards.

Manly (2012)

Manley (2012), was generally a positive review of Haeseker et al. 2012. The Manley review states (bold added by FPC):

> "Although I have suggested a reanalysis of the data it seems likely that the results obtained will not change much, and, in particular, that the **Spill and PDO variables will still be estimated to be important to the smolt to adult survival rates**."

The overarching concern of the Manley (2012) review was that Haeseker et al. (2012) did not include the detailed dataset and tables of all survival estimates, their variances, and the environmental variables for examination and alternate analyses. Due to space limitations in peer reviewed journal articles, tabular presentation of all the raw data and every estimate was not presented in Haeseker et al. (2012). However, all data are available upon request from the lead author.

Skalski et al. 2013

General Comments

Skalski et al. (2013) presents complicated statistical treatments to discount the conclusions of Haeseker et al. (2012). They utilize correlation analyses to argue against correlation analyses. Skalski et al (2013) select a variable that the subject population is not exposed to, in order to illustrate that correlation is not causation. In this analysis Skalski et al. (2013) shows that fish that are transported as smolts show better survival to adult when spill levels are high. Based on this analyses Skalski et al. (2013) concludes that the benefits of higher spill must be false since transported fish would not experience the higher spill levels. Although these statistical treatments may be impressive to some, the basic approach suffers from logical errors and does not provide a convincing argument against the Haeseker et al. (2012) analyses that documents the benefits of spill at multiple life stages. We provide specific comments on these analyses in the following discussion.

Skalski et al. (2013) selects a variable that the subject population is not exposed to, in order to illustrate that correlation is not causation. This is a fundamental principal, but good principals can also be used to buttress bad arguments (Gould 1991). This is similar to historic arguments regarding smoking and the occurrence of lung cancer in which Ronald A. Fisher (preeminent statistician and paid consultant for the Tobacco Standing Committee) argued that the considerable body of decades of data that showed a significant correlation between smoking and lung cancer did not establish causation. On that basis, legally required warnings and recognition that smoking caused lung cancer was delayed for decades at obvious costs. Skalski et al. (2013) argues against observational studies while recognizing that in the Columbia River system controlled experiments are not possible. Observational studies are scientifically well established, accepted, and extensively used in the fields of ecology, toxicology, paleontology, geology, and epidemiology in particular (Cochran 1983, Eberhardt and Thomas 1991, Rothman and Greenland 1998, Woodward 2005, Jewel 2005).

We do not believe that the complicated statistical treatments in Skalski et al. (2013) provide anything meaningful in terms of spill or transportation effects. For example, Skalski et al. (2013) failed to consider the fact that transported smolts and in-river smolts have shared experience in their downstream migration. They share a river experience as they migrate together to the first transportation collection site and share their migration experience below Bonneville Dam where transported fish are released and migrate with in-river migrants. In general high spill and high flow occur together in the Snake and Columbia rivers. Fish migrating in high flow conditions could arrive at the upstream transportation collection site faster and in better condition, and possibly better withstand the rigors of the transport system. In addition, in high flow years, which usually occur with high spill, transported and in-river smolts could experience better migration conditions through the lower Columbia River below Bonneville Dam due to higher flows. In attempting to disclaim the benefits of spill for fish passage, Skalski et al. (2013) presented a correlation analyses of an effect not experienced by their subject population but may not have considered the possibility that their spill variable actually represented a third variable, of high flow.

Specific comments

- Skalski et al. (2013) makes the comment that yearling Chinook have a maximum juvenile survival when spill is 20%, yet the model used for this same analysis predicts a maximum Chinook survival at 0% percent spill, which is obviously incorrect.

- Our attempt to replicate the multicollinearity analysis of Skalski et al. (2013) suggests that incorrect data or an incorrect calculation was used. Once this error is resolved, the section on multicollinearity is moot due to low levels of multicollinearity.

- Three methods of analyses show that the data in Haeseker et al. (2012) support linear relationships, but do not support higher-order processes, optima, thresholds, or spline relationships for the hydrosystem operations variables under management control (spill and water transit time).

- The comment by Skalski et al. (2013) that model averaging shouldn't be used is not referenced and is not supported by data or analyses. This point is at odds with a large body of peer-reviewed publications that use these techniques.

- Many studies support the use of the model averaging methods employed by Haeseker et al. (2012) as an appropriate and rigorous procedure to account for model selection uncertainty and to improve inference.

- Skalski et al. (2013) uses the length of wild and hatchery smolts marked and transported at LGR as a surrogate for fish condition. Since hatchery smolts are clearly larger than their wild counterparts, length does not seem to be a useful or realistic surrogate for fish condition.

