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Review of the Interior Columbia River Technical Recovery Team's Analyses of Survival Changes Needed to Meet Viability Criteria

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Introduction

In a November 27, 2007 letter from NOAA Fisheries, Dr. Usha Varanasi requested that the ISAB review the Interior Columbia River Technical Recovery Team's (ICTRT) November 2007 technical review drafts of "Assessing the Impact of Environmental Conditions and Hydropower on Population Productivity for Interior Columbia River Stream-type Chinook and Steelhead Populations" and "Required Survival Rate Changes to Meet Technical Recovery Team Abundance and Productivity Viability Criteria for Interior Columbia River Basin Salmon and Steelhead Populations" (Gaps document).

The review request included three specific questions:

1) Are the matrix modeling methods used to estimate changes in population productivity under alternative scenarios scientifically sound? Are the methods used to estimate population-specific survival changes required to meet ICTRT goals scientifically sound? Are there any significant conceptual or methodological flaws in the ICTRT's approach?

2) Is the range of assumptions about future climate and hydrosystem conditions reasonable and appropriate given currently available information?

3) Is the report clearly written? Are the methods described in sufficient detail for a reader to understand and replicate what was done? Are assumptions and uncertainties about the analyses clearly described?

The centerpiece of a science-based recovery plan should be a rigorous approach that has, as its foundation, a model or models by which one can judge the strategies for recovery, progress toward recovery, and that one can use to discover needs for further research, data collection, and analyses required for adaptive management decisions (ISAB 2007-4). The matrix models developed for this project form a basis for understanding the potential changes in survival required to meet ICTRT recovery goals. They also provide a platform for studying the potential impacts of climate change or alternative hydropower management scenarios on Columbia River Chinook salmon and steelhead. Models are not the real world, of course, so these projections provide *estimates* of the required survival changes or climate and hydropower impacts.

The authors develop two matrix models, one for Columbia River stream-type Chinook and one for steelhead. Both are life-history structured matrix models for the species in question and are parameterized from field data. The models appear to be scientifically sound and appropriately parameterized.

The authors propose three environmental scenarios. Among these, the “recent” and the “pessimistic” models are drawn from nearly the same time interval, so they consequently provide little contrast in years; however there is fairly significant contrast in survival. The recent scenario might be more appropriately called the “baseline scenario” since this corresponds to the baseline period used in the TRT viability assessments. Also, from what we have learned from our own climate change report[1] it appears that the pessimistic scenario may not be sufficiently pessimistic. In addition, although an optimistic scenario would seem to be extremely unlikely to transpire, it might be instructive to include one in the modeling report.

The Gaps document describes how the TRT team measured the gap between current survival and abundance statistics and those that would be required to meet ESA goals for the stocks and the ESUs in question. There are four criteria to be met: abundance, productivity, spatial structure, and diversity. The gaps determinations are based on just the first two of these, abundance and productivity, as these two are the only ones that are easily addressed through modeling.

The gap is expressed as the percentage increase in survival that is required to meet recovery goals. However, there are two dimensions to the gap, productivity and abundance. Where the stocks are vulnerable because of low productivity, a percentage increase in survival may indeed be a good measure of the gap. In the case of a shortfall in abundance, however, the gap may be a result of habitat limitations, not easily expressed in terms of an increase in survival (or alleviated by attempts to increase it).

Answers to Questions

1) Are the matrix modeling methods used to estimate changes in population productivity under alternative scenarios scientifically sound? Are the methods used to estimate population-specific survival changes required to meet ICTRT goals scientifically sound? Are there any significant conceptual or methodological flaws in the ICTRT's approach?

   The Interior Columbia Technical Recovery Team has developed two matrix models, the first based on the life history of Snake River spring summer Chinook salmon and the second for steelhead. The matrix models seem to be scientifically sound, but both models could be made clearer with the addition of a life history diagram which relates the various parameters and clearly delineates the life stages, their timing and their relationships. This is particularly true of the Chinook salmon model, where there are questions about timing of events, several undefined variables and parameters, and a number of typos.

