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UNITED STATES DISTRICT COURT
DISTRICT OF OREGON

NATIONAL WILDLIFE FEDERATION, *et al*. Plaintiffs, v. NATIONAL MARINE FISHERIES SERVICE, *et al.* Defendants.	Civil No. 01-640-RE 2008 REPLY DECLARATION OF CHRISTOPHER L. TOOLE, Ph.D.

I, Christopher L. Toole, declare and state as follows:

1. On October 24, 2008, I provided a declaration in support of the National Marine Fisheries Service's (NOAA Fisheries) 2008 Biological Opinion (BiOp) for the Federal Columbia River Power System (FCRPS) in this litigation. There, I described my qualifications as a fishery biologist and my role in the development of NOAA Fisheries' BiOp. In that declaration, I discussed issues raised in declarations prepared for the plaintiffs NWF and the State of Oregon by Mr. Frederick Olney, Dr. Steven Orzack, Mr. Edward Bowles, and Ms. Patty Glick.

2. I have reviewed a second round of reply declarations filed by Mr. Bowles, Mr. Olney, and Dr. Orzack and now provide this declaration to respond to further comments and criticisms raised in the reply declarations. In addition, I reply to one point raised in Ms. Glick's earlier declaration.

<u>Response to Reply Declaration of Edward Bowles</u>

3. Mr. Bowles, in Paragraph 1 of his declaration, claims that NOAA Fisheries used "a subset of the ICTRT data set" to calculate the metrics in the biological opinion. He further references a variation in time periods among populations for return-per-spawner (R/S) calculations and states that ODFW also ascertained that R/S time periods varied by population, and then argues that NOAA Fisheries explored leaving out some years in R/S calculations.

4. As I described in Paragraph 30 of my first declaration, NOAA Fisheries used the same "subset" of years that the Interior Columbia Technical Recovery Team (ICTRT) used to calculate average returns-per-spawner (R/S) in their Current Status Summaries (NOAA AR B.193) and to calculate intrinsic productivity and gaps (NOAA AR B.194 and B.196). These calculations start and end on different dates for different populations in the ICTRT's calculations

because the available data sets ended in different years and the ICTRT was attempting to include 20 brood years in every calculation.   NOAA Fisheries intentionally matched the ICTRT time periods, resulting in the same variation in starting and ending dates for different populations in the BiOp. As I also described in Paragraph 30 of my previous declaration, the ICTRT dropped years with low spawner numbers and NOAA Fisheries also dropped these years, in spite of the fact that this results in lower estimates of average R/S compared to estimates derived from leaving low spawner abundance years in the data set. As I also explained in my previous declaration, the exact years used for the metrics in the BiOp are available in NOAA AR C.1096, B.080, B.081, and B.082.

5. As I described in my previous declaration in Paragraphs 27, 28, and 31, the draft BiOp used median population growth rate (lambda) calculations and time periods identical to those in the ICTRT Current Status Summaries available at the time (NOAA AR B.193). The primary time period was approximately 1980 until the most recent year available. For the final BiOp, NOAA Fisheries ensured that a consistent 1980-present time period was used. Per Footnote 6 of my previous declaration, a 1990 to the present time period was also analyzed and included in an appendix to reflect the secondary time period described in the Lohn metrics memo as well as the most recent time period in the Biological Review Team (BRT) analysis (NOAA AR B.155). The ICTRT did not include a BRT trend analysis in their Current Status Summaries at the time, so the BRT trend time periods were chosen to match the lambda time periods. My previous declaration includes citations to relevant BiOp pages that describe this rationale.

6. While Mr. Bowles' new declaration no longer suggests that NOAA Fisheries selectively chose particular time periods in order to bias results, he does point out in Paragraph 1 that NOAA Fisheries considered alternative time periods. Of the administrative record

documents Mr. Bowles cites in his Paragraph 1, C.0673 is an e-mail that demonstrates NOAA Fisheries' insistence that the ICTRT method be used and that 1978 not be used as a starting point for analyses properly starting in 1980; C.1096 is an e-mail that shows that NOAA Fisheries conformed estimates to the most recent ICTRT data set and included all time periods discussed in the Lohn memo in its calculations (*see also* Footnote 6 of my first declaration); and the other four documents cited by Mr. Bowles do not appear to reflect any discussions by NOAA Fisheries of alternative time periods.

7. Mr. Bowles states in his Paragraph 2 that I referred to "ICTRT calculations" in my previous declaration, when in fact I was referring to calculations by NOAA Fisheries staff. I cannot find such a reference in my declaration. In addition to the descriptions in the BiOp and its appendices, I believe that Paragraphs 4-5 above and information in my previous declaration accurately describe the calculation of metrics and their relation to ICTRT products.

8. Mr. Bowles' Paragraph 3 cites various NOAA Fisheries documents related to the need to consider uncertainty and acknowledges that NOAA Fisheries addressed uncertainty in the BiOp, but then offers Mr. Bowles' opinion that NOAA Fisheries did not "adequately" address uncertainty. He offers no explanation for this opinion regarding adequacy. In Paragraphs 33-41 of my previous declaration, I responded to each of his previous criticisms by reviewing NOAA Fisheries' treatment of uncertainty and pointing out how it was considered throughout the BiOp and its appendices.

