KARIN J. IMMERGUT, OSB #96314
United States Attorney
STEPHEN J. ODELL, OSB #90353
Assistant United States Attorney
District of Oregon
600 United States Courthouse
1000 S.W. Third Avenue
Portland, OR 97204-2902
(503) 727-1000

RONALD J. TENPAS
Assistant Attorney General
SETH M. BARSKY, Assistant Section Chief
COBY HOWELL, Trial Attorney
BRIDGET McNEIL, Trial Attorney
MICHAEL R. EITEL, Trial Attorney
CYNTHIA J. MORRIS, Trial Attorney
Wildlife & Marine Resources Section
U.S. Department of Justice
Environment & Natural Resources Division
c/o U.S. Attorney's Office
1000 SW Third Avenue
Portland, OR 97204-2902
(503) 727-1023
(503) 727-1117 (fx)

Attorneys for Federal Defendants

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 RICHARD W. ZABEL, Ph.D.

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

1. On October 23, 2008, I provided a declaration in support of the NOAA Fisheries's 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 the Comprehensive Passage (COMPASS) model employed in the development of NOAA Fisheries' BiOp. 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 a second round of reply declarations filed by Mr. Olney and Mr. Bowles and now provide this declaration to respond to further comments and criticisms raised in their reply declarations.

Overall Comments

3. Both reply declarations primarily reiterate the points made in the original declarations and provide little new material. Their declarations attempt to discredit the COMPASS model by focusing on the few instances where the model results are not validated by actual observations of fish survival, the few critical comments made by the ISAB in their review that was generally supportive, or minor areas of scientific debate. The bigger picture is that COMPASS is clearly the best available model upon which to base management decisions for the FCRPS. The model is supported by an extensive data set, and generally fits those data well. It represents the efforts of many scientists from state, tribal, and federal agencies collaborating over a two-year period. It has been thoroughly reviewed by independent scientists to an unprecedented degree, and these scientists have provided a generally favorable response (eg

ISAB 2008)[1]. It is supported by an unprecedented amount of documentation and model diagnostics (see COMPASS Manual, NOAA AR B.367). It has successfully undergone the peer-review process and is published in a scientific journal. (See citation in footnote 9 of my earlier declaration.)

4. I agree that COMPASS has aspects that can be improved (as is true of all models), most of which are where data are limited. The COMPASS team has acknowledged these limitations and has identified areas where more and better data are needed. Further, there are still a few cases where the model does not fit the data, but the overall model fit is vastly superior to any predecessor model, and the model diagnostics have been displayed and scrutinized to a much greater extent than any other model of hydrosystem survival. One of the roles played by models is to identify data gaps and knowledge limitations. We have identified data limitations, such as imprecise survival estimates in the lower Columbia River, and brought these data limitations to the attention of the region's scientists and system managers. We also agree that under conditions of extremely low flow, we can improve model fit and have identified a currently unmodeled source of fish mortality (bird predation) that has led to discrepancies between model predictions and data. We continue to improve the model. In fact, several dozen scientists are currently working to update the model to expand the model to the Upper Columbia River, improve representation of model uncertainty, and improve the user's interface such that the model will be more accessible for users. If new model developments lead to a modified understanding of the response of salmon populations to hydrosystem operations, this information will be used in future management.

---

[1] ISAB (Independent Scientific Advisory Board). 2008. Review of the Comprehensive Passage (COMPASS) model – version 1.1. ISAB, Report 2008-3, Portland, Oregon, 6/2/2008.

5. The primary role of COMPASS modeling for the BiOp was to estimate the relative response (expressed as percentage changes in adult return rates) of salmon ESUs to alternative hydrosystem operations. These responses were determined using a 70-year water record of observations that represents a broad range of actual conditions in the lower Snake and Columbia Rivers. I believe these estimates are robust, and slight discrepancies under particular river conditions experienced in particular river segments have little effect on the overall results of the model.

