FISH PASSAGE CENTER

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MEMORANDUM

TO: Rich Alldredge, ISAB Chair
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: Michele DeHart, FPC

DATE: October 13, 2011

RE: Response to ISAB Comments, Review of Three Fish Passage Center Technical Memoranda September 16, 2011

We have reviewed the comments by the ISAB on three technical memorandums #134-10, #135-10, and #08-11. We offer the following response to ISAB comments.

- The FPC memorandums purposely, did not mention or draw conclusions regarding "weight of evidence" so it is not surprising that the ISAB did not find a "weight of evidence" in their review. Weight of evidence is a formal technical approach, which provides a framework for ecological risk assessment (Forbes and Callow 2002). The FPC memorandums simply point out that there is a broad range of evidence indicating that delayed/ latent mortality is associated with juvenile fish bypass through powerhouses. The FPC memorandums could be part of a basis for establishing a weight of evidence process to explore the occurrence of delayed mortality associated with powerhouse passage.
- The ISAB did not present any evidence that latent/delayed mortality is NOT associated with juvenile passage through powerhouses. The ISAB review only raised potential confounding factors. The ISAB review **never indicates or infers or presents analyses indicating that delayed/ latent mortality does not occur** as the result of powerhouse passage.

- Application of the precautionary approach or precautionary principle, in light of the broad range of analyses reviewed, shifts the burden of proof to showing that delayed mortality does not occur as a result of powerhouse passage. In light of the precautionary approach, the lines of evidence discussed in the FPC memorandums, generated from a wide range of analyses by different researchers, indicate that delayed mortality associated with powerhouse passage is likely to occur and should be addressed.
- In order to address all of the confounding factors identified and accomplish the "weight of evidence" objective of the ISAB review, a formal weight of evidence process, and a formal risk assessment would have to be established, which are the fundamental components to an adaptive management approach to decision making. Initiating or establishing these processes is beyond the scope of the Fish Passage Center activities. The FPC memorandums, reviewed by the ISAB, are simply raising the potential issue to the management agencies, who could initiate the weight of evidence, risk analysis process.
- As technical staff to fishery management agencies and tribes, the FPC has the responsibility of raising potential fish passage problems and issues to the managers. The FPC memorandums function to alert the managers that evidence for delayed mortality associated with powerhouse passage is developing and requires attention. The indications of delayed/latent mortality associated with powerhouse passage are present in many different analyses by many different researchers. This raises a concern. Whether or not these issues are pursued, addressed or resolved is the purview of the fishery managers.

Weight of Evidence

In response to the question:

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

The ISAB responded:

"Based on our review, 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".

FPC Response

The FPC memorandums purposely did not include any reference or conclusions regarding "weight of evidence" of latent and delayed mortality associated with powerhouse passage of juvenile salmon and steelhead. FPC memorandums included reference to "a broad scope and range of evidence" that indicate that latent/ delayed mortality occurs as a result of powerhouse passage.

Weight of Evidence (WOE) is a formal evaluation procedure for integrating the results of multiple measurements in environmental risk assessments. A weight of evidence approach takes into account the strengths and weaknesses of different measurement methods when determining whether results indicate a harmful environmental effect (Massachusetts WOE Workgroup 1995). Weight of Evidence can be defined methodologically or theoretically. In the methodological definition, WOE points to established interpretive methodologies including quantitative and qualitative criteria or where all of the evidence rather than a subset of the evidence are examined or quantitative weights for evidence are assigned. In the theoretical definition, WOE serves as a conceptual framework (Weed 2005). WOE as an approach is most often associated with Risk Assessment Analyses (Weed 2005). A formal weight of evidence evaluation, whether qualitative or quantitative provides a framework for rigorous consideration of the strengths and weaknesses of various measurements and of the nature of uncertainty associated with each of them (Massachusetts WOE Workgroup 1995).

The Fish Passage Center memorandums reviewed by the ISAB did not mention either **weight of evidence** or the larger objective of weight of evidence approaches, **risk assessment**. The Fish Passage Center memorandums concluded and we continue to maintain, that there is a broad scope and range of evidence that indicates that delayed/ latent mortality is occurring in juvenile salmon and steelhead that experience powerhouse passage. The **"weight of evidence"** objective pursued by the ISAB in their response was neither the objective of the FPC memorandums nor possible from a review of study results. The FPC conclusions and the ISAB response, particularly regarding confounding factors, both point to the need to address the delayed/ latent mortality associated with powerhouse passage in a formal WOE retrospective ecological risk analysis. The FPC memorandums provide the basis for the pursuit of a weight of evidence approach, if the management agencies agree that the evidence discussed and the ISAB review comments, particularly regarding confounding factors, raises adequate concern.

ISAB concerns over comparisons of upriver and downriver stocks

The CSS Oversight Committee formally addressed the ISAB concerns over upriver and downriver stocks in correspondence from the CSS Oversight Committee to the ISAB and ISRP chairpersons, dated August 5, 2008. We have attached that response for reference. We are mindful of the ISAB concerns and believe that the attached correspondence addresses those concerns. Specifically, that the upstream and downstream comparisons conducted in CSS and other analyses are consistent with accepted scientific methodologies. The measurement and inferential approaches used in these analyses are consistent with those used within an extensive body of peer-reviewed literature. The ISAB comments regarding Schaller and Petrosky (2007) and Petrosky and Schaller (2010) do not diminish the fact that these published, peer reviewed analyses; include indications that delayed/latent mortality is associated with powerhouse passage.

The ISAB review (p. 5) expressed concerns about propagation of error for estimating productivity with Ricker models (and, indirectly, for estimating delayed mortality). However, we are aware of only a single Ricker model formulation that indicated negligible delayed mortality. Hinrichsen and Fisher (2009) found equivalent levels of delayed mortality as reported by Schaller and Petrosky (2007) when fitting either individual Ricker "a" values or a regional Ricker "a" value to upriver and downriver populations. Hinrichsen and Fisher (2009) estimated negligible delayed mortality when fitting a common Ricker "a" value to all populations; however, this formulation also implied unrealistically low dam passage mortality (0.01), and implied that all populations have equivalent productivity regardless of widely differing habitat quality. Their common Ricker "a" assumption is also at odds with analyses by the Interior Columbia Technical Recovery Team (ICTRT 2007), which estimated widely varying intrinsic productivity across populations and habitats. In addition, empirically estimated SARs (PIT tag) from upriver and downriver Chinook populations (Schaller et al. 2007; Tuomikoski et al 2010) yielded similar estimates of differential mortality (and, by extension, delayed mortality) as the Ricker-based formulations of spawner and recruit data used by Schaller and Petrosky (2007).

The ISAB review also stated that multiple powerhouse passages were not included in best-fit models for steelhead in Petrosky and Schaller (2010), and that this should be noted in the FPC memos. Although, Petrosky and Schaller (2010) did not present any tabular model outputs for steelhead that included number of powerhouse passages (N_Powerhouse), they graphically presented all model results in plots of BIC and R^2. We specifically note that a model containing the N_Powerhouse variable was consistently included among the top models (Δ AIC < 2.0) for both steelhead and Chinook in both life stages analyzed (Petrosky and Schaller, personal communication). Relative variable importance (Burnham and Anderson 2002) of N_Powerhouse was 38% and 35%, respectively, for steelhead SARs and first year ocean survival rates (Petrosky and Schaller, personal communication).

