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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 RITCHIE J. GRAVES

I, Ritchie J. Graves, declare and state as follows:

1. On October 24, 2008, I provided a declaration in support of the National Marine Fisheries Service's (NOAA or NOAA Fisheries) 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 NOAA's BiOp and its analysis of the effects of those actions on listed Columbia and Snake River (SR) 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. Frederick Olney and Mr. Edward Bowles.

2. I have reviewed the reply declarations filed by Mr. Olney and Mr. Bowles (November 14, and November 18, 2008, respectively) and now provide this declaration to respond to further comments and criticisms raised in their reply declarations.

2009 Operations

3. As indicated in my previous Declaration (Graves Declaration at ¶ 31), NOAA and the federal Action Agencies have considered the recommendations of the ISAB (2008-5) in the development of spill-transport operations for the 2009 spring outmigration. NOAA and the Action Agencies recently recommended to the Regional Implementation and Oversight Group (RIOG meeting on December 12, 2008) that 2008 FCRPS BiOp operations at the three Snake River collector projects be modified in 2009 to continue the spill program through the May 7-20 timeframe (as long as flows are expected to exceed 65 kcfs). In the fall of 2009, NOAA and the

Action Agencies will collect and review the newly available information and recommend, to the RIOG, spill/transport operations for the following year.

4. NOAA can support continuing spill at the three Snake River Collector Projects in 2009 for the following reasons:

a. Termination of spill at LMN and LGS in 2009 would negatively affect studies being conducted to determine the efficacy of recently installed RSWs / TSWs.

b. The operation would conform with advice from the ISAB to continue 2007-08 type operations while gathering information to assess the potential alteration of post-Bonneville inriver survival patterns resulting from recent passage improvements at the mainstem FCRPS projects (and risks to SR sockeye).

c. NOAA expects that about 50,000 SR sockeye may be PIT tagged (COE & BPA are looking into funding) - which would provide the best information to date regarding inriver survival estimates – useful for assessing tagging needs and statistical power tests for any future transport/inriver comparisons.

5. However, NOAA understands that this operation is not without risks to SR steelhead and, to a lesser extent, spring/summer Chinook salmon. COMPASS modeling indicates that in the 80-130 kcfs years (representing the most likely 2009 spring flow conditions in the Snake River based on long-range forecasting and the past distribution of flows observed in the 70-year water record (36 of 70 years were in the 80-130 kcfs range):

a. Average transport rates would be reduced to about 57% and 70% for both spring Chinook and steelhead, respectively); a reduction of about 6% (10% relative for Chinook and 7.5% for steelhead); and

b. Average relative LGR-LGR SARs would likely be decreased by about 1% for Chinook and 6% for steelhead (an absolute change of about 0.00010 for Chinook salmon and 0.00107 for steelhead).

6. This operational adjustment will not affect the overall ESU conclusions in the BiOp, which considers the full implementation of the RPAs (including adaptive management provisions) across a 10-year timeframe. The one-year timeframe for this proposed modification is consistent with the adaptive management provisions of the 2008 FCRPS BiOp (see RPA Actions 1-3, 23-24, 29-31, and 54-55) and may provide information pertinent to longer term operations and their affects on Snake River ESUs. Further, the impacts of the operation are likely to be mitigated because ocean trends have been favorable in recent years and there is no indication at present that these trends will be reversed in 2009 – meaning that SARs will likely be higher for 2009 outmigrants than the "average" estimates in the COMPASS modeling would predict (about 0.9% for Chinook and 1.8% for steelhead). If this proves true, there is no additional risk (in terms of negatively affecting population growth rates) to SR steelhead (or spring/summer Chinook salmon) populations resulting from 2009 operations.

Flow Management and Objectives

7. In Mr. Bowles' Second Declaration (at ¶ 38) he states that "Mr. Graves appears not to agree that the 2008 Biological Opinion dramatically reduces the amount of flow augmentation in the summer and provides no more flow augmentation in the spring than provided in the 1999 Proposed Action (the same as the 2000 Biological Opinion), as stated in my previous declaration." I acknowledge (and did not dispute in my prior Declaration) that the 2008 BiOp adopts an ecosystem-based water management plan (NPPC 2003, NOAA AR B.385) that

focuses on obtaining the most biological benefit to anadromous fish with available water while still providing for the needs of other resident species. This flow regime, which is consistent with the best available data, modestly reduces the water volumes available for Snake River fall Chinook salmon and summer flow augmentation in the Columbia River. The ecosystem plan (NPPC 2003, NOAA AR.385) was reviewed by the Independent Scientific Advisory Board (ISAB 2004, NOAA AR B207) and adopted by the Northwest Power Planning and Conservation Council. NOAA, the ISAB, and the NPPC have carefully reviewed the anticipated effects of adopting the plan and have found, on balance, that the likely effects on anadromous salmonids would be difficult to measure. The 2008 BiOp adopts this measure on an experimental basis and the result of these studies will inform future reservoir management decisions.

8. Mr. Bowles wholly ignores that maintaining a certain volume of water is not the goal of the flow augmentation program. The goal of the program is to increase fish survival by increasing flows during key periods to reduce juvenile fish travel times, either to the sea or points of collection and transport. As I noted in my first Declaration (at ¶ 14), substantial improvements in system operations and configuration have been established since 1995 that reduce juvenile fish travel times and increase fish survival. Flow augmentation is only one tool being applied to this effort. NOAA Fisheries considered the likely effects of these changes in the flow augmentation program in developing its analysis of effects and its conclusions. Mr. Bowles goes on to state that much of the increase in water volumes available for flow augmentation since 1995 identified in my prior Declaration (at ¶ 12) were “implemented beginning with the 2000 Biological Opinion.” Although several of these measures were included in the 2000 BiOp, the actions took several more years to fully implement. For example, the alternative variable

discharge flood control strategy (VARQ) was implemented on an interim basis beginning in 2002 at Hungry Horse dam and 2003 at Libby dam.

9. Mr. Bowles (Second Declaration at ¶ 39) points out that, in total, changes to the flow augmentation program would reduce the volume available for summer flow augmentation in virtually all years and that benefits from recent actions typically vary by water year type (i.e. wet, dry, average) and that these benefits will accrue primarily during the spring. In other words, there is a shift in benefits from summer to the spring to reflect the new data on SR fall Chinook. I concur. However, NOAA Fisheries evaluated the effect of these changes during the spring in terms of fish survival (COMPASS modeling) and population metrics, not terms of water volumes. Mr. Bowles emphasis on water volumes is misplaced.

10. Mr. Bowles has (Second Declaration at ¶ 39) misconstrued statements in my earlier Declaration, that the 2008 BiOp relaxes requirements for annual drawdown at Grand Coulee dam for drum gate maintenance, by suggesting that prior Biological Opinions had not authorized drawdowns for this purpose. NOAA's prior FCRPS BiOps have considered both the operations and maintenance of the FCRPS projects. The U.S. Bureau of Reclamation (Reclamation) has simply clarified its authority to draft Lake Roosevelt to elevation 1255 to accommodate drum gate maintenance for dam safety reasons. Because of concerns that, during dry years when the required drafting for flood control is substantially less, drafting to elevation 1255 would reduce flows downstream from Grand Coulee Dam during the spring as Lake Roosevelt is refilled and would adversely affect migrating fish, efforts were taken during the collaborative process to reduce this adverse effect of dam safety maintenance. Following investigation by Reclamation of the frequency that drum gate maintenance is needed and the frequency and magnitude of reservoir drafting for flood control, Reclamation proposed

modifying the drum gate maintenance schedule as described in the 2008 FCRPS BiOp Reasonable and Prudent Alternative (2008 RPA) 4, which states: "Take advantage of reservoir draft for flood control during high water years to perform drum gate maintenance. Drum gate maintenance must occur at a minimum one time in a 3-year period, two times in a 5-year period, and three times in a 7-year period." Thus, by acknowledging the potential to safely defer needed work for a year or more, and by attempting to schedule drum gate maintenance to avoid dry years, the 2008 Biological Opinion minimizes the effects of drum gate maintenance on Columbia River spring flows.

