

DAVID H. BECKER (OSB # 081507)  
Law Office of David H. Becker, LLC  
917 SW Oak Street, Suite 409  
Portland, OR 97205  
(503) 388-9160  
davebeckerlaw@gmail.com

Attorney for Plaintiff Native Fish Society

PETER M.K. FROST (OSB # 91184)  
Western Environmental Law Center  
1216 Lincoln Street  
Eugene, OR 97401  
Tel: (541) 359-3238  
Fax: (541) 485-2457  
frost@westernlaw.org

Attorney for Plaintiff McKenzie Flyfishers

**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**

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

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

Defendants.

---

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 on behalf of the plaintiffs. My qualifications and interest are described therein. In this declaration I respond to certain of the matters raised in the defendants' reply briefs and declarations to clarify my opinion, respond to some new information, and dispel some erroneous characterizations of my words.

2. Documents I reviewed in preparation of this second declaration include Federal Defendants' Opposition (NMFS Brief), State defendants' Opposition (ODFW Brief), First Declaration of Richard B. Turner, Declaration of Todd Alsbury, Declaration of Kirk Schroeder, Declaration of Richard Carmichael, and Declaration of Mark Lewis. I also belatedly received a copy of K. Arendt and G. Wanner, USFS Notes on the 2012 Spring Chinook Spawning Survey With Relation to Historic Trends, February 2013 (submitted to the case record as Exhibit 1 of the Sherwood declaration, cited hereafter as "USFS 2013"). I cite additional scientific literature and data sources in the text and where applicable list full citations at the end of this document.

**“PROTECTION” OF THE SANDY RIVER HATCHERY SPRING CHINOOK STOCK IS OF HIGHLY QUESTIONABLE IMPORTANCE**

3. NMFS on p.34-35 of its brief argues that an injunction on release of hatchery fish in the Sandy Basin “would irreparably harm listed hatchery stocks.” They further argue that not releasing hatchery fish would cause immediate and irreparable harm” to those hatchery fish.

First, I find it curious and perplexing that a single year of harm to hatchery fish would be considered irreparable, where as when harms to wild fish are assessed, they are usually assumed

by NMFS or ODFW to be “reparable” with the rationale that as long as we do not drive them to extinction, wild fish can recover or be replaced by survivors after we stop doing the harmful actions in the future. In my opinion, harms to wild fish should reasonably be considered irreparable, because the losses of genetic integrity and population productivity and consequently, population viability suffered on an annual or ongoing basis cannot be retroactively corrected, mitigated, or otherwise effectively compensated by ODFW or NMFS. Once dead, wild fish cannot be resurrected by ODFW or NMFS.

4. While yes, wild populations not driven to a non-viable condition can hopefully "self-repair" in the future, we cannot recover and have forever foregone the years of lost function and productivity and its impact on the ecosystems they inhabit (e.g., providing food to bald eagles, otters, kingfishers, other fishes, and other predators and scavengers that are not particularly tolerated at hatcheries). The management strategy of isolating hatchery from wild stocks, while it may create some sport fishing opportunities, further restricts the external ecosystem functions and values of hatchery-origin salmon. These foregone values and benefits are analogous to loss of fishery values to humans that sometimes occurs when hatchery populations are curtailed. This puzzling asymmetry of criteria in determining harm and assessing environmental costs favors hatchery fish whenever there is a direct or calculable tradeoff (as there is in the case of the Sandy spring Chinook, where the straying of hatchery fish into the wild has been recognized as a principle threat the wild population).

5. NMFS (p.40, reflecting Turner para 52) claims the Sandy hatchery programs could serve as a “gene reserve” for populations of fish in the Sandy River, and that failing to release hatchery fish would harm this “gene reserve” function. While I do not dispute that the concept of a “gene reserve” is allowed under NMFS policy, nowhere in the record that I can find

any argument or evidence presented that recovery of Sandy River salmon rests upon the need for a “gene reserve.”

6. Moreover, as a biological matter I do not agree that a hatchery population of salmon is an irreplaceable resource or even persistent biological entity, the way native wild populations are, or can be. Hatchery populations are biologically best described as transitory “satellite” populations derived from the “core” or “parent” population that is the natural wild stock (except in the case—not true in the Sandy—where the habitat or population of the wild stock is vulnerable to imminent and complete loss). In fact ODFW in its recent history with developing a new, in-basin Sandy Hatchery stock of spring Chinook has demonstrated beyond a doubt that a hatchery stock of spring Chinook salmon can be created—not without effort, but in relatively short order—as long as there is a wild stock present to draw from. The reverse has not been shown to be true.

