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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 Rich Hinrichsen

I, Rich Hinrichsen, declare as follows:

INTRODUCTION

1. In this declaration, I respond to several comments in the Reply Declarations of Dr. Steven Orzack and Mr. Edward Bowles. These responses are based on my knowledge of the extinction risk analysis, which I developed and implemented. The purpose of these responses is to clarify the nature of the analytic work I did in the Comprehensive Analysis, which was reviewed and used by NOAA for the 2008 BiOp, and correct any mischaracterizations of that work. See, NOAA AR A.2, SCA Attachment I, Aggregate Analysis Appendix.

Responses to the Reply Declaration of Orzack

2. In paragraph 14 of the Orzack Reply Declaration, Dr. Orzack claims that it is irrelevant that NOAA Fisheries relies on point estimates that are the most accurate estimates possible for comparison with the standard. In fact, using maximum likelihood estimation is relevant because it is a method that has a proven track record in applied statistics. Uncertainty in the point estimates used in the BiOp arises because of the inherent variability in the data, not because a poor estimation method was used. No amount of statistical manipulation or use of more sophisticated techniques will remove or even appreciably reduce the uncertainty in the estimates.
3. In paragraph 14, Dr. Orzack goes on to state that "the allusion to likelihood by Dr. Hinrichsen is misleading and would appear to be an attempt to add the appearance, but not the substance of statistical sophistication. In fact, by definition, the likelihood approach does not rely on confidence intervals, instead, a meaningful application of

that approach would necessitate the use of so-called 'support sets.'" Dr. Orzack has mischaracterized our classical statistics approach of using maximum likelihood estimation and confidence intervals to describe uncertainty. He seems to suggest that we are using modern likelihood and Bayesian techniques that rely on so-called posterior intervals to describe uncertainty. My work relies solely on classical statistical methods of maximum likelihood and confidence intervals. These methods have a long and proven track record in applied science.

4. In paragraph 15 of the Orzack Reply Declaration, Dr. Orzack claims that there is no substantive way in which uncertainty is acknowledged. Yet he also acknowledges that 95% confidence intervals were presented. These confidence intervals themselves represent a substantive acknowledgement of uncertainty. Confidence intervals represent a widely accepted way to quantify uncertainty. Dr. Orzack confuses the scientific presentation of uncertainty (confidence intervals), with how that uncertainty is applied in decision making. It appears to me that he blurs the distinction between science and decision-making for policy purposes in the face of uncertainty.

5. Furthermore, as I stated in my earlier Declaration, the BiOp does indeed discuss the implications of the high degree of uncertainty around point estimates of extinction. The BiOp notes that its dual reliance on quantitative modeling results and a host of qualitative considerations is an appropriate response to the uncertainty in the point estimates. The BiOp also notes that uncertainty in the extinction risk estimate increases with the time horizon used in the analysis. Thus, the BiOp's reliance on 24-year risk estimates (as opposed to 100-year estimates) results in greater accuracy.

(See, for instance, NOAA AR A.1, FCRPS BiOp at pages 7-18 and 7-20. See also CA at pages A-6 – A-8.)

6. In paragraphs 16-20 of the Orzack Reply Declaration, Dr. Orzack, claims that accounting for uncertainty when assessing risk is evident in the recent work of the ICTRT. I agree that the ICTRT did account for some sources of uncertainty (productivity and abundance), but not all. And the sources of uncertainty unaccounted for in the ICTRT are important: namely, the uncertainty in the variance, auto-correlation, and density dependent parameters. To support his claim, Dr. Orzack points to a sensitivity analysis included in an appendix to the ICTRT's Viability Criteria report. In this analysis, the ICTRT shows the effect on a sample viability curve of modifying some of the input parameters, including variance and autocorrelation. This is an interesting exercise. However, a sensitivity analysis is not an adequate way to characterize the uncertainty produced by the errors in estimating these parameters (See paragraph 18 of the Orzack Reply Declaration). Because of these sources of uncertainty, the ICTRT's viability curves would properly be displayed by depicting the envelopes that describe the considerable uncertainty about the viability curves. An envelope is like a confidence interval, except it gives upper and lower bounds on an entire curve (e.g., viability curve), not just a point estimate. Based on my investigations, these envelopes will tend to be quite wide for all listed salmonid populations in the Columbia River Basin, especially when using 100-year extinction probabilities (See Appendix A to this Reply Declaration). The ICTRT's 100 year risk analysis, as manifested in their viability curves, is subject to a high

degree of uncertainty. This uncertainty is inherent in the data sets and cannot be explained away by using a different flavor of risk analysis.