   The point of the gaps document is to describe how the TRT team determined the gap between current survival and abundance statistics and what would be required to meet ESA goals for the stocks and ESUs in question. Their overall approach

[1] See ISAB2007-2 (www.nwcouncil.org/library/isab/isab2007-2.htm)

seems to be sound, but there are questions about the final metric they have chosen. The goal should be to summarize in a simple way as much insight as possible about the nature and extent of the factors preventing recovery. The gap measure described in this report is simple, but it is potentially misleading, because it seems not to distinguish between gaps caused by unacceptable natural productivity and unacceptable abundance resulting from a shortage of suitable habitat. A pair of gap measurements would shed more light on both the nature and extent of factors limiting recovery.

2) Is the range of assumptions about future climate and hydrosystem conditions reasonable and appropriate, given currently available information?

In describing the oceanic conditions for the three environmental scenarios, the authors define *recent* as the assessments for brood years 1978-1999, *historical* as the last 60 years, and *pessimistic* as brood years 1975-1997. The *recent* and *pessimistic* periods are almost completely congruent in time intervals (though there are measurable differences in survival and outcome). Moreover, oceanic conditions are probably going to get worse for salmon than they have been in recent years, perhaps substantially worse, so the pessimistic scenario may not be sufficiently pessimistic. Also, from the document, it is not entirely clear how the "baseline survival" in the hydropower scenarios was determined.

3) Is the report clearly written? Are the methods described in sufficient detail for a reader to understand and replicate what was done? Are assumptions and uncertainties about the analyses clearly described?

Both reports could use some good editing. Many points need clarification. The report is difficult to navigate without a Table of Contents, especially since the formatting of subheadings is inconsistent. The reports could be greatly improved just by assigning and using variable names precisely – ideally names or notation that are consistent with the viability criteria document that we recently reviewed. Also, much important information in the Gaps report is left to Appendix A of that document, at the very end of the report.

The approaches, some of which are documented in the literature and some of which are *ad hoc*, *may* be useful, appropriate, and valid, but there is essentially no outside-the-group documentation (i.e., literature, peer-reviewed or not) for the methodologies and the rationale for using them. This may not be critical to the report, but if it is, then some more use of important literature is necessary. There is plenty of it out there for some of the topics. For example, using the low/increasing portion of a stock-recruitment curve as an index of stock resiliency (i.e., productivity) has been evaluated in several papers (Myers 2001, Denney et al. 2002). A very useful report on the Hockey-stick model, its strengths, and limitations, and comparisons with the Beverton-Holt model, is Barrowman and Myers (2000).

Specific Comments

Many of the questions below were suitably addressed by authors of the documents during a briefing at the ISAB's January 25, 2008 meeting. They are repeated here for reference.

Chinook Model

The Interior Columbia Technical Recovery Team has developed two matrix models, the first based on the life history of Snake River spring summer Chinook salmon and the second on steelhead life history. Each of the models could be made clearer with the addition of a life history diagram that relates the various parameters and clearly delineates the life stages and their relationships. There are several undefined variables and parameters, and a number of typos.

The chinook model is in particular need of clarification. It operates on a one year time step, apparently enumerating the fish just before each birthday, but there is no table that clearly lays out the definitions for each life stage. In addition, the authors have, by their own admission, taken some liberties with the life history:

1) One stage includes survival from Bonneville to the spawning stream, spawning, development and emergence of the young all the way up to their first birthday. Juveniles are first counted at the end of this stage.

2) The second stage (survival from their first birthday through the second year) apparently includes parr to smolt survival and survival of the downriver migration through the hydrosystem.

3) The third stage (survival from the second birthday through the third year of life) represents survival through the first year in salt water.

4) The last two stages represent up to two years of further survival in salt water.

5) The timing in the model and the reality of the life history do not seem to correspond completely. Inasmuch as survival is multiplicative, over a life span, one may still arrive at the correct number of fish, but numbers in the mid-life stages and simulation experiments involving specific interventions internal to this life cycle could be somewhat misleading.

6) The model would be clearer if the third fertility term (first row, third column in the matrix) was listed as $b_3\, S_A\, F_3(t)$, instead of zero. Granted that $F_3(t)$ is later set to zero, but at some point it may be necessary to give it some other value.

7) S_o is never defined.

8) It would be nice to have the equation given when a term is defined by one, e.g., $S_A = S_u\,(1 - h_r)\,S_{sb}$. In general, transparency of exposition should be the objective.