- An analysis of the dataset of length for wild and hatchery smolts marked and transported at LGR found a correlation between smolt length and offshore upwelling. The authors seem to be suggesting that the lengths of wild and hatchery fish are somehow related to nearshore ocean processes, which defies logic and conflicts with Skalski et al.'s (2013) earlier point of causation and correlation.

- The authors' finding of a correlation between transported SAR and in-river variables isn't unexpected as the distance from barge release to the river mouth is approximately one-third the distance from LGR to the river mouth. It is not a biologically reasonable hypothesis that smolt survival is independent of river conditions, in particular flow or water transit time, for the remaining 145 miles from Skamania Landing to the river mouth.

- The list of predictions presented by Skalski et al. (2013) are illogical convolutions of the hydrosystem-related delayed-mortality hypothesis of Budy et al. (2002). The analyses presented to investigate their predictions fail to use as they stated earlier, "appropriate spatial and temporal scales of the survival processes…in order to measure the potential covariates at the right geographic scale."

- The power analysis presented in Skalski et al. (2013) greatly exaggerates the amount of time that would be required to detect changes in survival at multiple life stages associated with a spill management experiment.

Yearling Chinook have maximum survival at 20% spill: Williams et al. reference

Skalski et al. (2013) references Williams et al. (2005) to note that, "For yearling Chinook salmon, survival reached a maximum at 20.6% spill." The general additive model (GAM) that Skalski et al. (2013) refers to also predicts a maximum Chinook survival when spill is zero (Figure 5). Many lines of evidence based on empirical data show that zero spill is detrimental for juvenile survival and in fact we are not aware of any other analysis available that would show that the highest possible survival would occur under zero spill operations.
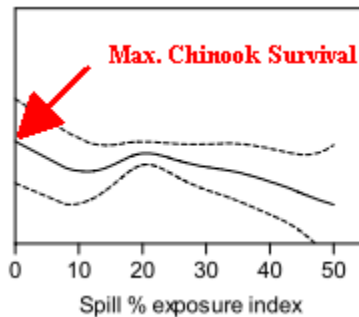
Figure 5. Results of generalized additive model for Chinook showing the highest survival is when spill is zero. From Figure 37 in Williams et al. (2005). Williams et al. (2005) notes that the y-axis units are not meaningful but that the relative influence of spill % on survival can be ascertained from the model. Red arrow and text added by FPC.

For their own GAM analysis Skalski et al. (2013) used survival data from FPC. However their method for combining separate reach survival estimates of LGR to MCN and MCN to BON to achieve LGR to BON estimates is questionable. The FPC data are from 1-week cohorts in the Snake River and 2-week cohorts in the Columbia. Skalski et al. (2013) combined data into an annual LGR to BON single cohort by multiplying the geomean survivals for each reach. This hybrid survival estimate was then coupled with environmental covariates which were weighted by the number of dams in each reach. With this sort of manipulation it is dubious if the resulting data resemble the initial data set from FPC. There is no mention of the methods used to fit their GAM (e.g., how many knots were allowed in the models? How did they assess as fit?). Since they used such an unusual method for reach survival calculations it seems unlikely that any form of weighting was used to account for different variances in the survival estimates. Estimates for the MCN to BON reach were much more variable than those for the Snake River. And recent estimates were less precise. There was no mention of how these were accounted for, which could lead to biased model coefficients if all observations were treated equally. In addition, there is a substantial risk of overparameterization associated with fitting thirteen observations with a three-variable generalized additive model. This overparameterization would be expected to result in increased bias and poor predictive performance.

Multicollinearity

On May 31, 2013, Rebecca Buchanan (University of Washington) requested the survival estimates from Haeseker et al. (2012) in order to "perform analyses using the same data that you used" and "to explore additional relationships associated with hydrosystem operations." Those survival estimates, their variances, and the environmental variables were sent to Rebecca Buchanan on June 6, 2013. Despite the fact that the necessary data was provided to them, Skalski et al. (2013) failed to examine whether multicollinearity was an issue in the data analyzed in Haeseker et al. (2012).

Skalski et al. (2013) states that serious concerns over multicollinearity arise when the variance inflation factor for any of the covariates exceeds the value of 10 (Neter et al. 2004). To address whether multicollinearity was an issue in the analyses presented in Haeseker et al. (2012), we calculated the variance inflation factors as specified in Neter et al. (2004). The variance inflation

factors for the environmental variables used in Haeseker et al. (2012) were all less than 3.5 (Table 5) indicating that multicollinearity was not a serious issue in the regression results presented there. It is important to note that even when correlations among predictor variables are present, these correlations do not inhibit the ability to obtain a good fit, the ability to make predictions of new observations, or the ability to make inferences about mean responses (Neter et al. 1996).