11. Mr. Bowles misunderstands (Second Declaration at ¶ 42) the dry year plan for Lake Roosevelt and Grand Coulee Dam included in the 2008 RPA. Under the 2008 FCRPS BiOp, Lake Roosevelt would continue to be drafted to elevation 1278 by the end of August in the lowest 50 percentile of water years and the 2008 RPA leaves a future option to draft to elevation 1278 only during the lowest 20 percentile of years depending upon study results and the outcome of regional collaboration. [FCRPS BA (page 2.1-9, AR B.89) and FCRPS BiOp (at RPA 4)]. The purpose for evaluating the effectiveness of a less frequent draft to elevation 1278 is to better balance operations to best meet the needs of both ESA-listed salmon and resident fish important to both the Spokane Tribe and the Confederated Colville Tribe.[1]

[1] In his declaration dated 19 November, 2008, Charles E. Pace, Ph.D., addresses the concern of the Spokane Tribe over the operations of Lake Roosevelt and suggests that operations be optimized to meet the needs of resident fish as well as anadromous fish. See Exhibit 4 (Pace/Spokane): "…ensure that in-season water management operations at GCL are optimized to meet the needs of anadromous and resident fish. If in-season water management operations that are not optimized to meet the needs of resident fish, they must; 1) be premised upon and proportional to demonstrable biological benefits for anadromous fish…" and "Develop and use the technical capability to analyze differing GCL operations on salmon, steelhead, sturgeon and resident fish migration, survival, spawning and rearing…" See Exhibit 5 (Pace/Spokane): "GCL operations based solely on achieving flow targets for anadromous fish are avoided." "The criteria ensure that in-season water management operations of GCL are optimized to meet the needs of anadromous and resident fish."

12. Mr. Bowles (Second Declaration at ¶¶ 40 and 41) addresses the operation of the Hells Canyon Complex (HCC) hydroelectric project owned by Idaho Power Company (IPC) and NOAA's prior actions aimed at managing Snake River water. Mr. Bowles is factually correct when he states that the 1995 BiOp (at pg. 101) required that the Technical Management Team coordinate with IPC to obtain 110 kaf for flow augmentation during the spring and up to 237 kaf in total by the end of August or September. In all other respects, Mr. Bowles assertions are wrong. There is no doubt that the existence of the HCC, under the ownership of non-federal parties and under the regulatory authority of another federal agency, the Federal Energy Regulatory Commission (FERC), has complicated managing the Snake River system. However, IPC has actively participated in the Snake River flow augmentation program since the late 1980s when it was part of the NPPC's water budget, and we have no reason to believe that IPC would now intentionally interfere with federal actions designed to benefit anadromous fish. Further, while NOAA cannot prescribe RPA measures for the operations of the HCC through its consultations on the FCRPS - as these facilities are operated by IPC and FERC's approval of them in a new license is a separate agency action subject to its own consultation - through involvement in the FERC relicensing efforts currently underway for this project, NOAA has been successful in securing 237 kaf of flow augmentation during the summer (July-August) from HCC under an interim operations agreement.[2]

[2] The "Hells Canyon Hydroelectric Project Settlement Process – Interim Agreement" was signed by most parties to the settlement negotiation, including IPC, NOAA, and the Oregon Department of Fish and Wildlife in December 2004 and submitted by IPC to the Federal Energy Regulatory Commission on January 7, 2005. In this document, IPC agreed to "implement certain additional measures on an annual basis, provided that the parties remain engaged in settlement discussions intended to resolve long-term relicensing issues." Although the settlement agreements ended unsuccessfully in 2005, IPC has continued to implement most of the additional measures on a voluntary basis in subsequent years (including spring and summer reservoir operations) and continues to have discussions with NOAA and other parties to resolve remaining relicensing issues. And there has been no indication from IPC that it will not be providing the 237 kaf again this year.

13. The HCC Interim Agreement defines both spring and summer operations. IPC attempts to refill Brownlee Reservoir, after being released from its flood control requirements, by late June. As the reservoir is refilled to approximately the same elevation at about the same time each year, the total volume of upper Snake River water released during the spring migration period would pass through the HCC prior to this time, and only a slight modification of delivery timing would thus be possible in most years. IPC has displayed good cooperation in attempting to protect SR fall Chinook and other anadromous species in recent years, and we have no reason to believe, nor has there ever been a previous attempt, that IPC would intentionally use Brownlee reservoir to intercept Reclamation's spring water releases. As stated previously, the HCC is currently undergoing relicensing by FERC. In the Final Environmental Impact Statement for the project (FERC 2007, NOAA AR B.121), FERC staff adopted all of NOAA's recommended spring and summer operations with the single exception of spring ramping rates (which we continue to discuss with IPC) aimed at protecting rearing fall Chinook juveniles in shallow-water habitat.[3] IPC has not disputed these operations, which include a requirement for IPC to maintain Brownlee reservoir within 1 foot of the minimum elevations necessary to meet its April 15 and April 30 flood control requirements – ensuring that the volume of water necessary to refill the project is minimized in each year. This package of reservoir management conditions are expected to be a key element of a new license for the project. During the summer period, IPC must draft Brownlee reservoir to elevation 2059 by August 7 of each year and cannot refill above

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[3] Federal Energy Regulatory Commission. August 31, 2007. Final Environmental Impact Statement for the Hells Canyon Hydroelectric Project (NOAA AR B.121).

this elevation through August 31 – eliminating any potential for intercepting Reclamation water deliveries in July and August.

14. Mr. Bowles is correct (Second Declaration at ¶ 41) that there is no longer a shaping agreement to move water delivered by Reclamation outside of the primary juvenile migration season into the season by drafting Brownlee in advance of this water's arrival. The prior shaping agreement proved difficult to administer and the volume of water provided by Reclamation outside of the juvenile migration season has been minimized. Rather than reviving the shaping agreement, Reclamation has modified its delivery timing (all water is released by August 31) and increased the volume of augmentation water available. These changes are expected to result in increased the benefits to salmon beyond those that were obtained in the previous shaping agreement.

15. Mr. Olney (Reply Declaration at ¶¶ 3 and 4) continues to rely upon a statement made in the 1995 FCRPS BiOp – that summer flow objectives (40-55 kcfs in the Snake River and 200 kcfs in the Columbia River) represent a "… low estimate of flow that is likely to avoid high mortality" to argue that NOAA's current view of these objectives must be the same as articulated in 1995. Mr. Olney also indicates that "these are the same flow objectives contained in all of the subsequent FCRPS BiOps" (including the 2008 FCRPS BiOp – RPA 4). This is incorrect. As I previously noted (Graves Declaration at ¶ 21), considerable amounts of information have been gathered during the past 13 years that has influenced NOAA's current thinking with respect to summer flow objectives and the effective use of summer water releases on the Snake River fall Chinook salmon ESU. These factors include the fact that Snake River fall Chinook salmon are now known to exhibit a decreasing propensity to migrate through July and are expressing an alternative, and successful, "stream-type" life history, and that there are

potential negative effects of increasing flows from the warmer Snake River during the late summer when Dworshak releases are being used primarily to control temperatures in the lower Snake River for rearing Chinook salmon juveniles and adult migrants in the lower Snake River.

16. With respect to spring flow objectives, Mr. Olney (Reply Declaration at ¶ 5) indicates that he is "not aware of any documentation that supports Mr. Graves' contention that NOAA has a different view about spring and summer flow objectives and he does not cite any new study of flow objectives." Again, Mr. Olney is in error. Mr. Olney does not dispute, but rather ignores the information I presented in my earlier Declaration (at ¶¶ 15, 18, and 21) as well as that provided in the 2008 SCA (Section 5.1.3, NOAA AR A.2) and in NOAA's issue summary (NOAA AR S.77) and response to comments (Issue 26: Comments on Juvenile Migration Flows, NOAA AR C.1155) documents regarding information that has led to NOAA's most recent thinking with respect to migrating SR fall Chinook salmon. With respect to spring migrating salmon and steelhead, the information gathered since 1995 has lead to more subtle changes in NOAA's thinking. For example, as I mentioned in my earlier Declaration (at ¶ 19 and Table 1) spring migrating juveniles appear to be surviving at relatively high rates under lower flow conditions, which has, over time, led to the adoption of increasingly lower flow triggers used for initiating seasonal "maximum transport" operations at the Snake River collector projects. However, unlike the case for Snake River fall Chinook salmon and the discovery of an alternative life history strategy, the information obtained for spring migrants in the intervening years has not indicated that major revisions in NOAA's position on the topic of overall flow operations is necessary or warranted.

17. Mr. Olney indicates his belief (Reply Declaration at ¶ 6) that the action agencies "are managing for the low end of the low estimate 'of the flow that is likely to avoid mortality'

on an average basis” instead of “meeting flow objectives where further increases in flow above that level no longer reduce mortality. Mr. Olney (Reply Declaration at ¶ 6) also asserts that my earlier statements (Graves Declaration at ¶ 19) regarding flow triggers for implementing maximum transportation in the Snake River during the spring period imply a belief that “fish migrating in-river will do well with even less flow.” This is not the case. My comment was intended only to demonstrate that survival rates of inriver migrants through the FCRPS during lower flow years (between roughly 65 and 85 kcfs) have been deemed high enough to justify the elimination of maximum transport operations in lower flow conditions at present (<65 kcfs) than was allowed in the 2000 FCRPS BiOp RPA (<85 kcfs).

