A review of
Effects of the FCRPS on Salmon Populations
Williams et al. (2003)

Overview and Landscape Issues:

Formatting

The preliminary draft "Effect of the Federal Columbia River Power system on the Salmon Populations" contains no table of contents and has minor editorial problems characteristic of a preliminary draft. The document contains four sections: A Historical background, Methods, Results, Discussion, and Conclusions/Summary. The Results Section is organized according processes/measures with a subdivisions addressing run type. The Discussion Section is organizes by run type and with subdivisions of processes/measures. A priority during the redrafting of this document should be a reader friendly organization structure.

Background

The historical background is informative and covers issues developed in PATH, in particular the explanation of the decline of the stocks as related to three hypothesis: a hydro effect, a regime shift effect, and a stock viability effect. The document discusses factors related to these hypotheses but it does not address them in a well-organized manner. Missing from the introduction is any discussion about the direct effect of hydrosystems operations on fish survival.

In particular, the historical background lacks the recent discussions on the impact of flow on fish survival, impact of spill, especially summer spill, and any basis for the spread the risk strategy for passage. Also missing are discussions on the impact of harvest and hydrosystem operations on adult salmon. While discussions of harvest may not be germane to the topic of the paper, the fact that harvest is not included as a separate technical memo is a critical omission.. The almost minimal treatment of adult survival and the factors influencing it, are especially noticeable due to the recurrent mention throughout many of the draft papers of evolutionary fitness and the importance of diversity. Empirical support for changes in fitness or diversity, requiring generations to be manifested is difficult enough, without further complicating the task by basing such a hypothesis primarily on the survival of juveniles in an anadromous life cycle.

The background's most evident omissions involve the impacts of flow, water augmentation, and water withdrawals on fish survival and the effect of different seasonal spill strategies on the populations. The flow-survival relationship is an important issue. The Council has devoted considerable time to the issue and has received presentations by NOAAF, the state and tribal fisheries agencies, ISRB, and NSF.

Methods Section

The Methods Section contains a number of topics but the linkage between them is somewhat confusing. Numbered headings would help here and throughout the report.

The Methods Section has several omissions. The estimate of in-river survival to Bonneville Dam, which is required to compute D, is not described. A second omission is a description of the flow index calculation. A significant part of the report was devoted to an analysis of selective, or size dependent, mortality but the methods were not described, presumably because the analysis is in review.

Results Section

The Results Section discusses a number of issues including: population trends, travel time, yearly survival estimates above and within the hydrosystem, differences between hatchery and wild fish, yearly and temporal estimates of SARs as a function of passage history, differential delayed mortality (D), and the influence of fish size on mortality (selective mortality). The Results Section principally updates the yearly estimates of SAR and in-river survival. New material includes the seasonal SARS and D values over a number of years and a number of figures relating fish tagging length to collection efficiency and survival.

The yearly data are valuable in that they update the ongoing and evolving datasets. The seasonal SAR and D data and the survival/collection efficiency vs. size graphs are interesting but they lack the actual data and the relationships are only presented as lines, which are presumably regression lines. The cursory treatment of this information makes it difficult to assess the significance of the trends.

Missing from the Results Section are relationships of SAR and in-river survival with passage conditions including flow, spill, temperature, and turbidity. These are significant omissions since flow and spill are used in managing fish passage and research indicates that temperature may be a predominant factor in fish survival. In presentations to the NPCC, NOAA claimed that flow may have a broken stick relationship where mainstem passage survival is dependent on flow below 100 kcfs. Essentially none of this analysis and alternative interpretations is mentioned in the Document. Further, there was no assessment of the impact of spill on fish survival. It would be particularly valuable for NOAA to evaluate the seasonal value of spill on fish survival in much the same way they evaluated the seasonal impact of D.

Discussion Section

The Discussion Section varies significantly from the Results Section and some issues raised in the Discussion are not mentioned in the Results Section. For example, the discussion mentions the spread the risk policy although it is not mentioned elsewhere in the Document.

NOAA states the ocean is the most important factor in the temporal pattern of the populations and speculates that ocean conditions may be worse with global warming. However, they do not address the issues of river temperature on the direct and delayed fish mortality, which are both mechanisms through which global warming can affect the populations. Furthermore, the above statements seem in disagreement with other studies by NOAA (McClure et al. 2003) that conclude that oceans factors are not that significant

in determining the stock trends, or that global warming is not a significant consideration. This apparent conflict is worth resolving or at least noting in the Document.

Evidence that SARs are lower for marked fish compared to unmarked fish is significant and suggests cumulative stress in handling contributes to delayed mortality. How do these findings affect the estimates of stock productivity and extinction? And do you have any suggestions on how to further define this differential effect? Without definition, it seems an insolvable dilemma.

