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UNITED STATES DISTRICT COURT
DISTRICT OF OREGON
PORTLAND DIVISION

NATIONAL WILDLIFE FEDERATION, et al.,

Plaintiffs,

v.

NATIONAL MARINE FISHERIES SERVICE, et al.,

Defendants.

Case No.: 3:01-CV-00640-SI

2015 REPLY DECLARATION OF RICHARD W. ZABEL,Ph.D. NATIONAL MARINE FISHERIES SERVICE, WEST COAST REGION
In support of Federal Defendants' Cross-Motion for Summary Judgment

I, Richard W. Zabel, Ph.D., declare and state as follows:

1. On March 5, 2015, I provided a declaration in support of the NOAA Fisheries' 2014 Supplemental Biological Opinion (BiOp) for the Federal Columbia River Power System (FCRPS). There, I described my qualifications and experience. I also raised some concerns regarding declarations prepared by Dr. Brendan Connors for the plaintiffs NWF and by Mr. Anthony Nigro for the State of Oregon.

2. I have reviewed the reply declarations filed by Dr. Connors (Connors) and Ms. Kathryn Kostow (Kostow), who prepared the reply to critiques of the Nigro declaration. I now provide this declaration to respond to further comments and criticisms raised in their reply declarations.

Response to Connors Declaration

3. In his reply declaration, Connors continues to support his hypothesis that the reason we are seeing density dependence at relatively low abundances is because populations are not using all available spawning habitat. He speculates that as populations increase in abundance, as they have done in recent years, individuals bypass available quality habitat and instead crowd into sites that were occupied by previous generations when populations were at lower abundances. He argues that habitat actions are of little use because plenty of habitat is available to increase capacity. Instead, Connors argues that we need actions outside of freshwater habitat to boost populations up to the point where they exploit all available habitat.

4. As in his previous declaration, Connors offers little empirical support for his hypothesis. He cites studies by Isaak and Thurow (2006) and Ward (2000) in support. With regard to Isaak and Thurow, Connors claims that I selectively represented results, and that the paper actually supports his hypothesis. Connors claims that I only discussed results from the two populations with the highest density while ignoring the three with the lowest density. In fact, the two types

of populations exhibited very similar behavior across the lower range of densities experienced by all these populations (see Figure 5, Isaak and Thurow (2006)). In all five populations, as population density increased, the number of habitat units occupied increased in a regular and consistent pattern. In fact, in referring to all five populations, the authors state, "occupation of previously unused stream segments was rapid." I highlighted the two high-density populations because these two populations reached higher densities than those experienced by the other three populations. For the two populations reaching higher densities, they demonstrated a leveling off at about 70% occupation of habitat units, with the remaining 30% of habitat units likely unsuitable for spawning. This pattern of rapid utilization of habitat units as populations rebounded from low levels (a pattern present for all five populations) is not consistent with the Connors' hypothesis, which states that only a portion of available habitat is utilized as populations expand.

5. Connors also cites Ward (2000) as support for his hypothesis. However, this study offers limited support. First, the study population is a coastal population of steelhead on a small river (Keogh River) on Vancouver Island. The Keogh River is 35 km long and drains a watershed of 130 km^2. Connor is suggesting we can apply the results from this system to all populations in the Columbia River basin, with an area of over 415,000 km^2, and with some populations migrating over 1000 km to high elevation habitats. Given these substantial differences, extrapolating the results in this manner is not appropriate. Moreover, the study doesn't really support his hypothesis at all. Similar to the predictions of Connors hypothesis, the study demonstrates a decreased spawner-recruit relationship in recent years compared to an earlier time period (Figure 4 in Ward (2000)), suggesting the existence of different recruitment regimes. Although Ward (2000) suggest several hypotheses to explain this observation – change in

environmental conditions, increased exposure to UV light, genetic effects due to a population bottleneck, decreased nutrient availability – he does not include overcrowding in a limited set of habitat areas (the Connors hypothesis) among the proposed hypotheses. It should be noted that this is a highly studied system, with many papers published on it (see several examples in the references for Ward (2000)), so if habitat underutilization were a factor in reduced freshwater productivity, Ward would have likely mentioned it. Finally, Ward (2000) observed a third productivity regime that occurred in the time period between the current and past regimes that was substantially more productive than either. This occurred as the result of a nutrient enhancement program, exactly the type of action that could be considered as part of a habitat restoration program – the type of action Connors argues wouldn't be effective. So in fact, this study actually contradicts Connors' hypothesis and his conclusion that habitat actions will not be effective for Columbia River basin salmonid populations.