7. Hence as a matter of conservation decision-making regarding protection and recovery of a listed species, any reasoned calculus of tradeoffs between hatchery fish and wild fish should strongly favor the persistence and recovery of wild fish as its priority. Indeed, if wild fish prosper and are eventually delisted, then by definition the wild population will be able to sustain a robust fishery, entirely replacing this function of the hatchery. To reiterate, I have seen nothing in the record that makes an even slightly compelling case that hatchery salmon and steelhead in the Sandy are necessary, or even practically beneficial, for conservation and recovery, either as a “gene reserve” or otherwise. To the contrary, it is clear that the purposes of maintaining these hatchery stocks are other than recovery, and among the consequences of maintaining these hatchery stocks is that they are in fact an acute threat, a source of take, and a major impediment to recovery.

8. If the Sandy spring Chinook stock should at some future time decline to the point where an attempt at "rescue" by hatchery stocking might be imminently warranted, ODFW has successfully demonstrated it can establish a wild-origin brood stock in the span for the Sandy in the span of a few years. Even if one grants, as NMFS and Turner argue, that the present hatchery brood needs to be maintained as a hatchery-only stock for future potential "rescue" purposes, given the rates of survival to return cited by in the Alsbury declaration of 1-3%, only small releases on the order of a 50,000 hatchery Chinook would be necessary to sustain the brood stock size and diversity necessary to maintain a self-standing "gene reserve" in the hatchery. Even much smaller releases of hatchery smolts could support a hatchery stock if regular supplementation with wild-origin brood stock is determined to be appropriate as natural population abundance increases.

#### **IMPACT ON WILD POPULATIONS OF COMMINGLING HATCHERY ADULTS**

9. The declaration of Mr. Turner refers to Hess et al. (2012) on the population-level effects of "supportive breeding" of spring Chinook salmon in the Johnson Creek in Idaho as evidence hatchery fish that spawn in the wild can have rates of reproductive success equal to or higher than wild-origin spawners. That hatchery program is operated as a conservation hatchery and only natural-origin fish are taken as brood stock each year. It is a small hatchery program designed to affect a single small wild salmon population, involving hundreds of returning adult fish per year, not thousands as in the Sandy River basin. This is substantially different kind of hatchery management, possibly with less potential for adverse domestication selection than is the situation on the Sandy, where after an initial period of wild broodstock collection only second-generation hatchery-origin (adipose fin clipped) adults are now taken for broodstock. Moreover

the Hess et al. results are quite unusual, in that they directly contradict results from a number previously published studies conducted in similar circumstances.

10. Mr. Turner is mistaken in stating that the numbers I cited for relative reproductive success of hatchery-origin fish spawning in the wild are not representative or that they exaggerate the known adverse impact of hatchery fish on wild populations. In fact the most robust analysis to enlighten this question was published by Chilcote et al. in 2011, with a slightly revised addendum published just this month (*ibid* 2013). Chilcote's study looked across 89 salmon and steelhead hatchery-influenced populations of Chinook salmon, coho salmon, and salmon and steelhead in the Pacific Northwest. The populations in the study spanned a range of hatchery management and habitat conditions, including a number of cases comparable in magnitude and management to the Sandy basin. The data examined included long time series of adult returns, accounting for full life cycle effects.

11. Chilcote et al. concluded that reproductive performance measured at the population level declined as a function of the proportion of hatchery fish in the population, for all species and conditions. They further reported that the difference in reproductive success was so large that "the recruitment performance for a population composed entirely of hatchery fish would be 0.128 of that for a population composed entirely of wild fish." They also found that as a rule, "the impact of hatchery fish from 'wild type' hatchery broodstocks was no less adverse than hatchery fish from traditional, domesticated broodstocks," and that the magnitude of the impact of hatchery fish was no greater in populations with long-time exposure to hatchery programs than those with short-time exposure. They conclude that in most cases, minimizing the exposure of wild populations to hatchery-origin fish is the best conservation strategy for wild populations. Indeed this finding, which reflects many previous findings of studies of more

limited scope, is perhaps the primary basis for ODFW's decision to reduce the incidence of hatchery fish spawning in the Sandy River!

12. I should take this opportunity to point out that specifically for Chinook salmon, Chilcote et al. (2013, Figure 4) show that for river systems with in-basin hatcheries (fish do not originate from out-of basin hatchery stocks) and no dam influence on the migratory corridor (both reflect the present situation for the Upper Sandy where spring chinook primarily spawn), a ten percent incidence of hatchery-origin fish in spawning populations produced an estimated roughly 25 percent decline in reproductive success (expressed as intrinsic productivity, or next-generation recruits per this-generation spawner) compared to a population with no incidence of hatchery-origin fish. In fact the first 10 percent incidence of hatchery fish spawning imposes a larger absolute loss of wild population productivity than subsequent increments of hatchery fish occurrence (i.e., the per capita impact of a hatchery fish declines slightly at higher stray rates). From the standpoint of a threatened species, a 25 percent loss of productivity is of a magnitude that can cause dramatic declines of population viability under common circumstances.