6. Although Mr. Bowles and Mr. Olney provide criticisms of COMPASS, they offer little suggestion of alternative methods for assessing alternative scenarios of hydrosystem operation. I strongly believe that a well-constructed model is the best tool to assess the alternative operations of the complex FCRPS. The alternative expert judgment approach, including that referred to as the "weight of evidence approach" by Mr. Bowles, is fraught with subjectivity. In particular, the opinions expressed by Mr. Bowles and Mr. Olney have not received nearly the level of scientific scrutiny as the COMPASS model.

Reply to the Bowles Declaration

7. In the following paragraphs, I respond to sections of the declaration of Mr. Bowles regarding the COMPASS model (paragraphs 68-72), much of which consists of restatements from his previous declaration. Mr. Bowles raises three main points: 1) PIT-tag survival estimates in the lower Columbia River have poor precision; 2) COMPASS over-predicted steelhead survival in the Snake River in 2001 and under-predicted steelhead survival in the Snake River in 2007; and 3) Non-detected fish are the only appropriate population to represent in-river fish for estimating adult return rates.

Pit-tag survival estimates in the lower Columbia River

8. In paragraphs 69-70, Mr. Bowles states that the precision of PIT-tag survival estimates in the lower Columbia River are poor. He indicates that because of this, COMPASS has limited utility as a management tool. In particular, this issue concerns the ability of the model to predict the response of survival of juvenile salmon and steelhead to varying river conditions while migrating through the reservoirs between the dams of the lower Columbia River, from McNary Dam to Bonneville Dam. The data underlying the survival relationships have limited samples sizes, and thus the confidence intervals about the PIT-tag survival estimates are broad, which limits the ability of the model to capture signals.

9. As I stated previously, I agree that the available data for this component of the model are limited. In fact, the COMPASS modeling team was the first to identify this issue, and this issue would confront any effort to model survival through this region. The quality of retrospective, previously collected data cannot be improved afterwards. Identification of this issue should lead to improved data quality in the future. Indeed, we plan to increase sample sizes in the coming years, and efforts are underway to improve PIT-tag detection rates. Both of these factors will lead to more reliable survival estimates in the future.

10. Nevertheless, the COMPASS results are still reliable for management decisions. Reservoir survival in the lower Columbia River is but one component in one region of the larger COMPASS model. Mr. Bowles fails to mention that the other components of COMPASS in this region – travel time, dam passage routing and survival, and hydrological modeling – are quite strong and help to compensate for data limitations about reservoir survival. Because of this, overall adult salmon return rate model results reasonably reflect the effects of alternative management actions in the Columbia River, such as increased spill, because the model’s dam

passage survival and estuary arrival timing components compensate for any imprecision in reservoir survival predictions. Regarding the quality of reservoir survival models in the lower Columbia River, we have adopted a conservative approach and consequently have used simple models that are still responsive to spill, flow, and travel time. These are the best available data, and we have constructed the best model possible.

COMPASS predictions for steelhead in the Snake River in 2001 and 2007

11. In paragraphs 71-75, Mr. Bowles once again raises the point that for steelhead migrating through the Snake River, COMPASS under-predicted survival in 2007 and over-predicted survival in 2001. Mr. Bowles indicates that these discrepancies are entirely due to COMPASS' inability to capture the full benefits of spill under low flow conditions. I disagree with Mr. Bowles' opinion, and I provide my reasoning below.

12. I do not, as Mr. Bowles implies, think these discrepancies are just random aberrations. Conditions the fish experienced in these years were unique, and models cannot always capture all fluctuations in natural systems. Nevertheless, as borne out in independent peer review, the COMPASS model is sufficiently accurate for its intended application.

13. In this case, there is strong evidence that survival from Lower Monumental to McNary Dam is related to bird predation rate, which was high in 2001 and low in 2007. I agree with Mr. Bowles that the density of smolts in the river plays a role, but I did not mention this in my previous declaration because the relationship between smolt density and bird predation rates presented in Faulkner et al. (2008, Exhibit 3 of my previous declaration) was not statistically significant, indicating that other factors (see below) were likely important. Mr. Bowles statement that bird predation "decreases dramatically" as density increases is unsupported by the

available science and therefore overstates the weight management should place on this relationship.