As noted above, the strengths, weaknesses and implications of data and analyses relative to delayed mortality from powerhouse passages would be appropriately considered in a formal WOE ecological risk assessment, which is beyond the scope of the FPC memorandums.

<u>Size selectivity of bypass systems</u>

The ISAB comments regarding size selectivity of bypass systems has been addressed in independent analyses. In Comparative Survival Study analyses size selectivity of bypass systems was addressed. The effect of smolt size on collection probability has been evaluated as part of the CSS study and no clear indication of a strong relationship between size and collection probability was found. The magnitude and relationship between smolt size and collection on efficiency at the Snake River collection dams has been evaluated for wild Chinook (Chapter 9; CSS 2006 Annual Report) and for wild and hatchery Chinook and steelhead (Chapter 4; CSS 2008 Annual Report). No evidence of a size bias for wild or hatchery Chinook at LGR or LGS was found in these analyses; steelhead may exhibit a small size bias in the bypass system. The results of these analyses did not show convincing evidence of size selectivity. Conversely, Zabel et al. (2005) and Williams et al. (2005) did find a bias in the recapture probability at dams downriver of LGR (i.e., LGS, LMN, MCN) for Chinook and steelhead. The reason for this lack of agreement is not known. The population of fish analyzed by Zabel et al. (2005) and Williams et al. (2005) was captured, tagged and released at LGR while the population of fish studied in the CSS was captured, tagged, and released upriver of LGR at various traps.

However, the ISAB comments and concerns regarding size selectivity of bypass systems fail to recognize an important point. In total, size selectivity, (if it actually occurred) delayed mortality, or any other effect on fish is the result of the operation of these hydrosystem projects and their various systems. The argument that delayed mortality of powerhouse passage is associated with fish size would infer that the delayed mortality associated with powerhouse passage is the most severe for wild/natural fish which are generally smaller than hatchery produced fish. This implies that the hydrosytem projects do more harm to wild/naturally produced fish than hatchery fish. In any case the specific and wide ranging impacts of powerhouse systems are ultimately a result of the operation of the hydrosystem projects.

Additional analyses, articles indicating delayed/ latent mortality

We agree with the ISAB that there are many more analyses and publications that indicate that and illuminate the occurrence of delayed mortality associated with powerhouse passage. A formal weight of evidence approach and risk assessment would allow the weighting of additional work in the risk assessment. Fishery management agencies technical staffs continue to pursue the issue of delayed mortality associated with powerhouse passage. A significant body of work has been assembled and considered. Analyses of delayed mortality associated with bypass passage, is continuing.

Delayed mortality associated with turbine passage

We disagree with the ISAB conclusion that the Ferguson et al. (2006) provides little or no support for the latent mortality hypothesis. The ISAB correctly states that Ferguson et al. focuses on fish passage through turbines. The FPC memorandums state that direct route specific survival estimates do not account for the effects of delayed mortality. Turbine passage survival is a component of overall project survival which is the specific performance standard established for mainstem hydroprojects. These studies indicate that it is likely that delayed/latent mortality occurs through all routes of powerhouse passage which would have implications for the direct project survival estimates that comprise the test of project performance standards.

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COMPARATIVE SURVIVAL STUDY
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August 5, 2008

Dear Chairpersons Huntley and Loudenslager:

Since the inception of the state, federal and tribal fishery managers' collaborative Comparative Survival Study (CSS), both the ISAB and ISRP committees have reviewed CSS study design, analyses and reports. Over the past decade the CSS Oversight Committee has provided extensive presentations and analysis at the request of the ISAB and ISRP committees. The ISAB and ISRP reviews have helped improve the CSS study in numerous ways. As a matter of record, the CSS Oversight Committee has given the highest priority to addressing the comments and recommendations of the ISAB and ISRP committees. Consistent with our past approach to the ISRP/ ISAB comments and recommendations, the CSS Oversight Committee has given the recent comments and recommendations on the 10 year retrospective report careful consideration and has expended considerable time and effort researching the literature and body of scientific work in order to fully understand and consider the most recent comments. Our review is presented in the following discussion, which supports reconsideration of the ISRP/ISAB recommendation that resulted in the elimination of funding for tagging specific downstream mark groups from the CSS study. The CSS Oversight Committee believes that the elimination of tagging and analysis resulting from implementation of the ISAB/ISRP recommendation hinders scientific understanding of and resolution of the critical status of Chinook and steelhead populations in the Columbia Basin.

While the ISAB and ISRP Review (ISAB and ISRP 2007-6, hereafter "Review") of the CSS 10-year Retrospective Report (Schaller et al. 2007) was otherwise quite positive and helpful, the end result was the ISAB and ISRP recommendation that ongoing PIT-tagging efforts on Carson hatchery Chinook (an eleven-year time series of tagging efforts) should be terminated and that CSS-proposed PIT-tagging efforts on wild Chinook from the Warm Springs River (Deschutes River Basin) should not occur within the CSS. Following these recommendations, fisheries management agencies including the Washington Department of Fish and Wildlife (WDFW), the Oregon Department of Fish and Wildlife (ODFW), the Idaho Department of Fish and Game (IDFG), the Columbia River Inter-Tribal Fish Commission (CRITFC), and the United States Fish and Wildlife Service (USFWS) sent a letter (Joint Staff Letter, November 29, 2007) to the Northwest Power and Conservation Council staff (Jim Ruff) expressing their objections and concerns over the impacts and ramifications of these recommendations. The Council subsequently adopted the ISRP and ISAB recommendations by eliminating funding for these two tagging efforts in FY 2008 and 2009 from the CSS contract.

We find that the ISAB and ISRP recommendations to terminate existing and disallow proposed tag group monitoring data in the CSS disregard key management data needs that the CSS fulfilled and appear inconsistent with accepted scientific methodologies. In this response, we will show that:

- **The CSS study design is appropriate for measuring and monitoring life-cycle survival rates, for examining environmental factors associated with those rates, and for evaluating hypothesized mechanisms for variation in those rates,**
- **The measurement and inferential approaches used within the CSS are consistent with those used within an extensive body of peer-reviewed literature, including studies conducted by many of the ISAB/ISRP members themselves,**
- **The recommendations by the ISAB and ISRP to terminate existing (Carson hatchery Chinook) and to disallow proposed (Warm Springs wild Chinook) PIT-tagging efforts are inconsistent with their other Review recommendations, are inconsistent with past recommendations on other monitoring studies in the region, and are incompatible with several key management information needs,**
- **The scope of the CSS is primarily to establish a long-term dataset that measures the survival rate of annual generations of salmon and secondarily to examine empirical evidence relating to explanatory hypotheses for developing functional relationships useful for informing management decisions. In their Review, the ISAB and ISRP misinterpret the scope of the CSS as attempting to determine "unambiguous assignment of cause(s)" for all observed differences. These misinterpretations form the foundation for their rationale to eliminate the existing, and prevent the proposed, tagging efforts, and**
- **In addition to these issues associated with the elimination of monitoring tag groups, we would like to note several misconceptions apparent in the ISAB and ISRP interpretations of the CSS Report.**

The management issues affected by the ISAB and ISRP recommendations are simple, but critical. Upriver populations of spring/summer Chinook and steelhead populations have demonstrated poor life-cycle survival rates. Downriver populations of spring/summer Chinook salmon and steelhead have demonstrated much better, and in some cases self-sustaining, life-cycle survival rates. The upriver and downriver tagging efforts in the CSS are focused on determining where the life-cycle survival bottlenecks are occurring. Knowing where survival bottlenecks occur is critical for management decisions (Good et al. 2007). The ISAB and ISRP recommendations resulted in eliminating the possibility of the CSS fulfilling these critical management information needs.