18. However, in considering Mr. Olney’s comment (Reply Declaration at ¶ 6), I again reviewed recent (2002-2006) survival and flow information (Table 1) as well as survival and flow information from the higher flow years of 1996-1999 (Table 2). It is apparent that survival rates in the recent lower flow years (presumably due to the implementation of actions since 2000 to improve the survival of inriver migrating fish) are comparable to the survival rates observed during the higher flow years of 1996-1999. In-river survival was roughly the same in years of high flow as it was in years of low flow after the operational and structural changes were made (post-2000). This information does not support Mr. Olney’s assertions (Reply Declaration at ¶¶ 5 and 6) that the spring flow objectives represent “a low estimate of the flow that is likely to avoid high mortality.”

Table 1. Mean flows (kcfs) at Lower Granite Dam (April 10 – May 31) and mean estimated survival and standard error (s.e.) of yearling Chinook and steelhead migrating from Lower Granite to Bonneville Dam, 2002 through 2008. Hatchery and wild fish combined. Source: Flow data used to estimate average flows was downloaded from Columbia River DART on December 5, 2008; survival estimates were obtained from Tables 2 and 3 of Ferguson (2008).[4]

	YEAR						
	2002	2003	2004	2005	2006	2007	2008 Prelim.est.
Mean Flows at Lower Granite Dam (kcfs)	80.1	85.8	67.2	71.7	134.6	67.8	92.6
Est. Yearling Chinook Survival (s.e.)	0.578 (0.060)	0.532 (0.023)	0.395 (0.050)	0.577 (0.069)	0.643 (0.017)	0.597 (0.035)	0.419 (0.037)
Est. Steelhead Survival (s.e.)	0.262 (0.050)	0.309 (0.011)	___	___	0.455 (0.056)	0.364 (0.045)	0.458 (0.015)

Table 2. Mean flows (kcfs) at Lower Granite Dam (April 10 – May 31) and mean estimated survival of yearling Chinook and steelhead migrating from Lower Granite to Bonneville Dam, 1996-1998. Hatchery and wild fish combined. Source: Flow data used to estimate average flows was downloaded from Columbia River DART on December 5, 2008; survival estimates are from Table 6.2-7 of the 2000 FCRPS BiOp.

	YEAR			
	1996	1997	1998	1999
Mean Flows at Lower Granite Dam (kcfs)	127.2	155.2	111.8	107.3
Est. Yearling Chinook Survival (s.e.)	0.406	0.384	0.451	0.519
Est. Steelhead Survival (s.e.)	0.428	0.455	0.418	0.402

[4] J. Ferguson (NOAA Northwest Fisheries Science Center). September 8, 2008 memorandum for B. Suzumoto (NOAA). Subject: Preliminary survival estimates for passage during the spring migration of juvenile salmonids through Snake and Columbia River reservoirs and dams, 2008.

Spill and Transport Operations

19. Mr. Bowles (Second Declaration at ¶ 49) asserts that the 2008 FCRPS BiOp RPA depends heavily on maximizing transportation of steelhead and that the BiOp does not address the risks of transportation to the Mid-Columbia steelhead DPS. This issue is also raised by Mr. Olney (Reply Declaration at 67). I continue to disagree with these assertions. As explained in my earlier Declaration (at ¶ 24), COMPASS modeling shows that under the starting operation of maximizing transport from May 7 to May 20 (when SARs of transported steelhead are typically higher than SARs of fish that have migrated inriver to Bonneville Dam), future transport rates are expected to decrease by about 4% across the 70 year water record compared to the "Current" operation. Expected transport rates would be even lower under the proposed 2009 operations (see Reply Declaration at ¶¶ 3-6), further reducing potential impacts to Mid-Columbia steelhead.

20. Mr. Bowles (Second Declaration at ¶ 50) takes issue with my discussion (see Graves Declaration at ¶ 26) of potential longer term effects of management actions that increase the number of preferred prey available to avian predator colonies. I used, for the purpose of illustration, results from a report by Dr. Dan Roby (Roby et al. 2008) studying consumption and nesting success rates of the Crescent Island Caspian tern colony in 2007 (and comparisons to results from previous years). Mr. Bowles' response (Second Declaration at ¶¶ 50 and 51) relies upon preliminary 2008 data obtained via a November 8, 2008 e-mail from Dr. Dan Roby (that has not yet been distributed through normal regional processes) to argue that consumption rates of this tern colony (especially of steelhead) were reduced and that (contrary to my discussion about potential longer-term implications to nesting success leading to growth of the colony based on 2007 data) nesting success was also reduced in 2008. Mr. Bowles (Second Declaration at ¶ 51) indicates that the researchers (presumably referring to Ph.D. Roby or someone participating

in his research) “speculated that the higher flows in 2008 apparently reduced residualization by steelhead, and few were available to terns during chick rearing thus reducing chick survival and fledgling success.”

21. However, a cursory review of environmental data at Ice Harbor dam (data from Columbia River DART) indicates that other factors were likely responsible as well – primarily differences in temperature. Similar flows and spill rates were observed through mid-May at Ice Harbor Dam in both 2007 and 2008, except that 2007 daily average temperatures were about one degree centigrade warmer during this time than in 2008. In 2007, after mid-May, flows decreased dramatically and temperatures increased to from <14^o C to 15^o C by month’s end. In 2008, after mid-May, flows increased and temperatures decreased from 12^o C down to less than 11^o C before returning to 12^o C by month’s end. These temperatures were about 3^o C cooler in 2008 than in 2007. Thus, in the latter part of May, both reduced flows and increased temperatures were likely contributing to the increased residualization of steelhead smolts in 2007. This, on the whole, supports the larger point in my earlier discussion (see Graves Declaration at 27) “that predator-prey interactions are complex and predictions… may or may not prove accurate.”

22. Mr. Bowles (Second Declaration at ¶ 52) and Mr. Olney (Reply Declaration at ¶ 67) disagree with my statement that “the magnitude of this effect (increased inriver survival rates due to predator swamping) may be offset by the relatively large differentials observed between the post-Bonneville survival of transported vs in-river migrating steelhead” (Graves Declaration at 27) and provides several arguments to prove that this claim is “not supported by available information, nor is the basis for the claim transparent within the Biological Opinion or

supporting materials” (Bowles Second Declaration at ¶ 52). I rebut each of these arguments in the following paragraphs.

23. Mr. Bowles (Second Declaration at ¶ 53) and Mr. Olney (Reply Declaration at ¶ 65) correctly note that NOAA ultimately decided against using predictions based on recently obtained information on the seasonal patterns of post-Bonneville survival of both transported and inriver migrating steelhead in the formulation of a Base to Current adjustment for this species (see Graves Declaration at 33-38). As I previously explained, there were valid reasons for questioning whether or not these data could serve as appropriate surrogates for calculating Base period survival estimates – when inriver conditions and transportation protocols were quite different from those observed today. Mr. Bowles is also correct in asserting that had we decided it was appropriate to apply the post-Bonneville SAR relationships to the Base to Current adjustments they would have shown a substantial reduction in Snake River steelhead SARs between the Base and Current periods. However, NOAA did not do so, and, in the face of scientific disagreement regarding the suitability of the Current steelhead SAR information for use as a surrogate during the Base period, NOAA exercised its best professional judgment.

24. Mr. Olney (Reply Declaration at ¶ 65) asserts that I failed “to note that NOAA rejected the use of SARs for their Snake River steelhead base-to-current survival adjustment…” in my previous Declaration. This is not true (see Graves Declaration at 33-38).

25. Both Mr. Bowles (Second Declaration at ¶ 54) and Mr. Olney are correct in noting that the post-Bonneville survival relationships were used for estimating Base to Current SAR estimates for Snake River spring/summer Chinook salmon, whose post-Bonneville transport and inriver SARs are less sensitive (the differentials between transported and inriver SARs are less divergent). However, his insinuation that there is a real difference for the other

three ESUs for which COMPASS model runs were made is completely in error. Because Upper Columbia steelhead and spring Chinook salmon are not transported (all are migrating in-river), the differential between transport and inriver SARs that were the root of NOAA's concern for Snake River steelhead Base estimates do not exist for these ESUs. Also, no post-Bonneville SAR relationships were used for mid-Columbia River steelhead as they only migrate through 1-4 dams, which NOAA determined was a substantial enough difference from the Snake River fish that the post-Bonneville inriver SARs (these fish are not transported either) were also not applicable as surrogates. Thus, contrary to Mr. Bowles assertion, the Base to Current adjustment for Snake River steelhead was done in exactly the same manner (using system survival) as was done for mid-Columbia River steelhead (as noted in my earlier Declaration at ¶ 35).