9. In Paragraph 4, Mr. Bowles states that NOAA Fisheries interpreted positive lambda and BRT trends as changes in population status over the base period, independent of changing environmental conditions. This appears to be the same point Mr. Bowles attempted to raise in Paragraph 72 of his first declaration and it continues to be incorrect, as I pointed out in

Paragraph 45 of my previous declaration. The base period trends represent demographic changes, which are influenced both by management actions and by environmental factors (*see also* BiOp page 7-36). Base-to-current and current-to-future multipliers are based on management actions and are independent of changing environmental conditions. The combination of the base period trends and the multipliers affect the BiOp conclusions, so these conclusions are not reliant solely on favorable environmental conditions in the latter part of the base period.

10. The larger point in Mr. Bowles' comments in Paragraph 4 seems to be that NOAA Fisheries' analysis should have attempted to adjust the lambda and BRT trend metrics to factor out environmental trends from the demographic trends. While Mr. Bowles believes that NOAA Fisheries should have done this for the BiOp, he does not mention that the ICTRT did not determine that such an adjustment was necessary for its calculations of lambda in the Current Status Summaries (NOAA AR B.193) nor did NOAA Fisheries' Biological Review Team determine that such an adjustment was necessary to calculate either lambda or the BRT trend for NOAA Fisheries' 2005 status review (NOAA AR B.155).

11. It is also important to note that the R/S recovery prong metric, which is an average of individual brood year returns calculated for all years in the base period, is not affected by any trends in environmental conditions during that period. That is, the average R/S metric used in the BiOp provides the same estimate regardless of whether the most favorable environmental conditions occur at the beginning, the end, or the middle of the base period. NOAA Fisheries used all three metrics to indicate a trend towards recovery but stated that R/S is the most realistic of the recovery indicator metrics (BiOp page 7-23) and it was the primary quantitative factor relied upon for most conclusions regarding the recovery prong of the jeopardy

standard. Most recovery prong conclusions relied primarily on R/S estimates greater than 1.0[1], so these conclusions were not influenced by potential effects of trends in environmental factors during the base period.

12. Mr. Bowles' Paragraph 7 states that estimates of recovery prong metrics for which the lower 95% confidence interval was less than 1.0 indicate that "it is uncertain" whether the standard has been met. This statement appears to acknowledge that unless there is 100% statistical certainty, a quantitative estimate is "uncertain," which is not particularly informative. However, it leaves the impression that there must be 95% certainty for the estimate to be valid for the recovery prong of the jeopardy standard, which is not true. Oregon raised this point during the public comment period for the BiOp and NOAA Fisheries responded to it in its Response to Comments. *See* Response 2-L of NOAA AR C.1155. Briefly, NOAA Fisheries did not base its decision solely on quantitative estimates, nor on a pass/fail criterion associated with meeting indicator metric goals with 95% confidence. NOAA Fisheries based its conclusions, in part, on quantitative estimates (means, medians[2]) for the survival and recovery metrics, while additionally considering the uncertainty associated with those estimates. 95% confidence limits were calculated and displayed for all metrics and the probability of the metric being above 1.0

[1] Estimates of mean prospective R/S were greater than 1.0 for Snake River fall Chinook (Table 8.2.6.1-1), for 19 of 23 populations of Snake River spring/summer Chinook (Table 8.3.6.1-1), for 18-20 of 24 populations of Snake River steelhead; for all populations of Upper Columbia River spring Chinook (Table 8.6.6.1-1), and for all populations of Mid-Columbia River steelhead (Table 8.8.6.1-1). R/S was not relied upon for Snake River sockeye and Upper Columbia River steelhead recovery prong conclusions. NOAA Fisheries acknowledged that the trends for Upper Columbia River steelhead are strongly influenced by hatchery supplementation programs.

[2] See also 2008 Declaration of Rich Hinrichsen at Paragraph 8: "But, the estimates developed for the BiOp use maximum likelihood estimation, which is standard in statistical practice. The point estimates represent the most accurate estimates possible for comparison with the standard (e.g. 1.0 for trend, or 5% for extinction probability)."

was calculated and displayed for lambda under both the HF=0 and HF=1 hatchery assumptions[3], which yield similar results to the BRT trend and R/S estimates, respectively (BiOp pages 7-24 and 7-25). These uncertainty estimates allowed NOAA Fisheries to understand the range of uncertainty associated with the quantitative estimates when reaching its conclusions.

