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November 11, 2011

Dave Jepsen
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Dear Dave,

In your July 20, 2011 letter you requested the Independent Multidisciplinary Science Team (IMST) to review and assess the scientific and technical underpinnings of the Oregon Department of Fish and Wildlife's (ODFW) document titled *Conservation Plan for Fall Chinook Salmon in the Rogue Species Management Unit* (July 2011 draft).

The IMST's completed review is attached. In the review, the IMST offers several constructive comments on overarching and specific issues. IMST realizes that this draft conservation plan represents a 'work in progress' intended to function as a living document that ODFW will revise on a regular basis as new information and tools become available. IMST offers these review comments with the intent of aiding ODFW efforts to better identify information needs and filling data gaps. By doing so, ODFW will strengthen the scientific basis of the conservation plan and better assess the long-term trends of fall Chinook populations and their habitat in the Rogue River basin. This review does not include any formal recommendations.

Please feel free to contact us if you or the plan's authors have questions or concerns regarding this review.

Sincerely,

Carl Schreck
IMST Co-Chair

Nancy Molina
IMST Co-Chair

cc: Ed Bowles
Tom Satterthwaite

IMST Review of Oregon Department of Fish and Wildlife's *Conservation Plan for Fall Chinook Salmon in the Rogue Species Management Unit* (*July 8, 2011 draft*)

Released on November 11, 2011

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Citation: Independent Multidisciplinary Science Team. 2011. IMST Review of Oregon Department of Fish and Wildlife's Conservation Plan for Fall Chinook Salmon in the Rogue Species Management Unit (July 8,l 2011 draft). Oregon Watershed Enhancement Board, Salem, Oregon.

Review Preparation: This review was prepared by the IMST based on an initial draft by an IMST subcommittee (Bob Hughes, Carl Schreck, and Vic Kaczynski with Kathy Maas-Hebner providing administrative support). Tom Satterthwaite, Dave Jespsen, and Kevin Goodson (ODFW) were present at the IMST's October 31, 2011 public meeting to discuss the IMST's draft comments and to provide clarification of ODFW's process. The IMST discussed initial review comments at its October 31, 2011 meeting and the review was unanimously adopted at its October 31, 2011 meeting.

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ACRONYMS AND ABBREVIATIONS

C	Celsius
CHF	fall Chinook salmon
ESU	evolutionarily significant units
IMST	Independent Multidisciplinary Science Team
MSY	maximum sustained yield
NP CHF	naturally produced fall Chinook salmon
ODFW	Oregon Department of Fish and Wildlife
PVA	Population Viability Analysis
SMU	species management unit
USACE	US Army Corps of Engineers
USGS	US Geological Survey

INTRODUCTION

The Independent Multidisciplinary Science Team (IMST) reviewed the document titled *Conservation Plan for Fall Chinook Salmon in the Rogue Species Management Unit* (July 8, 2011 draft; hereafter referred to as the "Plan") at the request of the Oregon Department of Fish and Wildlife (ODFW; letter from Dave Jepsen dated July 20, 2011). This review addresses whether the Plan's approach and analyses are credible and consistent with accepted scientific standards, whether Plan assumptions are supported by best available science, and whether uncertainties are characterized adequately. IMST approached these issues from the perspectives of ODFW's Native Fish Conservation Policy goals.

We appreciate the opportunity to offer our comments. There are strong aspects to the plan, and IMST compliments ODFW for its serious effort. We particularly appreciate the holistic approach to the overview of biological mortality factors. We also appreciated the fact that two very conceptually different population models were used in the analyses. This Plan is a marked improvement over the Rogue spring Chinook plan that the IMST previously reviewed. We had a limited time to provide this review, and hence we could not get into specific background material used in development of the Plan to ensure accuracy of statements, etc.

This review consists of three sections, *Overarching Issues*, *Major Comments*, and *Specific Comments*. In the *Overarching Issues* section IMST provides extended discussion of several topics that arose repeatedly during review of the Plan. The majority of concerns raised in this section are relevant not only to the Rogue fall Chinook Conservation Plan but also to forthcoming conservation plans and future plan revisions. Subsequent sections contain detailed comments that are directly relevant to the Plan. Many topics discussed in the *Overarching Issues* section are revisited in subsequent sections of this review. The purpose of this repetition is to highlight where these overarching issues arose in the Plan and to provide additional details and examples relevant to those issues.

OVERARCHING ISSUES

Fall vs. Spring Chinook

The classification of fish into *fall Chinook salmon* versus *spring Chinook salmon* appears arbitrary. To be scientifically rigorous the Plan really needs a discussion of the importance to productivity of the segment of the population of Rogue fall Chinook salmon that is present in the system prior to August 15th. What risk to the adequate management of fall Chinook salmon is there by not including this segment of the population in the Plan? The fall Chinook salmon in the Rogue appear to be one of the ESUs where molecular taxonomy can classify fish into spring and fall runs. It seems that such an approach should be built into monitoring programs for the fish to ensure that the right fish are being considered at the right times. Further, perhaps such monitoring data could be used to more accurately classify fish into spring or fall runs than results from a fixed date. IMST suspects that the timing of spring and fall runs can shift considerably among years and with climate change. If a fixed date is used, there will be years when the population status is overestimated and others where it is underestimated, which could contribute an unreasonable amount of error. In

addition, it would be helpful if the Plan could address the question of possible hybridization between spring and fall fish. Given that fall Chinook salmon in the Rogue system produce some yearling smolts that migrate in spring, more like a spring Chinook salmon phenotype, does this suggest considerable hybridization or phenotypic plasticity such that fall Chinook salmon can produce spring Chinook salmon and/or vice versa? It would provide a more scientific basis if such genetic considerations were fully developed. We know that it takes very few strays to introgress into a population that are critical for maintaining population genetic variation. There is some genetic information available suggesting discreteness of the fall and spring taxa in the Rogue system (presentation made to the Rogue River Advisory Committee in 2005 by Renee Bellinger). It would be useful if this information was discussed. We believe that this presentation also brought up the notion of "mid-run" fish that could be useful to consider relative to the questions raised above.