26. Mr. Bowles (Second Declaration at ¶ 55) asserts that there is an inconsistency in my earlier Declaration, suggesting that NOAA viewed the post-Bonneville survival estimates as too flawed to be used for Snake River steelhead, but "good enough to demonstrate that the effects of predator swamping on in-river survival estimates are essentially inconsequential" (Bowles Second Declaration at ¶ 55). Mr. Bowles is again in error. NOAA is confident that the post-Bonneville survival relationships are applicable to both the Current and Prospective periods. This information is completely applicable to a discussion of whether increasing inriver survival by some amount at the present time is likely to compensate for the observed differences in post-Bonneville survival between transported and inriver migrants. Determining that this information is not appropriate as a surrogate during the Base period is a completely separate issue – which I have addressed previously in the preceding paragraph.

27. Mr. Bowles (Second Declaration at ¶ 56) disputes my statement that NOAA "recognizes that predator-prey interactions are complex and predictions based on theoretical

constructs – rather than on empirical data – may or may not prove accurate" (Graves Declaration at ¶ 27). This is simply taken out of context, as Mr. Bowles himself (Second Declaration at ¶ 52) notes that NOAA "does not disagree with me that some predator swamping is likely to occur in any given year" (source: Graves Declaration at ¶ 27). Clearly, the applicability of the empirically derived post-Bonneville survival patterns is an issue of scientific disagreement. But, as should be apparent from my responses on this general topic above, I am not persuaded by Mr. Bowles' arguments that NOAA made any fundamental errors in the use of this data in the 2008 FCRPS BiOp.

28. Mr. Olney (Reply Declaration at ¶ 66) asserts that "NOAA's analysis did not take into account the negative effect that transportation has on prey densities and predation dynamics." This statement is in error. The potential negative effect of transport – resulting from its decreasing the numbers of juvenile fish left to migrate inriver – is fully incorporated in the COMPASS results, as these were generally the conditions which provided the survival estimates used to calibrate the model. NOAA (2008 Supplemental Comprehensive Analysis – sections 5.4.1 and 8.1.1.2) has clearly indicated that predators substantially affect juvenile survival within the mainstem migration corridor and that the relative efficacy of inriver migration vs collection and transportation is affected by the status of predatory species. As was acknowledged by Mr. Bowles (Second Declaration at ¶ 52), I have also (Graves Declaration at ¶ 25) noted that predator swamping is likely to occur. The issue for NOAA, for which the ISAB report poses no remedy, is how to identify and assign a likely benefit of predator swamping (resulting from many, varying rates of transportation) to the various ESUs migrating as inriver migrants, how the resultant decreased numbers of fish surviving to below Bonneville Dam might affect the survival of these juveniles through the Columbia River and estuary downstream of Bonneville Dam, and

ultimately how would this affect the relative post-Bonneville SAR patterns of inriver and transported migrants. At present, there is insufficient information to undertake a quantitative analysis of this kind (see Zabel Reply Declaration at ¶¶ 11-17), although NOAA expects that such tools might be developed once the adults have returned from the past few years when transport rates were substantially reduced compared to previous years with similar flows. However, no such tool now exists.

29. Mr. Olney (Reply Declaration at ¶ 65) cites the recent ISAB report (2008-5, pg 3) and states: "If such [predator] compensation occurs, the relative benefit of transportation could decrease as spill percentages increase. It seems that this potential benefit of spill has not been considered in comparing alternative spill-transport scenarios." As I have noted in the previous paragraph (and in Graves Declaration at ¶ 27), NOAA does not dispute that improving passage conditions at the mainstem FCRPS projects would likely improve the inriver survival of juvenile steelhead. The fundamental scientific dispute is over whether or not the magnitude of this survival improvement would reasonably be expected to exceed the relative survival advantage observed in the post-Bonneville SARs of transported vs inriver migrating fish in mid- to late May. In contrast to Mr. Olney, NOAA does not expect that this is likely based on the information available at present. However, the proposed 2009 operations (see Reply Declaration at ¶¶ 3-6) should provide additional information on this subject.

30. Nor does the ISAB provide absolute assurance that substantial survival benefits from predator swamping will occur or are likely to continue indefinitely. Immediately prior to the quote selected by Mr. Olney (ISAB 2008-5, pg 3.), they state that "increasing spill percentage could increase the number of in-river migrants and temporarily buffer all potential prey species inhabiting the river from predation risk" (emphasis added). The ISAB quote -

"however, the magnitude of the benefits in smolt-to-adult return ratios (SARs), fish travel times, and survival rates vary substantially among species, within the migration season, and between years" (ISAB 2008-5 pg 1) - that Mr. Olney cites (Reply Declaration at ¶ 67) is an accurate description of the multiple years of post-Bonneville SAR relationships for both transported and inriver migrating yearling Chinook salmon and steelhead used in NOAA's COMPASS modeling. This statement does not refute NOAA's fundamental interpretation of the currently available data, that transport benefits tend to increase as the migration season progresses for wild and hatchery yearling Chinook salmon and steelhead (see ISAB 2008-5, Figures 1-4). NOAA agrees with the ISAB that data are currently insufficient to determine whether transportation benefits or harms Snake River sockeye.

31. I disagree with Mr. Olney's (Reply Declaration at ¶ 68) implied characterization - that past data is uncertain and predation compensation is important - of the ISAB's basis for their recommendation to "continue spill-transport operations like those in 2006 and 2007" (ISAB 2008-5, pg 3). The major points identified by the ISAB (pg 37) that appear to have most affected their recommendation to continue recent operations are 1) recent structural and operational changes look promising to improve the survival of in-river migrants (which might also positively affect their post-Bonneville SAR patterns); 2) concerns for the effects of juvenile bypass system (including screens) and transport on lamprey and juvenile sockeye; 3) concerns for the potential impacts of transported fish on other ESUs; and 4) the desire to gather more data (using multiple types of analysis) to support future decision making.

Surface Passage Routes

32. Mr. Bowles (Second Declaration at ¶ 57) disagrees with my earlier statement that surface passage structures are "proving beneficial – especially for steelhead," but then acknowledges, citing a September 15, 2008 Fish Passage Center memorandum, that "surface passage structures… show promise." This latter view is also held by the Independent Scientific Advisory Board – (2008-5, pg. 21- 25). I agree that more testing (which is ongoing) is necessary in order to maximize the effectiveness of the surface route tool, and that many complex factors must be considered when assessing the benefits of surface passage routes on juvenile salmon and steelhead. I also agree that one of the primary benefits ascribed to these structures are substantial reductions in forebay residence times (Bowles Second Declaration at ¶ 56) – which NOAA expects will substantially reduce predation rates at some projects (an additional survival benefit considered only qualitatively in the 2008 BiOp).

33. Mr. Bowles (Second Declaration at ¶ 58), correctly notes that spill patterns (which typically include specified spill volumes and spill-gate openings across a wide range of project flows) are necessary for juvenile salmon and steelhead passing through surface passage routes. However, the primary purpose of spill patterns is not to optimize attraction to the spillway and surface passage structures, but to optimize tailrace egress conditions, thus minimizing the foraging capabilities of both avian and fish predators. This also holds true for fish passing through conventional non-surface oriented spillbays. Mr. Bowles confuses spill patterns used to "train" flows with "bulk" spill – the latter being the concept of using wide spillbay gate openings to reduce the likelihood of fish encountering "edges" as they pass through spillbays. Nearly all spill patterns, under nearly all flow conditions, result in the creation of eddies – regardless of

whether or not "bulk" spill is included in the pattern. However, well designed spill patterns minimize these conditions and the potential for predation in the tailrace environment.

34. Lastly, with respect to downstream mortalities that "may have occurred during testing of TSWs at John Day Dam in 2008" (Bowles Second Declaration at ¶ 58), preliminary information presented by M. Weiland (Pacific Northwest National Laboratory) at the Corps of Engineers' Anadromous Fish Evaluation Program Annual Review (December 8-11, 2008) indicates that survival was relatively high through the juvenile bypass system, spillbays 1-14, and the TSWs in spillbays 15 and 16 (93% for subyearling Chinook salmon, 96% for yearling Chinook salmon, and 98% for steelhead). Preliminary study results also indicated that night-time survival rates were substantially higher than were day-time survival rates for juvenile migrants passing through turbine units and spillbays 17-20 – which are adjacent to a slackwater area in the tailrace caused by four empty turbine bays located near the center of the river. Thus, predators do appear to have substantially impacted juveniles using the latter passage routes at John Day Dam in 2008, underscoring the importance of developing overall project operations (including spill patterns) to minimize areas that are advantageous to predators.