13. In Paragraphs 8-10, Mr. Bowles points out that there is uncertainty in the estimation of survival changes expected from ongoing actions and RPA actions, a point that the BiOp and previous declarations of NOAA Fisheries staff also make. *See* BiOp pages 7-37 through 7-49 and referenced appendices (discussing methods for all survival change estimates); Kratz 2008 Declaration at ¶ 27 (regarding estuary habitat action uncertainty); Kratz 2008 Declaration at ¶ 11 (discussing uncertainty in tributary habitat estimates and RPA requirements for ensuring that the expected survival changes are realized); Graves 2008 Declaration at ¶ 43 (discussing uncertainty in kelt reconditioning estimates). NOAA Fisheries did capture uncertainty for these estimates quantitatively when alternative approaches or data sets were available by treating each alternative as a range of results (for example, the range of estimated future harvest rates for Snake River fall Chinook [e.g., BiOp pages 8.2-37 and 8.2-38] and Snake River steelhead [e.g., BiOp pages 8.5-56 through 8.5-60] and the range of estimated base-to-current hatchery effects for Upper Columbia River steelhead [e.g., BiOp pages 8.7-47 and 8.7-48] resulted in ranges of BiOp prospective survival and recovery prong metrics).

14. The main point of disagreement is that Mr. Bowles appears to believe that NOAA Fisheries should have quantified this uncertainty for all action effect estimates and explored its effect on subsequent results through propagation of error (*See also* Dr. Orzack's Reply

[3] HF=0 indicates that hatchery-origin natural spawners are not effective in producing progeny that survive to spawn as adults. HF=1 indicates that hatchery-origin spawners are equally as effective as natural-origin spawners.

Declaration Paragraphs 13 and 30). As the BiOp makes clear (*see* § 7.2.2), most of the survival change estimates are based on expert opinion, limited empirical data, or a combination of both. These estimates are not amenable to calculating statistical variance. Because of this, "exploring" the effect quantitatively would essentially mean arbitrarily choosing a variance and evaluating how it changes the calculated confidence limits for each metric when each term is multiplied together, which would yield results with little meaning or justification.

15. These action effect multipliers reflect actions which will be monitored and, if necessary, refined or augmented to ensure achievement of the estimated benefits. Each conclusion about RPA benefits in the BiOp describes and relies on the importance of a strong monitoring program, review and reporting requirements, and procedures for implementing contingent actions if necessary (e.g., BiOp page 8.3-41 for Snake River spring/summer Chinook). For example, with respect to the tributary habitat actions, as Dr. Kratz pointed out in his previous declaration (¶ 11), NOAA Fisheries' primary approach for dealing with this uncertainty is to have a strong monitoring program, specific review criteria (e.g., RPA 57 for tributary habitat; *see also* SCA Appendix F "Inriver Juvenile Survival" monitoring plan and standards), and an adaptive management program for making changes if effects are not realized as anticipated.

16. In addition to increasing the certainty that action effects will be realized through monitoring and adaptive management, some of the assumptions used to generate quantitative estimates were pessimistic, thereby potentially over-estimating risk. BiOp Table 7.4-1 lists five pessimistic assumptions that affect quantitative risk estimates, including ignoring habitat improvements that are expected to accrue after 2018 and considering only density independent

habitat action effects[4]. Similarly, some qualitative considerations indicate a level of greater certainty that survival and recovery metrics will be achieved than is indicated by the quantitative estimates. For example, the presence of safety-net hatcheries reduces the short-term risk of extinction estimated for those populations and species for which these hatcheries were not taken into account in a quantitative manner.

17. In Paragraph 11, Mr. Bowles expresses concerns that NOAA Fisheries may not have accounted for density-dependent factors which might, for example, affect capacity but not influence productivity in its estimation of expected survival changes. As discussed in the BiOp on page 7-7 and described above, NOAA Fisheries followed the ICTRT by applying only density-independent changes in survival to meet the needed survival change ("gap") necessary for species to be on a trend toward recovery.

> The "survival gap," as defined by NOAA Fisheries, refers to the change in density-independent survival[1] that would need to occur in order to change the current value to the desired value of a given metric.
>
> Footnote 1: "Density-independent" refers to a change in survival that is not influenced by the number of fish in the population. Generally speaking, most factors influencing survival after the smolt stage are assumed to be density independent. During the egg-to-smolt stage, the density of adults and juveniles can influence survival as a result of competition for limited habitat or other factors. For evaluation of survival gaps, estimates of survival changes resulting from actions affecting early life stages of salmon and steelhead are made under the assumption of low density. (Biop page 7-7)

18. The method to estimate effects of tributary habitat improvements explicitly considered only density-independent survival changes (Footnote 12, Attachment C-1, page C-1-36 of NOAA AR B.89). Additionally, Paragraph 7 in Dr. Kratz's 2008 Declaration addressed Mr. Bowles' concern about interference or synergistic effects among actions, stating that these

[4] As described on BiOp page 7-31, some habitat improvement actions such as removing barriers to increase rearing area capacity will have greater benefits at high density than at low

were taken into account when the expert panels estimated survival improvements expected from habitat actions. Potential compensatory effects of predator removal were also taken into account in the BiOp (e.g., page 8.3-27).