14. I believe the dynamics in these river segments does not lead to a simple relationship between smolt survival and smolt density. For instance, the abundance of birds in 2001 was more than twice as large as it was in 2007 (Faulkner et al. 2008), and the estimated bird consumption of steelhead also increased in 2001. Due to the lack of supporting data, the COMPASS model does not currently contain a function to model lower fish survival at higher bird predator densities, just as it does not contain a function for higher fish survival simply because smolt density increases. As a result, COMPASS' overestimate of survival in 2001,when more birds were feeding than in average years, and underestimate in 2007, when fewer than normal numbers of birds were feeding, is consistent with the hypothesis that the number of birds feeding on migrating smolts, in addition to smolt density, will affect smolt survival rates in this segment of the river. This is an appropriate issue for future refinements in the modeling of hydrosystem actions as our understanding of these mechanisms improves with better data and studies.

15. From a management standpoint, Mr. Bowles prescription of increasing smolt density to improve in-river survival may yield no net benefit in the return rate of adult fish if the *total* number of smolts taken by bird colonies is not determined by smolt density but rather by bird abundance (Faulkner et al. 2008) as preliminary indications from the evidence suggest.

16. Mr. Bowles states that COMPASS already accounts for all the necessary components that lead to changes in smolt density and consequently should capture changes in predation rate. He asserts that this "argues against Dr. Zabel's suggestion of high predation rates on PIT-tagged steelhead smolts in 2001 and low rates in 2007 as likely explanations of the poor

agreement between observed and COMPASS predicted survival rates in the Lower Granite Dam to McNary Dam reach." This argument is unreasonable for several reasons. First, the density of fish in the river is strongly influenced by the abundance of smolts arriving at Lower Granite Dam (the first dam the Snake River fish encounter) in any given year. Further, although COMPASS does relate survival to spill, it does not relate in-river survival to smolt density, which is determined, to a certain extent, by the proportion fish transported. Furthermore, as mentioned above, predator densities fluctuate from year to year, further complicating the predator-prey dynamics.

17. As I mentioned in my previous declaration, the COMPASS modeling team will address this issue in the near future. We plan to modify the model by first incorporating smolt density, and then examine for responses of PIT-tag survival estimates to smolt density.

Bypassed versus non-bypassed fish

18. In paragraphs 76-80, Mr. Bowles addresses the issue of whether, when modeling the smolt-to-adult survival rate, the survival rate of only non-bypassed, PIT-tagged fish should be used to represent the survival rate of all fish that are not transported but instead migrate in-river. Non-bypassed fish are non-transported, PIT-tagged fish that pass all transportation collection dams (Lower Granite, Little Goose, and Lower Monumental) without going through a bypass system where they would be detected by a PIT-tag detector. Instead, they pass these dams undetected through a spillway, surface collector or turbine. This is an important issue because previous research has demonstrated that non-bypassed smolts return as adults at greater rates than those PIT-tagged smolts that were detected going through 1-3 bypass systems (see Williams et al. 2005; NOAA AR B.538)[2]. Consequently, the choice of which population of PIT-

---

[2] Williams, J.G., S.G. Smith, R.W. Zabel, et al. 2005. Effects of the Federal Columbia River Power System on

tagged fish (non-bypassed versus all PIT-tagged fish) used to represent the in-river migrants can have strong implications on their performance relative to transported fish.