Table 5. Variance inflation factors for environmental variables analyzed in Haeseker et al. (2012).

Variable	Chinook	Steelhead
Day	1.2	1.2
% Hatch	1.1	1.8
WTT	3.2	3.5
Spill	3.0	3.3
SST	1.6	2.2
PDO	1.4	2.2
Upwelling	1.3	1.3

Skalski et al. (2013) devotes a large section to the idea of multicollinearity and the potential confounding results on models. Despite having the data from Haeseker et al. (2012), Skalski et al. (2013) did not conduct analysis to examine whether or not their concerns regarding multicollinearity were valid. Instead, as proof of concept, Skalski et al. (2013) constructs a new data set of several in-river, oceanic, and terrestrial variables. Multicollinearity in the new Skalski et al. (2013) dataset is tested using the variance inflation factor (VIF) for each variable. When VIF exceeds 10 then there are multicollinearity issues. Skalski et al. (2013) states that VIF values among the in-river variables in the dataset ranged from 30.31 to 185.69 suggesting "extreme" issues of multicollinearity. ***In fact these VIF values are so high that an error in calculation or an incorrect dataset is likely.*** A VIF value of 185.69 between two predictor variables means that a linear regression of those two variables would have an R^2 value of 99.46%. (Table 6).

Table 6. Calculations of VIF presented in Skalski et al. (2013) and those calculated by FPC (highlighted) using the data from Table 2 presented in Skalski et al. (2013). Skalski et al. (2013) notes that model results would be a matter of concern when VIF exceeds 10. FPC calculations of VIF for in-river variables are always less than 10 and 20–27 times smaller than those presented by the authors.

Publication	Range in VIF for Ocean	Range in VIF for In-river
Skalski et al. 2013	1.09-1.60	30.01-185.69
FPC	1.09-1.60	1.56 - 6.90

Skalski et al. (2013) provides a correlation matrix for the new dataset in their Table 2. Estimates for VIF can be easily calculated as the diagonals from the inverse matrix of portions of Table 2. FPC calculated the VIFs among the ocean variables and then among the river variables from the correlations presented in Table 5. As compared to Skalski et al. (2013) our calculation was identical for the ocean variables and 20 to 27 times smaller for in-river variables (Table 6). The

values we calculated for in-river variables were all less than 10, the cutoff suggested by the authors.

Comments on Generalized Additive Models and non-linear relationships

Skalski et al. (2013) faults regression analyses that ignore higher-order processes, the possibility of optima, thresholds, or spline relationships and recommend that advanced regression techniques such as generalized additive models be used. When the data support these approaches, they may indeed improve model fit. However, when the data do not support these approaches, overparameterization, increased bias, and poor predictive performance will result. Skalski et al. (2013) fails to mention these risks.