We believe that several of these issues continue to reflect some basic misunderstandings about the CSS, and could have easily been resolved through discussion prior to decisions with long-term consequences being made. Unfortunately, the end result is that key, management-oriented information needs will not be met as a result of these ISAB and ISRP recommendations. Given the long-term implications and consequences of these recommendations, we believe that these recommendations warrant reconsideration. We are willing to discuss these issues with the ISAB and ISRP at any time.

The CSS study design, which includes downriver population groups and comparisons among population groups, is appropriate for measuring and monitoring life-cycle survival rates, for examining environmental factors associated with those rates, and for evaluating hypothesized mechanisms for variation in those rates.

First and foremost, the CSS is a management-oriented, large-scale monitoring study of spring/summer Chinook and steelhead. The foundational objective of the CSS is to establish a long-term dataset that measures the survival rate of annual generations of salmon from their outmigration as smolts to their return to freshwater as adults to spawn (smolt-to-adult return rate; SAR) (Schaller et al. 2007, p. 1). Through PIT-tags, survival rates and other important demographic responses (e.g., migration rates, migration timing) can be partitioned over the smolt-to-adult life-cycle (e.g., during the juvenile freshwater migration, during ocean residency, and during adult upriver migration). PIT-tags provide much finer level of resolution of monitoring information than can be obtained by less intensive methods (e.g., stock-recruitment data). By design, the CSS PIT-tagging efforts address several of the basin-wide monitoring needs, providing the "basic data on performance of upriver and downriver stocks [that] remain of value in monitoring and evaluation" (ISAB and ISRP 2007-6). Monitoring survival rates over the life-cycle can help identify where survival bottlenecks are occurring, which are critical for informing management decisions (Good et al. 2007). Again, the primary objective of the CSS is measurement of survival rates and other demographic responses for upriver and downriver stocks of Chinook salmon and steelhead in the Columbia Basin.

The CSS is consistent with a number of study design frameworks. Under the classification of Hurlbert (1984), the CSS is a comparative mensurative experiment. Under the classification of Rasmussen et al. (2001), the CSS could be considered an example of a time series intervention analysis. Under the classification of Roni et al. (2005), the CSS shares characteristics of both the extensive post-treatment design (through the monitoring of multiple population groups, with population groups providing spatial replication) and the intensive post-treatment design (through the monitoring of population groups over multiple years, with population groups providing both spatial and temporal replication). Under the classification of ISRP (2005-14), the CSS can be considered an effectiveness monitoring study, where the objective is to evaluate the effectiveness of various management actions (e.g., mainstem operations, mainstem and ocean environmental conditions, and transportation). However, the CSS design and analytical methods are most consistent with that of an "observational study" (Cochran 1983, Eberhardt and Thomas 1991, McDonald et al. 2007). Cochran (1983, p. 1) defines an observational study as having two characteristics:

"1) The objective is to study the causal effects of certain agents, procedures, treatments, or programs.
2) For one reason or another, the investigator cannot use controlled experimentation, that is, the investigator cannot impose on a subject, or withhold from the subject, a procedure or treatment whose effects he desires to discover, or cannot assign subjects at random to different procedures."

Observational studies are used extensively within 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). Cochran (1983, p. 2) describes that "a basic difference between observational studies and controlled experiments is that the groups of people whom the investigator wishes to compare are already selected by some means not chosen by the investigator." Examples of observational studies include epidemiological studies that measured and compared lung cancer rates among population groups of smoking and non-smoking individuals. Through those comparisons, the effects of smoking and other risk factors could be examined, despite the ethical and logistic difficulties of randomly assigning individuals to the smoking or non-smoking treatment groups (Eberhardt and Thomas 1991). Analogously, it is obvious that population groups monitored within the CSS cannot be randomly assigned to different hydrosystem entrance locations or assigned to pre-determined environmental conditions (e.g., various levels of flow, spill, ocean upwelling conditions). The observational study design simply measures the biological responses of population groups to the conditions (i.e., treatment effects or management actions) that they experience. Thus, the CSS monitors the differences and similarities among population groups caused by the "natural experiment" within the Columbia Basin (Diamond 1986). However, the inability to randomly assign population groups to treatments or management actions does not preclude valid measurements of biological responses for those population groups or valid comparisons of biological responses among groups (Hurlbert 1984, Jewel 2005).
Diamond (1986) defines a "natural experiment" as a study where the "experimenter does not establish the perturbation but instead selects sites where the perturbation is already running or has run." In the selection of population groups to monitor, "along with the experimental sites, the investigator selects control sites so that the two types of sites differ in the presence or absence of the perturbation but are as similar as possible in other respects" (Diamond 1986). This selection of groups for study that are as similar as possible is known as "matching" or "blocking," and is one of three techniques for reducing the potential for confounding in observational studies (Cochran 1983). Examples of matching and blocking within the CSS include comparisons between population groups of the same species (matching species groups), of the same rearing type (matching rearing type- hatchery or wild), and of the same arrival timing at Bonneville Dam (temporal blocking). The other two techniques described by Cochran

(1983) for reducing the potential for confounding are refinements in technique to reduce measurement error and control during the statistical analysis. Both of these techniques are applied in the CSS. Examples of efforts to reduce measurement error include the use of PIT-tag data, which have high precision relative to other measurement methods, and statistical analyses to remove the effects of sampling error from SARs (Chapter 4, Schaller et al. 2007). Examples of control during the statistical analysis include accounting for transportation location in the analysis of adult upriver migration success (Chapter 6) and analyses of the fork length, tributary emigration timing, and first-to-third dam migration rate relative to water travel time (Chapter 5). Many of those environmental conditions that may influence the observed biological responses are measurable, varying across and within years, providing contrast in the environmental conditions experienced. If time series data of sufficient length are collected over contrasting environmental conditions, the development of functional correlations between the measured treatment effects and candidate associative factors may be possible (ISAB 2003-1, ISAB 2006-3, ISRP 2007-8). Statistical adjustment techniques such as regression and analysis of covariance provide analytical tools that can account for, and potentially remove, the effects of confounding factors in observational studies (Cochran 1983, Rothman and Greenland 1998, Woodward 2005, Jewel 2005).