13. Although Chilcote et al.'s curves translating hatchery stray rates into productivity losses have been available for two years now (and likely longer than that inside the agencies), it appears NMFS in the BiOp and ODFW in the HGMP made no attempt to make a transparent and plain English explanation or formal or informal assessment of the expected impact of its target of 10 percent hatchery stray rates on viability of listed spring Chinook salmon in the Sandy. Nor has Mr. Turner in his declaration done so. The agencies' silence on this point leaves the impression in the public's mind that a 10 percent stray rate is either deemed nearly harmless, or has been through some formal and population-specific analysis determined to be acceptable or a level where benefits to the listed species outweigh the biological costs. As far as I can discern

from the record, no such analysis has been done for Sandy River stocks. In my opinion, Chilcote's work (and much other published research) raises grave doubts about the adequacy of the 10 percent stray rate as a conservation benchmark; it appears to be an arbitrary threshold picked as a generic recommendation with no specific assessment or reference to its likely biological effects in the Sandy Basin and its Chinook populations. (A 10 percent threshold "is constituent [sic] with recommendations made by the HSRG and Interior Columbia Basin Technical Recovery Team," NMFS Brief at p. 26). Given that NMFS and ODFW in their briefs extoll the virtues of site-specific analysis, it appears strange that no such site-specific analysis was done to justify or evaluate their site-specific hatchery program stray rate targets in the Sandy basin. Furthermore, why cannot expected losses of reproductive success, estimated based on the best and readily available science such as Chilcote et al. (2011 and 2013), be considered *a direct and robust measure of life cycle take* caused by interbreeding with hatchery fish—the central issue, after all, of the HGMP? NMFS is silent on this question.

14. Given the perspective that Chilcote et al.'s work provides on the observed magnitude of impact of hatchery straying on the productivity of wild salmon populations across the region, I have no reservations about the opinion expressed in my first declaration that the lack of recovery, or relatively slow rate of recovery, of salmon in the Sandy Basin in recent years (not just in response to removal of Marmot dam in 2007, but a host of other habitat protection and restoration actions) is highly likely to have been in large measure a consequence of straying hatchery salmon and steelhead.

## **LIKELIHOOD OF SUCCESS FROM OUTPLANTING OF CHINOOK SMOLTS IN BULL RUN**

15. A principle measure emphasized in the briefs from NFMS and ODFW (citing the declarations of Alsbury and Schroeder) intended to reduce future straying of hatchery spring Chinook is the use of off-site acclimation ponds and release of smolts into the Bull Run River, a tributary that meets the Sandy downstream of the Sandy Hatchery location. ODFW is also seeking permission from the Portland Water Bureau to construct a weir and trap in the lower Bull Run River to capture returning hatchery adult spring Chinook and remove them from the system. There are several major reasons to not presume that this action will produce the desired result of greatly reducing the incidence of hatchery strays in to upper Sandy spawning areas.

16. My foremost concern is that the pattern of migration expected is utterly antithetical to the long-term evolutionary adaption of the Sandy River spring Chinook (we know that the wild stock and the hatchery stock derived from it originated largely if not wholly in the Upper Sandy). As Mr. Schroeder documents in his declaration, upper Bull Run spring Chinook were virtually eliminated early in the previous century with the construction of the Bull Run dams. Spring Chinook habitat in the accessible lower reaches of Bull Run River and the Little Sandy River is of limited quality and quantity, and sees some but limited use by wild spring Chinook (perhaps including descendants of displaced upper Bull Run populations, but this is apparently unknown). ODFW expects short-term, pre-smolt acclimation to imprint hatchery fish released into the Bull Run to cause them to return there and avoid the Upper Sandy, but to accomplish this, behavioral imprinting will have to overcome several thousand years or more of strong natural selection for rapid migration into the Upper Sandy and its tributaries.

17. Migratory behavior of Pacific salmon has been intensively studied, and it is well-known to result from the interaction of imprinting and learning during juvenile life and a longer-term, genetically inherited, instinctive behavioral imprint (Dittman and Quinn 1996, and many others). Consequently, I doubt that brief exposure to Bull Run water will reprogram or overwhelmingly influence the migratory behavior of Sandy River Spring Chinook to the complete extent ODFW anticipates. If the stock of origin consisted of Bull Run fish or fish from another lower-Sandy tributary, there would not be such a vast discrepancy between the imprinting signal and the evolutionary template, and ODFW's expectation might be more reasonable. But given the ecological and evolutionary discrepancy, in my opinion ODFW's hopeful expectation is biologically untenable and therefore, not very reasonable.