FPC used three methods to assess whether non-linear, higher-order processes or generalized additive models would have improved model fit in the data presented in Haeseker et al. (2012). First, we conducted simple linear regressions between the hydrosystem operations under management control (water transit time and average percent spill) and the stage-specific survival rates presented in Haeseker et al. (2012). These simple linear regressions suggest that there is a linear response between hydrosystem operations and freshwater survival, ocean-adult survival, and smolt-to-adult survival rates (Figures 6 and 7). These data also do not show indications of higher-order processes, optima, or thresholds. Contrary to the coarse-scale, and likely overparameterized, analysis on freshwater survival presented in Skalski et al. (2013), the data in Haeseker et al. (2012) indicate increasing survival at all life stages with increasing spill levels and reductions in water transit time (Figures 6 and 7). The data simply do not support Skalski et al.'s (2013) conclusion that survival is maximized at 35% spill levels.
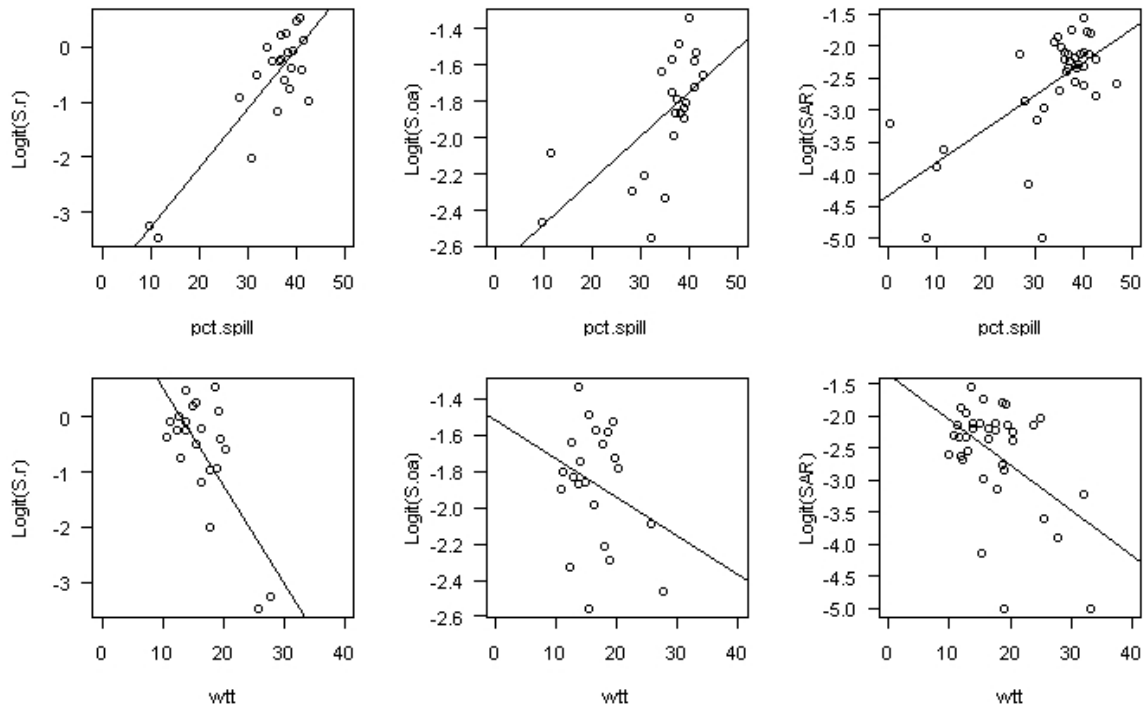
Figure 6. Simple linear regressions of logit-transformed freshwater survival (S.r), ocean-adult survival (S.oa), and smolt-to-adult survival (SAR) versus average percent spill and water transit time for spring/summer Chinook salmon using data presented in Haeseker et al. (2012).
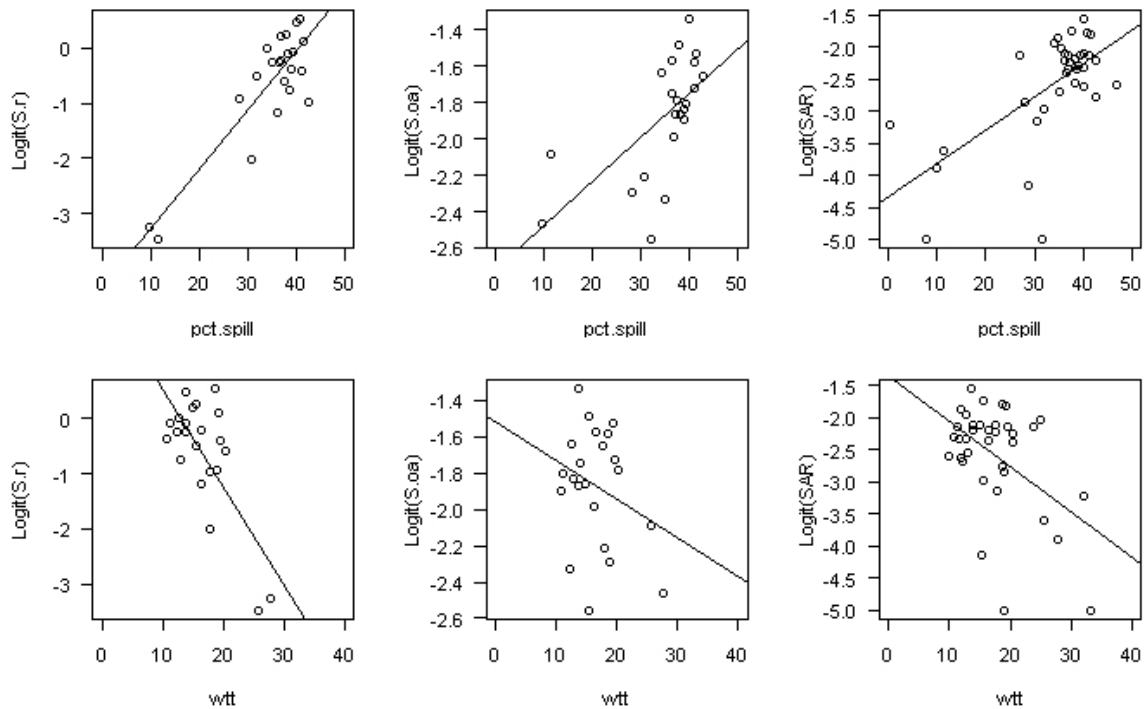

Figure 7. Simple linear regressions of logit-transformed freshwater survival (S.r), ocean-adult survival (S.oa), and smolt-to-adult survival (SAR) versus average percent spill and water transit time for steelhead using data presented in Haeseker et al. (2012).