19. In Paragraphs 12-14, Mr. Bowles discusses NOAA Fisheries' procedure for reaching species-level conclusions as a third major source of uncertainty. Mr. Bowles' comments appear to assume that NOAA Fisheries' jeopardy analysis required multiplying together estimates for each population or major population group (MPG) to get a composite numerical estimate of risk for the species. Mr. Bowles also claims that neither the BiOp nor the administrative record explains how NOAA Fisheries ultimately decided that species were trending towards recovery. These assumptions and statements are not correct. I summarized NOAA Fisheries' method of considering population and MPG results in reaching conclusions at the species level in Paragraphs 50-51 of my previous declaration and a full description of the approach can be found on BiOp pages 7-49 through 7-51. In my previous declaration, I pointed out how this approach considered available recovery planning information and population structure and provided citations showing how it was applied to conclusions for various species. Mr. Bowles apparently does not like NOAA Fisheries' method or the BiOp's description of this method, and his comments in Paragraph 13 also suggest that he disagrees with NOAA Fisheries' risk tolerance for this decision; however, he offers no technical arguments indicating flaws in the species-level methodology or conclusions.

20. In Paragraphs 16-27, Mr. Bowles claims that NOAA Fisheries did not take pessimistic results into account in its conclusions, instead relying only on optimistic assumptions. This contention is disproved by Mr. Bowles' own review of the BiOp analyses

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density, but only the low-density benefits, were assumed.

and conclusions, which indicate that all information was presented in the BiOp and its appendices and considered for each population, MPG, and species. Mr. Bowles does not describe any scientific information or analyses that NOAA Fisheries failed to include or consider in the BiOp. What Mr. Bowles appears to object to is the particular judgment that NOAA Fisheries made after considering all of this information. The basis for that judgment is described for interior Columbia species, for example, in the BiOp sections 8.2.7 (Snake River fall Chinook), 8.3.6 and 8.3.7 (Snake River spring/summer Chinook), 8.4.6 Snake River sockeye), 8.5.6 and 8.5.7 (Snake River steelhead), 8.6.7 (Upper Columbia River spring Chinook), 8.7.7 (Upper Columbia River steelhead), and 8.8.6 and 8.8.7 (Mid-Columbia River steelhead). That description speaks for itself. While Mr. Bowles would like to see much more explanation for NOAA Fisheries' conclusions, I see no technical issues that he has raised regarding flaws in the analysis. Indeed, in some cases he appears to be making assertions that find no support whatsoever; for example, the sentence attributed to me by Mr. Bowles in Paragraph 18 is not to be found in AR C.1089.

21. In Paragraph 29, Mr. Bowles reiterates his belief that NOAA Fisheries should have specified an explicit minimum abundance standard as part of the jeopardy analysis. NOAA Fisheries has previously explained in the BiOp (pages 7-27 through 7-29), in the response to comments document (Response 2-H of NOAA AR C.1155), and in my previous declaration (Paragraphs 8-16) how the BiOp considered abundance and why it did not include an explicit recovery abundance standard in the jeopardy analysis. Mr. Bowles' offers no new information or technical points to explain his belief.

22. Because Mr. Bowles and the plaintiffs refer to the recovery analysis in the 2000 BiOp, I would like to briefly describe what NOAA Fisheries actually did in that analysis. The

methods and details of the analysis can be found in Appendix A of the 2000 BiOp. Two quantitative methods were used for the 2000 BiOp recovery analysis, with variations on each based on treatment of hatchery fish.

23. The first method was to calculate the survival change needed for lambda to be greater than 1.0. This method was identical to the method used in the 2008 BiOp. This method was the only recovery analysis option available for populations for which a recovery abundance target had not yet been defined. That is, it was the only recovery analysis option available for all but 12 populations and one ESU aggregate in the entire Columbia Basin (2000 BiOp Tables A-4 and A-5)[5]. For example, for Snake River spring/summer Chinook, abundance targets were available for only seven populations, so the lambda $\geq$1 approach was applied to the remaining 37 "populations" in this ESU that had been identified at the time (comparison of 2000 BiOp Tables A-4 and A-5 with Table A-6).

24. The second method was applied to 12 populations and one aggregate ESU in the basin (Tables A-4 and A-5 of the 2000 BiOp). This method estimated the survival change needed to have a 50% likelihood of reaching a recovery abundance level in either 48 or 100 years. The "CRI" approach to calculating this metric, which was applied to most populations, assumed unconstrained exponential growth - that is, the population growth rate was not assumed to decrease as abundance neared the carrying capacity. For three Upper Columbia River spring Chinook populations, a "QAR" approach, which did assume density-dependent population growth, was applied.

[5] The 2000 BiOp analyzed a total of 92 "populations" (including A-run and B-run Snake River steelhead aggregates) and 7 combined-"population"aggregates in 11 ESUs or DPSs (2000 BiOp Tables A-4, A-5, and A-6).

25. For both the lambda >1.0 and the abundance target methods, the survival change needed to meet the standard was calculated from the recent population growth rate (generally 1980 to the most recent available year) under the assumption that this population growth rate would continue indefinitely into the future. That is, the estimation of the needed survival change to meet the standard (the survival "gap") assumed that the survival rates that influenced the population growth rate during the 1980-present base period would continue into the future.