The CSS provides extensive analyses that examine multiple hypotheses that attempt to explain the observed patterns of differential mortality between upriver and downriver stocks as well as hypotheses on the effects of freshwater and marine habitat conditions on the survival and migration rates of population groups (Chapter 5, Schaller et al. 2007). These evaluations are conducted to determine functional relationships that may be useful for informing management decisions (ISAB 2003-1, ISAB 2006-3, ISRP 2007-8). The CSS maintains “multiple working hypotheses” (Chamberlin 1890) for investigating these differences among population groups, migration experience, and over time. Our intent is to determine the support that the data offer for each competing model or hypothesis (Hilborn and Mangel 1997, Burnham and Anderson 2002). We agree with and apply the view of Hilborn and Mangel (1997) that “science consists of confronting different descriptions of how the world works with data, using the data to arbitrate between the different descriptions, and using the “best” description to make additional predictions or decisions.”

While investigating candidate hypotheses and examining functional relationships are objectives of the CSS, we recognize that the identification of functional correlations does not establish causality. Rothman and Greenland (1998) provide an excellent discussion of the issues surrounding causality in scientific investigations. They state that “perhaps the most important common thread that emerges from the debated philosophies is Hume’s legacy that proof is impossible in empiric science” and that “even when they are possible, experiments (including randomized trials) do not provide anything approaching proof.” As a result,

“All of the fruits of scientific work, in epidemiology or other disciplines, are at best only tentative formulations of a description of nature, even when the work itself is carried out without mistakes. The tentativeness of our knowledge does not prevent practical applications, but it should keep us skeptical and critical, not only of everyone else’s work but of our own as well.”

They contend that the appropriate scientific response to the tentative nature of these formulations is to “focus on testing the negation of the causal hypothesis” and that “observations can provide crucial tests of competing non-null causal hypotheses.” The methods and analyses used within the CSS are consistent with these views. None of the analyses in the CSS (or any other study in the region, including fully randomized experiments) are capable of proving cause-effect relationships (Rothman and Greenland 1998). However, consistent with accepted analytical techniques, extensive efforts were undertaken in the CSS to evaluate, examine, and in some cases negate proposed causal hypotheses for observed differences between population groups. The CSS examinations of the evidence for various hypotheses using assembly rules constitutes a "weight-of-evidence" approach, which has been recommended and applied to examine causal links for treatment effects in observational studies (Fox 1991, Gilbertson 1997, Beyers 1998, Lowell et al. 2000, Morales et al. 2003, Adams 2003, Brown et al. 2003, Collier 2003, Culp and Baird 2006).

Several of the key hypotheses relating to upriver/downriver comparisons that were examined in the CSS Report included investigations of differences in smolt emigration timing, differences in smolt size, differences in migration rate, and differences in smolt arrival timing to below Bonneville Dam. These were the primary hypotheses that had been proposed for explaining the observed differences between upstream/downstream stocks. Despite finding little evidence in support of these primary hypotheses, the ISAB and ISRP speculate that additional hypotheses are the cause of the observed differences and conclude, without scientific analysis, that it would be impossible to examine the degree of evidence for those hypotheses. The hypotheses proposed by the ISAB and ISRP (many of which could be addressed in future analyses) include differences in productivity, predator populations, local climatic conditions, life history, and "that the salmon have likely adapted and evolved (as well as they can) in the time elapsed" and that "Snake River stocks are, in all likelihood, no longer the same as their pre-hydrosystem ancestors." However, recent genetic evidence from the standardized region-wide database indicates that John Day and Snake Basin spring/summer Chinook stocks are closely related (Seeb et al. 2007, Fig. 3). This study provides evidence that Snake Basin population groups have not diverged from the John Day population groups, which migrate through fewer dams.

Making comparisons among population groups and the inferential approaches used within the CSS are consistent with those used within an extensive body of peer-reviewed literature, including studies conducted by many of the ISAB/ISRP members themselves. Measuring and making comparisons in survival, productivity, and other demographic measures among salmonid population groups is a common, well-established, and accepted scientific methodology for drawing inferences on factors associated with measured variation, both within the Columbia Basin (Schaller et al. 1999; Botsford and Paulsen 2000; Deriso et al. 2001; Petrosky et al. 2001; Budy et al. 2002; Wilson 2003; Zabel and Achord 2004; McHugh et al. 2004; Scheuerell 2005; Schaller and Petrosky 2007) and across the northeast Pacific Ocean (Adkison et al. 1996; Botsford and Lawrence 2002; Bradford 1995; Brodeur et al. 2004, 2007; Fisher et al. 2007; Hare et al. 1999; Hodgson et al. 2006; Holt and Peterman 2004; Mantua et al. 1997; Morris et al. 2007; Mueter et al. 2002a, 2002b, 2005, 2007; Orsi et al. 2007; Peterman et al. 1998, 2003; Peterman 2004; Pyper et al. 1999, 2001, 2002, 2005; Pyper and Peterman 1999; Schindler et al. 2003; Su et al. 2004; Trudel et al. 2007a, 2007b; Quinn et al. 2005). The spatial scales of these comparisons range from small (< 10 km) to large (up to 3,000 km) in the distances between population group spawning locations. Within the Columbia Basin, the range in distances and level of complexity between population groups for comparisons are obviously much smaller than those that have been examined across the northeast Pacific Ocean and published in the peer-reviewed literature.

Consistent with the scientific methods used within this body of work on salmonid population group comparisons, for over fifteen years researchers have compared and identified differences in the survival and productivity of upriver and downriver spring/summer Chinook stocks in the Columbia Basin using stock-recruitment data (Petrosky and Schaller 1992, 1996; Schaller et al. 1996, 1999; Deriso et al. 1996, 2001; Botsford and Paulsen 2000; Schaller and Petrosky 2007). Obviously, these studies have met the scientific standards of the peer-review process and were published in the peer-reviewed literature, in opposition with the viewpoints of the ISAB and ISRP that making comparisons among these same population groups is compromised by "inevitable confounding of all differences" (ISAB and ISRP 2007-6) and therefore should not occur within the CSS. Additionally, the primary criticism against using stock-recruitment data is that the data are imprecise and require assumptions about the stock-recruitment relationship. However, the CSS overcomes these issues by directly estimating differential survival through PIT-tag SARs. That is, no stock-recruitment relationship is necessary and the PIT-tag survival estimates are highly precise.

Consistent with the peer-reviewed literature cited above, the CSS measures and compares survival rates within and among salmonid population groups and across migration experiences. Contrary to the perspectives of the ISAB and ISRP, the process of making comparisons does not require that all potential causative factors must be controlled and accounted for (Hurlbert 1984, Jewel 2005). It is simply not possible to quantify or eliminate all possible interacting factors affecting species in a large ecosystem. However, the basic process of scientific inquiry is to use observational data to examine the degree of evidence for or against proposed hypotheses on the putative causes of observed variation. The ISAB and ISRP (2007-6) state that comparisons between population groups in the CSS do not meet scientific review criteria due to the inability to "unambiguously assign cause(s)" (p. 9), populations are not replicates of other populations (p. 12), "the inevitable confounding of all differences" (p. 12), and "the system is too complex" (p. 9). However all 39 of the publications listed above used similar observational study approaches as the CSS and met the scientific criteria of highly respected journals. Other comparative observational studies conducted by ISAB and ISRP members themselves using observational data (e.g., Poe et al. 1991; Bilby et al. 1998, 2003; Pearcy et al. 1996; Sork et al. 2005; Weeder et al. 2005) also applied observational study approaches and met the scientific criteria of peer-reviewed journals.