18. A second and coupled concern is that when salmon home to less than optimal or crowded habitat, it is a plain fact that many of them choose to stray. Research demonstrates that homing and straying decisions are made (even by hatchery fish) based on a complex set of cues that includes the availability of habitat of higher quality in areas other than their point of acclimation or stocking (Cram et al. 2012). Habitat conditions in the lower Bull Run River simply do not appear to be particularly conducive to attracting and holding of adult spring Chinook in large numbers (though I agree they are less hostile than Cedar Creek, which was impossible for returning salmon to enter much of the time). The lower Bull Run River is relatively warm in summer and fall. A check of Portland Water Bureau and USGS water temperature data for the lower Bull Run River (USGS data for Bull Run River at Larson's Bridge near Bull Run, OR, as reported in the Portland Water Bureau's *Bull Run Water Supply HCP Compliance Report 2011*) indicates extended periods during July and August when flows are at their lowest and water temperature exceeds 17-18 degrees C. While this is below the absolute

tolerance of adult spring Chinook salmon, it nevertheless can be considered stressfully warm for holding them over extended periods. In the Yakima River basin, for example, Berman and Quinn (1991) found that migrating spring Chinook sought out coolwater habitats that averaged 2.5°C cooler than the ambient 12°C-19°C temperatures of the river. Unless extensive coldwater thermal refugia exist there, and their availability is somehow detected by the fish, the thermal regime of Bull Run appears unlikely to naturally attract large numbers of migrating spring Chinook from the mainstem Sandy River, which appears to be somewhat cooler during than Bull Run much of the summer. The strongest attraction will continue to be the dramatically cooler Upper Sandy River tributaries. (For example, USGS stream record data for the summer of 2007 show that Sandy River below Marmot Dam site exceeded 18°C for only a few days in July, while the Bull Run River at Larson's Bridge exceeded 18°C for several weeks during August and September; further upriver at Brightwood the Sandy mainstem appears to generally run 2-3 °C cooler than Bull Run during most of the summer and fall months, and upper river tributaries can run much colder.) I can find no evidence in the record showing that ODFW or NMFS have undertaken a systematic review of the seasonal thermal template and other physical conditions in the Sandy system insofar they are likely to significantly affect migration and holding behavior of returning adult spring Chinook salmon.

19. A third and also coupled concern is the possibility that if large numbers of returning adult hatchery fish are attracted to Bull Run, prevailing temperatures and crowding in that system during low flow intervals could send the stage for disease outbreaks that propagate further to both hatchery and wild fish, adults and juveniles alike. Many forms of salmon disease become virulent at water temperatures above about 15.6°C (McCullough 1999 p. 97), and this is a particular challenge for spring Chinook adults that hold in freshwater for extended periods

under prevailing temperatures. I have been unable to locate in the record consideration by ODFW or NMFS of this possible hazard of releasing large numbers of hatchery salmon into the lower Bull Run River, but records I examined (USGS data for Bull Run River at Larson's Bridge nr Bull Run, OR, as reported in the Portland Water Bureau's *Bull Run Water Supply HCP Compliance Report 2011*) indicate that temperatures exceeding 15.6°C prevail in lower Bull Run River during July and August, and may occur well into September.

20. A final concern is the apparent spectacular clash between management goals for the Bull Run River between Portland Water Bureau (PWB) and ODFW. The Bureau holds an approved Habitat Conservation Plan with NMFS that, among other requirements, provides flow restoration the lower Bull Run for the express purpose of recovery of wild spring Chinook salmon, and other species. The Kucas letter of January 19, 2013 from PWB to ODFW (I understand it will be attached to Plaintiff's Brief) underscores the PWB's opposition to stocking of hatchery fish in the Bull Run River because that action potentially cripples its ability to restore wild salmon in the system (the PWB expects wild-origin returns to exceed 400-500 fish in future years). Moreover the first condition imposed on the potential grant of access that is needed in order for ODFW to construct a weir is that the weir and trap be "able to operate under high streamflow conditions...From July 1 to October 31, which is a minimum typical spawning season for spring Chinook, streamflows in the lower Bull Run River can be greater than 500 cfs and as low as 30 cfs." This is a tall order for a weir and may require a very expensive and large structure to achieve, depending on the site. This recent correspondence indicates the two agencies have not worked out even the most basic agreement about biological and management priorities for Bull Run system.

21. The NMFS Brief (p.15 second paragraph) erroneously mischaracterizes the simple analysis in my first declaration of the potential effectiveness of weirs/traps and altered rearing regimes (Frissell first dec. para 24). I presented that analysis to show that it is possible with reasonable assumptions to assess and compare the likely success of various proposed actions under the HGMP to reduce stray rates below the 10 percent target. I presented this simple example because the record appears strangely lacking in any such attempted rational calculus to provide a reasoned context for the large stack of assumptions embodied in the HGMP and BiOp. Here is the relevant excerpt of my declaration (Frissell first para24):

“If we assume that modified rearing measures might reduce the pre-trap incidence of straying hatchery Chinook into the Upper Sandy from the present 60-70% to nearer 50% of the prevailing average total return, a 33% reduction in the total number of Chinook smolts released is most likely to produce a roughly proportional one-third reduction in those strays, producing a total stray rate in the neighborhood of 30-40% into the Upper Sandy. If we very generously assume that perfected weirs and traps can reduce that figure by another 50%, that could result in an incidence of hatchery strays of roughly 14-20 percent of total Chinook returns to the upper Sandy Basin. A few further simple trial-and-error calculations suggest that on average, cutting Chinook smolt releases from their authorized 300,000 to the range of 75,000 to 100,000 (25-30% of recent releases) or less—in tandem with significant reductions from altered rearing strategies, and greatly improved trapping efficiency—is probably necessary to achieve a stray rate of 10% or less.”