26. There were two quantitative adjustments made to the estimated survival gaps to determine if it was likely that the targets would be met. As in the 2008 BiOp, the first was a "base-to-current" adjustment that reflected ongoing management actions that had changed in comparison to the actions that occurred on average during the base period. The second was a "base-to-future" adjustment that reflected the RPA and additionally, unlike the 2008 BiOp, other actions affecting survival and recovery of listed species. NOAA Fisheries made quantitative estimates of survival changes associated with hydro operations and harvest management. Included was an adjustment for achievement of expected hydro survival rates resulting from the Mid-Columbia Habitat Conservation Plan for Upper Columbia River spring Chinook and steelhead. As in the 2008 BiOp, the survival multipliers were either point estimates or ranges based on alternative assumptions. Unlike the 2008 BiOp, 95% confidence limits were not estimated.

27. In Paragraph 30, Mr. Bowles states that there is an "indication" that I considered quasi-extinction thresholds (QET) to be minimum abundance standards (relative to recovery) in my previous declaration. The reference to QETs in Paragraph 10 of my previous declaration clearly referred to the survival prong of the jeopardy analysis, not to the recovery prong. Thus, Mr. Bowles' assertion is incorrect. Also, contrary to Mr. Bowles' "indication," NOAA

Fisheries' discussion of abundance in relation to QET in the BiOp also is confined exclusively to the analysis of extinction risk and the survival prong of the jeopardy standard (e.g., BiOp pages 8.3-30, 8.3-33, 8.3-34, 8.3-37, 8.3-39, 8.3-44 through 8.3-46 for Snake River spring/summer Chinook).

28. Mr. Bowles' remaining discussion in Paragraph 30 and in Paragraph 31 does not negate or contradict the point of Paragraphs 17-21 in my previous declaration. These paragraphs continue to demonstrate that there is not a specific time period beyond which recovery of Columbia basin salmon and steelhead is precluded. That would be the only biological justification for Mr. Bowles' assertion that NOAA Fisheries is required to evaluate attainment of a particular abundance level within a particular period of time for a valid Section 7(a)(2) jeopardy analysis. Yet, Mr. Bowles includes no new information on this subject in his declaration, other than to clarify that he did not intend to suggest that ICTRT abundance thresholds represent abundances associated with genetic bottlenecks.

29. In Paragraph 32, Mr. Bowles criticizes the BiOp for not explaining how it can be easier to meet a recovery standard than a survival standard. As stated in the BiOp, the two standards are complementary and both must be achieved to avoid jeopardy (BiOp pages 1-12, 1-3, 7-5). The current status of a species might be so poor at the moment that it is unlikely to survive while recovery actions are being implemented. On the other hand, a large population might have a very low extinction risk but be unable to reach recovery until factors limiting recovery are addressed. The assessment of the risk of precluding recovery is augmented by the assessment of short-term extinction risk. Therefore, Mr. Bowles' contention is irrelevant because they are joint assessments.

30. Mr. Bowles' Paragraph 33 disputes my characterization of a 1% drop in prospective R/S for the Marsh Creek population (from 1.08 with 0's to 1.07 with 1's) and a 1% drop for the Sulphur Creek population (from 1.10 with 0's to 1.09 with 1's) as "trivial." (Paragraphs 42-44 and Exhibit 1 of my previous declaration). I continue to consider these changes trivial, especially since the expectation of R/S greater than 1.0 is unchanged, and I find no basis for Mr. Bowles' statement that "there was a chance for some conclusions to change from no GAP to a GAP."

31. Mr. Bowles' Paragraphs 34-37 go to great lengths to demonstrate that a greater change in survival is necessary for most populations to achieve recovery, as defined by the ICTRT, than is necessary for populations to be on a trend towards recovery, as defined for the recovery prong of the BiOp jeopardy analysis. This observation is undisputed but irrelevant:

> It is important to understand that the "survival gap" terminology applies to the needed survival change associated with achieving any goal, based on any survival-based metric. Here, it applies to the goal of being on a trend toward recovery and having a low short-term risk of extinction. The ICTRT (2007c, 2006) also uses the "survival gap" terminology. The ICTRT defines survival gaps associated with the long-term viability of populations. These ICTRT viability survival gaps are based on somewhat different target metrics, and represent the gap between the condition of populations over approximately the last two decades and the condition that the ICTRT considers viable. If a sufficient mixture of populations reaches this level, then the species is considered viable.
>
> In contrast, this analysis is directed at a different question than the ICTRT's analysis of long-term recovery. This analysis focuses on the survival changes needed to ensure that populations support species (ESU or DPS) that are on a "trend toward recovery;" i.e., moving toward recovery even though full recovery of the species may not be achievable during the period of the Prospective Actions. In general, the needed survival changes for full recovery are higher than the needed survival changes associated with the "trend toward recovery." (BiOp page 7-7).