Additionally, the fate of the Rogue River fall Chinook salmon seems to be inevitably intertwined with the Rogue River spring Chinook salmon (page 52). Not all actions to benefit fall Chinook will be beneficial to spring Chinook salmon that inhabit the same watersheds, and vice versa. So shouldn't these races be considered in a single integrated conservation plan, perhaps with other interacting species? It seems as if fish conservation plans and priorities would be more rational if the plans were integrated.

SMU and Populations

Over the Rogue River basins, the Plan found that certain populations of fall Chinook oscillate up and down together. The Plan proposes to use the same management criteria for all Rogue populations. This approach seems risky, defeating the purpose of defining populations in the first place, and may not meet the objectives of the Oregon Native Fish Conservation Policy. The conclusion was drawn several places in the conservation plan that because these oscillations are correlated, management practices that favor one population in a given basin would also be beneficial for another population in a related basin. While this seems plausible, it is also likely that the specific detrimental human activities might differ among sub-basins. The intensity of motorized boating, rafting, loss of riparian vegetation, urban expansion, intensity of runoff, forest management practices, poor control of irrigation-induced erosion and agricultural runoff, etc. may pose vastly different pressures in the various sub-basins. Different negative pressures might be differentially managed from place to place. Therefore, different management options may be really important for fish populations from basins that nevertheless, oscillate up and down together as influenced by overarching climatic and ocean conditions (e.g., page 17 paragraph 2, pages 23–24, and page 100 paragraph 4 lines 8–12).

Additionally, the Rogue populations appear to be based on simple USGS hydrologic accounting units. If so, the population designations have no basis in ecology, biology, or hydrology (see Omernik 2003). Unless there are clear and substantial genetic and life history differences among populations, perhaps all three mainstem populations should be considered as 1. Table 1 (page 8) denotes unexplained different characteristics, so there may be genetic and life history differences among the populations that may be significant. The Plan acknowledges the uncertainty of population identification when discussing research needs and when considering dependent versus independent populations (page 8). It would strengthen the scientific credibility of the Plan if these differences were more clearly explained and described.

It seems as if the Applegate and Illinois are likely to be independent populations. Other plans that we have reviewed have classified fish in such widely separated areas as distinct populations. It is unclear why they are not monitored and treated as such.

Additionally, fall Chinook salmon in the Rogue and Klamath basins are "mostly grouped in the same Evolutionarily Significant Unit based on genetic assessments"(page 66) and population numbers appear to co-vary. Therefore explanation is needed for why it is scientifically defensible to exclude the Klamath fish from the Rogue conservation plan. If both populations are essentially one gene pool moving through time, then the Rogue is just one stratum of an ESU. Might it be more scientifically sound to consider Rogue and Klamath populations similar to the way separate, equally major basins were treated in the mid-Columbia steelhead recovery plan? If not, why not?

Monitoring

IMST is concerned that Rogue Chinook salmon populations and Coastal Chinook salmon populations in the Rogue SMU have been and will continue to be inadequately monitored under the proposed conservation plan. In several instances (Middle Rogue, Upper Rogue, Applegate) the authors have stated that because no monitoring has been done or that the Gold Ray Dam was removed (page 17) they cannot recommend management criteria for a particular population, therefore Huntley Park would be used as a surrogate. This is not a scientifically sound reason for not drafting interim management criteria based on known habitat and biological conditions affecting those particular populations. Even after stating that there is inadequate data for these populations, the Plan neither identifies key indicators that need to be monitored within these population nor does it propose any new monitoring efforts.

Apparently ODFW is proposing to estimate the entire Rogue natural fall Chinook salmon population by seining at Huntley Park. What is the error in such a survey and what is the rationale for such limited sampling, especially given the value of the Rogue fishery and the differences in run timing among populations? Management of the Rogue SMU populations is dependent primarily on Huntley Park monitoring. Without consistent monitoring methods applied consistently throughout the basins and in the ocean, it will be extremely difficult to conserve and manage those populations in a scientifically rigorous manner. It is impossible to have adaptive management without rigorous monitoring. It is particularly troubling that ODFW does not appear to include improved monitoring in any of its management alternatives, nor does it appear to propose monitoring the effects of its proposed management actions. Not until the monitoring and research needs sections is monitoring proposed; perhaps the needs for more monitoring could be indicated as they arise by referencing the later sections. Also, see Firman & Jacobs (2001), LaVigne *et al.* (2008a,b), Roni *et al.* (2008), Bouwes *et al.* (2010), Anlauf *et al.* (2011) and Hughes *et al.* (2011) for pertinent freshwater survey designs, methods, and indicators. Based on the information presented in the Plan, ODFW has not made a strong case that it can meet its claim on page 134 that it will use results from its monitoring, evaluation, and research efforts to determine the efficacy of management strategies and actions that are outlined in this conservation plan.

It also appears to us that a habitat monitoring plan more extensive than presented (page 129) is warranted to ensure that the various habitats and habitat components are not degrading. In the face of the lack of actual fish population monitoring, it appears critical to us that the extent and quality of spawning habitat be assessed periodically. This is particularly so because temperature appears to be such an important population regulatory factor affecting spawners and nesting and rearing success. The only reference to spawning habitat we found in the Plan was a 1944 report on gravel

distribution. Given the construction and removal of dams since the time of this study and other changes to the landscape, we do not believe that it is scientifically defensible to assume that the physical attributes of habitat have remained stable for nearly 70 years. Also, explain why temperatures at Agnes are sufficient for reflecting thermal conditions in the upstream basin. Explain why it is sufficient to monitor shade once every 10 years, etc. Under the "evaluation needs," the need for surveys is mentioned (item 3) but exactly what will be surveyed, where and what sampling design will be used? There is also no discussion regarding how such monitoring/sampling plans would be formulated in the future. The use of the ODFW survey methods (item 4) clearly adds scientific credibility to this section for wadeable streams—but not for nonwadeable rivers. A discussion of why redd success and juvenile abundance monitoring are not important evaluation needs could help also.

