Independent Scientific Advisory Board
for the Northwest Power and Conservation Council,
Columbia River Basin Indian Tribes,
and National Marine Fisheries Service
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Memorandum (ISAB 2012-1) **January 3, 2012**

To: ISAB Administrative Oversight Panel
Bruce Measure, Chair, Northwest Power and Conservation Council
Paul Lumley, Executive Director, Columbia River Inter-Tribal Fish Commission
John Stein, Science Director, NOAA-Fisheries Northwest Fisheries Science Center

From: Rich Alldredge, ISAB Chair

Subject: Follow-up to ISAB reviews of three FPC memos and CSS annual reports regarding latent mortality of in-river migrants due to route of dam passage

Summary

In this memo, the ISAB re-examines analyses conducted by the Fish Passage Center (FPC) and Comparative Survival Study (CSS) to evaluate whether the route of dam passage affects subsequent survival ("latent mortality") of in-river migrants. The ISAB finds that collectively these analyses demonstrate that fish bypass systems are associated with some latent mortality, but the factors responsible for latent mortality remain poorly understood and inadequately evaluated. The significant association between fish bypass and latent mortality might only reflect a non-random sampling of smolts at the bypass collectors (the selection hypothesis) rather than injury or stress caused by the bypass event (the damage hypothesis). Because these hypotheses have very different implications for hydrosystem operations, FPC and CSS conclusions should be re-examined to consider alternative explanations discussed in this review. Further research will be needed to resolve this issue. Details are provided below.

Background

The Northwest Power and Conservation Council's 2009 amendments to the Columbia River Basin Fish and Wildlife Program call for the continuation of the fish passage related functions of the Fish Passage Center and a regular system of independent science reviews of the FPC's analytical products. The FPC Oversight Board, ISAB, and FPC director have established guidelines for this regular review. The guidelines specify that the ISAB will examine FPC and CSS draft annual reports when these reports are released for public comment. Since this regular review was initiated in 2010, the ISAB has reviewed two draft FPC annual reports (2009 and 2010) and two draft CSS annual reports (2010 and 2011).

The guidelines also include criteria for identifying FPC analyses/products for ISAB review. These criteria include the introduction of new or novel analyses; new conditions or data that bring old analyses into question; and/or situations where consensus cannot be reached in the region on the science involved in the product. In 2011, the ISAB identified three FPC technical memos on the topics of latent mortality and effects on in-river survival as meeting the criteria for review. Specifically, the three memos addressed latent mortality of in-river migrants due to route of dam passage. The ISAB conducted a review of the three memos (ISAB 2011-3) and concluded under Question (f), "the studies and analyses cited in these technical memos do not provide an adequate base of reliable information to support a 'weight of evidence' conclusion on the strength of a relationship between multiple bypass passage and latent mortality of juvenile Chinook and steelhead. That is, the relationships observed between latent mortality and bypass passage are confounded with other factors that obscure unambiguous interpretation."

Upon review of the ISAB report, Dr. Dan Goodman, a member of the FPC Oversight Board, had questions about the ISAB's conclusion stated above. Specifically, he suggested that the "strongest, and most direct, evidence now available on bypass-attributable delayed (post-Bonneville) mortality is that in Chapter 7 of the CSS 2010 draft annual report, cited in the 3 memos." He states, "My initial reading of that CSS Chapter, and Chapter 5 of the CSS 2009 report which it cites, is that the methodology fairly addresses much of the longstanding criticisms of earlier less direct methods for estimating this component of mortality. That earlier criticism was well summarized in the ISAB 2007 report (ISAB 2007-1) reviewing latent mortality as it was understood at that time. It looks to me that CSS 2009 and CSS 2010 made an effort to implement a number of the ISAB 2007-1 recommendations. Yet the current ISAB review seems not to have engaged on the specific strengths or weaknesses of the new technicalities of Chapter 7 of the CSS 2010 report. In fact, the separate ISAB review of the 2010 CSS draft annual report (ISAB 2010-5) recommends explicitly that Chapter 7 warrants further independent peer review."

