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UNITED STATES DISTRICT COURT
DISTRICT OF OREGON

NATIONAL WILDLIFE FEDERATION, *et al.* Plaintiffs, v. NWR NATIONAL MARINE FISHERIES SERVICE, *et al.* Defendants.	Civil No. 01-640-RE 2008 REPLY DECLARATION OF ROBERT P. JONES Jr., NOAA

I, Robert P. Jones Jr., declare and state as follows:

1. On October 24, 2008, I provided a declaration in support of the National Marine Fisheries Service's (NMFS') 2008 Biological Opinion (BiOp) for the Federal Columbia River Power System (FCRPS) in this litigation. There, I described my qualifications and experience. I also explained certain technical issues concerning actions required by the NMFS' BiOp and its analysis of the effects of those actions on listed Columbia and Snake River salmon and steelhead. The issues I discussed in that declaration were raised in declarations prepared for the plaintiffs NWF and the State of Oregon by Mr. Jack Williams and Mr. Edward Bowles.

2. On November 18, 2008, Mr. Jack Williams filed a Reply Declaration in support of NWF's motion for summary judgment. I have reviewed this reply and now provide this declaration to respond to further comments and questions raised therein.

3. In Mr. Williams' Reply Declaration, he states at paragraph 23 that consultation on the funding of hatcheries, conditioned on criteria such as best management practices being implemented at the hatchery, is inappropriate because the criteria do not yet exist. "[T]he 'proposed funding decision criteria' that Mr. Jones refers to and that are identified in RPA 39 and the Action Agencies' BA did not exist and had not actually been adopted at the time NOAA prepared the 2008 BiOp. Any actual consultation on these funding criteria, therefore, is necessarily still in the future." (paragraph 23 of the Williams Reply Declaration). This statement misunderstands what NMFS did with respect to consulting on hatchery funding.

4. First, hatchery funding is part of the proposed action. RPA 39 states that the Action Agencies will adopt "programmatic criteria for funding decisions" and that these criteria will be further defined from objectives provided in the Biological Assessment and from recommendations and guidance developed through the collaboration. These criteria or objectives for operating hatchery programs do exist and have been provided. The Action

Agencies Biological Assessment (BA) at B.2.3-4 provides "guidelines for hatchery operation" and Appendix C of NMFS' SCA provides "Recommendations for Operating Hatchery Programs Consistent with Conservation and Sustainable Fisheries Mandates". See also SCA 5.5, Hatchery Effects. Because hatchery funding is part of the proposed action, it must be addressed in the BiOp.

5. Next, as stated in my prior declaration at paragraph 10, NMFS did not assign any quantitative benefits to the hatchery funding action as a result of the consultation, precisely because the benefits of an action that will be tailored to each hatchery program in the future are impossible to assess at this time. "NMFS will not know what actual quantifiable effects the adoption of BMPs may have on listed ESUs or steelhead DPSs until RPA 39 is implemented at each hatchery program and we can analyze the resulting program changes in site-specific ESA section 7 consultations." The BiOp calls for hatchery reforms, provides principles for reform, and establishes process for implementing reforms. RPA 39 requires new Hatchery and Genetic Management Plans (HGMPs) and establishes principles for operating hatcheries (BA at B.2.3-4 and Appendix C of the SCA). The NMFS BiOp states that the application of these principles and criteria will be determined at the site-specific scale in "ESA Section 7, Section 10, or Section 4(d) consultations with NOAA Fisheries," and this work is already underway in the Upper Columbia River (UCR) per RPA 39. Thus, while some discussion in the BiOp of RPA 39 is required, as these measures will likely lead to effects, the assignment of quantitative benefits cannot be done until these site-specific consultations are performed in the future.

6. At paragraph 24, Mr. Williams states that NMFS did not analyze the HSRG guidelines in assessing hatchery effects. In fact, NMFS did consider HSRG guidelines in assessing hatchery effects. In my prior declaration at paragraph 13, I state that NMFS analyzed

specific hatchery actions identified in RPAs 40, 41 and 42 and "significant past management changes that would be expected – based on the scientific literature – to result in a change in the reproductive fitness of naturally spawning hatchery-origin fish relative to naturally spawning natural-origin fish." NMFS also followed Appendix C of the SCA, "Assessing Benefits and Risks & Recommendations for Operating Hatchery Programs Consistent with Conservation and Sustainable Fisheries Mandates", which considers principles for operating hatchery programs developed by the HSRG, to analyze the specific hatchery actions referenced above (for example, see SCA, Appendix C page 25).