19. However, controversy exists as to the mechanisms (causes of mortality) that lead to this differential return rate. One hypothesis (first published in Budy et al. 2002; NOAA AR B.52), suggests that bypass systems at the dams stress fish, but the mortality suffered because of this stress does not occur until after the fish leave the hydrosystem. An alternative hypothesis, proposed by Zabel et al. (2005, see Exhibit 4 of my previous declaration) and Williams et al. (2005), is that smaller fish tend to pass through bypass systems while larger fish tend to pass the dams undetected through other routes (as demonstrated in Zabel et al. (2005)). Smaller fish return at lower rates than larger ones (as demonstrated by Zabel and Williams (2002), see exhibit 5 of my previous declaration). Another possible mechanism that can lead to the observations is that the majority of non-detected fish pass through the spillway, and spilled fish pass dams substantially more quickly than fish that pass through bypass systems. Thus, non-detected fish arrive to the estuary up to several days earlier than multiply-bypassed ones, when environmental conditions there are more favorable, and thus are expected to return at greater rates based on the relationships described in Scheuerell and Zabel (2006; NOAA AR B. 455). COMPASS already contains this effect because fish that pass through spillways have decreased travel times and arrive to the estuary earlier in the season. These earlier arriving fish then survive to adult return at greater rates than fish that passed via bypass systems.

20. In contrast to Mr. Bowles' unequivocal support for the first hypothesis (that bypass systems harm fish), I believe all three possible mechanisms may be operating to a certain degree, and a great deal of uncertainty exists over the relative contributions of each. In fact, a

---

salmonid populations, U. S. Dept. of Commerce, NOAA Tech. Memo., NMFS-NWFSC-63., 2/1/2005.

great amount of evidence exists to support the notion that the first hypothesis is not the only mechanism. Therefore, it is my opinion that using the survival rate for all PIT-tagged, in-river fish, whether by-passed or not, to represent the survival rate for all in-river fish, as the COMPASS model runs for the BiOp did, is likely to be closest to the actual survival rate.

21. Mr. Bowles (and Mr. Olney) appears to accept that bypassed fish should be included to represent in-river migrating fish at certain times of the season when no transportation is occurring. The issue is therefore narrowed to how to represent in-river migrating fish when a large proportion of the PIT-tagged fish is either bypassed and returned to the river or collected on barges for transportation.

22. In his declaration, Mr. Bowles attacks the size-selectivity hypothesis in an effort to bolster his argument that the bypass-induced stress hypothesis is the only viable one. He states that the CSS annual report for 2006 (Berggren et al. 2006) “strongly refutes” the hypothesis that detection of juveniles in bypass systems is related to their size. Mr. Bowles’ characterization of the CSS report is misleading and incorrect. In the next several paragraphs, I present details of the CSS report cited by Mr. Bowles and demonstrate how it did not “strongly refute” the size selectivity hypothesis.

23. The CSS team adopted the same methodology that was used by Zabel et al. (2005) for the first part of their analysis where they examined size-selectivity relationships in bypass systems. This simplifies the comparison of results. However, the first part of their analysis departed in an important way from the Zabel et al. (2005) analysis: the CSS team analyzed fish tagged and measured at Snake and Clearwater traps above Lower Granite Dam, while Zabel et al. (2005) analyzed fish tagged and measured at Lower Granite Dam. This allowed the CSS study to analyze size selectivity of fish entering the bypass system at Lower

Granite Dam, whereas the fish studied by the Zabel et al. (2005) team were all initially collected in the bypass system at Lower Granite and then studied for passage route preference at downriver dams. The CSS sample size, however, was greatly reduced compared to Zabel et al. (2005). The CSS researchers acknowledged that this limited their ability to analyze relationships at the downstream sites – Little Goose and Lower Monumental Dams where the Zabel et al. (2005) results were observed. Nonetheless, they observed a very similar level of size selectivity at Little Goose Dam to that observed by Zabel et al. (2005), confirming the Zabel et al. (2005) results. At Lower Monumental Dam, Zabel et al. (2005) observed size selectivity, but the CSS report did not. However, as mentioned above, the CSS authors had little faith in this part of their analysis due to limited sample sizes.