In particular, ISAB report 2010-5 states "A more complete review of this chapter [7] is warranted, perhaps in combination with an overall examination of the regional bypass evaluation efforts. The caveats stated in this chapter that caution should be used when interpreting route-specific survival estimates and SARs conditioned on bypass detection provide support for a careful, independent review of the complexity of the data, modeling, and interpretation of results." In response to Dr. Goodman's suggestion and the earlier ISAB recommendation, Jim Ruff (the FPC Oversight Board and ISAB liaison), Dr. Rich Alldredge (ISAB Chair), and Erik Merrill (ISAB coordinator) agreed that the ISAB should undertake a further review of Chapter 7 of the CSS 2010 Annual Report, Chapter 5 of the CSS 2009 Annual Report, and any additional recent and relevant analyses such as information presented at the CSS 2011 annual workshop.

In this review, the ISAB again considers the six questions (Q1-Q6 below) from the FPC Oversight Board that framed the previous review of the three technical memos. For convenience, the six questions are grouped under four topics: 1) defensibility of the FPC's original analyses; 2) inclusiveness of evidence from other studies; 3) adequacy of consideration of alternative explanations; and 4) overall strength of support for FPC's conclusions regarding latent mortality.

Answers to Questions Organized by Topic

Defensibility of the original analyses

> *Q1 - Are the original analyses cited in the chapters scientifically rigorous and relevant to the topics in the chapters?*

Chapter 7 of the CSS 2010 Annual Report includes original analyses designed to improve understanding of the effects of bypass systems on travel time and subsequent survival of Chinook salmon and steelhead. Three analytical approaches were explored: 1) multiple linear regression to test for bypass effects on fish travel time between Lower Granite Dam (LGR) and Bonneville Dam (BON); 2) logistic regression within an information theoretic framework to test for bypass effects on post-Bonneville smolt-to-adult survival (SAR); and 3) a random effects meta-analysis to estimate the ratio of SAR for bypassed (C1) fish to SAR for undetected (C0) fish. These approaches provide appropriate statistical analyses of the PIT-tagged data sets, but some relevant information is lacking.

Delay in travel time: More justification is warranted for the use of a third-order polynomial function to account for within-season variability in the travel time model. In particular, it would have been useful to provide more quantitative description of how much variability is explained by the third-order polynomial. The travel time analysis indicated that for statistically significant cases of delay, the average delay per dam was about 0.7 d (range 0.13-5.32 d for Chinook and 0.19-2.71 d for steelhead). Given this skewed distribution, it would be more reasonable to report the median delay time (0.5 d for Chinook and 0.4 d for steelhead). This delay is brief, but as noted in the report, it may be enough to significantly increase exposure to predators in the dam forebays, and thus, could explain higher in-river mortality of bypassed fish.

Effect of bypass on post-Bonneville SAR. Although clearly written, the brief description of the logistic regression analyses glosses over some fundamental problems inherent in the complex and variable nature of the data. Any analysis of these data faces a plethora of interactions, and an overly simplistic interpretation could yield misleading conclusions. These interactions become obvious in graphical comparisons of survival rates of different rearing types (hatchery or wild) passing different dams in different years. For example, Figures 53 to 55 in Williams et al. (2005), illustrate that the apparent effect of bypass on SAR differs greatly among rearing types, dams, and years. In some subsets of the data, bypass has no obvious effect on SAR. In other subsets, bypass is associated with reduced SAR, but there is no obvious "dose effect" – the reduction is similar for fish bypassed 1, 2, 3, or 4 times – whereas in other data sets, a dose effect is apparent with SARs progressively decreasing as the number of bypasses increases; in still other data, bypassed fish survive better than never-bypassed (never-detected) fish.

The ISAB recommends the plots provided in Williams et al. (2005) be updated to complement the CSS analysis. The complexity revealed by these plots indicates that even if some data indicate bypass events are significantly correlated with reduced SARs, the factors actually driving bypass-survival relationships remain poorly understood, and 2) the models may have little predictive value for any individual bypass site. More consideration of potential explanations for the observed interaction between year and rearing type is also warranted. For example, is it possible to attribute these interactions to inconsistent annual differences in size or migration timing between hatchery and wild smolts? Year-to-year variations in stress from temperature changes in reservoirs, or other confounding events during river passage, as well as year-to-year variations in estuary and ocean conditions might also affect the magnitude of (or ability to detect) latent mortality, and these potential effects warrant further consideration (e.g., Haeseker et al. in press).