In short, Mr. Bowles' point is irrelevant because the survival gap that he seeks to close is that to a recovered population, which is not the same as the survival gap for a trend to recovery, which is relevant to this Section 7(a)(2) consultation.

Response to Reply Declaration of Frederick E. Olney

32. Mr. Olney, in Paragraphs 82 and 83, repeats a point he raised in his previous declaration at Paragraph 95, asking how it is possible to offset problems with poorer-performing B-run populations by considering benefits from stronger A-run populations for Snake River steelhead. In Paragraph 52 of my previous declaration, I demonstrated that even under criteria for a "viable" ESU, as set forth by the ICTRT, A-run and B-run populations can be substituted to meet viability goals for Snake River steelhead. I used the Salmon River MPG as an example of how a stronger A-run population can meet viability goals in place of a weaker B-run population to satisfy some of the ICTRT's criteria. Mr. Olney's confusion appears to stem from my statement in Paragraph 52 that "NMFS did not adopt the ICTRT's viability scenarios as a requirement for reaching a no-jeopardy conclusion," implying that this invalidated my example. It is true that NOAA Fisheries did not *require* that ICTRT viability scenarios be met to make species-level decisions, but the BiOp clearly stated that these viability scenarios were relevant. In Paragraphs 50 and 51 of my previous declaration, I described the relevant citations and evidence showing that NOAA Fisheries took this information into account in the BiOp.

33. As is clear from the summary tables for each species in the BiOp (e.g., Tables 8.3-56 and 8.3-58 for Snake River spring/summer Chinook), the ICTRT described various combinations of populations at particular viability levels that could meet some of the MPG and species viability goals. Thus, the general approach of stronger populations being able to help offset problems with poorly performing populations in the BiOp also applies beyond the example of Snake River steelhead raised by Mr. Olney.

34. In Paragraphs 84 and 85, Mr. Olney states that NOAA Fisheries did not fully disclose limitations of lambda calculations in its analysis of Snake River steelhead. The reason that NOAA Fisheries did not disclose this limitation is because it was unaware of it at the time. The important question, however, is what effect this limitation would have on the BiOp analysis. As described in the following paragraphs, this would have little or no effect on the BiOp analysis.

35. In Paragraph 54 of my previous declaration I noted that BiOp estimates of R/S, lambda calculated with the HF=0 hatchery assumption, and BRT trend estimates applied to Snake River steelhead were correct, but that lambda estimates with the HF=1 hatchery assumption were incorrect, affecting two B-run populations (out of eight total B-run populations) and about half of the A-run populations. I also noted that, for those populations affected, the lambda results with the HF=1 hatchery assumption would be similar to R/S estimates for populations with significant hatchery influences. Mr. Olney did not dispute these statements.

36. Prospective R/S estimates were greater than 1.0 for all A-run populations (Table 8.5.6.1-1), ranging between 1.09 and 1.60. This means that NOAA Fisheries' determination of a positive prospective trend for all A-run populations was not affected by a lack of valid HF=1 lambda estimates for these populations, since the HF=1 lambda estimates would be similar to the R/S estimates.

37. One of the two B-run steelhead populations with a hatchery influence is the South Fork Clearwater population (e.g., *see* Mr. Olney's first declaration at Paragraphs 32 and 33). Like the A-run populations, the prospective R/S estimate for this population is greater than 1.0 (1.06-1.10, depending upon harvest assumption; Table 8.5.6.1-1). Again, NOAA Fisheries' characterization of an expected positive trend for this population would not be affected by lack of a valid HF=1 lambda estimate.

38. We are now down to one population potentially affected by lack of a valid HF=1 lambda estimate. At the time I prepared my first declaration, I had assumed that this population was the Lolo Creek population, for which NOAA Fisheries estimated prospective R/S between 0.99-1.04. However, as Mr. Olney points out in Paragraph 33 of his previous declaration, the hatchery releases into that basin were too recent to affect the base period and therefore would not have affected calculation of HF=1 lambda for this population. Therefore, the HF=0 lambda estimates are the only relevant lambda estimates for this population and lack of a valid HF=1 lambda estimate would have no effect. Mr. Olney also mentions a B-run population in the East Fork Salmon River affected by hatchery releases during the base period. However, the ICTRT has classified the East Fork Salmon population as an A-run population (NOAA AR B.194 Attachment 2, "Viability Scenarios") so it is already accounted for in my description of A-run populations above. Frankly, I am now no longer certain that there are any Snake River steelhead populations whose conclusions in the BiOp might be affected by the lack of valid HF=1 lambda calculations and, if so, at most only one of the 24 populations in this DPS would be affected. This would result in little or no effect on the BiOp analysis.