It appears that the ISAB and ISRP apply these criteria and judge them to be insurmountable flaws of one minor aspect of the CSS objectives. These criteria are then used as justification for their recommendations to terminate and disallow the collection of key monitoring data that are needed for the identification of survival bottlenecks and the evaluation of candidate hypotheses. The recommendation that "the additional monitoring and evaluation of downstream stocks should not be directed toward upstream-downstream comparisons" stands in stark contrast against the vast body of peer-reviewed and published scientific work on salmonid population dynamics and the comparisons made therin.

The recommendations by the ISAB and ISRP to terminate existing (Carson hatchery Chinook) and to disallow proposed (Warm Springs wild Chinook) PIT-tagging efforts are inconsistent with their other Review recommendations, are inconsistent with past

recommendations on other monitoring studies in the region, and are incompatible with several key management information needs.

As stated above, the primary objective of the CSS is to establish a long-term dataset that measures the survival rate of annual generations of salmon from their outmigration as smolts to their return to freshwater as adults to spawn (smolt-to-adult return rate; SAR). Objectives 1-3 of the CSS proposal, which met "scientific review criteria," describe how that long-term dataset is to be established using each tagging group proposed, including the downriver tagging groups. At points in the Review, the ISAB and ISRP appear to recognize the value of these downriver monitoring data collected by the CSS. For example, they note:

"future effort on downriver stocks might be directed at monitoring and evaluation of wild stocks, including determination of SARs and other population metrics for these stocks. This would be a logical part of a regional monitoring and evaluation program to inform managers of stock status and could be useful for assessing recovery of threatened and endangered stocks."

Because these first three CSS objectives apply to the downriver tagging groups proposed, and these three monitoring objectives "meet scientific review criteria," then therefore the downriver tagging groups must meet scientific review criteria on the merits of their monitoring value alone. The ISAB and ISRP recommendations to terminate the existing and disallow the proposed downriver tag groups in the CSS is therefore inconsistent with their support of the CSS monitoring objectives 1-3 in the Review.

Based on previous ISRP reviews, the region has invested in a long-term (11-year) time series of monitoring data for the Carson hatchery stock. Terminating this time series and disallowing the collection of other lower-river tagging time series within the CSS precludes the identification of where life-cycle survival bottlenecks are occurring, and knowing where survival bottlenecks occur is critical for management decisions (Good et al. 2007). Terminating and disallowing the lower river tagging efforts also precludes future investigations on the factors that may be affecting the performance of these stocks. These recommendations directly conflict with the guidance provided by the ISAB themselves on using multiple population groups to examine causal links (McDonald et al. 2007):

"Monitoring multiple, independent, but similar observational studies, and analyzing them as if they were replicates subject to study-specific random effects, may provide convincing evidence for the effect of an action if results are consistent across the studies. In an inductive sense, monitoring numerous observational studies can suggest causal relations (e.g., Shipley 2000)."

It appears that the ISAB and ISRP have been inconsistent in their criticism of comparisons between population groups in the CSS relative to their recommendations on other large-scale monitoring efforts. As mentioned above, the CSS is an example of a large-scale effectiveness monitoring study and is classified as a comparative mensurative experiment. As defined by the ISRP (2005-14; page 20), the objective of an effectiveness monitoring study is:

"Establishment of mechanistic or causal links between management actions and population responses with conclusions justified by replicated results and subjective judgment."

The ISRP (2005-14; page 25-26) then go on to state that when mensurative experiments are replicated over space and time,

"corroborative results of the studies can provide compelling evidence for general conclusions. Such a mensurative study can be quite powerful and quite convincing when many replications of experimental results occur, i.e. there is establishment of the same relationships over several or many smaller studies. In this inductive sense, census and statistical monitoring in mensurative experiments do support research on cause of effects. The ISRP believes that this is the best study design for establishment of effectiveness of management actions in a large ecosystem such as the Columbia River basin. However, such conclusions require subjective judgment."

The two main points of the ISRP appear to be that: 1) powerful, convincing and compelling evidence for causal links can be established when mensurative experiments are replicated over space and time, and 2) some degree of subjective judgment must be used in interpreting resulting evidence on potential causal links arising from effectiveness monitoring studies. With these points, we fully concur. However, when considering the mensurative experiments replicated over space and time within the CSS, the ISRP and ISAB (2007-6) conclude, without scientific analysis, that comparisons within the CSS insurmountably preclude

"unambiguous assignment of cause(s) impossible even if convincing, statistically significant differences in fish performance were established between upriver and downriver stocks. In sum, the system is too complex, and the possible sampling design necessarily too constrained in time and place, to reach conclusive findings on causation from this type of comparison."

These polar viewpoints are highly inconsistent. Either mensurative experiments replicated over space and time, combined with subjective judgment, are capable of providing compelling evidence of causal links or they are not. If they are, then the CSS mensurative experiments replicated over space and time, combined with subjective judgment, are capable of examining evidence for causal links. If they are not, then the recommendations provided by ISRP (2005-14) and McDonald et al. (2007) are false. Instead of supporting increased spatial and temporal replication efforts, as recommended by ISRP (2005-14), ISAB (2006-3), ISRP (2006-6), and McDonald et al. (2007) through the Carson and Warm Springs tagging efforts, the ISAB and ISRP recommend the termination or prevention of those replication efforts. Additionally, we found little or no evidence that the ISAB or ISRP found the scientific judgment used in the CSS was in any way inappropriate. In ISAB and ISRP (2007-6) there was no criticism of the extensive analyses that examined the main hypotheses on differences between upriver and downriver smolt characteristics, smolt responses to environmental conditions, or the temporal blocking comparisons between groups arriving at Bonneville at the same time. There was no criticism that the scientific judgment used by the CSS authors overstepped proper scientific interpretation or was in any way inappropriate when interpreting the results from these comparisons. But there also was no allowance for continued investigation of the newly-postulated hypotheses offered by the ISAB and ISRP to explain away the differences between upriver and downriver population performance. Instead, the ISAB and ISRP recommend terminating the future temporal replication that would have been provided by the Carson Hatchery tagging efforts (an 11-year time series) from the CSS and did not support augmenting the spatial and temporal replication efforts that would be provided by the Warm Springs wild

Chinook tagging proposed by the CSS. These recommendations against replicating mensurative experiments over space and time directly oppose what the ISRP (2005-14) believes to be the "best study design for establishment of effectiveness of management actions in a large ecosystem."