6. In conclusion, I maintain that Connors' hypothesis lacks empirical support. Nonetheless, Connors prescribes taking drastic management action (curtailing habitat restoration efforts) based on a hypothesis with very limited empirical support.

Response to Kostow Declaration

7. In my previous declaration, I raised several concerns about the analyses represented in the Nigro declaration. In particular, I noted that the extensive analyses had not received any form of peer review, and accordingly, I found inconsistencies in data and citations. Kostow, in her declaration, responds to the issues I raised about the Nigro declaration.

8. In response to my claim that the analyses contained within the Nigro declaration were not peer-reviewed, Kostow replied that the analyses were essentially peer-reviewed because they were based on the Ricker model, which had been peer-reviewed. However, in a peer-review

process, all aspects of the analysis are considered, including: the data that go into the model; how models are combined together; the assumptions required to conduct the analysis; and the conclusions drawn from the results. My concern was not about the Ricker model itself, which we used in Appendix C, but about the other aspects of the analysis.

9. Regarding the data that went into the analysis, I raised several concerns about inconsistencies across figures, unclear citations, and inconsistencies between data presented in figures and those provided in the sources. Kostow provided several clarifications on how data were derived. For example, she explained how different life-history types were combined to produce estimates of smolt abundance from the Copeland et al. (2014) paper. In response to concerns about interpretation of smolt data for the Oregon populations based on the Jonasson et al. (2014) report, Kostow shifted the data points for these populations (comparing Kostow's Figure A to Nigro's Figure 8). These shifts substantially reduced the gap needed to achieve replacement, according to Kostow's Figure A. These clarifications and modifications indicate that the analyses are still a work in progress and have not received the rigorous scrutiny necessary for it to be ready for management purposes.

10. Regarding Figure A, some issues with the data still remain. For example, in the South Fork Salmon River, the smolt trap is high up in the watershed, and a substantial proportion of the population spawns below the trap. Nigro and Kostow used total spawner abundance to calculate smolts per spawner, which greatly underestimates this value. When we adjusted smolts per spawner to account for only adults that spawned above the trap, smolts per spawner increased from 33 to 91, which places this population close to the replacement line. In addition, some of the populations in the figure have experienced strong influences from hatchery supplementation. It is unclear from the Kostow and Nigro declarations how this influence was dealt with. For

example, the Pahsimeroi was treated as a hatchery supplementation study stream and was subject to drastic hatchery actions. For a generation in the mid-80s, all returns to the weir at the mouth of the Pahsimeroi were taken into the hatchery to initiate a local broodstock program. In the subsequent 5 years, virtually all returns were from releases from that hatchery program and a limited number were passed over the weir into the natural spawning areas (the bulk were used as hatchery broodstock during that cycle). After that, the weir has been used to control the proportion hatchery fish into natural areas, pretty high proportions in the 1990s, with much lower proportions recently. These actions clearly have impacted this population.

11. In defending the Nigro approach, Kostow claims that the model is just "simple arithmetic". However, by using an overly simplified approach, the analyses ignore important components of population dynamics that drive population trajectories. For example, as demonstrated by Kostow's Figure I, SARs are highly variable from year-to-year and they also exhibit auto-correlation across years (bad years are typically followed by bad years, etc.). So just using a single SAR value misses important dynamics. Further, recruits per spawner are typically log-normally distributed (as depicted in the Ricker model), which means that there are more high-recruitment events than compared to a symmetric normal distribution. These high recruitment events can sustain the populations for a number of years. The simplified analysis of Kostow and Nigro ignores these dynamics with unclear consequences. It should be noted that Life-cycle modeling approach takes these factors into account, and several of these models have been peer-reviewed (e.g., Zabel et al. 2006). These types of approaches would be better suited to conducting the analyses of the Nigro and Kostow declarations.