It is important to recognize that rigorous monitoring can be a costly enterprise in terms of dollars and human time; however inadequate monitoring is costly in terms of ignorance of the resource and potential eventual listing or loss of the resource. Listings and lost resources limit the recreational and commercial value of the resource, increase management costs, and restrict other human activities that might affect the threatened resource. In other words, protection of a resource generally costs less than the herculean actions often taken to recover a resource from a depleted state. This is the fundamental principle of conservation (and conservation plans), but such plans can only be as good as the monitoring and adaptive management employed in plan implementation. An agency concerned with a resource such as a fall Chinook salmon population and its environment must balance the costs of insufficient monitoring against the costs of rigorous monitoring. Insufficient or inappropriate monitoring produces poor data upon which to base management decisions; overly rigorous monitoring and an excessive number of inappropriate indicators are too costly to sustain through time and space. Thus, in designing and implementing a cost-effective monitoring program, it is important to recognize that a good one is neither fast nor inexpensive, and it requires a clear set of management objectives as regards assessment questions, geographic scope, study populations, and indicators (Hughes & Peck 2008; IMST 2007, 2009).

Presentation of Monitoring and Modeling Results

The Plan would be greatly strengthened scientifically by a discussion of the sources of error in the various parameter estimates used in the two population models. Further, what such error means in terms of confidence in management actions needs to be considered. We suggest an approach similar to that used in the upper Willamette Conservation Plan for Steelhead and Chinook Salmon. Given such large error bounds around the Huntley Park estimates, it would be very easy to miss a population crash suggesting increased risk to the population, much like the decline of the Oregon coastal coho salmon (e.g., Hughes *et al.* 2000).

While the regressions for the Huntley Park estimates with spawner abundances appear to be quite good, they are driven by a few data points to the far right and display considerable variation to the left. We suspect that analyzing the data unimodally may be misleading and that a two compartment model may be more appropriate.

On figures 5, 35, 42, 55, and 56 most of the data points lie outside of the 95% confidence intervals. Can the confidence intervals still be accurate? Other graphs are missing confidence intervals and annual estimates of population sizes should include confidence limits. See Figures 3, 6, 9, 10, 14, 16, 18, 19, 21, 24, 25 and Table 18. The absence of sufficient sampling or adequate sampling designs is NOT a satisfactory reason for omitting confidence limits around estimates based on data.

Even if the estimates are professional guesses, such as weather forecasts, some sorts of ranges in those guesses are appropriate.

At the end of the SMU status section, it would be useful to have a summary stacked line graph relating the estimated mean annual abundances of each SMU population.

Better support is needed, coupled with freshwater, estuarine and oceanic monitoring data, for continued hatchery releases. See Chilcote *et al.* (2011) for modeling results and discussion of the impacts of even low levels of hatchery fish on wild salmon and steelhead recruitment. IMST's concern goes well beyond potential genetic effects. It does not seem that fisheries amplification warrants the potential risks of increased competition and increased bycatch of wild fish as a result of hatchery production. Historically, a substantial fishery persisted on wild fish without hatcheries. It would be helpful to explain what has changed to require the considerable risk and expense of continued hatchery operations in the basin.

Land Use Impacts

Some but not all of the matrix of land management tradeoffs are listed in the fall Chinook management plan. For example high forest cover, timber harvest, and fire suppression can alter water storage capacity, total forest water use, peak flows, total flows, soil disturbances, and nutrient losses and to different degrees. The degrees of these different effects are all part of management and all affect stream flows all year. See paragraph 2 on page 69 where all land management factors are discounted.

For example, what significant water (and sediment, nutrient, and thermal) return flows occur from agricultural fields and ditches in the various basins? What is the potential to reduce return flows?

For example, the middle of the Rogue River is identified as an area of sparse spawning habitat (page 18), but isn't this section of the river also subject to frequent motor boat activity as well as rafting and urban encroachment?

Demographics, Climate Change and Adaptive Management

While the Plan mentions human demographics, climate change, and adaptive management, it does not deal with them in a sufficient manner. The effects of the large projected increases in Oregon's population and land use changes combined with effects of climate change and ocean regime shifts need to be considered if long range forecasts are to be scientifically credible. Additionally, given that climate change and human population increase each exert differing pressures on habitat, it is important to consider these issues not just jointly, but also separately and carefully, when projecting consequences for salmonid population viability. Incorporating such considerations explicitly are important elements of any adaptive management or conservation plan.

The Plan does discuss climate change specifically in a focused section (p. 107), and makes a reasonable effort in projecting the effects of climate change. The use of a study with models specific to the Rogue River basin is helpful, although it would also be good to corroborate that approach with relevant peer-reviewed literature (e.g. Markoff & Cullen 2008). Integrating those projections within specific sections throughout the Plan would strengthen ODFW's ability to anticipate localized changes.

Further, the pressure from human population increase should be emphasized more clearly in the Plan. Population increase and land use change will affect streamflow, water quality, physical habitat, and the presence of toxic chemicals including those present in pharmaceuticals personal

care products, among other damaging effects to the aquatic environment (IMST 2010). While the Plan does mention some of these effects within specific sections (e.g., 3rd paragraph, p. 51, regarding expected water demand increases in Jackson County), there is not a focused section drawing attention to the projected effects in the Plan, and more could be done to also examine projected changes on these parameters due to population increase and land use change throughout the Plan.

