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**IN THE UNITED STATES DISTRICT COURT  
FOR THE DISTRICT OF OREGON  
PORTLAND DIVISION**

**NATIVE FISH SOCIETY,  
MCKENZIE FLYFISHERS,**

Plaintiffs,

v.

**Case No.: 3:12-cv-431-HA**

**FOURTH DECLARATION OF  
CHRISTOPHER A. FRISSELL, Ph.D.**

**NATIONAL MARINE FISHERIES  
SERVICE, PENNY PRITZKER,  
Secretary of Commerce, WILLIAM STELLE,  
Regional Administrator, NMFS, OREGON  
DEPARTMENT OF FISH & WILDLIFE,  
ROY ELICKER, Director, ODFW,  
BRUCE McINTOSH, Acting Northwest Region  
Manager, ODFW, ED BOWLES, Fish  
Division Administrator, ODFW,**

Defendants.

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I, Christopher A. Frissell, declare the following matters are personally known to me and to which I am competent to testify under penalty of perjury of the laws of the United States:

1. I previously submitted an expert declaration in support of the plaintiffs's motion for partial summary judgment (Third Frissell Declaration). My qualifications and interest are described therein. I submit this declaration in support of the plaintiffs' motion to strike to explain that several points raised by Richard Turner in his Second Declaration and by Dr. Craig Busack in his First Declaration are erroneous and have little or no value as evidence, and to clarify points raised in those declarations. To prepare this declaration, I reviewed both the Second Turner and Busack Declarations, the NMFS Memorandum Support of Defendants' Cross Motion for Partial Summary Judgment and Opposition, and I consulted a number of relevant scientific papers, including those cited in relevant sections of Busack and Turner and additional relevant articles not mentioned in those declarations.

2. Turner (2<sup>nd</sup> Declaration, p. 24) argues the Kostow and Zhou (2006) study of harmful interactions between stocked hatchery summer steelhead and wild winter steelhead in the Upper Clackamas River is not applicable to the Sandy River because in the Clackamas, returning hatchery summer steelhead comprised over 70 percent of the total steelhead run. While Turner aptly points out that the Sandy is stocked with the very same non-indigenous variety of hatchery summer steelhead as used in the Clackamas study, nevertheless he implies that behavioral impacts of hatchery steelhead on wild winter steelhead would not prevail in the Sandy because the proportion of hatchery summer steelhead in the total steelhead run is smaller. I disagree. Because the harms to Clackamas wild winter steelhead reported by Kostow and Zhou were demonstrated to be occurring primarily during the juvenile freshwater phase of their coexistence, Turner is using an inappropriate metric in his dismissal. In the Clackamas, Kostow

and Zhou reported (See Figure 1, p. 832 of that paper) that, while the number of adult summer steelhead returning exceeded that of wild winter steelhead in many years, the numbers of outmigrating wild and hatchery steelhead smolts passing over North Fork dam on their way to sea were roughly equal across most years that hatchery stocking was occurring. To be specific, Kostow and Zhou concluded that stocking hatchery summer Chinook resulted in displacement of wild winter steelhead because of the (immediate, first-generation) behavioral and size advantages enjoyed by the stocked summer steelhead juvenile fish. This corresponded with a later higher rate of smolt-to-adult return of hatchery summer steelhead than wild winter steelhead for some of those generations of fish affected by hatchery stocking.

3. Turner's interpretation of the applicability of this study to the Sandy errs in two major ways: 1) Turner is inappropriately measuring the magnitude of effect by comparing abundance of returning adults, not the abundance of juvenile steelhead, the life stage at which the adverse interaction of hatchery and wild fish is shown to occur by Kostow and Zhou. Perhaps one reason for this is that, as far as I am aware, the record presents no attempt by NMFS to assess or analyze the density or relative abundance or density of hatchery and wild juvenile steelhead as they coexist in the Sandy system. 2) Kostow and Zhou did NOT establish a lower limit below which hatchery stocking has no effect on wild winter steelhead; they could only conclude that in many years, total carrying capacity of the habitat in the Clackamas was apparently exceeded, and the addition of hatchery smolts led to displacement, death, or other harm to wild fish. This is important because we can expect a steelhead stock like the wild winter steelhead in the Sandy River to exist at densities that hover at or near its present biological carrying capacity of its habitat, with some variation across years due to ocean conditions and other factors. Harm from stocking could be avoided only if studies establish that the Sandy

River winter steelhead population exists *below* its freshwater carrying capacity (a difficult biological question that cannot be established by guesswork). In addition, within a large basin there are many localized streams and sites where adverse behavioral actions could occur where hatchery fish are locally concentrated after release. Therefore, even if the hatchery steelhead are not dispersed through the whole Sandy Basin in large numbers, localized concentrations of hatchery fish in the mainstem Sandy could still affect all wild winter steelhead that must migrate through the river mainstem to get to sea.