9) The Equation on line 245 needs clarification: clearly explain the time indices $parr_{t+1}$, R_t, and SAR_{t+2}. Alternatively, redefine the time subscripts in a way that is more intuitive.

10) Is $S_2 = S_d(t)$, or is it $S_2 = S_d(t)\ S_{ps}$, or possibly something else?

11) At what point is differential delayed mortality (D) applied and why?

The Steelhead Model

Thanks to a more complete table of parameters with their definitions, and a clear statement of the time of enumeration of the fish, the steelhead model is much easier to decipher, even though the steelhead life history is somewhat more complex. There are again some typos, but the basic model seems to be in good order.

Scenarios

It is not entirely clear from the document how the "baseline survival" in the hydropower scenarios was determined and exactly how it differs from "Current Operations" and "Biop." (note: Tables 5 and 6 are mis-numbered)

In the environmental scenarios, the "recent" and the "pessimistic" are drawn from nearly the same time interval, leaving us with essentially two scenarios, not three. It might be useful to have optimistic (though extremely unlikely to occur), historic, and pessimistic scenarios. Also, the ISAB's climate change report (ISAB 2007-2) suggests that the pessimistic scenario may not be sufficiently pessimistic.

The Gaps Report

1) The point of the document is to describe how the TRT team determined the gap between current survival and abundance statistics and what would be required to meet ESA goals for the stocks and ESUs in question. We should begin with the reminder that there were four criteria that had to be met, collectively labeled as *VSP* (acronym not defined) *parameter*s: (a) abundance, (b) productivity, (c) spatial structure, and (d) diversity. Here, items (c) and (d) are alluded to in passing, and the reader is directed to a website for the detailed description of how these features, which are described as difficult to quantify, are to be assessed. They were apparently considered in defining the ESUs and the recovery criteria, but they do not seem to bear on the subsequent assessment. Something needs to be said about those two criteria, even if no more than an indication of how the relevant information is to be assessed.

2) The "gap" is verbalized as change from current condition that is needed for the stock or ESU to meet the TRT criterion for recovery, but is presented as a sort of % improvement in survival that would be needed to yield recovery. Calculation of this number is fraught with difficulties, and Appendix A spends a considerable amount of

space explaining how those difficulties have been dealt with. It develops that it is not (quite) a survival improvement, as calculated.

3) The gap measure described in this report is simple, but it is potentially misleading, because it seems not to distinguish between gaps caused by (i) unacceptable natural productivity (low r_{max}), or (ii) unacceptable extent of suitable habitat (low K or natural carrying capacity). A pair of gap measurements would provide more insight - about both the nature and extent of factors limiting recovery. In any case, if a single distance measure is to be used, it seems odd to call it a "survival gap" when it depends on abundance as well as survival. "Viability gap" would be a better term, but perhaps misleading, given that the other components of viability (spatial structure and diversity) have not been considered; "demographic gap" might be preferable.

4) Although there are stage-specific demographic models that are used to compute the various survival numbers, the end result is presented as a single number, not subdivided, though the report makes it clear that stage-specific strategic changes will be needed in developing effective intervention strategies for particular ESUs. The inputs for the various scenarios are available to the TRT team, who rely on the accumulated databases for the region, so detailed dissection of particular life histories can presumably be mounted as needed, stock-by-stock, ESU-by-ESU.

5) ICTRT targets are set by a combination of 20-year averages and 10-year geomean averages, but the nature of the combination is unclear. Is the 20-year average also a geomean, and if not, why are two different averages being used? Why not subdivide into 20-11 and 10-1? Whatever is done for this report, the reader needs more information.

6) There are a number of points in the delivery where the authors lapse into jargon whose meaning is unclear to the outside reader. Just for example, on Page 3, what does "**. . .** spawning level associated with achieving juvenile capacity" mean? Later, what is "spawner escapement"? These terms may be "industry standards", but clarity would be served by defining them.

7) On Page 4, the authors comment that they have not developed scenarios to address the impact of future habitat changes (further degradation or amelioration), but point out that the matrix tools in the other report can be used to do that. As configured here, the "gap" caused by inadequate or insufficient habitat is reported as a survival target that is said to be equivalent to habitat changes that would be necessary to increase abundance to the required level. Hockey stick algorithmics aside, however, an increase in available habitat should not to be confused with an increased R/S ratio. Survival and abundance are two different things, and to reduce them to a single number obfuscates the strategic issues. There are situations where increasing the R/S ratio (via increases in survival) will increase abundance to the required level, and other situations where it will not, due to inadequate habitat.