22. In its brief, NMFS stated (p.15, third para, mis-citing the Schroeder dec. para 57):  
*“Dr. Frissell either did not consider in his analysis or heavily discounted the key strategy that*

*ODFW is planning to reduce the stray rate: acclimation and release from Bull Run....* thus Dr. Frissell’s opinion about whether ODFW is likely to achieve the pHOS in the future should be accorded little weight.” It should be apparent to any careful reader that my opinion about the likely range of stray rate reductions achievable by acclimation and releasing smolts in Bull Run is explicitly included in the first sentence of my excerpt above, and further reiterated in the concluding sentence. For the reasons that I explain in detail earlier in this declaration, I find Mr Schroeder’s assumptions about the effectiveness of Bull Run acclimation and stocking in reducing hatchery stray rates (which are laid out clearly for the first time in his declaration) to be ill-justified and unreasonably optimistic.

23. It is important to point out at this juncture that the experiments on acclimation in the Clackamas River that Mr. Schroeder cites to support his optimism (Schroeder para 58-59) used a foreign, out-of basin stock of spring Chinook salmon, the McKenzie River Hatchery stock. Hence the behavior of those returning fish was not necessarily evolved or adapted to the habitat conditions and geography of the Clackamas River Basin. Therefore the Clackamas experiment was much different from the proposed Bull Run experiment in the Sandy, which would instead involve a much smaller scale alteration of the rearing location of a locally evolved stock—a group of fish that has long adapted through natural selection to migrate to the upper Sandy Basin to hold and spawn. While the relative roles of genetic heritage and environmental experience in homing and straying of salmon remains a matter of scientific uncertainty, both are known to play a role (e.g., Dittman and Quinn 1996), and it is my opinion that it is not reasonable to simply assume to obtain the same outcome using a locally derived Sandy hatchery stock (intentionally just one or two generations removed from its wild source) that was observed

in the Clackamas Basin using wholesale dislocation of a domesticated hatchery stock from an entirely different and highly altered river system. This is not the same experiment.

### **WEIRS AND TRAPS AND THEIR ANCILLARY IMPACTS**

24. The declaration of Mr. Schroeder was very helpful in clarifying the basis of ODFW's metrics and decision criteria for monitoring of hatchery and wild salmon and the effects of the weirs in the Upper Sandy system under the HGMP. While I profoundly trust Mr. Schroeder's account, I still find it quite difficult to extract many of the key facts from the available NMFS and ODFW record. I also remain more convinced than before that the proposed protocol and in particular, the HGMP goals or "triggering" criteria are a very blunt instrument and are not well-suited to the job of describing or ensuring the viability and recovery of wild stocks in the face of hatchery straying in the Sandy. I noted also that Mr. Schroeder reiterates the important point he made in his 2011 ODFW report (as cited in my previous declaration) that differences in survey breaks and weir locations seriously complicate and limit applicability of the HGMP's proposed monitoring and benchmarking of weir effects by comparing red counts above and below weirs between years (moreover, ODFW reports that weir locations may move again in the future, which could further compound the evaluative problem). Overall, lack of clarity and ambiguities in the public record and HGMP itself seem unfortunate and unproductive given that public scrutiny and interest in the outcome of the Sandy Hatchery program have been obviously high for many years.

25. Mr. Schroeder's uncertainty (Schroeder para. 45) about my reference to "premature spawning" associated with weirs warrants response, because it opens the broader question of effect of potentially harmful effects of weirs on behavior of wild and hatchery salmon that are not measured in ODFW's survey sampling of carcass and redd distribution and

prespawning mortality (the presence of eggs in female carcasses). Because migrating adult salmon are on a fixed energy budget and must spawn prior to the depletion of energy reserves, delayed migration and altered migration timing can be harmful (Crossin et al. 2004). Quoting Donaldson (2008):

Prolonged delays that prevent salmon from reaching suitable resting or spawning areas in time may obviously reduce their reproductive success ... This may especially be of importance in rivers with many migration barriers, which have a cumulative effect on the total delay. The fish may either have to spawn in unsuitable areas, or they may reach suitable spawning areas too late. Delayed arrival at spawning grounds results in fewer opportunities for selection of prime spawning sites, shorter time spent on spawning grounds, and fewer opportunities for reproduction (Smoker et al. 1998; Hodgson and Berg 1999; Crossin et al. 2004; Keefer et al. 2006).