12. In paragraph 13, Kostow agrees that Ricker curves with limited data points typically have a great deal of variance. She then presents results from a simulation for a single population

(Minam River Chinook salmon) where she allowed for variance in the estimation of SARs. She then concluded that the plot "indicates that SARs need to approximately double … in order to reach and maintain the target abundance." Although this analysis begins to incorporate some of the population dynamics I mentioned above, it is quite unclear how she reached this conclusion. In particular, she didn't define some the key symbols in Figure B (diamonds and triangles), so it is unclear how the simulation contributes to the ongoing discussion.

13. More importantly, Kostow appears to have missed my point about the importance of variance in the Ricker relationship. My main point was that with small sample sizes and most of the data representing low abundances, the Ricker curve has a great deal of uncertainty. In particular, estimating the key parameter, Smax (the estimated maximum number of spawners sustainable by a population), is problematic because of the lack of data at higher abundances, which results in the few points at high abundance having very high leverage in determining this parameter. This is undesirable statistically, and thus using a point estimate of Smax without representing the uncertainty in the parameter is not desirable. Yet this is what Kostow does in the simulation in Figure B, and the abundance standards specified in Figure C.

14. The analysis described in paragraph 13 highlights another issues with the general approach described in the Nigro and Kostow declarations. In Figure A of the Kostow declaration, the Minam population is slightly above the replacement line (dashed line), meaning the population is above the standard depicted in Figure A. However, in Paragraph 13, Kostow claims SARs would have to double for this population to reach the specified recovery goal. This incongruence across analyses arises because the plot in Figure B depicts the SARs required to instantly achieve recovery *in a single generation*. This is not a realistic expectation as most salmon recovery plans are aimed at making survival improvements that lead to achieving

recovery goals across several or more generations. Thus a population like Minam that is above the replacement threshold could increase in abundance to reach recovery goals without drastic increases in SAR as concluded by Kostow. In fact, this draws into question the relevance of these types of analyses (Figure B and Appendix A) in a discussion of meeting targets under this Biological Opinion. In particular, it is clear that using this analysis to specify target SARs is inappropriate.

15. In paragraph 15, Kostow argues that the recent ISAB report supports her claim that a point estimate of Smax is an appropriate recovery goal. Although the ISAB does support the use of reference points based on recruitment models, they clearly do not specify Smax as an example. In fact, on page 31, the ISAB identifies maximum sustainable yield (MSY), which is less than Smax, as a typical abundance goal. Further, as Kostow points out, the ISAB endorses the use of ecosystem-based management to establish abundance goals "given the existing uncertainty about density dependent … processes." Their clarification in the quotation marks supports my concerns about using Smax as a recovery target. Finally, I agree with ISABs conclusion that there are benefits from abundances that exceed capacity. However, this type of discussion is more appropriate for a recovery plan.

16. In paragraph 18, Kostow describes Figure C, where for several example populations, she compares current abundance to the ICTRT abundance goals and Smax. She concludes that when Smax is greater than the ICTRT goals (as is the case in the wilderness populations), enough freshwater habitat exists to achieve ICTRT recovery goals. When the opposite is true (as is the case in the degraded habitats), more habitat capacity is needed to achieve ICTRT recovery goals. In all cases, she concludes that out of basin improvements are also needed and that the best candidate for these survival improvements is the hydrosystem. First, as I outlined above, I do

not believe a point estimate Smax is appropriate for an abundance goal due to several statistical concerns. Therefore, using it as a measure of habitat capacity is even more questionable. Nonetheless, I agree with some of Kostow's conclusions in this paragraph. I agree that there are more opportunities for survival and capacity improvements in degraded habitats. In fact these types of populations are the focus of proposed habitat actions. I also agree that we need to look to opportunities outside of spawning/rearing habitats for potential improvements. However, opportunities for survival improvements exist across the salmon life cycle in all Hs (harvest, habitat, hatchery, hydro), not just in hydro.

I declare under penalty of perjury that the foregoing is true and correct. Executed on May 1, 2015, in Seattle, Washington.

Richard W Zabel

Richard W. Zabel, Ph.D.