As a second method to assess whether higher-order processes were supported by the data presented in Haeseker et al. (2012), we conducted F-tests comparing the full models at each life stage to models that also included quadratic terms for spill and water transit time. None of those tests supported the inclusion of quadratic terms for spill or water transit time (all P-values > 0.36). As a third method to assess whether non-linear processes were supported by the data, we fit generalized additive models to the data presented in Haeseker et al. (2012). Both smoothing splines and locally-weighted scatterplot smoothing (loess) forms of generalized additive models performed far worse than linear regression for both species and at all survival rate stages based on Akaike's Information Criterion (Table 7). In addition, the generalized additive models required the estimation of double to triple the number of parameters as the linear regression approach. In the case of steelhead ocean-adult survival, the generalized additive model approach required the estimation of more parameters than the number of observations. Clearly, applying a generalized additive model approach to the data in Haeseker et al. (2012) would have been inappropriate and would have resulted in an overparameterized model with increased bias and poor predictive performance.

Skalski et al. (2013) failed to examine whether generalized additive models would have improved model fit in Haeseker et al. (2012), despite being provided with the data to do so. In summary, the three sets of analyses presented here show that the data in Haeseker et al. (2012) support linear relationships, but do not support higher-order processes, optima, thresholds, or spline relationships for the hydrosystem operations variables under management control (spill and water transit time). Furthermore, the linear regressions presented in Haeseker et al. (2012) are likely to have less bias and better predictive performance than would be obtained by a generalized additive model approach that would suffer from overparameterization.

Table 7. Summary statistics (n = number of observations, k = number of estimated parameters, AIC = Akaike's Information Criterion) for linear regression, generalized additive model regression using smoothing splines, and generalized additive model regression using locally weighted scatterplot smoothing (loess) approaches for modeling freshwater (S.r), ocean-adult survival (S.oa), and smolt-to-adult survival (SAR) using data presented in Haeseker et al. (2012).

Method	Species	S.r			S.oa			SAR		
		n	k	AIC	n	k	AIC	n	k	AIC
Linear Regression	Sp/Su Chinook	33	6	-10.1	33	9	8.1	36	9	2.3
GAM (spline)	Sp/Su Chinook	33	13	-6.1	33	22	13.5	36	22	5.7
GAM (loess)	Sp/Su Chinook	33	14	-9.4	33	26	22.2	36	26	14.3
Linear Regression	Steelhead	22	6	-9.5	22	9	0.2	36	9	42.5
GAM (spline)	Steelhead	22	13	-0.1	22	22	NA	36	22	45.6
GAM (loess)	Steelhead	22	14	1.8	22	26	NA	36	28	58.8

Comments on Model Averaging and Model Selection Uncertainty

Skalski et al. (2013) acknowledges that model averaged predictions account for model selection uncertainty but says no theoretical basis has been given for model averaged regression or model averaged coefficients. However, Burnham and Anderson (2002) state that with closely related

models, selecting a single model is often unsatisfactory because it can incorporate a "model selection bias" which will affect the coefficient estimates. To account for model selection uncertainty, Burnham and Anderson (2002) provide a formal method for inference from more than one model. Although the method of model averaging and the use of Akaike's Information Criterion (AIC) is not promoted by Skalski et al. (2013), it has been used extensively in the Columbia River Basin (Scheuerell et al. 2009, Holsman et al. 2012) and in particular it is used in mark recapture studies. A Google Scholar search revealed that Burnham and Anderson (2002) has been cited *twenty thousand, one hundred and forty nine times* as of September 18, 2013.

In fact, in some cases model averaging may be the preferred tool of inference over the use of a single model. Lukacs et al. (2010) used a Monte Carlo simulation to compare model averaged results using similar methods as in Haeseker et al. (2012) versus results from a single model selected with stepwise regression. Those authors found that using a single model for inference instead of employing model averaging produced confidence intervals with poor coverage and estimated coefficients that were biased up to three times larger than the true value. Finally, the authors note that stepwise regression in particular is not the problem, but rather that inference from a single model can produce a biased result. These results were confirmed by Claeskens and Hjort (2008) who demonstrated both mathematically and with simulations that using a single model for inference can underestimate standard deviations, bias coefficient estimates, and underrepresent type I error. These and other studies support the use of model averaging as an appropriate and rigorous procedure to account for model selection uncertainty and improve reference.