4. Kostow and Zhou (p. 839) in fact concluded their results were not unique, but likely to be widely applicable to steelhead stocking programs. Their words are unmistakable: “...we do not believe the impacts we detected are restricted to the Clackamas River Basin. Similar density-dependent ecological effects could occur in any hatchery program that causes basin carrying capacity to be exceeded, whether or not interbreeding effects also occur....the addition of large numbers of hatchery fish, regardless of the intention of managers, would further depress the productivity of the wild population by introducing greater density-dependent effects.” Therefore it appears NMFS had reasons other than the scientific content of this paper (and several other articles in the literature cited in Kostow and Zhou that presented consistent results or supporting analyses) to conclude that the Sandy summer steelhead hatchery program is inconsequential in its harm; however as far as I can discern the record does not state what those reasons might be.

5. Turner (p.25) discusses the return timing of adult steelhead in the Sandy River system, arguing there is only small overlap between the spawning time of wild winter steelhead and hatchery summer steelhead that return to spawn in the wild. He argues this limited overlap seen in one particular case would “not have any substantial effect because the genetic

introgression...would not be measurable.” However, Turner does not provide what criteria he employed to “measure” genetic introgression. It appears that NMFS is not in fact measuring genetic introgression between hatchery and wild steelhead in the Sandy, nor does the HGMP seem require it to be measured in the future. Moreover Turner’s conclusion appears curiously out of step with available published studies that clearly shows that genetic introgression has been measured between early and late-spawning steelhead populations even where the observed overlap is very small (Seamons et al. 2012). In clear terms, Seamons et al. (p. 716-717) admonishes fishery managers to not assume that small overlap in spawning time and location leads to effective segregation, and to seek other means of actively reducing genetic harms of hatchery fish on wild fish.

6. Finally, I will briefly address Turner’s comments on the proposed spring Chinook acclimation rearing and hatchery stocking program in the Bull Run River, tributary to the Sandy. Turner (p. 26) states that the “Bull Run River has not had suitable habitat for spring Chinook salmon production since...1921.” He cites the lower 6 miles of Bull Run River as “of little value for salmon.” Yet then he recognizes that up to 34 spring Chinook salmon redds were observed in the gravel in lower Bull Run in recent years. Moreover, I am aware of a report produced by a contractor for the City of Portland that reported for 2005 a peak count of 60 adult Spring Chinook holding in lower Bull Run, and observed evidence of adult fish moving both up and downstream within the lower 6 miles through mid summer and the fall months, likely constrained by high temperatures for summer that kept fish holding in the mainstem Sandy River. In that 2005 study the reported total redd count exceeding 60. Turner seems to be saying on one hand that the Bull Run is suitable habitat for stocking, rearing, and retrieving large numbers of hatchery fish, but that it does not support a “significant” population of wild spring

Chinook. To the contrary, I will reiterate that I think available data show the Bull Run supports a population of wild spring Chinook large enough to be considered biologically “significant,” but that they are likely limited by commonly stressful thermal and flow conditions. Such stark contradictions of data together with the overall rather cavalier mode of the accounts in Turner’s declaration indicate to me that neither NMFS nor ODFW yet has taken a hard look at the potential harms to wild fish that could be caused by 1) constructing weirs that could partly obstruct wild fish movements in lower Bull Run River, 2) by the stocking of large numbers of hatchery Chinook on top of wild juvenile spring Chinook rearing in Bull Run River, and 3) by the potential harmful effects of large concentrations of hatchery adults returning to Bull Run River in future years and concentrating near and downstream of the Bull Run weir, and potentially competing with wild adult salmon for limited staging, holding, and spawning habitat that are likely in short supply.

7. Dr. Busack (Para. 21-22) questions the veracity of the results of Chilcote et al. (2011, 2013) for the purposes of assessing impact and harm from hatchery fish stocking on wild fish in the Sandy Basin and in general based on two arguments. First, he complains that Chilcote’s results are not derived from a controlled experiment but rather from a meta-analysis of results of studies of 89 salmon and steelhead populations reported from sites across the Pacific Northwest. Dr. Busack, however does not point to any scientific sources that meet his criteria of a controlled experiment, nor does he specify what suitable “data quality” would be in such a study. The intrinsic extreme difficulty (perhaps near impossibility) of undertaking such ideal, fully controlled experimental studies on real salmonid populations is not a weakness of the research, but is in fact the primary justification for Chilcote et al.’s alternative approach. Chilcote’s data are imperfect, of course (as are virtually all long-term fish population data) but

the results are validated and underscored by their repeated strong pattern of response seen across streams, species, and time frames—the classic measure of utility and veracity in a meta-analysis or (analogous in medicine) epidemiological study. In the case of this study, the strength of the underlying biological relationships unequivocally overpowered the data gaps and errors, and for this the reason both papers passed what I have no doubt (given the subject matter) was intensely rigorous peer review, and now stand as scientific benchmarks in published in the primary literature. And inexplicably, NMFS did not address the Chilcote (2011) study, or raise any of the arguments Dr. Busack now makes, in developing its biological opinion or environmental assessment.