35. Mr. Bowles (Second Declaration at ¶ 59) asserts that 1) surface passage structures have not been tested while spilling to the gas caps at most dams where they exist, especially Lower Granite, Ice Harbor, McNary and John Day dams, and 2) the relative benefits of surface passage structures diminish at high spill levels due to the relatively smaller proportion of flow going through the structure compared to spillways. These statements are generally true, but greatly oversimplify complex relationships. As an example, increasing the volume of spill at Lower Granite dam to the gas cap for 24 hours daily would have the undesirable consequence of reducing spill at Little Goose dam to below the current 30% level in order to comply with the

total dissolved gas waiver issued by Washington Department of Environmental Quality, thus increasing the proportion of fish passing through the powerhouse at Little Goose dam.

36. In addition, Mr. Bowles' statements (Second Declaration at ¶ 59) ignore broader regulatory requirements. In the long-term, the action agencies must achieve both ESA requirements (for salmon and steelhead) and Clean Water Act responsibilities. The Corps of Engineers coordinates with the Oregon Department of Environmental Quality (ODEQ) and Washington Department of Ecology (WDOE) to obtain the applicable total dissolved gas (TDG) waiver or criteria adjustment for fish passage spill, currently 120% as measured in the tailrace and 115% in the forebay. In addition, the TDG total Maximum Daily Load (TMDL), which was developed by the States of Oregon and Washington for the lower Columbia River includes the objective of attaining 110% TDG. NOAA views the development of surface passage routes as an important tool for achieving these longer-term objectives of improving passage conditions for inriver migrating salmon and steelhead, reducing TDG, and harmonizing both the ESA and CWA, with respect to the FCRPS mainstem dams.

Snake River Fall Chinook Salmon

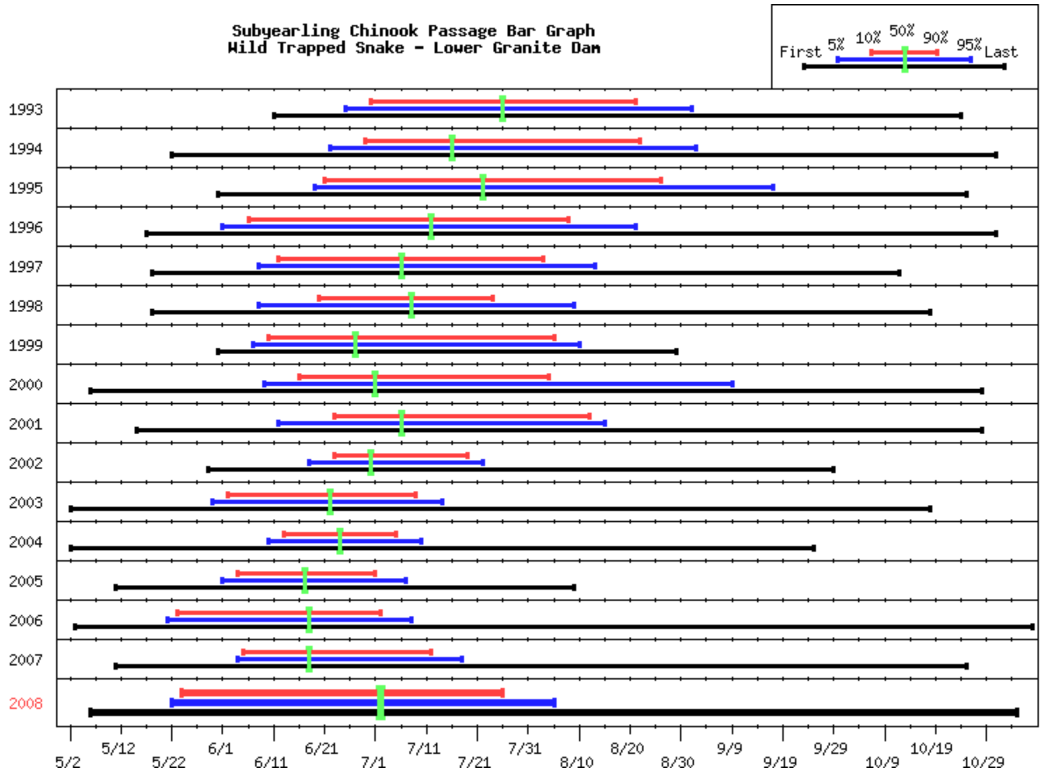
37. Mr. Bowles' (Second Declaration at ¶¶ 60-67) and Mr. Olney's (Reply Declaration at ¶¶ 58-61) general theme with respect to Snake River fall Chinook salmon is that juveniles from this ESU are supposed to migrate as subyearlings (ocean-type life history), but that because of the hydrosystem and hatchery programs, a substantial portion of the juvenile migrants are now using an alternative stream-type life history strategy which is somehow maladaptive, and therefore actions should be taken that minimize or eliminate the expression of this alternative life history so that the status of the ESU can be improved.

38. Mr. Bowles and Mr. Olney fail to consider several important factors that are directly pertinent to their theses, and that are also essential for considering the overall effect of the 2008 RPA on SR fall Chinook salmon. First, there are substantial differences between the temperature regimes of the Snake and Clearwater Rivers that underlie the distinct differences in the behavior and migration timing of naturally produced fish from these two spawning areas. Second, the SR fall Chinook salmon ESU, though consisting of a single population, is robust and increasing in abundance throughout its current range, although the average harvest rate is currently at about 40% (SCA Section 5.6.1, NOAA AR A.2). Third, to the extent that the summer spill program included in the 2008 BiOp proves beneficial to migrating ocean-type subyearling fall Chinook salmon, the productivity of the ESU should increase compared to current estimates (although NOAA did not assume any quantitative increase in productivity from Hydro improvements for fall Chinook in the 2008 BiOp - see Table 8.2.5-1). Fourth, had the 2008 BiOp spill termination trigger been used during 2008 (less than 300 fish for three consecutive days), spill would have continued through August 30 (see Graves Declaration at 31). Finally, estimated return rates of stream-type fall Chinook salmon are high relative to those of ocean-type fall Chinook salmon (December 10, 2007 Fish Passage Center memorandum – see Graves Declaration at ¶ 58). It is quite possible that actions taken to discourage the stream-type life history strategy of late-migrating juveniles (especially those from the cooler Clearwater River) could reduce the productivity of spawners in the Clearwater River and of the entire ESU. The following paragraphs respond directly to specific comments made by Mr. Bowles and Mr. Olney and elaborate on these points.

39. Both Mr. Bowles (Second Declaration at ¶ 60) and Mr. Olney (Reply Declaration at ¶ 58) clarify that their comments regarding juvenile Snake River fall Chinook migration

timing was based on more recent (post-1975) information – as opposed to observations by Mains and Smith (1964 – cited in my earlier Declaration at ¶ 52) of migration timing before the 1960s. Mr. Bowles is correct that the great majority of the juvenile fall Chinook observed in the lower Snake River during the earlier period were from the Marsing Reach of the Snake River (upstream of Brownlee Reservoir). However, any juveniles rearing upstream of the present location of Little Goose Dam (including the spawning habitat inundated by Little Goose and Lower Granite reservoirs) are included in the Mains and Smith study. The point remains that these researchers observed no evidence of juveniles migrating or rearing in the lower Snake River after June – likely because summer temperatures in the lower Snake River often increased to harmful levels in July and August (Connor et al. 2005, NOAA AR B.76). Although the precise cause is unknown, there has been a marked change in the timing of juvnile fall Chinook salmon (that incubated and reared in the Snake River proper) passing Lower Granite Dam in recent years (Figure 1) such that the current migration timing of fish reared in the Snake River (not in the Clearwater River) more closely resembles the historical migration patterns (Mains and Smith 1964). For fish that incubated and reared in the Clearwater River, median migration times vary widely from year to year (see Figure 1). This is consistent with the behavior expected of juveniles that are adopting a yearling life-history strategy – in which they are using Lower Granite reservoir and in the other Snake River reservoirs primarily as rearing areas through the summer, fall, and winter – before migrating in earnest the following spring.