Comments on "Regression, Retroduction, and Beyond"

The authors state that drawing inferences from observations (retrogression) can be improved through the further development of hypotheses that can be tested (hypothetico-deductive model) through falsification of predictions. Budy et al. (2002) presented and discussed evidence that some of the mortality that occurs during the period of estuary and early ocean residence is related to earlier hydrosystem experience during downstream migration, a concept known as the hydrosystem-related, delayed-mortality hypothesis. Several analyses in Haeseker et al. (2012) provided support for the hydrosystem-related, delayed-mortality hypothesis for downstream-migrating spring/summer Chinook salmon and steelhead from the Snake River. Skalski et al. (2013) presents several predictions that would follow if the hydrosystem-related, delayed-mortality hypothesis of Budy et al. (2002) were true. However, our examination of these predictions show that the premises underlying the Skalski et al. (2013) predictions are illogical convolutions of the hydrosystem-related, delayed-mortality hypothesis of Budy et al. (2002). We discuss the illogic of each of their predictions below.

No correlation between ocean covariates and in-river covariates
Contrary to Skalski et al.'s (2013) assertion, the hydrosystem-related, delayed-mortality hypothesis makes no statements about whether there is or is not a correlation between freshwater and ocean covariates. The hypothesis simply states that some of the mortality that occurs during the period of estuary and early ocean residence is related to earlier hydrosystem experience during downstream migration. The prediction of no correlation between freshwater and ocean covariates based on the hydrosystem-related, delayed-mortality hypothesis is therefore an illogical premise. Although this premise is illogical, it should be noted that Haeseker et al.

(2012) found no correlation between freshwater and ocean covariates. Furthermore, FPC believes that Skalski et al. (2013) incorrectly calculated the amount of association between in-river and ocean variables (see the Multicollinearity section of this document).

No correlation between the adult return rate (i.e., SAR) of transported fish and in-river covariates because transported fish have little opportunity to experience the hydrosystem

This hypothesis and its relation to Haeseker et al. (2012) is not logical. The hydrosystem-related, delayed-mortality hypothesis states that some of the mortality that occurs during the period of estuary and early ocean residence is related to earlier hydrosystem experience during downstream migration. A key element of the hydrosystem-related, delayed-mortality hypothesis is exposure to a "hydrosystem experience during downstream migration." Because transported fish do not have a "hydrosystem experience during downstream migration," the hydrosystem-related, delayed-mortality hypothesis makes no predictions on the factors that influence the survival of transported fish. The premise behind this prediction does not follow from the hydrosystem-related, delayed-mortality hypothesis. It is important to note the transported fish do experience some portion of the hydrosystem prior to collection at the transportation sites, and flow conditions downstream of BON following release may influence their survival rates. Therefore, researchers should utilize the "appropriate spatial and temporal scales of the survival processes…in order to measure the potential covariates at the right geographic scale" following the admonishment of Skalski et al. (2013). Unfortunately, Skalski et al. (2013) fails to follow their own advice in the specification of appropriate temporal and geographic scaling for covariates related to transported fish. Transported fish are typically released at Skamania Landing which is very near Bonneville Dam. Therefore transported smolts are released approximately 145 miles from the mouth of the Columbia. This distance is more than a third of the distance from LGR to the river mouth. We agree that transported smolts have little opportunity to experience the hydrosystem. However, it is not likely or a biologically reasonable hypothesis that their survival is independent of river conditions for the remaining 145 miles from Skamania Landing to the river mouth. In addition, the flows likely affected both the survival and condition of the juvenile migrants prior to their collection for transportation.

No correlation between in-river smolt survival and ocean covariates

A reasonable explanation for a potential correlation between in-river survival and ocean covariates was not presented by the authors. The hydrosystem-related, delayed-mortality hypothesis states that some of the mortality that occurs during the period of estuary and early ocean residence is related to earlier hydrosystem experience during downstream migration. The mechanisms (injury, predation, physiological stress, migration delay, and disease) underlying the hydrosystem-related, delayed-mortality hypothesis were described by Budy et al. (2002). There is no plausible mechanism whereby freshwater smolt survival would influence abiotic features of the ocean. Therefore, there is no logical premise or mechanism behind this illogical prediction. This hypothesis was presented by the authors directly preceding an analysis that used only transported fish which do not emigrate in-river. This hypothesis was not tested by the authors.