7. In paragraph 21 of the Orzack Reply Declaration, Dr. Orzack implies that there is a method of dealing with uncertainty built into the IUCN criteria. That is not so. As stated in my previous declaration at paragraph 8, for example, an estimated population size reduction of 90% or greater over the most recent three generations can place a population in the IUCN's Critically Endangered category. This criterion says nothing about how to specifically treat uncertainty. Nor does the Annex I (see IUCN 2001, page 24, Annex 1 "Uncertainty" referenced in Dr. Orzack's Reply Declaration at paragraph 21) prescribe a scientific method for treating uncertainty. Science can quantify the uncertainty in its estimates. But how that uncertainty is used in the decision-making process is a policy determination. Again, Dr. Orzack blurs the distinction between science and policy.

8. In paragraphs 22-23 of the Orzack Reply Declaration, Dr. Orzack claims that the incorporation of uncertainty in the IUCN Red List Assessment of *Onchorhynchus nerka* is obvious. This is not so. The 50th percentile (median) of the decline-rate distribution is not a measure of uncertainty. The median itself is subject to uncertainties that are ignored. No confidence intervals were constructed for the 50th percentile to use as a measure against an IUCN standard (e.g., decline of 80%) to determine risk status._Therefore, Dr. Orzack's conclusion that uncertainty was incorporated in IUCN Red List Assessment of *Onchorhynchus nerka* is incorrect.

Responses to the Second Declaration of Bowles

9. In the Second Declaration of Mr. Edward Bowles, paragraph 2, Mr. Bowles rightly points out that the metrics used in the BiOp can be sensitive to time period chosen and that I suggested using confidence intervals to account for this uncertainty. But then Mr. Bowles goes on to claim that the BiOp "did not adequately address this uncertainty in its quantitative analysis." In fact, uncertainty in the metrics was addressed through the construction of confidence intervals, which is a standard statistical description of uncertainty.

10. In the Second Declaration of Bowles, paragraph 5, Mr. Bowles states that "the responses from Mr. Toole and Mr. Hinrichsen primarily underscore the failure of the 2008 FCRPS Biological Opinion to adequately consider uncertainty in its quantitative analysis." Actually, in the BiOp, uncertainty was considered in both the population dynamics models used to for the extinction risk calculations, and point estimates (through use of confidence intervals).

11. In the Second Declaration of Bowles, paragraph 14, Mr. Bowles mischaracterizes my work on population viability by claiming that "Hinrichsen appears to assume salmon populations have steady-state dynamics where the primary source of variation is random error and, if it is large it is most likely caused by faulty measurement methods." I make no such assumptions. Actually, the modeling assumption is that the population dynamics follow spawner-recruit models (Ricker or Beverton-Holt), both models with a long tradition in fisheries science. As is customary, the dynamics were modeled with a stochastic error term. This error term was assumed to contain autocorrelation, which describes how random variation in one year is connected with

random variation in the next. There was no assumption that these random errors were due to measurement error, as Mr. Bowles claims.

12. In the Second Declaration of Bowles, paragraph 15, Mr. Bowles claims that "instead of acknowledging the uncertainty of their quantitative analysis and objectively addressing it, NOAA Fisheries used a 'qualitative analysis' that provided the ultimate basis for their conclusions about jeopardy." Contrary to this assertion, uncertainty was acknowledged and objectively reported using confidence intervals.