8) On the top of Page 5, we see that *latent mortality* enters the survivorship calculations. We have commented extensively elsewhere on the difficulty of separating *latent* from other sources of mortality, so the inclusion is potentially a bit worrisome. As long as all the mortality is accounted for, terminology is a side issue, but we probably need some clarification on this point.

9) In describing the oceanic conditions for the three environmental scenarios, the authors define *recent* as the assessments for brood years 1978-1999, *historical* as the last 60 years (Figure 3 shows that as 1945-2001), and *pessimistic* as brood years 1975-1997. This is a less-than-ideal set of choices. (a) Everything we know about climatic warming makes it clear that we can expect different oceanic conditions in the next 100 years; there has been quite a bit of modeling of what to expect, and there are range of scenarios available to the NOAA team. (b) The *recent* and *pessimistic* periods are almost completely congruent, so they are not really different. Moreover, oceanic conditions are probably going to get worse for salmon than they have been in the last 25 years, perhaps substantially worse (see comment (a) above, along with the ISAB Climate Report, and references cited therein). (c) It would be nice to have an *optimistic* scenario to go along with the *pessimistic* scenario, though we are unlikely to experience it, but perhaps the period from 1945 to 1974 could provide the necessary numbers. Precise specification aside, it would be nice to have *high*, *medium* and *low* oceanic survival scenarios included in the assessment.

10) The precise meaning of the "gap" does not become evident until Appendix A, starting on page 43 of a 68-page report. We need at least a quick mental construct, presented early in the delivery. Even after navigating the methods used to compute it, the construct remains troublesome. Setting aside, for the moment, the question of whether it is wise to use a single number to deal with two different sets of issues, the point of increasing survival is that a population with an R/S ratio of a certain critical size can increase, and anything less than that number will lead to a decrease in population size. Moreover, the R/S ratio can be expected to vary somewhat from year to year, as described nicely in the report, and some sort of geomean would be in order (assuming approximately exponential growth, below threshold abundance). The threshold R/S ratios were established in the ESU establishment process, presumably outside the purview of this report. We need the projected R/S ratios (under different scenarios), the target R/S ratio, and some convenient measure of the difference between the two. In Appendix A, the authors seem to be defining the Gap as (NM – 1). This is not wrong, of course, but neither is it intuitively obvious. This is essentially a standard demographic problem, for which we have established demographic terminology. Why not use a demographic construct?

11) The tables are extensive, for each stock in each ESU, and for each evaluated scenario, but as currently presented, they are too information-dense. Half the space is empty, which we interpret as an indication that half the scenarios have yet to be completed. Whether that is true or not, the tabular entries are so tiny that they are virtually unreadable. One can magnify them in the word processor, of course, but larger font and fewer entries would be better.

12) At one point, the authors are using Chinook returns to model Steelhead returns (where there are no Steelhead data). They flag it explicitly and fairly, but the two species are quite different in their demography and in their responses to virtually every variable in the scenarios, and one has to wonder how well the Steelhead ESU is served by this choice. At the very least, the TRT team needs to be explicit about the limitations of this practice, and it would not be amiss to flag the need for better Steelhead data for this ESU in the report.

13) The really critical aspects of the report are presented in Appendix A. The details are voluminous enough that there really is no easy alternative, but the reader first has to wade through 42 pages of text and extensive tables, with no clear sense of what the numbers actually mean. The "Gap" has a non-obvious and non-standard definition. Some thought has to be devoted to reorganizing the report, if it is to be useful for anyone who has to make practical decisions.

14) The presentation of Gap computation is confusing. The growth model is either logistic or a saturation curve. The matrix models, all fairly standard, can be adapted to non-exponential growth over a wide range of abundance. The necessary techniques exist in the literature and are verbalized in the matrix document. As pointed out by the authors, some of these populations have inadequate habitat (thus, sub-critical abundance), but their productivity (R/S ratios) are inadequate to sustain even those levels of abundance. Other cases have reduced R/S ratios, because abundance exceeds the carrying capacity of the available habitat. Increasing the R/S ratio in density-limited cases is not going to be possible, without increasing the amount of habitat. We have two issues that need attention here, and we probably need two measures.