For confidence in success of the Plan, it is necessary to know if there is an adaptive management plan and, if not, how and when one will be formulated. As described in IMST (2011), "a formal adaptive management plan would give more confidence in the approaches proposed by ODFW. Goodman *et al.* (2011) provides a concise description of adaptive management, that could be used as a model for a Rogue Chinook salmon adaptive management plan. Based on Goodman *et al.* (2011; pp 23–24), an effective adaptive management plan should include *(1) a series of explicit expectations, models and indicators to evaluate status and trends and (2) explicit loops from monitoring results back to key trigger points. This approach forces managers to consider how they will measure and report results and how and when they will determine whether actions are successful or not. Key adaptive management elements include the following:*

- *Explicit statements of problems, objectives and goals, with trigger points and possible alternatives at those points described in advance.*
- *Clear conceptual models of processes of concern, and simulation models supported by data.*
- *Clear results of predictions and performance indicators from the proposed actions, along with potential alternatives if expectations are not met within explicit confidence bounds.*
- *A rigorous monitoring and assessment program with periodic analyses for evaluating progress and selecting alternative actions.*
- *A research and management team to evaluate results and, when needed, to revise goals, objectives, or actions. The team should be led by a Chief Scientist responsible and accountable for leading the program.*
- *An adequately funded lead agency willing to implement the recommended changes.*

Program duties include stimulating public discussion of scientific issues; facilitating rigorous peer review of important documents; supplying public and scientific reports; managing open retrievable databases; and revising models for continuous analysis and assessment."

Fisheries

Management with a focus on fishery production, i.e., increasing harvest in good years and decreasing it in poor years, means that salmon populations will fail to enrich spawning grounds with carcasses. Are the available spawning and rearing habitats fully seeded to allow increased fisheries? Is there any evidence of density dependent juvenile mortality or growth? Salmon evolved to gain periodically from excess enrichment as did their prey base. Fisheries management generally has failed to recognize this except in some Alaskan fisheries. Continuance of such practices effectively reduces the peaks in Lawson's (1993) conceptual model and ensures lower valleys.

IMST suggests that ODFW investigate the possibility of 2 or more sets of spawner-recruit and MSY curves. Such families of curves usually indicate years when environmental conditions and productivities (e.g., ocean temperatures and upwelling strengths, precipitation) differ markedly. This is often the case in natural ecosystems, and understanding the environmental drivers for lower and higher curves leads to better ecological understanding and more flexible and conservative management. Although it is a common practice, it seems highly unlikely that anything as complex as recruits per spawner in freshwater and salmon production in the ocean can be adequately graphed, understood, or managed by a single curve.

Overall Completeness of the Plan

It is probable that the actions enumerated in the Plan as "ODFW actions and support" alone will be insufficient to assure the survival of the Rogue River fall Chinook. There seems to be little broad connection between current citizen efforts, representative public involvement, and this Plan. The advisory committee for this Plan appears limited or selective and may have lacked participation of a sufficient number of citizens up stream who might have to lead the reconsideration of upstream behavior to make the Plan a success (e.g., urban planning, wastewater management, agricultural and forestry practitioners, watershed council leaders, and a representative of the USACE). The Rogue River area contains many watershed councils that are implementing restoration projects, but they do not seem to be sufficiently engaged here. Perhaps this concern could be alleviated via an Appendix listing the types and degrees of citizen involvement in the Plan (e.g., advisory board, public hearing locations and participants, reviewers, etc.).

Any fish conservation plan should not only consider the way that human activities affect the water supply and quality in the river but also seek to educate, to build an ethos, and to shape a political consensus to undertake specific actions. Watershed function depends on the capture, storage, use, and release of precipitation. Land management affects how the landscape stores and releases water and how vegetation covering the landscape uses water. Ultimately these factors affect river function and performance and they rely on broad community cooperation. Although community cooperation is central to the Plan's success, it is only enumerated in the conservation plan with no apparent involvement or "buy in" from the larger public.

GENERAL COMMENTS

- It would be very useful if the patterns of the population estimates for the various populations were compared. In other words, do estimated abundances for the various populations co-vary? If not, then estimates derived from the Huntley Park sampling may not be as robust as expected. Similarly, how do the estimates from the Huntley Park data (Figure 9) compare to those from the salmon-steelhead card returns (Figure 10)? If the two data sets do not co-vary, then which one is more correct and what might this mean for the Plan? In figure 10, do the harvest estimates for the total and the estuary catches co-vary or are there times when they do not? If the latter, then what does this mean for the Plan?
- It would be useful if more discussion were provided to demonstrate that two generations were sufficient to represent population status (page 101). The other recovery /conservation plans that we have reviewed all consider much longer timeframes and it would be good to explain why the Rogue would be different.

- Regarding the implementation actions, the wording of many does not suggest that a particular action will actually lead to the desired effect. Words such as “recommends”, “requests”, “develops”, “supports”, “manage”, “initiates”, etc. are all vague and open ended. They give no insight into how serious an attempt will be made to actually achieve the action or the extent of the action. Will there be follow up? What happens if the USACE ignores the requests, how extensive and intensive is the “support”, and how sufficient is the “development” and “support programs” in terms of staff and funding, etc.?
- The Plan appears to consider hatchery fish a minor problem in the basins, and no discussion is offered that hatcheries could become a future problem as basin conditions change (see Chilcote *et al.* 2011). Also, there is no indication in the Plan concerning the percentage of hatchery fish that have been marked over time, so estimates of spawning fish of hatchery origin could be significantly underestimated or overestimated.
- There is insufficient discussion of the local and cumulative effects of instream and floodplain aggregate and gold mining in the Plan. Given that California has recently banned suction dredging for gold from salmonid streams, this activity may shift northward from California, and along with current mining in the basin, lead to potentially substantial cumulative effects on salmonid spawning and rearing success.
- It is unfortunate that key Rogue research is not published in scientific journals (e.g., Amandi *et al.* 1982; Fustish *et al.* 1988; Martin 1973; 1975; McGie 1968; 1969; Rivers 1964; Reimers *et al.* 2001; ODFW 1990; 1992; 2000; Sattherthwaite 1995 as cited in the Plan). Journal publication would add considerably to the strength of the statements made in this Plan.