7. In paragraphs 26 and 27, Mr. Williams argues that hatchery effects should reduce natural-origin fish productivity in base-to-current adjustments and questions "whether the model NOAA employed can properly be applied under circumstances where the facts relevant to one of its core assumptions may not be met." The core assumption Mr. Williams refers to is "the average productivity of wild spawners must be the same during the baseline and future periods." This is not a core assumption and NOAA captures hatchery effects, at least hatchery affects that it has data for, in the model. The method used to estimate population productivity (which is consistent with the ICTRT's method) considers the contribution of *all* naturally-spawning fish to the next generation, both hatchery-origin and natural-origin. To the extent the productivity of the natural-origin component of the spawning population is impaired, that will manifest itself in the overall productivity estimate for the population. Regarding adjustments in productivity resulting from recent changes in hatchery management, hatchery practices in general have been steadily improving (see Appendix C Chapter 5) and these improvements benefit natural-origin fish. Appendix C of the SCA provides seven examples of how changes in hatchery management can benefit population abundance and productivity. SCA Appendix C, Figure 3. In fact, the changes

NOAA modeled were designed and implemented to benefit natural-origin fish productivity. For example, improving the fitness of hatchery fish that are included in an ESU or steelhead DPS and that are intended to spawn naturally for conservation purposes would provide a biological benefit to the natural-origin component of the population. As yet, NOAA does not have information that would support a quantitative analysis of these beneficial effects nor does it have data to quantify any adverse effects on the natural-origin component of any natural population. The modeling method that Mr. Williams criticizes simply provides an estimate of the *current* productivity of the naturally-spawning population (as opposed to the population's average productivity during a 20 year historical base period) resulting from certain significant changes in hatchery management. This is identical in concept to the analytic approach used for other "Hs." For example, juvenile salmonid survival through the hydrosystem is also adjusted (base-to-current) to reflect management changes that occurred partway through the base period, but are not fully reflected in the base period productivity estimate. BiOp 7-8 – 7-12. Finally, it may be that Mr. Williams is concerned that NOAA is not attempting to estimate the productivity of the natural-origin component of the population independent of the hatchery-origin spawners within that population. But this is also not true. As was the case in the 2000 BiOp, the 2008 BiOp uses two estimates of lambda (or median annual population growth rate) that assume different values for the relative reproductive effectiveness of hatchery-origin spawners. See for example BiOp at 8.3-56. This provides an estimate of the productivity of the natural-origin component of the population using two assumptions regarding the relative reproductive success of the hatchery-origin spawners within that population.

8. It should also be noted that obtaining more accurate estimates of hatchery fish relative fitness reduces uncertainty in assessments of natural population viability. In late 2004,

the paper, Review of Relative Fitness of Hatchery and Natural Salmon (Berejikian and Ford 2004) was published in part "to provide information that would be useful in updating the 2000 Federal Columbia River Power System Biological Opinion" and for "estimating long-term natural population growth rates." The authors assert that it is necessary to know or estimate the relative fitness of the hatchery fish compared to the natural fish in order to estimate the natural productivity of a population. Since the 2000 FCRPS BiOp, numerous additional studies on the relative fitness of hatchery fish and natural-origin fish have been published. The estimates of hatchery fish relative fitness provided in Berejikian and Ford 2004 use the latest information to "aid in refining parameter estimates for productivity models for natural populations in cases where hatchery and natural fish co-occur on the spawning grounds."

9. However, Mr. Williams is not focusing on the relative productivity of hatchery fish versus natural ones. Instead, he chooses to focus on the productivity of specifically the natural-origin fish. At this time, there is insufficient information to quantitatively predict effects on natural population productivity from interactions between hatchery and natural-origin fish. Therefore NOAAF assesses these effects qualitatively. Berejikian and Ford 2004, for example, report that "the large body of literature on genetic introgression of hatchery and natural populations does little to assist in reducing the uncertainty regarding relative hatchery and natural fish fitness." There are numerous variables that make it extremely difficult to accurately measure the effects on natural fish of interacting with hatchery-origin fish, including the intensity and duration of hatchery fish natural spawning, genetic similarity between hatchery fish and natural-origin fish, habitat quantity and quality, and selective fisheries. These and other factors can also vary from place to place and from year to year. Thus, NOAA's quantitative analysis is

well grounded in the latest science, whereas Mr. Williams is asking us to wade into uncertain territory.