39. In Paragraphs 86 and 87, Mr. Olney states that NOAA Fisheries' discussion of a QET less than 50 fish being appropriate for some smaller populations does not appear to be relevant because NOAA appears to be relying on a risk assessment threshold of 10 fish for three populations in the Middle Fork Salmon MPG. NOAA did not rely on a specific QET less than 50, but considered results at levels between 1 and 50 fish, both because of uncertainty about an appropriate QET level for short-term *versus* long-term extinction risk and because some smaller populations have fallen below 50 fish four years in a row and have not gone extinct:

> The ICTRT does not address nor recommend a reasonable QET for shorter-term extinction risk. The analysis in this SCA includes the 50-fish QET recommended for evaluating 100-year extinction risk by the ICTRT, but it includes also a sensitivity analyses to alternative choices of QET. These may also be appropriate for assessment of short-term extinction risk since many populations in the Columbia Basin have dropped below 50 fish and returned to higher abundance levels during the past 20 years. (BiOp page 7-17)
>
> NOAA Fisheries primarily considered QET=50 in evaluating extinction risk because this is the threshold used by the ICTRT for long-term extinction risk. For the reasons discussed above, this threshold may be overly conservative for smaller populations, particularly those that have demonstrated the ability to return to higher levels after dropping below 50 spawners (e.g., Figure 7.1-3), and it may also be conservative for short-term extinction risk since at least one of the ICTRT's reasons for QET=50 is related to long-term genetic considerations. .. Regarding emphasizing lower QET levels, NOAA Fisheries agrees that "true extinction" is defined as dropping to 0 or 1 spawner four years in a row. However, the ability of available data and models to accurately predict population behavior at this low level is extremely limited and the risk tolerance in such an analysis would have to be extremely low. It is reasonable to evaluate "quasi-extinction" thresholds above 1 fish, although there is little information favoring use of any particular level. (BiOp page 7-19)

40. For Snake River spring/summer Chinook survival prong conclusions, NOAA Fisheries acknowledges that it cannot demonstrate quantitatively that all populations or all MPGs will have low short-term extinction risk under all assumptions (BiOp page 8.3-46) and that the Middle Fork Salmon MPG "is a concern." (BiOp page 8.3-46) After considering a variety of factors, including the relevance of QET levels less than 50 for some of the smaller populations and consideration that Middle Fork Salmon MPG populations will be closely monitored to ensure that any changes in status are detected and appropriate actions taken, NOAA Fisheries concluded that "enough populations are likely to have a low enough risk of extinction to conclude that the ESU as a whole will have a low risk of short-term extinction." (BiOp page 8.3-46).

41. In paragraphs 88 and 89, Mr. Olney states that, in my previous declaration, I incorrectly asserted that NOAA Fisheries does not assume that all recovery actions occur *immediately* because I also pointed out that NOAA Fisheries assumes that the actions occur all at once, or *instantaneously*, in a single time step. I stand by my statements and the relevant citations from the

BiOp in paragraphs 58 and 59 of my previous declaration. Mr. Olney may be equating the two italicized adverbs, which is not consistent with the usage in the BiOp. As described on page 7-12,

> However, for productivity estimates, the time period associated with the estimates begins with full implementation of the expected survival changes. The best way to think of the productivity estimates is that they represent the initial productivity following achievement of the expected survival rate changes resulting from the Prospective Actions.

In other words, for the recovery prong analysis NOAA Fisheries assumes that at a point in the future (not immediately), following implementation of the RPA actions and achievement of the expected survival rate changes, the recovery metrics change in a single step, instantaneously.

<u>Response to Reply Declaration of Steven Orzack</u>

42. Paragraphs 1-10 of Dr. Orzack's declaration summarize the information I presented in Paragraphs 8-16 of my previous declaration, which responded to Mr. Bowles' contention that NOAA Fisheries did not consider abundance in its jeopardy analysis. This discussion demonstrated that NOAA Fisheries did consider abundance in each of the four steps of its jeopardy analysis. Dr. Orzack does not appear to dispute my description of the consideration of abundance throughout the BiOp, including the BiOp's comparison of current abundance with ICTRT abundance thresholds in Steps 1 and 2 of the jeopardy analysis and NOAA Fisheries' consideration of abundance in its conclusions. However, he expresses concern that NOAA Fisheries did not "employ" the ICTRT abundance thresholds "in any of its quantitative analyses" (Paragraph 8). I could find no explanation of the scientific basis for this concern in either Dr. Orzack's initial declaration or reply declaration.

43. As the BiOp explains on pages 7-27 through 7-29, and as I explained in Paragraphs 13-16 of my previous declaration, NOAA Fisheries did not conduct a quantitative analysis to predict future abundance, which would have required a more complex model. That is, there were no

abundance projections to which NOAA Fisheries could "employ" the ICTRT abundance thresholds. Instead, NOAA Fisheries displayed the best available information regarding abundance projections, which was a set of example matrix model runs from the ICTRT and Zabel (NOAA AR B.197) that showed the relationship between changes in productivity and resulting changes in abundance over time for a subset of BiOp actions. As Dr. Orzack confirms, increased productivity results in increased abundance, and the model runs in the BiOp showed that increased abundance also affects productivity. Both of these points were considered and factored into conclusions in the BiOp. For example, for BiOp's Snake River spring/summer Chinook conclusions section regarding the potential for recovery prong of the jeopardy standard states that:

> This does not mean that recovery will be achieved without additional improvements in various life stages. As discussed in Chapter 7, increased productivity will result in higher abundance, which in turn will lead to an eventual decrease in productivity due to density effects, until additional improvements resulting from recovery plan implementation are expressed. (BiOp page 8.3-43)

44. In Paragraph 10, Dr. Orzack quotes me as stating in my previous declaration at Paragraphs 21 and 23 that, of course, it is important for listed species to recover as quickly as possible. To use Dr. Orzack's terminology, this is a truism that I am sure reflects the sentiments of all involved in the recovery of Columbia basin salmon and steelhead. However, this was not a comment on a particular requirement for an ESA Section 7(a)(2) consultation. Dr. Orzack is incorrect in concluding that this statement contradicts my points in Paragraphs 17-25 that there is not a particular abundance level that must be achieved within a certain time period for Columbia basin salmon and steelhead to recover, or that there is not a technical reason why time to recovery must be evaluated in order to evaluate the recovery prong of the jeopardy analysis. It also does not contradict my description of the Upper Columbia River Spring Chinook Salmon and Steelhead Final Recovery Plan's analysis of the

likelihood of meeting recovery abundance and productivity goals (AR B.503, S.69), which was able to accomplish its goal without having to specify or evaluate a time to recovery.

45. Dr. Orzack, in Paragraphs 24-28, repeats his opinion that NOAA Fisheries should have specified a "recipe" for its treatment of qualitative information in the BiOp, which he previously expressed in Paragraphs 19 and 20 of his first declaration. Dr. Orzack offers no new scientific or technical information to support this contention. In contrast, NOAA Fisheries has previously pointed out that the BiOp enumerated each qualitative factor to be considered and described how that factor would be considered (BiOp pages 7-34 to 7-37), evaluated a consistent set of quantitative and qualitative factors for each ESU, reviewed those factors for each major population group and species, and explained how the quantitative and qualitative factors, taken together, led to conclusions regarding jeopardy (e.g., BiOp pages 8.3-39 through 8.3-45).

46. In Paragraph 29, Dr. Orzack selectively re-states the discussion in Paragraphs 42-44 of my previous declaration regarding the treatment of years with zero spawners for the Marsh Creek and Sulphur Creek populations. The key point of my analysis is not the base period estimates, but the prospective estimates of R/S under the RPA, which remained strongly positive following the adjustment, 1.07 for Marsh Creek and 1.09 for Sulphur Creek (*see also* Paragraph 23 above). These results remain as described, whether 1 is added to all annual estimates or only to years with 0 spawners.

Response to Declaration of Patty Glick

47. Paragraph 16 of Ms. Glick's declaration discussed results of three studies that predict various levels of increased air temperature in the Pacific Northwest in the future. Two of those studies were discussed and in part formed the basis of the Independent Scientific Advisory Board's (ISAB)

2007 review of climate change effects on Pacific Northwest salmon and steelhead (NOAA AR B.211), which NOAA Fisheries relied upon in the BiOp. As discussed in my previous declaration in Paragraphs 46-49, NOAA Fisheries summarized and relied upon the ISAB results, rather than extensively detailing the primary literature in the BiOp. One of the studies in Ms. Glick's Paragraph 16 (Miles et al. 2007[6]) was contemporaneous with the ISAB review and therefore not cited in it. This study presents explicit estimates for the 2020's, which are reproduced in her declaration. NWF's Reply Memorandum, Footnote 37, claims that NOAA "ignores" projected regional temperature increases described in this study.

48. NWF is incorrect. The BiOp at page 7-12 stated, based on the ISAB review, that "As discussed in Section 5.7, Pacific Northwest air temperatures have increased by approximately 1 degree C since 1900 and are expected to increase 0.1-0.6 degrees C per decade over the next century." The Miles et al. (2007) report, page 3, states under "Key Findings" that "Based on results from a number of Global Climate Models (GCMs), we can expect annual temperature to increase approximately 0.3^{o}C, or roughly 0.5^{o}F, per decade over the next 50 years." The Miles et al. (2007) estimate of air temperature change per decade is slightly lower than the midpoint of the range of temperature increases anticipated in the BiOp. Therefore, it is clear that the information in that report was not overlooked or ignored in the BiOp. SCA Pages 5-63 to 5-67, incorporated by reference into the BiOp discuss the implications of this increase in temperature on various aspects of salmonid life history.

[6] Miles, E.L., D.P. Lettenmaier, and 22 co-authors. 2007. HB 1303 Interim Report: A comprehensive assessment of the impacts of climate change on the State of Washington. 15 December 2007. Univ. of Washington JISAO CSES Climate Impacts Group. 51 pp.

was not overlooked or ignored in the BiOp. SCA Pages 5-63 to 5-67, incorporated by reference into the BiOp discuss the implications of this increase in temperature on various aspects of salmonid life history.

I declare under penalty of perjury that the foregoing is true and correct. Executed on December 12 2008, in Portland, Oregon.

Christopher L. Toole