Overall Document

IMST feels that the document would benefit from a careful editing to correct grammatical, spelling, and logical errors. The IMST did not have sufficient time to list specific editorial comments in this review. Additionally, the use of appropriate fonts for table and figure captions would prevent these from blending in with the text. Table captions are often split across pages or not on the same page as the table they are describing. Only a few examples of areas needing careful editing are presented here:

- The terms “spawning escapement”, freshwater escapement”, and “reach conservation criteria” are not defined within the body document or in Appendix A. Definitions.
- Agency jargon increases verbosity and detracts from the document’s overall readability by non-agency people. For example, on pages 16, 20, 29, 32, and elsewhere, the phrase “multiple metrics that provide context related to” could be deleted and “summaries of these metrics” could be replaced with something more understandable by laypersons such as “summaries of these population estimates”.
- The words “fishery” and “fisheries” are used with different and often vague meanings throughout the texts. For example, the word “fisheries” adds little to page 4, 4th paragraph, line 1.
- Check that terms are defined before the acronyms are used. For example, define MSY as maximum sustained yield, then use the acronym consistently throughout the manuscript.

SPECIFIC COMMENTS

Not all of the pages in printed version and the electronic version of the draft Plan provided by ODFW were consistent. Some text in the electronic version was on later pages than in the printed version. This section refers to the page numbers as they were shown in the printed version.

Page 6, 2nd paragraph – Revise this text given that Gold Ray Dam and its counting station were removed in summer 2010. How does removal affect ODFW's classification of spring and fall Chinook salmon?

Page 8, Table 1 – The letters in the Table 1 columns need explanation. This could help establish the justification for multiple independent populations beyond a pure geographic delineation.

Page 9, Table 2 – The table heading indicates results are for hatchery fish when at least 50 tags were recovered between 1973 and 2003. But the number of tags recovered totaled 78, meaning only 1 year of data or random effects. The fish originated from hatcheries, but the Plan is for natural fall Chinook salmon. This table does not make a strong case for life history differences among populations of natural fall Chinook salmon.

Page 9, Table 3 – It seems highly questionable that a difference of 3–18 miles would constitute a significant difference in life histories in a species capable of swimming much greater distances in a few hours.

Page 11 – Given the importance of flood flows for maintaining and improving spawning and rearing habitat for Chinook salmon it seems inappropriate to imply that these dams do not impair Chinook salmon habitat, life history, spawning, rearing, or migration. Explain briefly how the dam water releases enhance Chinook salmon and their habitats.

Page 12, Table 5 – Provide data indicating that the listed flow manipulations had overall non-detrimental or positive effects on Chinook salmon.

Page 15, Figure 2 – Given that Gold Ray Dam was removed, provide justification for separating Upper Rogue and Middle Rogue populations at that location.

Page 15 – It would be better to make the text clear that Lost Creek Dam in now called William Jess Dam and clearly label Figure 2 with "Lost Creek" in parentheses behind William Jess Dam. Also, it would be helpful to locate the middle range of the Rogue River Canyon (mentioned on page 18).

Page 16, Figure 3 – Given that these are numbers are estimated, provide the confidence intervals around the estimates.

Page 17, Figure 4 – Given that the Upper Rogue population is deemed an independent population, greater justification is needed for not monitoring it than the absence of a fish counting station. There are alternative methods for making population estimates, such as mark/recapture, redd counts, screw traps, etc. that ODFW uses in other systems. It seems risky to assume population estimates from Huntley Park are appropriate for Upper Rogue populations; 50% of the existing points are outside the 95% confidence intervals and the data only cover 20 years.

Pages 17 and 21 – Cross referencing the derivation of Recruits/Spawners, ocean harvest rates and freshwater harvest rates that appear in Appendix Table E-4 would aid readers' understanding of this information.

Page 18, Line 1 – 20 miles bracketing river mile 48 is unclear. Is this 10 above and ten below or 20 above and 20 below?

Page 18, Figure 5 – The cessation of Middle Rogue spawning surveys is given as the reason for proposing no specific management criteria for this population. It seems risky to assume population estimates from Huntley Park are appropriate for Middle Rogue populations; 57% of the existing points are outside the 95% confidence intervals and the data only cover 14 years.

Page 19, last paragraph – How good is the assumption that there are "…no difference in the age composition of NP CHF that returned to the Lower Rogue population area and all NP CHF that entered the Rogue River"?

Page 21, Figure 7 – It seems risky to assume population estimates from Huntley Park are appropriate for Lower Rogue populations; 45% of the existing points are outside the 95% confidence intervals and the data only cover 21 years.

Page 21, 1st paragraph – ODFW assumes that those populations not monitored are adequately covered by management actions for the aggregate populations on NP CHF which is a risky assumption without further knowledge about those putative populations.

Page 22 – Given the importance of the Applegate population, it certainly would be useful if the adult monitoring could be resumed.

Pages 22–24, and Figure 8 – Although these data may offer more support than many others as a basis for fishery management actions, it seems risky to assume population estimates from Huntley Park are appropriate for Applegate populations. The other putative populations corresponded well with the Huntley Park seining, but the Applegate population did not: 40% of the existing points are outside the 95% confidence intervals, the data only cover 15 years, and the relationship only explains 48% of the variability (a moderate correlation).

Pages 23–24 – It seems risky to assume population estimates from Huntley Park are appropriate for Illinois populations given the lack of Illinois data. Only the Lower Rogue spawning population was monitored and thus it seems unwise for ODFW to develop management actions for the other populations directly from those data alone. Where did the other 2 tags originate?