13. In the Second Declaration of Bowles, paragraphs 34-37, Mr. Bowles compares the results of the gaps methods used by the ICTRT and the BiOp. In this section he finds differences in the results. These differences are not surprising given that the BiOp uses a 24-year time frame instead of a 100-year time frame in its calculations. The gaps, whether based on a survival prong or recovery prong, represent needed changes in productivity to meet a certain goal. In this sense (and others described below), the ICTRT and BiOp methods are fundamentally similar.

14. Although the methods for calculating gaps differ between ICTRT and BiOp, the ICTRT and BiOp approaches are fundamentally similar. Both the ICTRT and BiOp gaps are based on the needed increase in productivity, expressed as recruits per spawner, to achieve a 5% extinction risk. The BiOp relies on data developed for the ICTRT and uses similar definitions of extinction. (Extinction occurs when spawner counts fall below a critical level in four consecutive years). See, NOAA AR A.1, FCRPS BiOp at 7-6 – 7-7. See also, NOAA AR A.2, SCA Attachment I, Aggregate Analysis Appendix.

15. In summary, Mr. Bowles blurs the distinction between the ICTRT's attempt to define the biological criteria that would represent a viable salmonid population over the long term (100 years) with the BiOp's attempt to quantify the risk that a population may become extinct within a relatively short timeframe (24 years). These are different inquiries that use very similar tools (as noted above). The gaps these inquiries attempt to estimate are in a similar currency (needed changes in productivity), but represent improvements needed to achieve different goals and standards.

I declare under penalty of perjury that the foregoing is true and correct. Executed on December 16, 2008, in Seattle, Washington.

Rich Hinrichsen

Appendix A. Viability curve and envelope

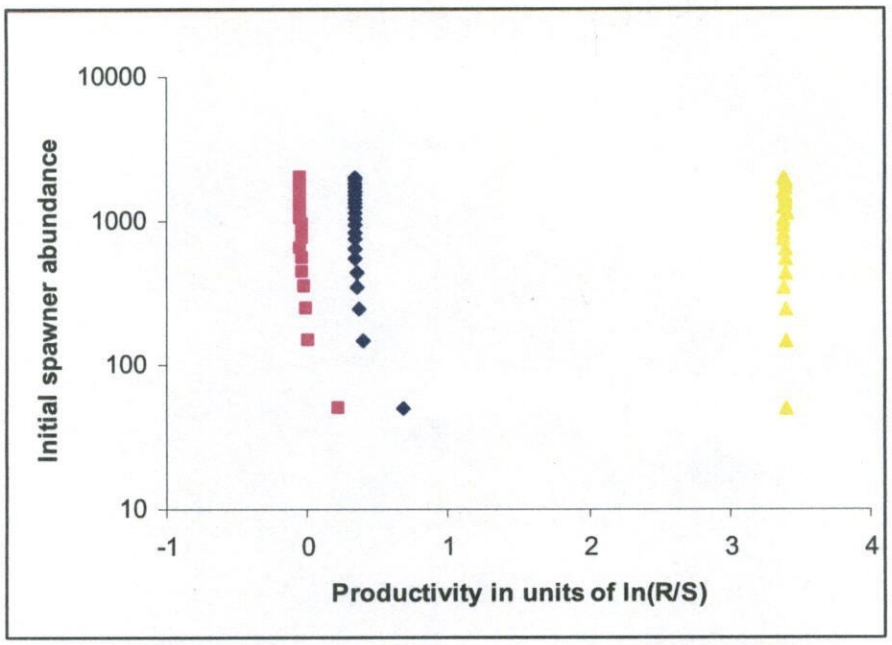

Figure 1. Sample viability curve with confidence envelope. The viability curve is given by the diamonds, the lower limit of the 95% confidence envelope is defined by the squares, and the upper limit is defined by the triangles. The viability curve represents combinations of abundance and productivity that achieve a 5% extinction risk over 100 years. Note the wide envelope. This is due to the high uncertainty in the parameter estimates for variance, auto-correlation, and density dependence. The data used, courtesy of the ICTRT, were from the population spring/summer chinook population South Fork Salmon East Fork (including Johnson Cr.).