In Chapter 7, the authors state "The Model 3 estimates for Chinook showed a 10% reduction in post-BON SARs per bypass experience at upriver dams (P = 0.0001, Table 7.5)." Is this conclusion based on the estimated logit coefficient for the bypass effect of -0.117 (reported in Table 7.5)? If so, it should be noted the conventional interpretation of this logit coefficient in logistic regression is that log-odds decreases by -0.117 (or 11.7%) for every 1-unit increase in this explanatory variable relative to the base, which may be defined in the analysis as either the bypass or in-river condition. Because the logit model describes a nonlinear relationship between the dependent variable (the probability of adult return at BON) and the explanatory variables, the change in the probability for a one-unit change in bypass experience varies according to where one begins on the probability curve. For this reason, an interpretation in terms of odds rather than probabilities is more valid. More explanation is needed as to how the results presented in Chapter 7 can be reconciled with the more conventional interpretation of logit coefficients in logistic regression analysis.

The estimated reduction in post-Bonneville SAR per bypass experience ("dose effect") is 10% for juvenile Chinook salmon and 6% for juvenile steelhead passing Snake River dams, and 22% for steelhead passing Columbia River dams. However, no confidence intervals are estimated, and given the variability in the data sets, a high degree of uncertainty might be expected. More consideration should be given to ways to determine (and improve) the reliability of the estimated dose effect.

The ISAB appreciated the general discussion of how the logistic regression results could be related to hydrosystem management. However, the relatively large dose effect (22%) for steelhead bypassed at the McNary and John Day dams warrants further critical consideration. Are these bypass systems configured differently than those encountered in the Snake River, and what factors (such as descaling) at these sites might have contributed to higher latent mortality?

Overall, the main conclusions from the original analyses in Chapter 7 seem to hinge on comparisons of the relative effect of adding particular variables (e.g., number of bypass events) to a multiple regression equation. The regression analyses of time delays and post-Bonneville SARs, and the meta-analysis of latent mortality, all have merit, but the interpretation of the results remains subject to the usual caveat when trying to explain biological relationships with indirect regression analyses – "correlation is not causation."

Inclusiveness of evidence from other studies

Q2 - Does the work by others cited in the chapters represent the "universe" of studies or information relevant to the topics addressed by the chapters?

Q3 - Do the chapters completely and accurately characterize the work by others cited in the chapters with respect to their relevance to the topics addressed in the chapters (e.g., do the chapters accurately and objectively describe what was done, why it was done, what was found and what it may mean)?

The effects of dams on salmon survival have been studied for many years in numerous species, especially Atlantic salmon. Despite this long history, the literature cited in the CSS annual reports and FPC memos is generally restricted to studies of Chinook and steelhead in the Columbia River. Surprisingly, the concept of latent mortality has not been raised by researchers in other contexts, even though the concept of latent mortality is not restricted to bypass systems and might be applied to dam passage more generally. However, the possible existence of latent mortality is acknowledged (as a hypothesis) by at least some researchers working on the effects of dams on Chinook salmon survival in other rivers. For example, Lindley et al. (2009) working on Sacramento River Fall Chinook write: "Delayed mortality from cumulative stress events has been hypothesized to explain the relatively poor survival to adulthood of fish that successfully pass more hydropower dams on the Columbia River (Budy et al. 2002). However, there is no *direct* evidence, to date, for delayed mortality in Chinook from the Columbia River (ISAB 2007), and its causes remain a mystery."

A key question is *why* bypassed fish tend to experience higher latent mortality than undetected fish. Williams et al. (2005) discussed the issue of bias caused by differences in size and disease condition of the fish. The potential of bias from selective sampling is discussed as an alternative explanation under the next topic (Q4 and Q5).

In general, consideration of studies by other researchers could be expanded in the CSS annual reports. Preliminary results from BPA Project 2003-041-00 are not considered or acknowledged, even though that study specifically targeted the problem of latent mortality (Marsh et al. 2010). Novel approaches to dam bypass mortality studies are required and have been recommended (Roscoe and Hinch 2010).

Consideration of alternative explanations

Q4 - Are the syntheses of the results from the relevant studies and original CSS analyses scientifically sound; i.e., are the interpretations of the weight of evidence represented by the body of work cited in the chapters reasonable and scientifically defensible?