40. Mr. Bowles (Second Declaration at ¶ 61) indicates that there is no propensity for fish to cease migrating in July and that the "stream-type" life history pattern can be explained by hatchery practices and hydrosystem operations. I disagree with this characterization for the following reasons. Recent tracking studies by McMichael et al. (2007) (NOAA AR B.262)



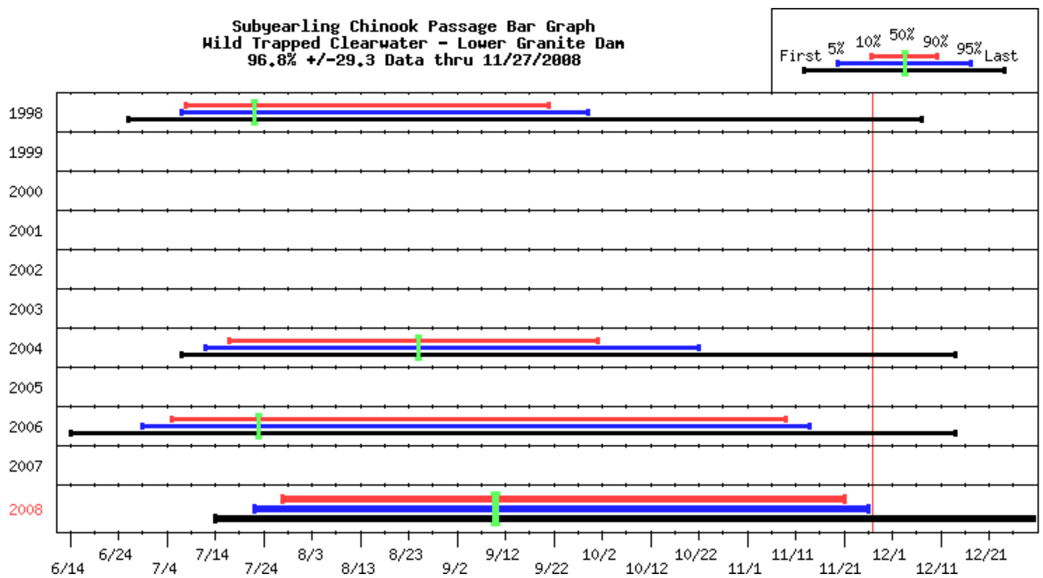


Figure 1. Passage Timing (PIT tag detections) of wild fall Chinook salmon trapped and tagged in the Snake River (1993-2008) (top) and in the Clearwater River (1998, 2004, 2006, and 2008) (bottom) and subsequently detected at Lower Granite Dam. (Note that dates differ substantially between the top and bottom graphics.)

Key: green vertical bar represents median passage date; red line = the middle 80% of the migration; blue horizontal line = the middle 90% of migration; and black horizontal line = first and last detections. (Source: Columbia River DART – data as of November 28, 2008).

clearly demonstrate that the likelihood of continued migration within Lower Monumental reservoir diminishes throughout July. There are substantial differences in the migration timing of wild juveniles from the Snake River versus Clearwater River that are better explained by differences in incubation and rearing temperatures than by effects of the FCRPS projects. Also, wild Snake River fall Chinook salmon, especially those that have reared in the Clearwater River, are successfully (See Bowles Declaration at 140) using a "stream-type" life history strategy (Connor et al, 2005, NOAA AR B.76) which is one of many life history patterns that are commonly observed in other fall Chinook salmon populations along the west coast (Good et al. 2005, NOAA AR B.155).

41. Mr. Bowles (Second Declaration at ¶ 62) and Mr. Olney (Reply Declaration at ¶¶ 58-59) assert that hatchery releases late in the migration season are responsible, at least in part, for the observed "stream-type" life history and suggests that the recently observed reduction in late season migration is due to changes in hatchery practices. This exaggerates the related statement in the Fish Passage Center's November 7, 2008 memorandum[5] (pg 2): "Beginning in 2004, these late season releases have been eliminated, which may be contributing to the observed decline in holdover prevalence in more recent years." Although hatchery practices cannot be ruled out as contributing to the observed patterns of migration behavior, their role, if any, is far from clear. For example, Figure 1 of the FPC memorandum does not support Mr. Bowles' and Mr. Olney's assertion, as it clearly indicates the estimated proportion of "holdovers" in 2007 (a recent year) was higher than estimates for 1997, 2000, and 2002 (for wild Snake River fish) and

[5] Both Mr. Bowles (Second Declaration at ¶ 62) and Mr. Olney (Reply Declaration at ¶¶58-59) rely upon the Fish Passage Center's November 7, 2008 memorandum to support their hypotheses that hatchery releases and hydropower operations are largely responsible for the well-documented "stream-type" life history exhibited predominantly by juveniles from the Clearwater River.

2001 (for wild Clearwater River fish – estimates could not be made for these fish in 1997 and 2000). Mr. Bowles' and Mr. Olney's hypothesis also fails to explain how any changes in hatchery practices could influence the behavior of naturally-produced juveniles from the Snake River proper, the vast majority of which have migrated past Lower Granite Dam prior to early August since 2002 (note that Figure 1 of this Declaration shows only naturally-produced fish). The available data has not wholly illuminated the causes of the recently observed stream-type life history strategy and several alternative hypotheses remain viable.

42. I also disagree with Mr. Bowles' assertion (Second Declaration at ¶ 62) that August spill percentage is "an important factor on the holdover rate of wild Snake and Clearwater fall Chinook." Figure 2 of the November 7, 2008 Fish Passage Center memorandum (pg 6) appears to show that the relationship between the average spill proportion in August and holdover rate for wild Snake River fish is weak (R2 = 0.25; P = 0.145) and insensitive (holdover rate is reduced from about 7% with no spill to about 2% with 50-60% average spill); though the relationship is somewhat stronger for wild Clearwater River juveniles (R2 = 0.43; P = 0.075) whose holdover rate is reduced from about 22% with no spill to 10% with 50-60% average spill. This also does not explain how lower estimated "holdover" rates were lower during a "no spill" year like 2004 than during 2007, when average spill levels were at least 50% at the Snake River projects – see Figure 1 of the 2008 Fish Passage Center memorandum (pg 5). Mr. Bowles also does not consider the potential trade-offs in terms of smolt to adult returns for actions that could affect the proportion of Clearwater River juveniles that employ either the stream-type life history strategy (which has produced relatively high SARs) or more typical "ocean-type" life history strategy (which has produced relatively low SARs – as would be expected for subyearling migrants) (Connor et al., 2005, NOAA AR B.76).

43. Mr. Bowles (Second Declaration at ¶ 63) asserts that the operation outlined in the 2008 FCRPS Biological Opinion is a reversion to pre-2005 spill operations. Mr. Bowles' statement fails to consider the relative proportions of actively migrating wild juveniles that are benefiting from spill in June and July versus August. The fact remains, which Mr. Bowles does not dispute, that there are relatively few fish passing the Snake River projects in August and that in 2008, the passage trigger would have worked as anticipated and spill would have been maintained through August 30. That is, the spill and transportation program presented in the 2008 BiOp is designed to respond in real-time to available smolt monitoring data and spill would not have been curtailed until August 30 if that program had been in place during 2008. Mr. Olney (Reply Declaration at ¶ 60) suggests that NOAA provided no analysis in support of its assertion that the 300 fish per day trigger would have provided spill through August 30. This is not true as I clearly cited Fish Passage Center data, accessed October 15, 2008 – which is publicly available information. However, to be clear, the daily collections of juvenile fall Chinook at Lower Granite Dam did not fall below 300 fish for three consecutive days before August 30. Collected fish at Lower Granite Dam (August 28-30) were: 242, 180, and 198.

44. Mr. Bowles (Second Declaration at ¶ 65) misinterprets statements in my earlier Declaration regarding the importance of fish produced naturally in the Clearwater River. Mr. Olney (Reply Declaration at ¶ 60) also argues for the importance of Clearwater River juveniles passing the Snake River projects in August. NOAA is well aware that the Clearwater fish have comprised (based on numbers of redds observed) about 28% of the ESU upstream of Lower Granite Dam (cited in my earlier Declaration)[6] and considers them to be an important

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[6] NOAA's January 24, 2006 preliminary 10(j) recommendations for the Idaho Power Company's Hells Canyon hydroelectric project (FERC No. 1971) to the Federal Energy Regulatory Commission.

component, though not a separate population, of the ESU. However, these fish would have to be actively migrating past the Snake River projects (as opposed to passing just one dam and continuing to rear in the next reservoir), in higher numbers than are typically observed in August before August spill would have a substantial effect on the overall number of adults returning to the ESU.