No correlation between smolt condition and ocean survival

The hydrosystem-related, delayed-mortality hypothesis states that some of the mortality that occurs during the period of estuary and early ocean residence is related to earlier hydrosystem experience during downstream migration. Given appropriate geographic and temporal scaling of

comparison groups, the hydrosystem-related, delayed-mortality hypothesis would make predictions that hydrosystem-related smolt condition indices would be correlated to ocean survival. For example, Tuomikoski et al. (2010) found that the number of bypass experiences influenced ocean survival rates for spring/summer Chinook salmon and steelhead, consistent with the hydrosystem-related, delayed-mortality hypothesis. Skalski et al.'s (2013) prediction of no correlation between hydrosystem-related indices of smolt condition and ocean survival is actually opposite that predicted by the hydrosystem-related, delayed-mortality hypothesis. Again, FPC agrees with Skalski et al.'s (2013) admonishment that researchers should utilize the "appropriate spatial and temporal scales of the survival processes…in order to measure the potential covariates at the right geographic scale." However, smolt length at time of transportation is not a hydrosystem-related index of smolt condition. Smolt length is a function of growth in hatcheries or rearing tributaries over the previous 2 years. Therefore, smolt length at transportation is not an appropriate metric to measure hydrosystem-related smolt condition. The data that the authors use to test this hypothesis are length data at LGR for hatchery and wild smolts marked at the same location. The authors do not show any supporting evidence for their use of length as a surrogate for condition. Further, given that these data contain a mixture of hatchery and wild smolts, and hatchery smolts are clearly larger than their wild counterparts, what does the length variable represent? This hypothesis was not tested adequately enough to support any conclusions by the authors. Finally, the finding of the authors that the dataset of hatchery and wild fish lengths are correlated with offshore upwelling is not supported by data. The authors appear to suggest that the length of hatchery fish at LGR is related to nearshore ocean processes measured at the same time. The authors do not offer any rationale, data, or mechanism to support their conclusion. This conclusion in their analysis contradicts their own caution regarding the dangers of not knowing the "appropriate spatial and temporal scales of the survival processes…in order to measure the potential covariates at the right geographic scale." The identification of a positive correlation between length and upwelling is an example of how poor specification of mechanisms can lead to erroneous conclusions.

In summary, the list of predictions presented by Skalski et al. (2013) are illogical convolutions of the hydrosystem-related, delayed-mortality hypothesis of Budy et al. (2002). The analyses presented to investigate their predictions fail to use "appropriate spatial and temporal scales of the survival processes…in order to measure the potential covariates at the right geographic scale." As a result, their analyses do not provide falsification of the hydrosystem-related, delayed-mortality hypothesis that was investigated by Haeseker et al. (2012). Given these inappropriate predictions and flawed analytical approaches, their criticisms of Haeseker et al. (2012) are not well founded and lack sufficient scientific credibility to raise any doubts about the conclusions of Haeseker et al. (2012). Their analysis would benefit from proper application of the hypo-deductive method using accurate predictions of the hydrosystem-related, delayed-mortality hypothesis and data collected at the appropriate spatial and temporal scales of the survival processes.

Power Analysis
The power analysis presented in the discussion section of Skalski et al. (2013) falls short for a number of reasons. First, the power analysis focuses on a short reach (LGR-MCN) that has less variability among years explained by spill than longer reaches (LGR-BON) or other life stages (e.g., ocean survival or smolt-to-adult survival). By focusing on a short reach with less

variability explained by spill effects, the resulting duration is overestimated compared to that expected by an experiment focusing on longer reaches or SARs. Second, the magnitude of change in LGR-MCN survival (10%) estimated by Skalski et al. (2013) is small relative to the changes in SARs expected through increased spill levels. Simulations presented in Hall and Marmorek (2013) indicate that SARs may increase by nearly 100% at high spill levels. With these larger expected differences in SARs, the expected amount of time that would be required to detect a change is much shorter. Because of these issues, the power analysis presented in Skalski et al. (2013) greatly exaggerates the amount of time that would be required to detect changes in survival at multiple life stages associated with a spill management experiment.

BioAnalysts, Inc. and Anchor QEA, LLC, 2013

- Assessing the success of actions in the 2008 Biological Opinion implementation on the basis of achieving performance standards is inappropriate and does not consider recent data and analyses.
- Smolt-to-adult return rates of Snake River spring Chinook and steelhead remain dangerously low.
- Comments provided in the critique of Haeseker et al. (2012) are out of date and do not consider recent updates to the analysis which include the effect of removable spillway weirs (RSWs) and surface passage. These updated analyses have been presented in public forums, have been reviewed, and are publicly available.
- Arguments made in this document regarding the inability to affect flow in the present configuration of the hydrosystem provides support for the consideration of Experimental Spill Management.
- The authors provide critiques of peer reviewed and published analyses that do not support their point of view, while presenting without consideration of public review critiques of published analyses that do support their point of view.