We are aware of several other examples of the ISAB and ISRP supporting the use of comparative reference groups for making inferences using observational studies. The ISRP has supported the methodologies of project 200311400, a study which makes upriver/downriver (Snake vs. Yakima) comparisons between hatchery spring Chinook survival and migration rates using small numbers of radio tags (ISRP 2006-6). In ISRP (2004-8), the ISRP compliment a study design that uses CWT fish from a downriver hatchery stock released into the estuary (Blind Slough) to improve understanding of the effects of the ocean environment on upriver (Snake) spring Chinook stocks (study title: "Evaluation of the relationship among time of ocean entry, physical, and biological characteristics of the estuary and plume environment and adult return rates"). Another example is the ISRP (2007-1) review of project 200001400 where they support the downriver monitoring of lamprey populations, because this monitoring would be "providing the opportunity to compare trends in abundance of lamprey populations not affected by mainstem dams with those occurring above the dams." In ISAB and ISRP (2005-15), evaluating the effects of supplementation "involves contrasting trends in treatment and reference locations" even though they recognize that "treatment and reference locations will undoubtedly differ from each other beyond the supplementation treatment." Obviously, reference streams are not perfect replicates of supplementation streams, but despite this limitation the ISAB and ISRP recommend that researchers compare and contrast trends between reference and supplementation streams. In contrast, when the ISAB and ISRP consider the CSS comparisons, their "core reason a contrast of salmon survival between upriver and downriver locations is not advised is that the populations in tributaries downriver of the dams are not replicates of the upper Snake River populations" (ISAB and ISRP 2007-6; page 12). How is this criticism of the CSS comparisons consistent with the ISRP's support of Snake/Yakima comparisons in ISRP (2006-6) or the ISAB and ISRP support of supplementation versus reference stream comparisons in ISAB and ISRP (2005-15)? All are examples of observational study comparisons between sites or groups that are not perfect replicates of each other in all other factors beyond the treatment effect.

However, the most dramatic reversal in opinion on the utility of reference groups came from ISRP (2006-6) themselves in their review of the CSS proposal. In regard to the downriver tag groups, the ISRP (2006-6) said the following (emphasis added):

"For this upriver-downriver comparison to be generally accepted, it seems prudent to **add more downriver sites in the future**. In response, **the CSS will add another downriver site in the Warms Springs River for wild Chinook tagging for 2007** to complement the ongoing tagging in the John Day River. **This is a positive action, however, additional downriver hatchery sites are even more important to add** because at this time, five upriver hatcheries are being used as tagging sites and only one downriver. There needs to be better hatchery to hatchery comparisons, and **adding several lower river hatcheries which show a range in return rates will provide a more realistic comparison in survival rates**."

At that time, the ISRP appeared to recognize that additional downriver tag groups would greatly improve understanding of between-population variation among the downriver population groups and between upriver and downriver groups. This improved understanding would also help identify life-stages of poor survival among populations (Good et al. 2007). In terms of additional downriver hatchery sites, Schaller et al. (2007; page 176) responded to the ISRP request for the identification of additional downriver hatchery sites, stating:

"Additional candidate populations relevant to these SAR comparisons from downriver hatcheries of the Interior Columbia include Klickitat, Warm Springs, and Round Butte (depending on fish health constraints). Future monitoring should also consider incorporating PIT-tag SARs from the upper Columbia region to expand these regional comparisons."

In terms of additional wild downriver tag groups, Schaller et al. (2007; page 175) responded to the ISRP interest in complementing ongoing tagging in the John Day River by stating that the CSS

"has proposed (but not received funding for) PIT-tagging wild spring Chinook smolts in the Warm Springs River (Deschutes Subbasin). Additional candidate populations relevant to these SAR comparisons from downriver areas of the Interior Columbia include Klickitat and Yakima rivers. Future monitoring should also consider incorporating PIT-tag SARs from the upper Columbia region to expand these regional comparisons."

Clearly, the CSS has identified several wild and hatchery population groups that could be used to quantify between-population variation in downriver areas. But the ISAB and ISRP have elected to terminate and not support the possibility of the CSS project collecting the data to monitor this variation.

These recommendations to terminate the Carson tagging effort and not support the proposed Warm Springs tagging effort are not compatible with key management information needs. The CSS monitors life-cycle survival rates and other demographic rates among upriver and downriver population groups within the Columbia Basin ecosystem. Good et al. (2007) describes the management importance of collecting these types of data at large spatial scales:

"The strategy of the recovery planning process has thus been to confront the large-scale biological and management challenges by incorporating of relevant scientific information at similarly large scales."

Good et al. (2007) goes on to describe how the productivity and life-cycle survival data, such as those collected within the CSS, are important for the assessment of managed populations:

"Stage-specific or lifetime productivity (i.e., population growth rate) provides information on important demographic processes. Abundance and productivity data are used to assess the status of populations of threatened and endangered ESUs."

Finally, when describing the management information needs of the Technical Recovery Teams (TRTs), Good et al. (2007) describes how large-scale observational data, including the natural variation in those data, is useful for quantifying cumulative effects:

"The TRTs have moved on to analyses of the cumulative effects of multiple factors over large spatial scales, employing metapopulation models where possible, and fostering the use of large-scale experimentation to manipulate or take advantage of natural variation in ecological factors."

The information collected from downriver groups within the CSS can be used to help fulfill these critical management information needs. Therefore terminating the Carson tagging efforts and not supporting the Warm Springs tagging efforts is not compatible with key management information needs.

The scope of the CSS is primarily to establish a long-term dataset that measures the survival rate of annual generations of salmon and steelhead. Secondarily, the scope is to examine empirical evidence relating to explanatory hypotheses and to determine functional relationships useful for informing management decisions. In their Review, the ISAB and ISRP misinterpret the scope of the CSS as attempting to determine "unambiguous assignment of cause(s)" for all observed differences. These misinterpretations form the foundation for their rationale to terminate the existing, and failure to support the proposed, tagging efforts.

The primary objective of the CSS is to establish a long-term dataset that measures the survival rate of annual generations of salmon from their outmigration as smolts to their return to freshwater as adults to spawn (smolt-to-adult return rate; SAR). Secondarily, the scope of the CSS is to examine multiple hypotheses that attempt to explain the observed patterns of mortality within and between upriver and downriver stocks as well as hypotheses on the effects of freshwater and marine habitat conditions on the survival and migration rates of population groups. These secondary evaluations are conducted to determine functional relationships that may be useful for informing management decisions.

In their Review, the ISAB and ISRP misinterpret the scope of the CSS as attempting to determine "unambiguous assignment of cause(s)" for all observed differences between upriver and downriver population groups. None of the analyses in the CSS (or any other study in the region, including randomized experiments) are capable of proving cause-effect relationships (Rothman and Greenland 1998). To be clear, as stated in the CSS Report, "our specific interest through the CSS was whether upriver/downriver differences in overall survival for wild and/or hatchery stream-type Chinook (with more precise estimates from PIT-tagged groups) *were consistent with the differential mortality estimated from S-R models for wild populations*" (Schaller et al. 2007; page 7). These types of comparisons would also be useful for indentifying specific periods within the life-cycle when survival may be a limiting factor among population groups. When the ISAB and ISRP infer that the scope of the CSS is to provide "unambiguous assignment of cause(s)" for differences between population groups compared in the CSS, we feel that this is a misinterpretation of our scope. The ISAB and ISRP then go on to cite reasons why "unambiguous assignment of cause(s)" is impossible (reasons that we are well-aware of), and use these reasons as foundation and justification for eliminating the downriver tagging efforts. These "unambiguous assignment of cause(s)" criteria are unreasonable and unattainable for all of the observational studies in the region that are used to inform management decisions, including the CSS.