Q5 - Are the conclusions reached as a result of the syntheses and interpretations of the relevant studies and original CSS analyses reasonable and scientifically defensible? Can one reach other reasonable and scientifically defensible conclusions based on the "universe" of studies or information relevant to the topics addressed by the memos?

The prevailing assumption is that a negative correlation between bypass exposure and survival is to be expected because exposure to collection/bypass systems at the dams is known to result in some degree of physical trauma and a strong stress response, with elevated levels of plasma cortisol, adrenaline, and other indicators of stress (Congleton et al. 2000). Deleterious effects of stress on disease resistance and other performance measures are most likely, however, when stressful situations are chronic rather than transitory.

An alternative hypothesis is that bypass systems do not sample populations of migrating fish randomly, but to some degree select fish of lower fitness. Bypass systems are as much sampling devices as seines, trawls, screw traps, and other devices used in fisheries research, and it is prudent (usually mandatory) practice in fisheries work to assess the selectivity of sampling gear. Selectivity for fish species, sex, size or condition may be of concern, depending upon the objective of the research (Hubert 1996). Size selectivity in bypasses at some Snake and Columbia River dams has been reported (Zabel et al. 2005), but the magnitude of the selection effect (difference in length between bypassed and non-bypassed fish) was not estimated. The health status of bypassed fish may be of even greater concern as selection for diseased fish would result in lower SARs.

The issue of sampling selectivity is further complicated in that each bypass system must be viewed as a distinct design with its own inherent selectivity and sampling biases. The differences are related to the specific design of the system and how each one collects fish. There is ample opportunity to collect evidence from various locations in the hydrosystem to clarify if and how site-specific selectivity occurs, and studies can be designed to evaluate it. As a result, numbers that are generated on migration delays (Study 1 in CSS Chapter 7) and survival (studies 2 and 3 in CSS Chapter 7) and the variances associated with those numbers must be viewed in light of potentially equally large, or larger biases in catch selectivity of various forms (size, sex, condition, health), all of which could affect travel times and survival rates to an equal or even greater extent than the effects of the treatments (bypass, non-bypass, etc.) themselves. This potential problem is aggravated when survival rates are low (as in Chapter 7) and variance estimates are not available for most of the numbers being forwarded on survival rates. In these situations, the selectivity of each site should be estimated to adjust for the biases found, or at least to understand the limitations on the estimates. Chapter 7 does not effectively address such potential biases. No information is provided on fish sizes between Chinook and steelhead as size differences between them might affect movements. The lack of sufficient information on these potential biases demands that caution be exercised in the interpretation of existing SARs.

In addition to this potential for selectivity, another potential source of bias arises from the variable percentages of fish passing (again, selectively) through each possible route (bypass, spill, turbines) as the percentage of spill changes. That is, if the bypass, spill, and turbines are all viewed as competing fishing gears for conveying fish, their relative effectiveness and biases will change with river conditions, especially spill, as competing conveyances for juvenile fish. This result was clearly depicted in 2001 when bypass became the safest route of passage as spill was eliminated. It is also supported in recent studies (e.g., Haeseker et al. in press)

More thorough applied studies are needed to evaluate differences in the selectivity of different passage routes, and the potential magnitude of biases in SARs that are determined from these data. In their response to the ISAB review of the three memos, the FPC correctly pointed to efforts to address these issues in the CSS 2006 (for wild Chinook) and 2008 (for hatchery Chinook and steelhead) annual reports, both of which indicated little or no bias for a sample of fish tagged upriver of LGR. However, it is also noted by the FPC and by the ISAB that results from those studies differ from those of Zabel et al. (2005), where bias was found for fish caught, tagged and released at LGR. The FPC concluded that “The reason for this lack of agreement is not known.” For this reason, it is prudent to resist drawing definite conclusions regarding differences in survival between bypassed and non-bypassed fish, especially those based on small observed differences, until the reasons for this lack of agreement are better understood.

Similarly, it is important to better understand the potential effects of selectivity based on fish health. The recent development of fast and cheap methods for pathogen screening would make investigation of bypass selectivity for disease carriers more feasible than in the recent past. It is also possible to test the assumption of random sampling at collectors by comparing bypass histories for individual PIT-tagged fish that survived in-river migration. Such comparisons of fish bypass histories at intermediate dams reveal whether some individuals are consistently more likely to be bypassed rather than spilled, or to pass undetected through turbines (Faulkner 2010).