10. Also at paragraph 27, Mr. Williams questions how NOAA can claim any productivity benefits when "they don't yet know the productivity effects of individual hatchery programs on the natural-origin spawners." NOAA found 18 studies on this subject that have been published since the 2000 Bi-Op (Berejikian and Ford 2004). From these we calculated changes in productivity based on these studies on top of other scientific information and pinpointed where new studies are required (see RPA 64 and 65). The SCA Appendix C described seven practices that hatchery programs can implement to benefit population abundance and productivity (Appendix C, Figure 3, items 1-7 at bottom of graphic). Scientific information was available to quantitatively model recent reductions in hatchery fish in the wild and the doubling of hatchery fish effectiveness in the wild when they are included in an ESU or steelhead DPS and intended to support recovery (SCA Appendix I Tables 1, 2, 3, 6, and 8).

11. Mr. Williams further states that Stier and Hinrichsen themselves argued against using a constant natural-origin fish productivity factor. They did not: after acknowledging that "adverse effects on the fitness of the natural-origin component of the spawning population could complicate the comparison of the relative reproductive effectiveness of hatchery-origin spawners to a hypothesized natural-origin fish…", the authors stated: "However, any reduction in the estimated survival improvements that might result from genetic fitness loss in natural-origin spawners could be negated by a long-term improvement in natural-origin spawner fitness as a result of the kinds of hatchery reforms considered in this analysis."

12. In paragraph 28, Mr. Williams claims that my previous declaration relied on low interbreeding among hatchery and natural-origin UCR steelhead in order to justify "sizeable

productivity benefits" from recent changes to the hatchery program at issue in the basin. This is not correct. There is no data on the level of interbreeding between hatchery and natural-origin natural spawners, so NOAA used the formula articulated in Berejikian and Ford (2004) to estimate the productivity of hatchery-origin steelhead and natural-origin steelhead in the UCR. Genetic introgression of hatchery fish into natural populations requires interbreeding and successful reproduction of hatchery fish in the natural environment. There is no data that measures interbreeding between hatchery and natural-origin steelhead, nor is there data that measures and compares the productivity of naturally spawning hatchery-origin steelhead and natural-origin steelhead in the UCR. In the absence of this data, hatchery fish relative reproductive effectiveness, or *"e"*, values were used to estimate changes in productivity and these values were derived from Berejikian and Ford 2004 and Araki et al. 2006 (see S.75 and S.76).

13. In paragraph 29, Mr. Williams questions why the recently integrated Wenatchee subbasin steelhead hatchery program should not be viewed as an immediate threat to the productivity of natural-origin fish. As described in Appendix D of the SCA and at S.75, this program implemented major reforms starting in 1998 and it has succeeded in preserving and developing steelhead genetic resources and in boosting the number of natural spawners (SCA Appendix D page 66). NMFS recommendations for operating hatchery programs (SCA Appendix C) emphasize that "risks from continued hatchery supplementation should be weighed against the risk of extinction in the absence of hatchery supplementation." For example, when the number of natural-origin spawners declined to dangerously low levels during the mid 1990s, the risk of extinction outweighed the potential risks to productivity posed by supplementing natural spawning with hatchery fish included in the steelhead DPS. Pursuant to RPA 39, site-

specific hatchery consultations, including the Wenatchee steelhead program, are underway in the UCR to ensure that hatchery programs are not impeding recovery of the steelhead DPS and the spring Chinook salmon ESU.

14. In paragraph 30, Mr. Williams highlights several uncertainties and argues that productivity gains for Methow steelhead are illusory "if hatchery and natural-origin spawners are not dramatically different." Available data are not sufficient to conclude that the Methow steelhead population is gone and that the only fish that remain are derivatives of hatchery spawners (i.e., that hatchery and natural-origin spawners are not different). In fact, current ESA Section 10 permits for operating hatchery programs and for regulating public fishing, and strategies employed by managers in the Upper Columbia, go to considerable effort to manage hatchery fish differently and to protect and promote a natural population (e.g., removing hatchery fish from the river before spawning). It is correct that the majority of steelhead returning to spawn above Wells Dam have been hatchery fish. Hatchery practices in the Methow before 1998 discouraged interbreeding between hatchery and natural-origin fish (S.75). Because of the crash in the population (natural-origin returns were < 200 fish for five of six years between 1993 and 1998), hatchery fish have been counted on to preserve genetic resources and boost the number of natural spawners (SCA Appendix C, page 67). As mentioned above, genetic introgression of hatchery fish into natural populations requires interbreeding and successful reproduction of hatchery fish in the natural environment, and there is no data that measures interbreeding between hatchery and natural-origin fish, nor is there any data that measures and compares the productivity of naturally spawning hatchery-origin fish and natural-origin steelhead in the Methow.