Snake River Sockeye Salmon

45. Mr. Olney (Reply Declaration at ¶¶ 62 and 63) asserts that NOAA did not discuss uncertainties or risks associated with transportation on the survival and recovery of Snake River sockeye salmon. This is not true. NOAA did considered all the relevant factors in reaching a decision with respect to the overall operation of the mainstem hydroelectric projects At the present time, transportation is not considered a limiting factor or threat for Snake River sockeye (SCA pg. 8.4-4 and 8.4-6). While information is nearly non-existent, the little available does not suggest that inriver migrating sockeye salmon are returning at higher rates than are transported individuals. Between 1990 and 2001, only three PIT tagged adult sockeye had returned: the transport SAR was 0.40% while the inriver SAR was only 0.03% (SCA pg 8.4-7).[7] Given the known benefit of transportation to steelhead and the paucity of information on sockeye effects, NOAA has chosen to carefully formulate the transportation program to benefit steelhead without harming other ESUs while continuing to study sockeye response to the program. However, the proposed 2009 operations (see Reply Declaration at ¶¶ 3-6) should provide some additional information for both steelhead and sockeye salmon relevant to the transportation issue.

[7] Since this time, it is my understanding that all detected juveniles are returned to the river – so no recent (including 2008) comparisons of transported versus inriver migrants are possible.

46. Mr. Olney challenges my assertion that the large Snake River sockeye salmon returns were mostly due to good ocean conditions (Olney Reply Declaration at ¶ 64), noting that I did not address the Fish Passage Center's memoranda on the subject (dated August 6, 2008), which is post-decisional. While I stand by my earlier assessment, I await NOAA's Northwest Fisheries Science Center's ongoing review of sockeye information (which includes new SAR estimates for the other Columbia basin sockeye salmon ESUs, relationships to freshwater and ocean conditions, and comparisons to Snake River sockeye salmon) to date to gain a better understanding of the factors which affect adult returns of sockeye salmon (including the Lake Wenatchee and Okanogan River sockeye salmon ESUs – which are not transported, but migrate through seven and nine mainstem dams, respectively). Mr. Olney's assertion that "similar increased returns should be evident for other sockeye populations along the west coast" (Olney Second Declaration at ¶ 64 quoting FPC's August 6, 2008 memorandum) does not persuade me that the primary cause of the notable returns in 2008 was something other than good ocean conditions. There is ample evidence that sockeye population dynamics for streams in close proximity to one another do not tend to be synchronous (Rogers and Schindler 2008),[8] so there is little reason to believe that such synchrony would be manifest throughout the West Coast.

Base to Current Survival Adjustments

47. Mr. Olney (Reply Declaration at ¶¶ 8-11) discusses the Snake River steelhead Base to Current adjustment issue (previously discussed in this Declaration at ¶¶ 23-26). The administrative record makes clear that NOAA technical staff did consider this issue as reflected

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[8] Rogers, L.A. and D.E. Schindler. 2008. Asynchrony in population dynamics of sockeye salmon in southwest Alaska. Oikos 117:1578-1586.

in the relevant tables (SCA – Hydro Modeling Appendix, Base to Current to Prospective Hydro Survival improvements for Snake River steelhead) that the Scheurell-Zabel hypothesis was NOT used in the calculation of a Base to Current adjustment for steelhead – in clear contrast to the calculation for yearling Chinook salmon (which is provided on the same page). In addition, as I previously indicated (Graves Reply Declaration at ¶ 25), the methodology used for Snake River steelhead is consistent with that used for mid-Columbia River steelhead, although – as Mr. Olney points out (Olney Reply Declaration at ¶ 22) – the reasons for this were different for each species.

48. In his Reply Declaration (at ¶ 12) Mr. Olney again states that had NOAA calculated the Base to Current Snake River steelhead adjustment using the post-Bonneville survival relationship, a negative impact of 10.2% (instead of -3.4%) would have resulted which would have affected the quantitative analysis metrics used in the 2008 FCRPS BiOp for this species. As explained previously, NOAA had sound technical reasons for rejecting the use of the post-Bonneville survival relationships as a surrogate for the Base period in its Base to Current adjustment calculation for Snake River steelhead (see Graves Reply Declaration at ¶¶ 23 and 25, above).

49. Mr. Olney reiterates the differences in NOAA's estimation of the Base to Current verses Current to Prospective hydro adjustments for Snake River steelhead (Olney Reply Declaration at ¶ 13) and reasserts that NOAA did not adequately explain the "apparently inconsistent treatment of the data." Again, NOAA determined that the post-Bonneville SAR relationships were likely poor surrogates for the Base period (Graves Reply Declaration at ¶¶ 23 and 25, and Graves Declaration at ¶¶ 33-37). Obviously there is a matter of scientific

disagreement regarding the use of the post-Bonneville SAR relationships as surrogates for Snake River steelhead during the Base period calling for NOAA's best professional judgment.

50. Mr. Olney (Reply Declaration at ¶ 15) suggests that because NOAA "did not explain its shifting view of the data, there has been no opportunity for anyone to assess the rationale Mr. Graves has now provided for changing the analysis of the base-to-current hydro survival adjustment for Snake River steelhead, or assess whether NOAA's inconsistent use of data in different analyses for this DPS is scientifically warranted." I have described NOAA's reasons for determining that the post-Bonneville SAR relationships were likely not good surrogates for the Base period, but are representative for the Current and Prospective periods (Reply Declaration at ¶¶ 23 and 25, and Graves Declaration at ¶¶ 33-37). Further, Mr. Olney and Mr. Bowles' were clearly able to identify the difference in methodology and calculate the numerical value that would result if NOAA had reached a different decision with regard to this issue. Again, it is obvious that there is disagreement on this point, but NOAA remains confident that it made the correct decision with respect to estimating the proper Base to Current hydro adjustment for Snake River steelhead.

Avian Predation

51. Mr. Olney again points out the discrepancy between two sections of the 2008 FCRPS BiOp with respect to a downward adjustment (resulting from compensatory mortality) of the estimated benefits of efforts to control avian predation (Olney Reply Declaration at ¶ 16). Mr. Olney repeatedly notes that failure to include this reduction "means that bird predation benefits are 50% higher than they should be" (Olney Reply Declaration at ¶ 17). Mr. Olney then insists that "this is the case regardless of why NOAA ultimately chose not to make the

adjustments they assumed they would in their methods section" and shows the negative reductions (totals ranging from 1.0% for spring/summer Chinook salmon, up to 1.7% for steelhead (Olney Reply Declaration at ¶ 18). I disagree with Mr. Olney's assertion. As I stated in my previous Declaration (at ¶ 45), there is a discrepancy between two sections of the 2008 FCRPS BiOp – but NOAA clearly chose to not apply the theoretical adjustment suggested by Roby et al., 2003.

52. Using Snake River steelhead as an example, Mr. Olney (Reply Declaration at ¶ 19) asserts that NOAA used several smaller adjustments (both positive and negative) in its quantitative analysis, therefore a reduction of 1.7% must be deemed "significant" by NOAA and should not have been omitted. I disagree with Mr. Olney's assessment. Mr. Olney goes on to show how NOAA's acceptance of this negative adjustment would have affected NOAA's mean recruit per spawner analysis had it been used (Olney Reply Declaration at ¶¶ 20-21). The issue before NOAA was whether or not to accept Mr. Roby's theoretical 50% adjustment. While NOAA obviously considered the use of such an adjustment (2008 FCRPS Biop at 7-48), it ultimately chose not to adopt Mr. Roby's hypothesis in its analysis (2008 FCRPS Biop at 8.3.5.6, which is referred to in other applicable species specific sections). The acceptance of Mr. Roby's theoretical 50% adjustment to represent compensatory mortality into NOAA's quantitative analysis is certainly a point of scientific disagreement.

53. With respect to predation in the estuary by double-crested cormorants, Mr. Olney (Reply Declaration at ¶ 26) disagrees that "double-crested cormorant populations appear to have stabilized at around 12,400 breeding pairs in the estuary between 2003 and 2006" (Fredricks 2008)" instead asserting that the population increased by about 27% between 2003 and 2006. Mr. Olney cites an e-mail from Gary Fredricks to me (Olney Reply Declaration - attachment 2) as

well as Roby et al. (2008) to support this assertion. Based on the information from Fredricks (2008 – see Graves Declaration, Exhibit 2, Table 4), the relative difference between 2002-2003 estimates (average of 10,563) and 2004-2005 estimates (average of 12,384) was 17% (1,821/12,384) and the relative difference between 2004-2005 and 2006-2007 estimates (average of 13,793) is about 11% (1,409/12,384). Viewed this way, it is correct to assert that the population may be growing, but the rate of growth appears to be slowing (both in absolute and relative terms) over the past 6 years. Also, Roby et al. (2008) similarly notes that there was little growth in the double-crested cormorant colony on East Sand Island between 2006 and 2007 (13,740 vs 13,770 breeding pairs). Lastly, Mr. Olney mischaracterized (Reply Declaration at ¶ 26) the content of Mr. Fredricks e-mail (Olney Reply Declaration, Exhibit 2) which goes on to indicate that "new and growing colonies occur in near the mouth of the Snake River, up the Snake River near Lewiston, the Yakima River and in the Potholes and Moses Lake areas." The fast growing colonies that Mr. Fredricks is primarily referring to in this e-mail are those located in the interior Columbia River basin.