In addition to these issues associated with the elimination of monitoring tag groups, we would like to note several misconceptions apparent in the ISAB and ISRP interpretations of the CSS Report. The ISAB and ISRP comments are listed below, with our responses following.

Page 12: "The core reason a contrast of salmon survival between upriver and downriver locations is not advised is that the populations in tributaries downriver of the dams are not replicates of the upper Snake River populations."

Response: While the upriver/downriver populations compared within the CSS Report were not presented or characterized as perfect replicates of each other, making comparisons among population groups is a well-established and common scientific method for examining and drawing inferences about patterns in survival, productivity, and other demographic measures among salmonid population groups (see 39 publications listed above and recommendations of McDonald et al. (2007)). Nowhere do we state or imply that populations are perfect replicates of other populations. On the contrary, we consider each marked group as a replicate only of itself, and only in that year. The information obtained from each marked group may or may not share similar patterns with other marked groups. As stated above, the primary objective of the CSS is to measure the biological responses of population groups. Secondarily, we describe which marked groups are similar and which are different by making comparisons, and then go on to examine which factors are associated and which factors are unassociated with those similarities and differences.

Page 12: "Geographical variation in habitat types, productivity, predator populations, and local climatic conditions makes cause and effect interpretation problematic, even if more hatchery and downriver wild stocks could be identified."

Response: We clearly stated that the purpose (p. 106) of the upriver/downriver SAR comparisons was to determine if the previously-established differences in mortality estimated from spawner-recruit (SR) analyses were also apparent in the PIT-tag derived SARs. Nowhere did we state or imply that the purpose of the upriver/downriver comparisons was to determine cause and effect relationships; this is a false premise and a mischaracterization of the scope of the CSS Report. Contrasts of the point estimates and 90% CI from the two types of data (p. 131-133) indicated SAR-based estimates of differential mortality agreed well with published SR-based estimates of differential mortality. We characterized the upriver-downriver comparison as a "natural experiment", which therefore has more limited scope for inference than manipulative experiments (p. 150), but the approach and methods are consistent with observational studies in scientific literature (see 39 publications above). Further, we investigated and tested hypotheses regarding possible biological causes (including alternative hypotheses previously suggested by the NOAA NWFSC) of differential mortality between upriver and downriver wild stream-type Chinook (p. 136-143).

The CSS Report thoroughly investigated all of the testable scientific hypotheses that had been proposed as candidates for the SR-based estimates of differential mortality. The CSS Report performed carefully-constructed comparisons to examine the evidence for and against those hypotheses. Neither the content nor the outcomes of these comparisons were recognized in the Review. Instead, the ISAB and ISRP present newly proposed hypotheses and then criticize the

CSS as being incapable of examining those hypotheses against existing data sets without allowing any opportunity for the CSS authors to investigate the evidence for or against those hypotheses.

Page 13: "The sponsors have presented evidence suggestive of a hydrosystem effect on differences in SARs between upriver and downriver sites, but little may be gained from further analysis of differences in SARs. The major conclusions of the research are already available for scrutiny by scientists and managers in peer-reviewed scientific literature and reports including the retrospective summary."

Response: Based on 5 years of PIT-tag SAR comparisons between wild Snake River and John Day River smolts, we have seen a consistent pattern of differential mortality across poor and favorable ocean conditions. Combined with estimates of in-river survival and relative survival of transported smolts, this is one line of (indirect) evidence that the magnitude of delayed hydrosystem mortality is large (e.g., Peters and Marmorek 2001; Schaller and Petrosky 2007). We strongly disagree with the opinion that little may be gained from further analyses of differences in SARs. On the contrary, we view the most critical task at hand as being to examine factors associated with those observed differences using life-stage-specific survival data, consistent with the recommendations of Good et al. (2007). We believe that these types of analyses, in conjunction with the detailed studies for fish travel time, reach survival, in-river SARs, transport SARs, and overall SARs are the critical measures that can and should be examined relative to candidate environmental and management factors. These passage characteristics and survival data provide critical information needed to inform management decisions regarding fish passage and survival through the FCRPS, and the likely effects of those decisions on adult returns (Peters and Marmorek 2001, Wilson 2003).

Page 9: "This is a single river system, without comparative measures of fish performance from before the hydrosystem was constructed, which makes unambiguous assignment of cause(s) impossible even if convincing, statistically significant differences in fish performance were established between upriver and downriver stocks."

Response: Contrary to this assertion, there are stock-recruitment data measuring comparative fish performances prior to and following full development of the FCRPS (e.g., John Day: 1959-1995; Imnaha: 1949-1995; Schaller et al. 1999). These stock-recruitment data have already established significant differences in fish performance between upriver (seven stocks) and downriver (six stocks) populations, and that the differences coincided with the construction of the hydrosystem (Schaller et al. 1999, Deriso et al. 2001). The task at hand is to examine factors currently associated with those observed stock-recruitment differences using life-stage-specific survival data (Good et al. 2007), and PIT-tag data provide the best opportunity to monitor life-stage-specific survival rates for making those comparisons.
Collection of the stock-recruitment data is an ongoing task among the fishery management agencies, and these data will provide ongoing opportunities to make comparisons with life-stage-specific survival data, but only if PIT-tag data are available. The primary limiting factor that is preventing further comparisons between stock-recruitment data with life-stage-specific survival data is the lack of downriver populations marked with PIT-tags. As mentioned above, nowhere

does the CSS Report state or imply that the purpose of the comparisons is to assign causes, unambiguous or otherwise.

Page 13: "We now doubt that there are a sufficient number of appropriate downriver wild stocks available to make a meaningful comparison…All differences between upriver and downriver stocks would be candidates for causal factors, and, as we note above, it seems impossible to adequately control or rule out all alternative causes."

Response: In the Report, we identified Warm Springs, Klickitat, and Yakima Rivers as other wild downriver groups that would be useful for expanding downriver tagging efforts. We also identified the Warm Springs, Klickitat, and Round Butte hatchery stocks as useful additions for expanding downriver tagging efforts. It is an unreasonable and unachievable standard to require that all causative factors must be identified and accounted for prior to drawing any inferences on factors associated or unassociated with observed patterns of variation in observational studies. If the ISAB and ISRP believe that this standard must be met before making comparisons and drawing inferences, then all scientific investigations of real-world phenomena using observational data would fail to meet this criterion. This would include each of the 39 publications listed above. This would also include comparisons made by ISAB and ISRP members themselves using observational data (e.g., Poe et al. 1991; Bilby et al. 1998, 2003; Pearcy et al. 1996; Sork et al. 2005; Weeder et al. 2005). Clearly, the ISAB and ISRP are applying an arbitrary and unreasonable standard in judging the upriver/downriver comparisons presented in the CSS Report.