Bonneville Power Administration (BPA) provided a draft report of "Federal Columbia River Power System Improvements and Operations under the Endangered Species Act – A Progress Report." The report is a review of the results of the implementation of the actions in the 2008 Biological Opinion, and is based on analyses conducted by BioAnalysts, Inc. and Anchor QEA, LLC. The report addresses improvements in juvenile survival through the FCRPS and concludes that:

> "…monitoring results and performance tests indicate that the new configuration and operation of dams have improved juvenile fish survival through the FCRPS to levels roughly comparable to those realized decades ago, when fewer dams were in place. Annual estimates indicate an upward trend in survival of juvenile steelhead and yearling Chinook salmon migrating through the Snake and Columbia rivers over the last two decades."

The draft paper is largely an endorsement of current implementation based on the studies conducted relative to performance standards at each hydroproject. The draft report does not address significant serious technical concerns that have been raised over the past several years

regarding the concept and approach of performance standards. NOAA has failed to address or consider recent data and analyses that raise serious issues regarding the validity of the performance standard concept and approach, specifically that route of dam passage affects later life stage survival (see our previous comments on performance standards). Recent data and analyses indicate that freshwater passage experience affects later life stages and adult returns, which are not considered in performance standard implementation in this Draft BIOP. Recent data indicate that a smolt-to-adult return rate would provide a more realistic performance standard.

Specifically, while juvenile survival rates may have improved compared to years when in-river conditions were hazardous to fish survival due to minimizing spill and the maximization of transportation, data regarding adult return rates show that on average spring Chinook from the Snake River have smolt-to-adult survival rates of less than 1% and steelhead smolt-to-adult survival rates average less than 2%. This should be put in context of the Northwest Power and Conservation Council goals of achieving smolt-to-adult return rates between 2% and 6%, with an average of 4%.

The BPA draft report also provides a critique of Haeseker at al. (2012) based on reviews conducted by Skalski et al. (2013) and Manly (2012) who were commissioned by the Action Agencies. (Note: these references are cited as 2012 and 2011, respectively, in the BPA paper. In light of the fact that Haeseker et al.(2012) was published in 2012, it would make a Manly review in 2011 most improbable). The reviews of Skalski et al. (2013) and Manly (2012) are addressed in detail elsewhere in this paper. However, The Progress Report (BioAnalysts Inc. and Anchor QEA, LLC 2013) excludes consideration of recent data and analyses, and mistakenly concludes that Haeseker et al. (2012) does not address adult returns past 2006 and therefore does not incorporate the benefits of RSWs. The recently updated analyses of Haeseker et al. (2012) incorporate the effect of surface passage structures. These recent analyses have been reviewed and presented in public forums and they are publicly available. The updated analyses of Haeseker et al. (2012), to incorporate the effect of surface passage structures, was completed in response to a direct recommendation from NOAA Fisheries.

One point that BPA brings up in the review is,

> "Water particle travel time (an index of river flow) was another key predictor variable in the Haeseker et al. (2012) analysis. They noted that lower SARs were associated with higher WTT indices and projected that reduced WTT would promote higher SARs. However, it is not clear what incremental changes in WTT are possible within any given water year, given water management operations that need to balance across sometimes competing demands including flood control, irrigation, recreation, water quality (total dissolved gas limits under the Clean Water Act), and international water treaties, as well as fish resources."

FPC agrees with the statement that given the present configuration, operation, and obligations of the hydrosystem there is little opportunity to provide additional changes in river flow. This is precisely why Experimental Spill Management is so appealing since it can be implemented in any flow year, without impacting reservoir operations. The question of TDG limits under the

Clean Water Act can easily be addressed through the assessment of biological information that has been collected under much higher spill and TDG limits (due to the impacts of the hydrosystem operation under the present FCRPS configuration) than presently contemplated under Experimental Spill Management.

It is unfortunate that, while BPA commissioned reviewers to assess the Haeseker et al. (2012) paper, they present Rechisky et al. (2013) without any such reservation. The Rechisky et al. (2013) paper presents information contrary to the conclusions in Haeseker et al. (2012). In fact, when the CSS Oversight Committee reviewed the Rechisky et al. (2013) study (review attached) they found significant flaws with the methods and assumptions used in the study. Results indicate significant confounding due to tagging and handling effects. Critical assumptions regarding offshore ocean migration patterns and array detection efficiency are inconsistent with the available data. Because of these issues, the conclusions regarding hydrosystem-related delayed mortality are overreaching and unsupportable. In addition, the Rechisky et al. (2013) study was conducted for only 3 years and had little contrast in ocean and river conditions as compared to recent long-term studies (9-60 years) that provide evidence of significant hydrosystem delayed mortality (Deriso et al. 2001, Schaller and Petrosky 2007, Petrosky and Schaller 2010, and Haeseker et al. 2012).

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Attachment

**Review of "Influence of multiple dam passage on survival
of juvenile Chinook salmon in the Columbia River estuary
and coastal ocean" by Rechisky, Welch, et al., April 23, 2013**