54. Mr. Olney (Reply Declaration at ¶ 27) appears to misunderstand my earlier comment (Graves Declaration at ¶ 46) "because no further quantitative benefit was assigned in the Current–to-Prospecitve adjustments, the analysis carries forward the current predation rates for the term of the BiOp." With respect to a Current to Prospective adjustment (the latter being after the 2008 FCRPS BiOp RPA actions are fully implemented), NOAA made no adjustment for implementing RPA 46, meaning that NOAA assumes this measure will at least be effective enough to prevent cormorants in the estuary from increasing their consumption of juveniles salmonids, beyond those rates currently being observed – a net Current to Prospective adjustment of zero. Mr. Only suggests that cormorant predation rates could continue to increase indefinitely

into the future. With the full implementation of RPA 46 and any additional measures that might arise as a result of the adaptive management process, such an occurrence is not anticipated.

55. Mr. Olney (Reply Declaration at ¶ 28) suggests that "the RPA, and in turn NOAA's analysis, thus appears to rely on a plan for a future plan to ensure that the cormorant colony does not increase its fish predation rates in the future." Mr. Olney mischaracterizes the objective of RPA 46. As clearly indicated in the 2008 FCRPS BiOp RPA Table, the objective of Predation Management Strategy 2 (RPA Table, pg. 65) is to "implement avian predation control measures to increase survival of juvenile salmonids in the lower Snake and Columbia Rivers." RPA 46 directs the FCRPS Action Agencies to develop a cormorant management plan and implement warranted actions in the estuary to achieve this objective.

56. Finally, Mr. Olney is inconsistent in his application of compensatory mortality. He does not argue that the negative impacts resulting from cormorant predation should be reduced to reflect compensatory mortality (Olney Reply Declaration at ¶¶ 23-26) - despite arguing that a reduction in the much smaller positive effects of reducing tern predation is warranted (Olney Reply Declaration at ¶¶ 16-20).

Kelt Reconditioning

57. Mr. Olney (Reply Declaration at ¶ 29) asserts that kelts cannot benefit from spill-related improvements in passage survival, such as RSW or TSW, during periods of no spill. This is true for the three collector dams, but not for the other five mainstem FCRPS dams (where spill will continue throughout the spring migration period). However, during periods of no spill at the three Snake River collector dams, higher proportions of kelts would enter the bypass system and become available for collection and inclusion in the rehabilitation project. The return rate of in-

river migrating kelts used in the analysis was .165%. The success rate of long-term rehabilitation used in the analysis was 35.7%. Even if this was modified by an estimated spawning success rate of 50%, and only 50% of the kelts collected were suitable for rehabilitation, a female B-run steelhead would be 54 times more likely to return to the spawning grounds through rehabilitation than through in-river migration and return. This improvement would easily compensate for the 10% reduction in dam passage survival during the two week long no-spill period described as the initial operation in the 2008 FCRPS BiOp RPA. The proposed 2009 operations (see Reply Declaration at ¶¶ 3-6) should provide increase the survival of migrating kelts and also provide some additional information regarding the number and condition of kelts available at the collector projects for use in the kelt reconditioning program under these operational conditions.

58. Mr. Olney (Reply Declaration at ¶ 30) asserts that NOAA has failed to display its assumption regarding the proportion of "kelts collected at Lower Granite and Little Goose dams that would be suitable for reconditioning because they did not take culling of poor condition fish into account." As Mr. Olney notes, there was no estimate of the number of fish that would be considered to be unsuitable for inclusion in a kelt rehabilitation program in the 2008 FCRPS BiOp analysis (SCA, Kelt Reconditioning Plan). Since it is not clear precisely what proportion of kelts would be considered good or fair, the number of fish available to the rehabilitation program learned during implementation will inform adaptive management to achieve expectations.

59. Mr. Olney (Reply Declaration at ¶ 30) is correct that assuming his speculation of a rejection rate of 20%, NOAA's point estimate would be reduced from 6% to 4.8% and the range of potential benefits would be reduced from 4.45-8.9% to 3.56-7.12% based on an assumption of 50% to 100% spawning effectiveness. NOAA did consider a full range of assumed spawning effectiveness, not just 50% as Mr. Olney asserts (Reply Declaration at ¶ 30-

35). A mistake in my earlier Declaration (at ¶ 42) (not in the 2008 Supplemental Comprehensive Analysis of FCRPS BiOp) led to Mr. Olney's misunderstanding (Reply Declaration at ¶ 30-35) that NOAA used only long term reconditioning with an assumed 50% viability rate to represent the maximum potential benefit that might be achieved from long term kelt reconditioning. Instead NOAA "recognized this by cutting the assumed success rate of the long-term reconditioned kelts by 50%" my earlier Declaration (at ¶ 42) should have read NOAA "recognized this by considering that the assumed success rate of the long-term reconditioned kelts might be 50%." This inadvertent mistake affected much of Mr. Olney's subsequent discussion relating to the kelt reconditioning plan, but does not call into question NOAA's analysis in the SCA Kelt Reconditioning Plan.

60. Mr. Olney (Reply Declaration at ¶ 31) was correct in his understanding that NOAA's 6% point estimate was based on a range of effectiveness of reconditioned kelts spawning in the wild of 50-100% (see discussion in previous paragraph). He could find no information in the SCA Steelhead Kelt Appendix or 2008 FCRPS BiOp specifying that NOAA had only used the low end estimate (50% effectiveness) because NOAA did not make this assumption, which also resolves the apparent inconsistency with my comment noting that a 6% survival improvement still falls within the adjusted range of calculated by Mr. Olney (3.56-7.12%) (Graves Declaration at ¶ 41).

61. Because of the mistake in my earlier Declaration (as noted above), Mr. Olney's assertion that NOAA had to have assumed a substantial improvement in in-river survival in order to find that a 6% benefit was reasonable (Reply Declaration at ¶ 32) is erroneous, and renders moot the technical points and expressed concerns made later in his Declaration (at ¶¶ 33-35).

62. Mr. Olney asserts (Reply Declaration at ¶ 36) that NOAA omits "important and relevant factors [raised by Mr. Olney at 33-35] from its analysis which, if addressed, would lead to a much lower survival benefit estimate" (Reply Declaration at ¶ 36) for the kelt reconditioning program. Again, the mistake in my earlier Declaration (at ¶ 42) contributed to Mr. Olney's misunderstanding with respect to the factors he discusses in his Reply Declaration (at ¶¶ 33-35). Based on the analysis provided in the SCA Kelt Reconditioning Plan, there remains a realistic potential for achieving the 6% survival goal.

Additional Hydro Actions

63. Mr. Bowles (Second Declaration at ¶ 86) lists Phase II actions "that can be taken in the hydro system to improve smolt-to-adult survival" and potential actions identified by NOAA staff (Attachment to an email from Gary Fredricks to R. Graves dated October 3, 2007) to benefit juvenile and adult migrants through the mainstem hydro projects. Mr. Bowles asserts that NOAA should have explained why it "disregards these actions in favor of other non-hydro based improvements in the Biological Opinion." The Phase II items that Mr. Bowles has extracted from the Action Agencies Biological Assessment (NOAA AR B.89) are intended to provide assurances in the event that the identified Phase I actions fail to achieve the expected benefits. Many of these items (e.g., tailrace divider walls, tailrace bathymetry modifications, installation of extended length guidance screens, powerhouse surface flow outlet systems) are extremely challenging undertakings that would require substantial regional discussion before implementation could begin. For example, I would be surprised if Mr. Bowles would support the installation of extended length guidance screens at Lower Monumental and Ice Harbor dams simply because it would likely improve survival and was identified as a Phase II action because

these actions would increase the proportion of fish that travel through the juvenile bypass systems and are transported. With respect to the NOAA staff document (attachment to email from G. Fredricks to R. Graves dated October 3, 2007 cited in Bowles Second Declaration at ¶ 86), it is my understanding, based on conversations with my staff that, contrary to Mr. Bowles assertion, each of the items in this list [with the sole exception of improvements to the adult trap and Bradford Island ladder system at Bonneville Dam] is forward within or is being investigated by the Fish Facility Design and Review Work Group or by the Operations and Management Team. In short, NOAA has not disregarded these actions in favor of other non-hydro improvements.

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

Ritchie J. Graves

Ritchie J. Graves