15. In the absence of this data, the BiOp uses best available scientific information (Berejikian and Ford 2004) to estimate the productivity of hatchery-origin steelhead in the Methow relative to natural-origin fish. In both cases, hatchery steelhead and natural-origin steelhead productivity are dramatically different, and therefore productivity gains for Methow steelhead are not illusory. Two scenarios were modeled to estimate changes in productivity resulting from changes in hatchery management and to address the range in uncertainty for the Methow (SCA Appendix I, page 11). Once scenario assumed that hatchery-origin fish were 30% as effective and one scenario assumed they were 45% as effective. In both cases, hatchery fish effectiveness *"e"*, prior to changes in the hatchery management, was assumed to be 0.2 (i.e., 20% as effective as natural-origin fish) based on broodstock practices and juvenile release strategies (S.75). Another action to deal with critical uncertainties is the major new investigation required by RPA 64 to determine hatchery-origin and natural-origin steelhead effectiveness and the effect of supplementation on the productivity of steelhead in the Methow.

16. In paragraph 31, Mr. Williams disputes my explanation of why NMFS expects to see 67% natural-origin fish on the spawning grounds of the Upper Grand Ronde, Catherine Creek and Lostine/Wallowa populations of Snake River Chinook. Specifically, he argues that "attaining an average of 67% natural-origin spawners under such circumstances (1) is beyond the broodstock management standards outlined in the draft HGMP for these three populations and (2) is not consistent with experience." The draft HGMP referenced by Mr. Williams is obsolete; in fact, NMFS and hatchery managers are working on new HGMPs that incorporate the most recent scientific information. RPA 39 establishes the schedule for completing these HGMPs. In the meantime however, the operating plan for Imnaha (Table 2 of Appendix C of the SCA) is not illusory and shows how goals for natural-origin spawners can be achieved. NMFS

recommendations for developing HGMPs and operating hatchery programs explain that the need for hatchery supplementation is based on the health or status of the natural population (SCA Appendix C). When the target natural population is at low abundance, greater supplementation may be warranted (based on site specific considerations) and the percent of natural spawners comprised of natural-origin fish is low (i.e., the *"f"* value used in Appendix 1 of the SCA). Conversely, when natural-origin spawner abundance is high, there is less need for hatchery supplementation and the percent of natural spawners comprised of natural-origin fish should be high. I interpret Mr. Williams' second point that attaining 67% "is not consistent with experience" to mean that this program does not have a track record for "attaining an average of 67% natural-origin" natural spawners (the *"f"* value). For these populations, the most important factor in attaining a high *"f"* value is the abundance of natural-origin fish, not past experience operating the hatchery supplementation program. Because these programs are relatively new (since substantial changes and reforms were implemented) their track record is limited. In the case of the Upper Grande Ronde (see Appendix 1 Table 1), *"f"* values have been above 67% for two years (0.95 and 0.80 in 2002 and 2003) and below 67% for two years (0.05 and 0.04 in 2004 and 2005).

17. In paragraphs 32, 33, and 34, Mr. Williams argues that NOAA tried to exclude from analysis changes to hatcheries which resulted in populations that "have had their productivity unfavorably affected by relatively recent changes in hatchery programs." In other words, Mr. Williams accuses NOAA of avoiding analyzing situations where the end results show negative impacts. Mr. Williams then uses two examples: Wenatchee spring Chinook and Imnaha spring/summer chinook. Mr. Williams is wrong to suggest that NOAA excluded consideration of these populations because they showed unfavorable effects. As stated in previous declaration

at paragraph 41, the standard for inclusion in the analysis "is populations which experienced effects from changes in hatchery management practices that were not captured in the baseline period" (1980-1999). In the case of the Imnaha program, native broodstock were used from its inception. There were no significant management changes during the life of the program and therefore it did not satisfy the criteria used for application of the model. The same is true of the program in the Wenatchee River subbasin. To repeat: neither of the programs to which Mr. Williams refers satisfied the objective criteria NOAA used to select populations for treatment with the model.

18. Mr. William's second point at paragraphs 33-34 is that NOAA did not apply its standard for calculating changes in productivity consistently, and he cites populations in the Minam and Wenaha as examples. Mr. Williams misunderstands the changes in the Minam and Wenaha River basins that NOAA is referring to. In fact, these "recent changes" in hatchery practices first took affect after 2002 (S.75), not during the base period. These changes are summarized in my declaration at paragraph 27: "However, recent changes in hatchery management meant that current strays in Tables 4 and 5 are now derived from local broodstock of the same EU and Major Population Group.... [T]hese changes reduce genetic risks to natural populations, but because they are strays and as such their productivity is still questionable, NMFS conservatively estimated their *e* factor to be 0.20." Again, NOAA followed best available information and applied this methodology consistently across all populations.

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

12/16/2008

Robert P. Jones Jr.