24. In the second part of their analysis, the CSS team compared the mean lengths of detected versus undectected fish at Lower Granite, Little Goose, and Lower Monumental Dams. This analysis is seriously flawed, because they were unable to address the size-selective mortality in the reservoir survival, a limitation they acknowledged. Similar to the effects demonstrated by Zabel et al. (2005), this can create strong biases. Further, the "undetected" population contains any fish that did not survive from release to the detection site, further introducing bias. Thus, this part of their analysis is invalid, and Bowles reference (in paragraph 80) to this part of the study is not valid.

25. I provide these details to make the point that the CSS report did not "strongly refute" the bypass-size relationship, but also to question Mr. Bowles capacity to represent the viability of the alternative hypotheses. The following statements, taken directly from the CSS report, demonstrate that the CSS authors themselves did not believe they "strongly refuted" the Zabel et al. (2005) study: "However, given the limited amount of data available for estimation

below LGR, we most emphasize our LGR findings." "Thus, overall there was no strong evidence for a consistent size-related bias in detection probability for our primary site (LGR), but some evidence for an effect at LGS." "Estimates of other years … [were] of similar magnitude to those reported for LGS and LMN by NOAA…" In sum, this analysis does not "strongly refute" the size-selectivity hypothesis, nor does it refute it at all.

26. I agree that it is important to consider whether the length selectivity is biologically meaningful. To demonstrate that the magnitude of length selectivity is biologically meaningful, I conducted an analysis based on two published studies – Zabel and Williams (2002) and Zabel et al. (2005). I applied the relationships (both for survival and detection probabilities) in Zabel et al. (2005) to estimate size differences between non-detected fish and those detected at both Little Goose and Lower Monumental Dams. For Snake River spring/summer Chinook, non-detected fish were approximately 0.8 mm larger than twice-detected fish, which was a highly significant difference (t-test, $P < 0.01$). I then applied the size-dependent survival differential for wild in-river Chinook published in Zabel and Williams (2002), assuming a linear relationship and a SAR of 1%. The differential SAR between non-detected and twice detected fish was approximately 7.5%. This magnitude of difference is certainly biologically relevant. This analysis is repeatable as it is based on published values and indicates that this is an important factor.

27. In sum, I believe that the entire population of PIT-tagged fish, including both bypassed and non-bypassed components, best represents the in-river population when estimating adult return rates. This is because of the following: 1) As acknowledged by Mr. Bowles and Mr. Olney, during times of no transportation (which represents 25-32% of the migration period under proposed operations, according to paragraph 75 of the Olney declaration), the entire PIT-tag

population is identical in behavior to the untagged in-river population; 2) It is likely that the size selectivity effect explains some of the differential return rates, and management actions will not alter this effect; and 3) The differential estuary arrival timing of spilled versus bypassed fish can account for some of the differential return rates, an effect that is already contained in COMPASS. I will not rule out that possibility that bypass-stress hypothesis operates to a certain degree, and we should continue to conduct research to examine this effect. However, given the points I just made, using only non-bypassed fish to represent in-river fish would unjustifiably exaggerate the magnitude of that hypothesis, thereby skewing COMPASS results to favor in-river migration.

Reply to Olney

28. Mr. Olney provides little new material in his most recent declaration. Mr. Olney generally lacks the qualifications to provide expert opinion about models such as COMPASS. He has apparently never authored any recent modeling documentation (since at least before the beginning of the PATH model in 1996) nor added to peer reviewed literature in the field of modeling salmon population dynamics.

29. The fact that he cites an ISAB review from 2001 (paragraph 78) further demonstrates that he is out of touch with recent modeling. His implication is that all models are similar, and that the state of data available has changed little since then. On the contrary, COMPASS reflects a concerted model development effort over a 2+ year period by dozens of scientists. Available data has greatly increased, with approximately 8 more years of PIT-tag survival data and dozens of radio telemetry studies at the face of dams. Further, COMPASS has undergone extensive review by the ISAB and anonymous peer reviewers. In light of this, the 2001 ISAB review is not relevant to the current discussion.