Page 28 – These numbers do not compute. How can 7 tags for 20–500 fish mean that 49% are hatchery fish?

Page 43, Table 15 – This is a very useful and informative table. The use of asterisks (or "stars" as stated in the caption) does not do not visually separate these from the rest of the list well. Ocean habitat, temperature, predators, competitors, and prey (consider adding to the list) conditions are manageable, they are simply much more difficult to do so than conditions in freshwater or estuaries. It would be useful to add physical habitat structure (e.g. large woody debris, substrate size) and flow regime (e.g., peak & low flow frequency, duration, and timing) to the list of habitat quality factors. What might the primary limiting factors be without using professional judgment?

Page 43 – Is spawning habitat patchy or continuous? How would the distribution of spawning habitat affect estimation of capacity?

Page 44 – Why would spawning gravel estimates from 1944 and 1955 be pertinent today after all the changes in logging, dams, push-up dams, water releases, and major storm events?

Page 44, Table 16 – Indicate how the recent removal of Savage Rapids and Gold Ray Dams might affect the distribution of spawning habitats. Although the miles of spawning habitat appear to be based on professional opinion, those opinions can still include ± estimates and they would be wise to include here.

Page 45, 1st paragraph – What is the cumulative impact of the known 20-plus push up dams on the system? What is the estimate of illegally constructed dams and diversions? And what potential impacts might they have on the system?

Page 45, 2nd paragraph – Indicate the area of increased spawning habitat resulting from low flow augmentation. Greater flows do not necessarily equate with increased spawning habitat, and the relationship is unlikely to be linear.

Page 48, 1st paragraph – Provide the extent of the riprap and aggregate extraction and the area of habitat increased by low flow augmentation and the Illinois ladder. It seems irrelevant that the former are outweighed by the increased habitat area provided by the latter.

Pages 48 and 52 – Is there any provision for estuarine rehabilitation (e.g., dike breaching), which could increase the habitat size and habitat structural complexity of the estuary? The small size of the estuary is likely not linearly related to its importance for migrating Chinook salmon smolts and juveniles. In other words, the small relative size of the estuary makes it a limiting factor.

Pages 49 and 57 – Unclear. If water temperature is the primary water quality factor influencing fall Chinook survival, and current management practices artificially result in net reductions in water temperature, this should be made clear from the outset. The context for most anticipated future management changes adding heat and providing a less beneficial environment would be clearer. Additionally, if high summer water temperatures are likely limiting factors for juvenile Chinook salmon in one paragraph, it seems as if low summer dissolved oxygen also would be a likely limiting factor in the preceding paragraph. Warm water temperature is probably a serious limiting factor but DO should co-vary with temperature. Has the interaction of temperature/food supply and growth been evaluated?

It is unclear how riparian vegetation removal increases temperatures in the mainstem Rogue River, because trees shade relatively little of a wide river. Or is the concern relative to small streams? More explanation is needed here on this issue. Also consider stream width, air temperature and stream flow. Retention of streambank trees provides more than shade: e.g., increased bank stability, allochthonous inputs, potential LWD, and perhaps floodplain sediment deposition.

Page 50 – Some discussion of pre-spawning mortality at temperatures below 19º C would be useful.

Page 50, Table 18 – The table caption indicates that the estimated mortality rates were for 1978–1986 but the table does not include any year after 1982. Why not list all years?

Page 52, Table 20 – It also would be useful to model a worse case scenario with the highest fall temperature (versus average). Incorporation of climate change model predictions of flow and temperature also would be enlightening.

Page 53 – Indicate the likely consequence of the Gold Ray Dam removal on the distribution of redside shiner and Umpqua pikeminnow.

It seems inappropriate to omit hatchery fall Chinook salmon from the list of potential competitors; given their close life histories, trophic, and habitat requirements, they would most likely compete with wild fall Chinook salmon in freshwater, estuaries, and the ocean.

Page 54, 2nd paragraph – There is no inference on how avian predation of fall Chinook changed after the introduction of redside shiners. The intent is left to the imagination of the reader. Was the illegal introduction of redside shiners intentional or an inadvertent mistake? Page 55, Table 21 and Page 83, Table 29 – Be more consistent in the predator species listing (some are listed by species, others by genus or water body type). Given its small mouth gape and body size, IMST finds it highly unlikely that reticulate sculpin predates juvenile salmon.

Page 58, Figures 30 & 31 – The data in Figure 30 suggests that the mathematical relationships are different than what has been calculated. It would be worthwhile to investigate the possibility of 2 sets of spawner-recruit curves: one as presented in the figure; the second incorporating only those points that fall above or on the current curve. Such families of curves usually indicate years when environmental conditions and productivities differ markedly. This is often the case in natural ecosystems, and understanding the environmental drivers for lower and higher spawner-recruit curves leads to better ecological understanding and more flexible and conservative management. It would also be useful to examine the square of the hatchery fish ocean survival rate because at low levels there may be a positive relationship and at high levels a negative relationship; again, it is unlikely that this is a linear relationship.

Pages 61 and 91 – As suggested above, it would be wise to examine the potential effect of hatchery fish on wild fish in the ocean, where both spend most of their lives and where food can be limiting and predators attracted. The absence of data indicating the effect of competition does not mean that it is not occurring. Hatchery fish affect natural fish through food and habitat competition as well as genetically.

Page 64, 1st paragraph – In the sentence starting "During years when less than 30,000 CHF return to the Rogue River, the freshwater fishery can harvest 10–20% of the run", does the word "can" mean that this **is** the level of harvest or that it is **appropriate** to harvest at that level?

Page 66 – High model precision offers little confidence when that modeling assumes constant ocean survival rates. IMST suggests considering including low, moderate, and high ocean survival rates in simulations because of the importance of ocean survival to Chinook salmon productivity.