The paper argues that differential delayed survival between transport and in-river fish, D, increases with season. Additionally, this leads to a conclusion that estuary entry timing interacting with the physical and biological status of the estuary appears to be the key. This may or may not be correct since temporally varying river properties interacting with fish condition could also produce a time varying D. More work needs to be done to resolve the mechanisms behind seasonally varying SARs and D. Irrespective of the causes; the idea that seasonal variation should be considered when developing transportation strategies is valuable. However, it is hard to know what to do with such suspicions unless the mechanisms are fully known.

NOAAF notes that the CSS studies support their hypothesis of seasonally varying SARs. This is useful information but the CSS studies are not otherwise mentioned in the Result Section. It would be appropriate for the NOAAF document to review a wider range of pertinent studies. Currently the focus in the Documents is on NOAAF studies. If the Document is to represent a comprehensive update of what is known about the effects of the hydrosystem on the fish then other studies besides NOAAF's should also be considered. This requires a significant reworking of the document. The current document is misleading in that it gives the impression that it is a comprehensive review, which it is not.

The Document takes issue with the Budy et al. (2002) hypothesis that stress or disease cause lower adult returns, stating that if this were true we would not see higher SARs later in the season. The NOAAF logic appears flawed. If fish are susceptible to stress early in the transportation season, for what ever reason; undeveloped immune system or smoltification, then we could obtain the observed seasonally increasing SARs. Generally, hypotheses on seasonal SARs have not been carefully considered by any group. The NOAAF hypothesis that time varying estuary factors are responsible for seasonally varying SARs and D is but one hypothesis. Fish condition, stress and diseases varying with river conditions or fish development may also be important. NOAAF incorrectly discounts these alternatives prematurely. There is enough information to suggest that fish condition during the freshwater period is important to the run size. Furthermore, the variation of D between dams suggests fish condition and stress affect D. In any case, although the hypotheses for the spatial and temporal variations in D are not fully articulated and clearly not resolved, the fact that NOAAF is addressing the issue is an important step to improving the transportation operations.

The discussion on the impact of the ocean on stock trends is excellent. However as mentioned above the conclusions reached in the Document may be somewhat in disagreement with the NOAAF studies estimating trends. These differences should be considered and clarified.

The section on Diversity is interesting but its foundation is not established in the Methods or Results section of the Document. That said, the arguments for diversity do not appear to have a quantitative foundation. The statement that a fish passage system may select for particular stocks or life history and could therefore reduce diversity if used exclusively is theory, as are all evolutionary selection processes. It should be carefully stated as such.

That said, the decline in SAR vs. number of detection suggests that bypass systems may be more stressful than transportation or even turbine passage. If this hypothesis proves correct, passing fish thorough bypass systems may not spread the risk. Rather it could increase risk to the stocks. Furthermore, population experiencing multiple passage routes over generations may be under additional stress compared to a population that adapts to a more stable life history experience. For example, a population that is transported in one generation (1.5 day migration time) and passes in-river in the next (migration time 4 weeks) may have lower fitness than an population that is allowed to evolve to a single migration timing. Mechanisms that allow individual stocks to adapt specific life history strategies will result in diversity but mechanisms that force the individual stocks to experience widely different life histories across generations may be undesirable.

Specific Comments:

Page 5: A precise definition of "D" would be valuable in the first paragraph.

Page 9: Does ICH have the bypass facilities referred to here, or do you mean TDA?

Page 13- Presumably the daily survival estimates were weighted by daily smolt population size passing the dam.

Page 14. Is it necessary to use a stratified approach of Sandford and Smith (2002). Does the analysis produce similar estimates of yearly SARs when seasonal averages are used instead of daily detection probabilities?

Page 16: (1) While you go on to mention it later in the report, discussion of the T:I ratio as a stand alone issue would seem to help perpetuate the notion that this relationship is significant, or even a reliable assessment of the overall passage experience and its affects. The fact that transportation benefits fish when D is greater than survival for inriver fish seems the far more pertinent point to make.

Page 16: (2) Why is the broken-stick or threshold survival model that appeared in presentations earlier this year absent in this report? It seems that was critical in depicting NMFS view of flow/temperature effects.

Page 17: (1) Recognizing that you could not keep track of all the ongoing passage and transportation studies and their idiosyncracies, it would seem the CSS fish were, at least, identifiable and tractable? In recent years they have been subject to a sort-by-code system that gives them a higher probability of transport than other hatchery fish. This violates the assumption of equal probabilities of transportation in the calculation of SARs by detection history. Have you plans to adjust in some way?

Page 17: (2) You refer to estimation of both SAR trends and of temporal D trends. Some folks may be skeptical. Can you share your methods?

Page 18: λ does not seem to be defined.

Page 19: Clearly there are important differences in SARs over the season, but if the pattern is not consistent or predictable, one will not generally be able to change operations to increase SARs for fish overall.

Page 20: That SARs of tagged fish are smaller than SARs for untagged fish seems valuable knowledge to pass on to decision makers if we are relatively sure it occurs. Is it possible to provide better documentation of the method for determining this relationship?