The percentage of water spilled at a dam likely sets an upper limit for the potential bias from selective sampling at bypass collectors. When the majority of fish are bypassed, the potential for bias from sampling only bypassed fish would be relatively low. Thus, other things being equal, latent mortality would appear to increase with spill proportion under the “selection” hypothesis (non-random entrance of less-fit fish into collectors), but not under the “damage” hypothesis (reduced fitness due to damage resulting from collection/bypass exposure). To help test these alternatives, more information and analyses should be presented on the relationships between the percentage of fish bypassed versus spilled in relation to percent spilled at each site.

It is important to recognize that the damage hypothesis and the selection hypothesis are not mutually exclusive. Both could be important, but if the effect of selection is relatively large, the benefit of diverting fish away from bypasses and over spillways would be less than anticipated.

<u>Overall strength of support for conclusions regarding latent mortality</u>

> *Q6 - Is there adequate evidence available to establish that latent mortality associated with bypass passage/powerhouse passage is indeed an issue for juvenile fish and fish passage management?*

The three studies presented in Chapter 7 are consistent with previously reviewed and more recent studies indicating that fish passing through the bypass system experience higher mortality both within the river and downstream of Bonneville Dam. A key assumption, however, is that fish entering the bypass system are randomly selected, or at least, that the sample of fish entering the bypass system are not disproportionately less fit in terms of their prospects for survival to return as adults. This assumption needs to be tested both with available data and with further experimental investigation (see recommendations above).

Haeseker et al. (in press) provide new evidence that bypassed fish experience higher mortality than undetected fish. Their evidence includes 1) a positive correlation between in-river and ocean survival rates, and 2) a positive correlation between ocean survival and percent spill (and the PDO index in a multivariate model). These correlations provide further indirect evidence for latent mortality as a consequence of bypass but do not provide definitive evidence of causation. The positive correlation between in-river and ocean survival rates could be related to other factors such as fish size or climate variables not considered explicitly in the analysis. For example, Lawson et al. (2004) concludes that survival of Oregon coho at sea is correlated with survival in fresh water, yet those fish did not migrate through bypass systems. Perhaps the correlation appears to arise because survivals in both systems are linked to larger scale climatic events influencing the ocean and freshwater systems simultaneously. Nevertheless, the correlation test reported by Haeseker et al. (in press) is still informative and strongly suggestive because a negative correlation would have weighed against the conclusion of latent mortality. Their finding of a significant correlation between ocean survival of smolts and the percent spilled during their downstream migration (together with PDO, but not water transit time) is especially interesting. Assuming that the percentage of water spilled reflects the proportions of smolts spilled, this correlation implies that ocean survival is higher when a lower percentage of fish are bypassed and could suggest that bypass systems can cause latent mortality.

The gold standard for evaluation of scientific hypotheses is publication of supporting data in peer-refereed journals. Although the analyses described in the CSS 2010 Annual Report and in various agency reports (e.g.,Williams et al. 2005) support a negative relationship between bypass exposure and SARs, only one published paper, using fish-passage and survival data from the 1990s, seems to have reported on this relationship (Sandford and Smith 2002). Other published papers (e.g., Budy et al. 2002, Petrosky and Schaller 2010) include speculation about a relationship between bypass exposure and survival, but not original data or analyses. None of the published or unpublished reports have included estimates of fish size effects with confidence bounds. We encourage submission to a peer-refereed journal of a manuscript dealing with bypass-survival correlations using data for recent years. Such a manuscript should include confidence bounds on body size effects, supplemented by graphical presentations of the data to illustrate variation and interactions among years, dams, and rearing types. The manuscript should discuss alternative hypotheses regarding underlying causes for a negative correlation between bypass and subsequent survival.

The ISAB concludes that the available evidence demonstrates that fish bypass systems are associated with some degree of latent mortality, but that its magnitude and the factors responsible for latent mortality remain poorly understood and inadequately evaluated. The significant association between fish bypass and latent mortality might only reflect a non-random sampling of smolts at the bypass collectors (the selection hypothesis) rather than injury or stress caused by the bypass event (the damage hypothesis). Because these hypotheses have very different implications for hydrosystem operations, FPC and CSS conclusions should be re-examined to consider the possibility of alternative explanations raised in this review. Further research is needed to resolve this issue.

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