8. Second, Dr. Busack suggests, without providing any numerical or other specific evidence to support his contention, that Chilcote et al.'s published relationships could break down to meaninglessness at the equivalent to percent of hatchery stray (pHOS) levels of 10 percent or less. Dr. Busack is correct that error could be high at this range of Chilcote et al.'s published curves; it could also be high in the middle, or at the upper end. But in effect Dr. Busack is contending that the observed relationships between hatchery fish straying and population productivity break down entirely or somehow reverse at pHOS levels below 10 percent. In fact all of the curves fit by Chilcote et al. clearly suggest that about 25-33% loss in productivity is associated with the first 10 percent of pHOS. In all likelihood it would be exceedingly difficult to make such a large initial decline in productivity disappear by simply changing the shape of curves fit to the data; for example one could revert to a straight line fit and the magnitude of the effect would still be on the order of 10-20%. Yes, it may well be that in some cases the initial effect is greatest in the first 10 percent of pHOS, while in others it is greater in the next 10-20 percent pHOS; for all we know in some cases the incremental loss

could be largest in the first 5 percent of pHOS. NMFS has neither established any of these, nor given us a way to analytically make that distinction. If Dr. Busack or other sources at NMFS have a contrary analysis, they should perhaps rush it to publication, or at least disclose it when they choose to discount the available science in making decisions affecting recovery of listed species, and they should do it as part of their public analysis of the consequences of approving hatchery programs. But it seems disingenuous to presume, with no data whatsoever, that Chilcote et al's results simply don't apply or are inversely incorrect at one portion of the graphs. Not only does Dr. Busack not provide any data to suggest an alternative curve for the Chilcote data set, he cites no studies to establish that no effect or minimal effects on survival and productivity would occur below 10 percent pHOS. Nor does he provide any alternative biological rationale why a potential avoidable 10-30% depression in the productivity of ESA listed salmon stocks caused by stocking hatchery fish should be ignored or permitted.

9. Dr. Busack (para. 20) questions my citation of the Chandler and Bjornn (2008) paper as one of several papers cited in evidence for how rapidly adverse genetic change can take place in the face of hatchery fish intermixing with wild fish. Dr. Busack argues the paper "is not a genetic paper," apparently on the basis of the fact Chandler and Bjornn did not report, nor sought to reveal, a measured change in genes. Chandler and Bjornn measured changes in fish growth rate and behavioral interaction—factors that are well-established to be heritable traits under a high degree of genetic control. Therefore changes in these behavioral and physiological features of wild fish, if and when caused by hatchery fish introduction, are exceedingly likely to correspond to genetic changes, whether or not "genes" are measured. Specifically I cited this paper to inform the conclusion that "Deleterious genetic changes can occur ... over several generations of hatchery breeding and rearing" (Frissell declaration Para. 13). Given that the

work of Chilcote et al. (2011, 2013) and many others cannot discriminate between harms caused by genetic alteration and those caused primarily by behavioral or ecological processes, it is important in making useful inference about harms from fish stocking that research into genetic and behavioral changes be linked and related when possible—not considered in isolation from each other as if they were unrelated events. Genetics can affect behavior and behavior, genetics.

10. Similarly I stand by my citation in Frissell Para. 13 to Emlen (1991) as among the most useful and relevant scientific evaluations of potential outbreeding consequences in salmon. In fact, we now know that hatchery populations demonstrably genetically diverge from wild populations of salmon, no matter what pains are taken to reduce this effect, as discussed at length in my declaration, and therefore “outbreeding depression,” is essentially one of the genetic consequences when hatchery salmon affect wild salmon in a deleterious way when the two interbreed. Dr. Busack (Para. 20) states that Emlen’s (1991) modeling is irrelevant, but he does not point to any other source that either contradicts Emlen’s conclusions or could serve as a more cogent reference.

11. In conclusion, in my opinion the Second Turner and Busack Declarations contain significant errors or shortcomings, including erroneous characterizations and unjustified conjecture about scientific literature and field conditions in the Sandy Basin, and therefore have little or no value as evidence. The record in this case demonstrates that best available scientific information has not been brought to bear by NMFS in its evaluation of hatchery practices and actions on the Sandy River, and as a result both undisclosed harms to wild salmon and steelhead, and a likely net loss in salmon and steelhead production are likely.

Pursuant to 28 U.S.C. § 1746, I declare under penalty of perjury that the foregoing is true and correct.

DATED this 15th day of November 2013.

s/ Christopher A. Frissell

Christopher A. Frissell

### **LITERATURE CITED**

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