15) The presentation at the top of page A-3 is not helpful. Everything is defined, but the logic of why we are doing it this way is unclear. The HR construct is not intuitively obvious; it may be perfectly sensible, but why we need to *adjust* at all is unclear, and how this *adjustment* deals with the situation is also unclear. It probably just needs clearer exposition. The meaning of the vertical green line in Figure A3a&b is unclear. Separation between sectors A and B also seems arbitrary.

16) The treatment of parameter uncertainty is unclear. The philosophy would seem to be that in the face of uncertainty, one should generally shoot higher, "just to be on the safe side," but it is not entirely clear how this is being done. An alternative, of course, would be to put a confidence band on the NM or gap figures that indicates just how uncertain they are. For example, an NM value reported as 1.6 ± 0.8 would tell the reader something different from 1.6 ± 0.1. The distribution on the NM values is probably closer to log-normal than normal, of course, so perhaps we need to have log (NM) ± SE reported. In any case, they should consider treating *uncertainty* as *uncertainty*, rather than as a *need for higher restoration target*. Given that they are simulating over a multi-year varying scenario, SE should be a computable number.

17) A future equilibrium escapement is projected by multiplying current abundance by the survival gap (example on page A-9). However, this projection might be inappropriate if the deficiency in survival to be remedied exists at a life history stage that precedes the stage limited by habitat capacity. In this case, the potential to increase abundance will be constrained by the subsequent bottleneck in habitat capacity. It seems to be assumed that the survival gap will always be reduced during or after downstream migration, and thus, after smolt abundance has been limited by constraints on the amount of suitable freshwater rearing habitat.

18) More explanation is required on page A-1 about the second method to estimate current maximum productivity (r_{max}) by selecting a subset of data in which data points from the last 20 years were deleted if the spawning escapement was greater than the median value. The following sentences (last on page A-1, first on A-2) which describe the process for choosing between these estimates of current productivity are confusing. As stated, the process would seem to ensure that high estimates of r_{max} become higher, whereas low estimates become lower.

19) Explain more clearly how the spawning abundance data have been adjusted to take annual variations in fishing mortality into account. The equations on page A-3 are unintelligible, in part because subscripts for brood year (t) and calendar year (t+1) are not distinct.

20) Text on page A-3 states that the estimate of minimum spawning escapement producing maximum recruitment (K) from stock-recruitment analysis was averaged with the value predicted from regression of historical spawner capacity on core area (weighted intrinsic potential). It is not clear whether this is the case for all populations. It would also help to explain why these estimates are averaged, instead of using the predicted values and recalculating the stock-recruit curves for a new estimate of r_{max}.

21) Explain more clearly how the "minimum productivity threshold" is determined (first mentioned on page A-5 and used to standardize productivity in figures A3 and A4). Contributing to the lack of clarity is the fact that the *x*-axis is scaled to the minimum threshold in Figure A3 but apparently not in Figure A4.

22) In Table A1 on page A-8, the abundance for zone A need not always be "below threshold", as currently stated.

23) Examples in Figures A5 and A6 and text on pages A-9 to A-11 are confusing. It would help to use consistent names for parameters in the hockey stick model (r_{max} or *a* for maximum productivity, and K or *b* for maximum recruitment). Contrary to the figure captions and text on A-12 (first paragraph), the scenarios in these examples that involve an increase in r_{max} also include an increase in K.

24) In Table 2 on page A-19, clarify the statistic "20-yr Prod." Presumably this is the ***geometric mean*** over the past 20 years.

25) “Observed gap” is used on page 3 before it is defined on page 4.

26) The four steps on page 3 do not correspond with the three steps in Appendix A.

27) First sentence of last paragraph on page 5 is almost unintelligible.

28) Page 15 an increase by a factor of 1.23 and a 123% increase are not the same. A 123% increase is a factor of 2.23. The index for measuring the gap needs to be clearly defined, and it needs to have intuitive appeal.

29) Caption for Table 1 is awkward due to incomplete sentences or punctuation.

30) Renumber bullets on page A-1; first bullet should include: “Estimate ***geometric means of***…”

References

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