Page 9: "In sum, the system is too complex, and the possible sampling design necessarily too constrained in time and place, to reach conclusive findings on causation from this type of comparison."

Response: Making comparisons among salmon population groups across space and time, and subsequently drawing inferences based on the resulting observed patterns is a well-established and common scientific methodology (see 39 publications listed above). The level of complexity among stocks in the Columbia Basin is highly unlikely to be greater than the level of complexity among stocks separated by up to 3,000 km in the northeastern Pacific Ocean, as has been investigated by other researchers (Adkison et al. 1996; Botsford and Lawrence 2002; Bradford 1995; Brodeur et al. 2004, 2007; Fisher et al. 2007; Hare et al. 1999; Hodgson et al. 2006; Holt and Peterman 2004; Mantua et al. 1997; Morris et al. 2007; Mueter et al. 2002a, 2002b, 2005, 2007; Orsi et al. 2007; Peterman et al. 1998, 2003; Peterman 2004; Pyper et al. 1999, 2001, 2002, 2005; Pyper and Peterman 1999; Schindler et al. 2003; Su et al. 2004; Trudel et al. 2007a, 2007b; Quinn et al. 2005). Again, nowhere does the CSS Report state or imply that the scope of the comparisons is to conclusively find or determine causes of the observed differences.

Page 9: "This is a single river system, without comparative measures of fish performance from before the hydrosystem was constructed, which makes unambiguous assignment of cause(s) impossible even if convincing, statistically significant differences in fish performance were established between upriver and downriver stocks. In sum, the system is too complex, and the possible sampling design necessarily too constrained in time and place, to reach conclusive findings on causation from this type of comparison."

Pages 12-13: "There is inevitable confounding of all differences between downriver and upriver stocks and their environments, precluding clear attribution of cause for any upriver/downriver differences that might be shown."

Response: Significant differences between upstream and downstream SARs are found (e.g. non-overlapping confidence intervals in Figure 5.12). Unambiguous assignment of cause from this study alone is not necessary for the analysis to be useful. Other lines of evidence address the question of effects of the hydrosystem. There are comparative measures of SARs and estuary/ocean survival from before the completion of the hydrosystem (see Petrosky et al. 2001 and Wilson 2003).

Page 15: "Additional analyses related to differential mortality and survival metrics related to dam passage such as route of passage, temperature, cumulative stress, and predation are lacking."

Response: Chapter 4 compares SARs of C_0 and C_1, which addresses questions of route of passage and cumulative stress. Cumulative stress is a possible explanation for differences in SARs between upriver and downriver stocks seen in Chapter 5. Given the purpose, scope, and focus of the CSS, analyzing predation impacts is not practical; there are other studies which examine that issue.

Page 18: "The C_0 cohort remains undetected until it is below BON, and its numbers and survival through the hydrosystem must be estimated, inevitably more by assumption than by checkable results. That translates into a substantial estimation variance."

Response: Fish in the C_0 group are defined as undetected only at the collector projects (LGR, LGO, and LMN). A substantial portion is detected downriver at other projects (MCN, JDA, BON, and the estuary PIT-trawl). The number of smolts in the C_0 category is estimated rather than observed, but estimation is done through standard CJS methodology and the accuracy of the estimation process is explored in Chapter 7 (and was also investigated indirectly in the 2002 Annual Report). The magnitude of estimation variance of number of C_0 smolts is detailed in Chapter 4, as well as Appendix E, and is usually quite small (1-6% CV). The number of returning adults in the C_0 category is not an estimate but a count of fish detected at LGR.

Page 19: "What is more bothersome is the fact that in the process of back-translating LGS- and LMN-transported stocks to LGR, we lose any opportunity to compare the SARs of these additional cohorts, call them T_1 and T_2 for convenience, with the SARs of T_0, C_0 and C_1. Such contrasts could be very useful indices of the success to be gained (or lost) by transportation from lower on the hydrosystem. ISAB asked for some resolution of this issue as early as the COMPASS (ISAB 2006-2) report, but this matter has yet to be clearly resolved."

Response: Transport project-specific SAR distributions are estimated and compared to in-river (C_0) SARs in Chapter 4. Given the limited annual sample sizes, and the fact that the LGR equivalent transport smolt number (T_0) is larger than the number of smolts transported from any project, reporting project-specific TIRs for individual years would be of limited value.

Page 19: "It seems unlikely that D will ever be definitively determined. Moreover, interpretation of D as the 'out of hydrosystem effect' requires that we separate the survival experience of BON-ocean-BON from that of BON-LGR, or at least the assumption that survival from BON-LGR is the same for T_0 as for C_0 (and/or for C_1). In spite of the small numbers returning for the upriver journey, it would be useful in the future to make an attempt to separate the effects."
Page 22: "We question, with reference to Chapter 3, whether it is reasonable to assume that D could be allocated to "out of hydrosystem effects," which at very least would require that the BON → spawning ground component of SAR(T_0) and SAR(C_0) could plausibly be as identical. This chapter clearly shows that assumption to be false; there are "within hydrosystem effects" embedded in D. There is unequivocal evidence that SAR(C_0) > SAR(T_0) for the BON-LGR leg of the upriver journey."

Response: There is no need for survival from BON-LGR of T_0 and C_0 or C_1 fish to be identical—surviving adults are enumerated at LGR. Any difference between groups is properly reflected in the ratio of SARs [TIRs or SAR(C_0) / SAR(C_1), or *D*]. *D* is intended to reflect effects *below the hydrosystem*, or "post-Bonneville", i.e. subsequent to the juvenile downstream migration up until returning to LGR as adults. Effects reflected in *D* (or TIR) do not necessarily have to be *outside the hydrosystem*, given that fish must migrate up the same hydrosystem they traversed downstream as juveniles. "Definitive determination" of *D*, like that of SARs, TIRs, or S_R is not possible. It is also not necessary in order to develop useful information for management decisions. These parameters are necessarily estimated with uncertainty; this uncertainty is reflected in the probability distributions presented and in the conclusions drawn.

Page 23: "Smolts transported from LGS (or from lower projects) fare better on the homeward journey than do those transported from LGR, but we have no reports on the compensating benefits on the outward journey, because the data on survival of T1, T2, and so on have been "back-adjusted to LGR" and are lumped with LGR-transported fish (T0). That is not necessary, and indeed, it is time for a direct assessment of SAR- and TIR-values for fish of the T1 (LGS-transported), T2 (LMN-transported), and other transported smolts as well, all in comparison with the C0 and T0 cohorts."

Response: Direct, project-specific estimates of transport SARs, TIRs, and *D*s were made for wild Chinook and wild steelhead in Chapter 4.

We hope that the ISAB/ISRP will receive this additional information in the spirit in which it is intended, of collaborative scientific exchange. The result of the ISAB/ISRP review comment was to limit tagging efforts and eliminate specific types of analysis. We believe that the censoring of analyses and prevention of data collection efforts, rather than constructive review and scientific criticism of analysis, does not advance our knowledge and tools to contribute to resolution of the current critical status of salmon and steelhead in the Columbia Basin.

Sincerely

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