Page 67, Figure 35 – Although Klamath and Rogue spawners are correlated, the former only explains 29% of the variability in the latter, indicating a weak relationship.

Page 68, 2nd paragraph – This is a good summary of limiting factors; it also would be useful to indicate the range in relative importance of the factors to aid ODFW in focusing on primary limits first. The high rates of gravel recruitment in the Chetco and Pistol Rivers are notable, but are the gravel bed loads stable? If not stable, spawning gravel area may be a limiting factor. A gravel bed load stability evaluation is needed.

Page 68, Table 24 – If the estimates are professional opinion, a likely range can be estimated.

Page 70, 2nd paragraph – Consider editing the line beginning "However, the values of this finding is somewhat academic..." to read "The value of this finding is confounded because stream flow in summer is also related to..." Also the same statement on page 73.

Page 73, Figure 38 – the data in Figure 38 seem to have been forced into a linear model yet they appear to show a non-linear functional relationship that is not graphed.

Page 75, 1st paragraph, lines 7–9 – As written, the statement is factually incorrect. Suggest editing to read" Mean daily maximum temperatures can reach or exceed 24ºC (75ºF) for several days..."

Page 75, 3rd paragraph – In lines 1–3, the Plan takes a general statement and apply it to a subset of items. This approach comes off logically flawed. Which streams have had riparian vegetation removed? Which ones of those have proven increases in temperature?

Page 76, 1st paragraph – Less shade and warmer stream temperatures are also associated with increased stream width.

Page 77, 1st paragraph, lines 3–5 – Why have these changes affected microhabitat?

Page 77, 2nd paragraph, line 7 – Words are missing. Edit to read "Based on <u>a</u> score... is <u>rated</u> from 10..."

Page 77, 3rd paragraph and Table 27 – An explanation of water sampling dates, durations, depths, locations, etc. would help the reader understand the representativeness of the temperatures reported here. Otherwise the reported conditions seem to be inconsistent with the climate.

Pages 79 and 80, Figures 39 and 40 – There seem to be too many data points on these graphs for too few site years. This is confusing.

Page 80 – Might the observed relationship between higher densities of juveniles and earlier downstream migration suggest density dependent food or habitat limitation?

Page 82, *Competitors* section – Indicate whether or not hatchery fish compete with juvenile Chinook salmon.

Page 83, 2nd paragraph, line 1 – Suggest editing statement to read "In summary, at this time it in not known whether..." It may not be known now but it probably can be determined.

Page 86, Figure 44; Page 87, Figure 45; Page 88, Figure 46; and Page 89, Figure 47 – It might be informative to plot two Beverton-Holt and Ricker curves for each figure based on the arrays of the upper and lower sets of points.

Page 94, Figure 50 and Page 96, Figure 52 – It would seem wise to use % harvest as the predictor variable and escapement as the response variable. Again there seem to be at least 2 curves in both figures.

Page 95, 1st paragraph – Explain for clarity, line 1 "...for a greater number of years" than what? What is this fishery being compared to?

Page 99, Figure 55 – The graphed relationship only explains 19% of the variability, indicating a weak relationship.

Page 100 – The biological status section is quite vague, particularly from the fourth paragraph starting with "Abundance of adult…."

Page 100 – Given the strong correlations among spawners and Huntley Park catch, perhaps the upper, middle, and lower Rogue populations are only one population; the Applegate population shows only moderate correlation with the Huntley Park catch and may be a different population.

Page 101 – Table 32 would benefit from more explanation. Where does the number 55,000 as a criterion come from and why? What scientific basis is there for the percentages used in the other criteria?

Page 101, 2nd paragraph – Ten years seems too short a time to evaluate population status and it is likely to coincide with cyclic patterns in ocean conditions, thereby leading to false assumptions regarding freshwater management options and true population status and trends.

Pages 101–102 – IMST suggests that ODFW limit run composition to 0–5% hatchery fish for all populations—at least until the desired production criteria for wild Chinook salmon are met or exceeded for 100 years. The risk to wild fish from hatchery fish does not seem to support higher numbers or percentages of hatchery fish. Also, in all cases but the Chetco, hatchery fish are <5% currently (Table 38). A better rationale is needed for the high hatchery numbers in the Chetco, which is otherwise one of the least-disturbed basins discussed in this Plan.

Page 102, 2nd paragraph – Aren't "population growth" and "population productivity quite different?

Page 102, Table 32 – If coastal population criteria are presented, it would seem wise to also present specific criteria for Applegate and Illinois populations, which are much larger basins.

Page 103 – It would seem that recruits/spawner coupled with river flow, temperature and macroinvertebrate drift biomass & composition (see Boues *et al.* 2010), and selected measures of ocean productivity (e.g., zooplankton, forage fish biomass, composition) would offer considerable insight into productivity.

Page 104, 3rd paragraph – The Rogue aggregate of 950 Chinook salmon needs further explanation here than that it is for five populations.

Page 105, 3rd paragraph – "Results from PVA simulations"—what models were used? What is the error associated with the models? Were the models validated, and if so, how?

Page 106, 3rd paragraph, line 4 – How will increased urbanization affect overall water quality.

Page 106 – The discussion of persistence ignores decadal changes in the strength of the California Current with corresponding changes in sea surface temperatures, temperature stratification, planktonic productivity, species assemblages, and predators on juvenile salmon—all affecting juvenile salmon growth and survival. ODFW appears to assume only fishing mortality and river habitat capacity are important.

Likely, toxic chemicals (e.g., endocrine disrupters, copper) could be an equal or greater threat to Chinook salmon than altered flow regimes from urbanization.

It does seem likely that the predicted climate changes will occur over the next 100 years; therefore, those changes should be incorporated in PVA modeling.