Thorstad et al. (2008) reviewed the many ways that instream barriers, whether they are physically impassable or serve as behavioral deterrents to upstream passage, can cause migration delays and other anomalies, including displacement of spawning of Atlantic salmon.

26. In my own experience with Chinook and other salmonid species, concentrations of spawning fish, and relatively early spawning behavior is often observed below temporary barriers to upstream migration, whether these are natural or man-made. I have observed such aggregations below constructed weirs on multiple occasions, even where traps were actively maintained on a continuous or multiple-times-per day basis. (Biologists who work with weirs and other forms of traps routinely observe that many individual fish appear to consistently show “trap-shy” behavior.) I note that the biologists with the Forest Service USFS (2013) observed

aggregations of spring Chinook immediate downstream of ODFS's weirs in the Salmon River and ZigZag River weirs on multiple occasions during 2012, and further reported (p.3) that "Redd superimposition immediately downstream of both weirs was unusually high. Evidence that fish may have been selecting to spawn downstream of weirs rather than moving through them."

Redd superimposition is evidence of habitat limitation and crowding to the extent that the spawning activities of one pair of salmon may cause damage or destruction of the eggs of other individuals. The same memo (USFS (2013, p. 3) documents illegal poaching activity in the vicinity of these aggregations, an intrinsic and common problem of weirs as I described in my first declaration.

27. The actual act of salmon spawning in the wild is not purely a matter of "hardwired" physiological and genetic programming; once a measure of physiological maturity is achieved, environmental and behavioral factors such as stress, and courtship and nearby spawning behavior of others of the species can trigger pairing and competitive spawning behavior. If a weir causes reluctance to migrate upstream, aggregations of fish below the weir form, and if the "ripeness" window has opened for at least some individual fish, a spate of spawning activity commonly results. In my experience this often begins (and peaks) well before the peak of spawning is observed upstream. Because of the location *below* the trap, both hatchery and wild fish participate in these aggregations, and as a result there is likely to occur a very high incidence of interbreeding of hatchery and wild fish at such a location. Such a localized high concentration of interbreeding (or breakdown of the isolation of hatchery and wild fish that ODFW and NMFS are striving for under the HGMP) directly results from behavioral and habitat alterations caused by the weir. These disruptions include "forced" spawning in a location downstream of what might have been a preferred upstream site, impairment of mate

selection and spawning segregation that wild fish might have exercised had they freely migrated, and increased likelihood of “sneaker male” matings and other mate selection distortions potentially associated with unusual crowding of hatchery and wild fish in proximity. Perhaps I would better have termed this impact “behaviorally premature spawning.”

28. Mr. Schroeder (para 45) correctly identifies one important mechanism by which that physical effects can interact with physiological effects at weirs to produce a bad outcome: If fish that hold below a weir are subject to a thermal regime different than that they would otherwise have experienced in preferred habitat upstream, that can initiate a cascade of harmful physiological and developmental disruptions for the individual and its offspring. It has been shown to alter such fine-scale adaptations as timing of egg development and maturation, propensity or preparedness to spawn, spawning site selection, and subsequently on down the life cycle to the duration of egg incubation, the timing of emergence of fry from the gravel, and the survival of emerged fry in freshets or icing events. Such a cascade of changes at critical life stages triggered by changes in temperature exposure have been associated with declines of spawning success, survival, and productivity of affected wild salmon populations in careful life cycle studies (e.g., Holtby et al. 1989).

29. All of these effects can result in spawning at less suitable locations and with less selective mating behavior than fish might displaced had the obstruction not been present. It is most critical to note that this list of harms is not effectively measured by the monitoring protocol that ODFW uses and that laid out in the HGMP and Mr. Schroeder’s and Mr. Lewis’ declarations, except in the unlikely instance an extremely large and exceptionally consistent degree of crowding should persist downstream of a weir. To the contrary, harms and take associated with behavioral alteration and mate selection can be manifest in concentrated fashion

over very short time frames and very short spatial distances when they occur in the proximity of spawning habitat. Many of these weirs, of course, are constructed in the midst of spring Chinook spawning areas (Schroeder declaration, para 46 and elsewhere).

30. In his comment (para 46) that “the area downstream of the weirs is not ‘bad’” for spawning Mr. Schroeder does not appear to take into account the science pertaining to micro-scale site selection of redds by salmon and trout. The literature is replete with studies showing what the human eye discerns as “not bad” for spawning can in fact produce highly variable egg and fry survival, depending on factors not readily apparent to us, including upwelling or downwelling of intragravel water, localized groundwater inflows, hydrodynamic stability of the specific gravel patch in the face of floods or drought conditions, and others (e.g., Geist and Dauble 1998, Baxter and Haurer 2000 Mull and Wilzbach 2007, McRae et al. 2012, Cram et al. 2012). While putative “good conditions” indeed do likely occur at some locations downstream of weirs, we also know that individual fish or pairs may seek very specific microsite conditions to place their eggs, depending on the fine-scale features of those sites and on their own physical and physiological features (including both adaptive morphology and condition factors, e.g., body size, girth, current energy reserves and muscular ability to excavate redds in very fast flows, physiological adaption of eggs to incubate in unusual groundwater-dominated temperature regimes) and other behaviorally-expressed preferences that reflect diverse individual biological needs. Because in this instance these factors are not measured and known, in fact fish that are displaced from their preferred spawning areas by a tendency to avoid weirs (or by the physical presence of a weir on top their preferred spawning site, or by delay caused by entrainment in a trap, or other behavioral effects) may be forced to spawn in locations that are far from their individual optima or preference. Moreover this appears to be a highly likely, and probably is an