Page 21: (1) The delayed mortality described here is synonymous with the Extra Mortality in PATH? It is distinct from "D", or delayed mortality specific to transport. Also, the same delayed mortality is discussed in Ferguson et al, as Extra Mortality. The result is confusion, potential contradiction and/or redundance. We suggest in our comments on the Passage paper the discussions be combined here. This also applies to the section on selective mortality.

Page 21: (2) "we set the non-detected category equal to 1" – an equation would be useful here for clarity.

Page 22: The section on using SARs to evaluate stocks other than the Snake River is, regretfully, true. A word or two more on the potential consequences to decision making may help bring the point home.

Page 26: "about 5%" should be 50%

Page 27- The conclusion statement following table 5 does not seem that solid for steelhead. Wild fish survived about 5 percentage points than hatchery fish, and 4 out of 6 years displayed higher survival. This is not very convincing evidence to purport that hatchery fish a re good surrogates for wild ones.

Page 31, Table 8: Standard Errors on SAR's would be useful (also Table 9 on p. 33). 1999 Hatchery summer chinook – what does "Wells Hatchery Wells" mean?

Page 34: "Yakima spring chinook had survival similar to Snake River spring chinook …" We believe you, but a table or figure would help demonstrate your veracity. The same for "If mortality averaged 50% …"

Page 36: As …" median travel times decreased" – did temperature, turbidity, spill, etc. also remain constant?

Page 37: "Based on Sandford and Smith … " – a discussion of their methods would be more convenient for the reader.

Page 51: Its difficult to follow the logic of "little room exists to presume any additional delayed mortality …". This point is important and controversial, and needs to be spelled out clearly and in detail.

Page 53: (1)"… ocean conditions may become worse than any we have experienced." – it seems they become a whole lot better, too. We understand the need to be conservative, but wonder if the conservatism can be applied fairly?

Page 53: (2) The reference to" 2 to 6% SAR" seems in need of citation & explanation.

Page 53: The observation that PIT tagged fish may not survive as well through to returning adult as the unmarked population is an interesting finding. However, the variances associated with SAR estimates are a bit large, which may draw into question the ability to identify a true difference in return rates.

An alternative explanation for different return rates may be that some tags are shed as adults mature. I believe NOAAF investigators documented this for some species held in captivity to maturity. Even so, whatever the causes, the conclusion offered at the end of page 53 is sound and important. The return rates estimated with PITs likely underestimate the true return rates of the populations. It would be appropriate for NWFSC

Page 57: The last sentence under the Flow, Temperature, and Migration Timing section, is not clear. If there are long, constant periods with steady flow, how could we expect to see a flow-survival relationship? More to the point, of what significance could it possibly be?

Page 63: We question the reference to few fish in the general population getting transported. Additionally, the conclusions in the Transportation section for fall chinook seem most speculative and unwarranted, given the extreme paucity of data they readily recognize. We believe this section may need reassessment by the investigators.

Addendum:

Alternative method to compare SAR's for PIT-tagged smolts vs. run-at-large

All calculations are made on a migration year basis, unless noted otherwise, and can be done for the usual Snake spring migrant groups (hatchery/wild, chinook/steelhead).

Let:

$Trans_i$ = Smolts collected for transport at LGR on day i (estimated by USACE);
$Phat_i$ = Proportion of PIT-tagged fish bypassed/detected at LGR, day i;

Then total smolts at LGR on day i is:

$Stot_i = Trans_i/Phat_i$.

$Phat_i$ can be taken directly from Sandford and Smith 2002 for earlier years, and readily calculated for more recent years.

The migration year total smolts at LGR, of course, is simply the sum of the $Stot_i$'s over the migration season.

This provides a back-check on the FPC's estimates of smolts at LGR, and, for hatchery fish, can also be checked against hatchery releases, since the annual total of $Stot_i$ for, say, hatchery chinook, should equal the sum of smolt releases from each hatchery multiplied by the PIT-tagged derived survival rate from the hatchery to LGR.

In addition, age-at-return to LGR can of course be derived from tagged jacks and adults for each downstream migration year. If need be, one could use only a subset of the releases (e.g. fish tagged at LGR) to get a potentially more representative sample of the run-at-large.

This method makes several assumptions, of course:

- The $Phat_i$'s are representative of the run at large;
- The age-at-return from PIT tags are representative of the run at large;
- (For the hatchery back-check) the survival rates from hatchery to LGR from tagged fish are representative of the run at large;

However, it does *not* assume that the SAR's for tagged fish are representative of the run at large, since the SAR's are derived from collected fish at LGR. Furthermore, assuming that the $Trans_i$'s are estimated without error, one can make many statistical inferences about the precision of the total smolt and SAR estimates.

Finally, it cannot be used for years prior to the early 1990's, when few fish were tagged.

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