Pages 107 and 108, Table 37 and other similar tables – Explain why the desired status is so much lower than the current status. Also, should not judgment of compliance be based on lowest

95% confidence interval, not the mean? Also, why is a 95% confidence interval appropriate rather than a more conservative confidence interval?

Page 107 – If the desired status of returns (7%) is out of compliance (8%), then shouldn't the conclusion be that the Rogue Stratum is NOT in compliance? It is more than 10% out of compliance. If so, are more protective or enhancing measures warranted?

Page 109 – Explain why the desired proportion of hatchery fish is so high in the Chetko population, especially given findings presented in Chilcote *et al.* (2011).

Page 112, Action 1.13 – ODFW needs to be cautious about recommending restoration of riparian trees for stream shade and stream coolness. Tree shade, on appropriate sized streams, can help maintain stream temperatures but can not be expected to reduce stream temperatures in wide reaches that have already increased in upstream reaches. Restoration of riparian and floodplain forests should be aimed at achieving all desirable functions (e.g., shading, streambank stability, creating and maintaining undercut banks, instream large wood)

Page 113, Action 1.13 and Page 119, Action 1.5 – What is considered mature riparian vegetation on a bar? By definition, bars lack trees. Are vegetated islands the desired future status of these areas? Are these expected to increase side channels?

Page 120, Action 1.14 – How does ODFW propose to enhance primary and secondary production, yet decrease nutrient flux into estuaries (Action 1.15)?

Page 121, Action 1.17 – Why not breach tidal blockages in estuaries?

Page 124, Management Strategy 6.5 – Why eliminate a natural cascade?

Page 127, Table 41 – It seems unreasonable to assume a single MSY curve or criterion given the marked periodic changes that occur in ocean productivity.

Page 127, Table 42, item 3. Specify what is <5%.

Page 127, Table 42 – A three year average of 10–15% hatchery fish in spawner composition for the populations seems excessive given the annual goal of <5% hatchery fish.

Page 128, Table 43 – A three year average of 10–20% hatchery fish seems excessive given the annual goal of <5% – <10% for all but the Chetco.

Page 129 – It seems advisable to monitor Upper and Middle Rogue, Applegate, and Illinois populations also. If each Coastal population is proposed for monitoring, why not monitor all the Rogue populations rather than depend on a single station?

Page 130, "Weekly Monitoring" – It is advisable and reasonable to monitor flows and temperatures continuously, rather than weekly. Concerning shade and gravel monitoring methods and survey designs, see Anlauf *et al.* (2011), Bouwes *et al.* (2010), and Peck *et al.* (2006; In Press); a 10-year return interval will hinder timely trend estimates. Universities and contractors are additional cost-effective options for the field work and timely data analyses. Current ODFW stream habitat survey methods are inappropriate for non-wadeable streams. See Peck *et al.* (In Press) for alternatives.

Page 131, Items 1 and 3 – It would be wise to monitor the effects of flows and ramping rates on Chinook salmon annually to reduce the probability of covariance effects resulting from monitoring only during low flow years.

Page 131, Item 4 – The ODFW habitat protocol is for wadeable streams; see Peck *et al.* (In Press) for methods for nonwadeable rivers.

Page 131 – ODFW needs recruitment estimates, which are difficult to obtain because of prolonged years of ocean rearing by Chinook salmon. But a running spread sheet of return age (based on relative lengths) numbers per year could aid in making such recruit estimates.

Page 131, Item 7 – IMST suggests that ODFW consider using a rigorous probabilistic ecosystem survey of the mainstem Rogue, Applegate, and Illinois Rivers about every 10 years. There are multiple chemical, physical, and biological variables that could be affecting Chinook salmon and their habitats, those mainstems have never received such a survey, recent removal of mainstem dams are likely to have system wide effects, and the proposed management and temporal changes are likely to alter river conditions further. See Peck *et al.* (In Press), LaVigne *et al.* (2008a,b) and Hughes *et al.* (2011) for examples.

Page 131, "Coastal Stratum" Item 1 – It seems wise to conduct such evaluations annually, not just every 15 years.

Page 134, "Reporting" – It would be beneficial to ODFW and other fisheries managers if some of these reports were also prepared for submission to peer review journals.

Page 156 – How good are the spawner counts in tributaries of the lower Rogue for estimating spawner abundance of the entire Lower Rogue population?

Page 162, 1st paragraph – Prespawn mortality of 2% is not high, but elsewhere in the text prespawn mortality is described as being high.

Page 162–163 – The estimates of spawning escapements have many assumptions and limited data. This potentially introduces errors in subsequent applications.

Page 166 – Spawning escapements for the Lower Rogue and Coastal populations appear to be over-estimates.

Page 167, 4th paragraph – Because of disagreements between biologists, a composite index of spawning habitat quality was developed by simply multiplying the score of each biologist. While this would yield some value, it would increase scientific credibility if some discussion was offered that convinced the reader that the resultant index was some reasonable approximation of reality.

Page 171–174. Explain why the assumed percentages of hatchery fish are relatively much higher than coded wire tag percentages would indicate.

Page 226, Items A8e and A8f – IMST does not support transferring hatchery broodstock into habitat not accessible to naturally migrating adults. See Chilcote *et al.* (2011) for the hazards associated with such practices.

Page 227, Item A11a – Because of the substantial potential loss of fish from unscreened diversions, IMST supports placing and maintaining fish screens or removal of diversions that fail to protect migrants.

Page 227 – The reason for Item A12c is not clear.

Page 227, Items A12e and A12f – Can the pinnipeds be trapped and moved elsewhere? Has relocation proved effective elsewhere?

Page 232, Item A1j – Because they restrict the natural movement of fish, water, large wood debris and bed sediments, IMST does not support dam/reservoir construction in the Chetco or Winchuck Basins

Page 233, Item A4a – Briefly explain how increasing channel complexity would help decrease the amounts of phosphorous and nitrogen (e.g., by increased nutrient spiraling rates).

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