inevitable result of any action that disrupts adult migratory and spawning behavior of a wild population of salmon.

31. Any assumption or implication that artificially constructed weirs can be made to function perfectly without disrupting this fine-scale, interlocked and adaptive architecture of individual fish behavior and physiology interacting with microhabitat choice, strikes me as ludicrous—particularly when weirs are constructed in the immediate vicinity of spawning habitat. I think it's safe to presume this is among the reasons why NMFS and other state and federal state agencies routinely discourage other parties from the placement of potential migration-blocking structures in habitats critical for threatened and endangered salmon.

32. Mr. Carmicheal's (Dec., esp. para 13-19) experience that weirs, if carefully designed and managed, can "function effectively" in northeast Oregon rivers is certainly relevant. However, in his statement that with appropriate design and maintenance, weir operations can be held to "low acceptable level of impact to wild fish," he provides no criteria for how "low" and "acceptable" are defined and measured. The statement strikes me as facially dismissive of the behavioral concerns cited above. (The purpose of these weirs, as I understand it from Mr. Carmicheal's publications and presentations, is to collect hatchery fish and data from captured wild fish, and to collect selected wild fish for broodstock, their "effectiveness" has primarily been evaluated by ODFW on the basis of how well those particular jobs are accomplished.)

33. Secondly, in the spirit of the refrain in NMFS and ODFW briefs that local evidence is of extreme importance and may controvert global knowledge, I would point out that hydrological and other stream conditions in northeast Oregon rivers (where I also have worked), such as seasonal streamflow variability, water temperatures, and water clarity) differ from those

in the Sandy in ways that may affect weir performance and fish response to them. There may also be behavioral and ecological differences in how salmon stocks in different rivers respond to habitat or migration blockages that affect the degree of harms that a weir of given design imposes. I do not see in the record evidence that a systematic analysis of past experience with weirs has been undertaken by ODFW or NMFS to evaluate and prescribe appropriate designs for the several prescribed Sandy River weirs. Rather, it appears that in the BiOp NMFS gave generic approval for “weirs” and mandated only a relatively crude monitoring protocol sufficient to ensure consistently disastrous biological outcomes are not evident.

34. In summary, to put these various concerns about adverse biological effects of weirs in perspective: I concur that as long as the Sandy River system is under pressure from very large numbers of hatchery-origin salmon returning to the river system, weirs and traps are the best available means of reducing potential interbreeding of wild and hatchery fish. But given the intrinsic potential for weirs and traps themselves to cause ongoing harm, and the intensity of management required to reduce that impact, it appears far more prudent, with minimal or no potential for ancillary harm, to reduce the releases of hatchery fish to levels, and employ methods for rearing and release such that weir and trap operations are not required to meet or exceed HGMP goals.

## **CONCLUSION**

35. In my view, the law holds that protection and recovery of a federally listed species rests on accurately identifying threats and implementing the necessary and prudent measures to alleviate them, within the authority and discretion of the responsible parties (in particular, government agencies). As I understand it, such actions should be taken when they provide needed biological relief from threats and promote recovery, even when they happen to

prove inconvenient to the ongoing management programs of those parties. The statute mandates that scientists provide their best analysis given the facts and scientific knowledge currently available. Neither the law nor scientific expedience asks that we delay scientific judgment in the expectation that the future effects of recent or anticipated incremental changes in management might be incrementally different. When it comes to protection and recovery of a listed species, the concept of adaptive management is abused when it is used to stave off sweeping but prudent actions of known effectiveness in the fond hope that smaller actions might produce results that are incrementally less harmful than past similar actions. In other words, stocking massive numbers of hatchery fish in a river basin is itself a sweeping action, and as a matter of biological and physical reality, it is highly unlikely to be effectively ameliorated by a less than sweeping change of practices. It doesn't take adaptive management to demonstrate this with high probability; extant facts suffice.