30. Mr. Olney begins his declaration (paragraph 69) by restating several paragraphs from his previous declaration regarding use of COMPASS in the BiOp analyses, and claims I did not address these comments. This is inaccurate, as I did address these comments extensively in paragraphs 21-22 in my previous declaration. Mr. Olney goes on (paragraph 71) to quote my previous declaration where I state, "differences in avian predation among years can easily explain the discrepancy between predictions and observations." Mr. Olney then states that I did "not cite any evidence to support this statement." However, my original statement was preceded by the supporting evidence, which included a reference to Faulkner et al. (2008). Mr. Olney, then states that I implied "that high spill in a low flow year had little or no effect on reach survival." This is a false representation of my statement. I believe that high levels of spill likely accounted for some of the relatively high survival observed in 2007 (which was reflected in COMPASS), but the discrepancy between COMPASS-predicted and observed survival was due to the relatively low bird predation rates in that year (see paragraphs 11-17 above). Mr. Olney's statements in these first three paragraphs demonstrate that he confuses the issues.

31. In paragraph 72, Mr. Olney misrepresents my declaration by stating that I did "not disclose that 2008 had above average flows and Court-ordered spill throughout the migration season." In fact, I stated (paragraph 21), "for 2008, when passage conditions were similar to those in 2006-7, COMPASS accurately predicted seasonal average survival …" In this statement, I was responding to Mr. Bowles comment that COMPASS could not capture the full effects of spill, and I noted that 2008 indeed was characterized by high levels of spill. His implication that I was somehow hiding the fact that 2008 was a high flow year is unreasonable. Mr. Olney then states, "NOAA did not discuss or disclose the accuracy of COMPASS in the

2008 BiOp." In fact, COMPASS had an unprecedented amount documentation and model diagnostics that were referred to in the BiOp.

32. Regarding Mr. Olney's statement concerning predator-prey dynamics, I addressed this issue extensively above (paragraphs 11-17).

33. Mr. Olney's comments on the use of bypassed fish ignores my comments on size differences between bypassed on non-bypassed fish in my previous declaration. In addition, I addressed this issue in detail above (paragraphs 18-27).

34. In Olney's paragraph 76, he states that although Williams et al. (2005) examined seasonal effects of transportation, they did not observe any benefits of transportation. In fact, Williams et al. (2005) concluded, "for wild yearling Chinook salmon and steelhead, in almost all cases fish transported after 1 May returned at similar or higher rates than fish that migrated through the FCRPS reservoirs and dams. In some years, fish transported as early as 15-20 April returned at higher rates than in-river fish." These results are quite similar to those obtained by Scheurell and Zabel (2006). Thus, Mr. Olney's conclusion that previous studies using non-detected fish did not observe differences in return rates between transported and in-river fish is erroneous.

35. In paragraph 79, Mr. Olney's clarification does little to clarify his original statement. He seems to be saying that if management decisions change within season, then pre-season COMPASS predictions will not be able to predict survival for that year. This point seems irrelevant as COMPASS could be run for several different scenarios to cover the range of possibilities, or model runs could be modified mid-season to reflect changes in operations. Further, the prospective modeling runs conducted in support of the BiOp reflected the fact that

operations can change within a season. HYDSIM updates its yearly predictions as the season progresses.

36. Regarding the NOAA response to the USFWS critique of COMPASS, Mr. Olney completely misrepresents the situation. In fact, NOAA had several discussions with USFWS management regarding this issue, and it was resolved to everyone's satisfaction. Although the NOAA memo (NOAA AR C. 409) was internal, it was public information, available to everyone, including USFWS. Mr. Olney's misguided speculation that the situation was somehow secretive once again reveals his general lack of understanding of the collaborative process that produced COMPASS.

I declare under penalty of perjury that the foregoing is true and correct. Executed on December 15, 2008, in Seattle, Washington.

Richard W. Zabel

Richard W. Zabel