36. In the case of the Sandy River, I conclude from the record and my understanding of the relevant science that it is a straightforward matter that a dramatic reduction in ODFW's release of hatchery salmon (in particular, spring Chinook salmon) from current and anticipated levels is the principle prudent conservation measure that would effectively and directly alleviate threat—coupled with continued weir operation for five to six years until the last fish from prior hatchery releases return. A prudent adaptive management plan would rest on 1) establishing a minimum baseline level of hatchery fish releases necessary to sustain a Sandy Hatchery stock of spring chinook salmon, followed by 2) carefully evaluating straying and possible genetic and ecological impacts at that minimum level over a period of years, and then 3) to carefully and incrementally increase hatchery releases using new rearing and release methods and locations, while continuing to monitor to ensure biological harm is kept in check and to accurately quantify

and limit take under the new management measures. It remains my opinion that the proposed release of 200,000 spring Chinook salmon smolts into the Sandy River system in 2013 will cause irreparable harm to wild spring Chinook, and is likely to substantially hinder or prevent their recovery over the coming several years.

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 March 2013.

\_\_\_\_\_  
s/ Christopher A. Frissell

Christopher A. Frissell

#### **LITERATURE CITED**

Baxter, C.V. and F. R. Hauer. 2000. Geomorphology, hyporheic exchange, and selection of spawning habitat by bull trout (*Salvelinus confluentus*). *Canadian Journal of Fisheries and Aquatic Sciences*. 57: 1470-1481.

Berman, C.H. and T.P. Quinn. 1991. Behavioral thermoregulation and homing by spring chinook salmon, *Oncorhynchus tshawytscha* (Walbaum), in the Yakima River. *Journal of Fish Biology* 39:301-312.

Chilcote, M. W., K. W. Goodson, and M. R. Falcy. 2011. Reduced recruitment performance in natural populations of anadromous salmonids associated with hatchery-reared fish. *Canadian Journal of Fisheries and Aquatic Sciences* 68:511-522.

Chilcote, M.W., K.W. Goodson, and M.R. Falcy 2013. Corrigendum: Reduced recruitment performance in natural populations of anadromous salmonids associated with hatchery-reared fish. *Canadian Journal of Fisheries and Aquatic Sciences* 10.1139/cjfas-2012-0542.

Crossin, G.T., S.G. Hinch, S.J. Cooke, and nine others. 2008. Exposure to high temperature influences the behavior, physiology, and survival of sockeye salmon during spawning migration. *Canadian Journal of Zoology* 86: 127-140.

Dittman, A.H., and T.P. Quinn. 1996. Homing in Pacific salmon: mechanisms and ecological basis. *The Journal of Experimental Biology* 19: 83–91.

Donaldson, M.R. 2008. *The Physiology, Behaviour, and Fate of Up-river Migrating Sockeye Salmon (*Oncorhynchus nerka*) in Relation to Environmental Conditions in the Fraser River, British Columbia*. M.S. Thesis, Carleton University, Ottawa, Ontario.

Hess, M.A., C. D. Rabe, J.L. Vogel, J. J. Stephenson, D.D. Nelson, and S.R. Narum. 2012. Supportive breeding boosts natural population abundance with minimal negative impacts on fitness of a wild population of Chinook salmon. *Molecular Ecology* Nov;21(21):5236-50. doi: 10.1111/mec.12046. Epub 2012 Oct 1.

Holtby, L.B., T.E. McMahon, and J.C. Scrivener. 1989. Stream temperatures and interannual variability in the emigration timing of coho salmon smolts and fry and chum salmon fry from Carnation Creek, British Columbia. *Canadian Journal of Fisheries and Aquatic Sciences* 46: 1396-1405.

McCullough, D.A. 1999. A Review and Synthesis of Effects of Alterations to the Water Temperature Regime on Freshwater Life Stages of Salmonids, with Special Reference to Chinook Salmon. Columbia Intertribal Fisheries Commission, Portland, OR. Prepared for the U.S. Environmental Protection Agency Region 10. Published as EPA 910-R-99-010. [http://www.krisweb.com/biblio/gen\\_usepa\\_mccullough\\_1999.pdf](http://www.krisweb.com/biblio/gen_usepa_mccullough_1999.pdf)

McRae, C.J., K.D. Warren, and J.M. Shrimpton. 2012. Spawning site selection in interior Fraser River coho salmon *Oncorhynchus kisutch*: an imperiled population of anadromous salmon from a snow-dominated watershed. *Endangered Species Research* 16: 249–260, 2012.

Mull, K.E., and M.A. Wilzbach. 2007. Selection of spawning sites by coho salmon in a northern California stream. *North American Journal of Fisheries Management* 27:1343–1354.

Thorstad, E.B., F. Økland, K. Aarestrup and T.G. Heggberget. 2008. Factors affecting the within-river spawning migration of Atlantic salmon, with emphasis on human impacts. *Reviews in Fish Biology and Fisheries* 18:345–371. DOI 10.1007/s11160-007-9076-4. <http://www.lakseelver.no/Nyheter/2008/oktober/Thorstad%20et%20al%20migration%20review.pdf>

USGS, National Water Information System, Surface-Water Data, available at <http://waterdata.usgs.gov/nwis/sw>