# NATIVE FISH CONSERVATION PLAN FOR FALL CHINOOK SALMON IN THE ROGUE SPECIES MANAGEMENT UNIT

**Comments and ODFW responses on the Technical and Co-Manager Review Draft of July 8, 2011**

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Part 1. Review submitted by Dr. David Hankin, Humboldt State University, and ODFW responses
Part 2. IMST review and ODFW responses
Part 3. ODEQ comments and ODFW responses

# CONSERVATION PLAN FOR FALL CHINOOK SALMON IN THE ROGUE SPECIES MANAGEMENT UNIT

## FINAL REVIEW – David Hankin, 10 October 2011

**General Impressions and Major Concerns**

I was greatly impressed by the obviously very large (huge!) effort that went into developing this Conservation Plan and, for the most part, I find myself in support of many of the assumptions that were made to develop the calculated stock-specific values of spawning escapements and other attributes that are reported in the Plan. Although I feel that many calculations are right at the edge of "pushing available data too far", and a very few are "pushed too far", the conclusions that are reached seem generally well supported and in the practical world fishery managers are often faced with making decisions with poor or imperfect information. Overall, the authors are to be highly commended for having the nerve and energy to undertake such an ambitious task (merely assembling all the available data would have been a daunting task in itself, much less trying to make any sense of it all) and, as on previous occasions, I commend ODFW for taking these planning exercises so seriously.

The Fall Chinook Conservation Plan attributes the recent increases in apparent abundance of upper Rogue River and Applegate River fall Chinook salmon to increased control of water temperatures and flow at Lost Creek and Applegate dams. It is difficult to argue with the logic of this conclusion given the rapid response of these populations to dam operations. I remain concerned, however, by the Plan's recommendations to operate Lost Creek Dam (renamed William Jess Dam, I guess) so as to continue to favor survival and reproduction of upper Rogue fall Chinook salmon. My recollection is that I expressed concern about this same issue in my previous review of the Conservation Plan for Rogue Spring Chinook. If dam operations continue to favor increased production of upper Rogue fall Chinook, it is possible that the long-term integrity of the two racial phenotypes (spring and fall) may be compromised. In my local Trinity River system, construction of Lewiston and Trinity dams eliminated most upriver spawning habitat for spring Chinook, leaving spring Chinook no option but to spawn in the mainstem, immediately below Lewiston dam, where they interbreed with fall Chinook salmon that were previously present. The long-term prognosis for maintenance of distinct races in this situation is not good in my opinion. In the Rogue River, my understanding is that thermal barriers during late summer historically prevented fall Chinook from developing a large population in the upper Rogue, thereby preventing much exchange between spring and fall runs. Based on Figure 2 of the Conservation Plan, which indicates "primary spawning areas" for fall Chinook, it is my hope that my concern is misplaced and that most spawning of upper Rogue fall Chinook takes place far below spawning areas for Rogue spring Chinook. If, however, there is significant overlap in spawning locations of the two run types, due to further upriver penetration of fall Chinook resulting from operation of Lost Creek Dam, then I think this issue requires very serious consideration. It also points out the deficiencies in

development of conservation plans at the level of runs of specific species rather than at the watershed levels with at least all run types of a specific species being considered in the same plan. That, of course, is a planning issue that is well beyond the scope of this review! Nevertheless, I feel strongly that at least a brief discussion of this issue should be presented in the Conservation Plan and I'd like to see a figure which clearly illustrates potential overlap in spawning areas of spring and fall Chinook in the Rogue system.

*ODFW response: ODFW agrees that the draft CHF conservation plan should be modified to clearly address this matter, rather than depending solely on an executive summary to do so. During plan development, there was extensive consideration of this matter by both the public advisory committee and by ODFW, and the related options are conveyed in the draft plan in Table 39 on page 110. Both suites of management strategies, that received support from members of the public advisory committee and from ODFW, state that restoration of wild CHS production is of greater priority as compared to the maintenance of wild CHF production in the Rogue River Basin. As such, there is integration between the CHS and CHF conservation plans. To help clarify this matter, ODFW crafted and included a new section, and revised another section, within the conservation plan.*

I found what I regard as serious analysis flaws at only two points in the Conservation Plan. First, as typeset, the equation used for cohort reconstructions is in error. I have learned, from Tom Satterthwaite, that this is a typesetting error, so that means that cohort reconstructions will not need to be recalculated (thanks goodness!). However, it is very important to correct this typesetting error. *ODFW response: Typesetting error was corrected.* Second, the method used to measure recruitment (sum of catches plus escapements) is not an appropriate metric for a stock recruitment analysis and leads to errors in calculation of MSY harvest rates and spawning escapements. Recommendations for calculation of a more appropriate "adult-equivalent" recruitment metric are provided later in this review. Revised stock-recruitment analyses should be carried out using this adult-equivalent metric. *ODFW response: ODFW agrees with comment. Recruit estimates and the relevant analyses were revised, as well as relevant text within the plan.*

In preparation of this review, I first carefully read through the full conservation plan (July 2011 Technical Review draft). I made numerous suggestions for changes in the text (using track changes) that might clarify meaning and I made numerous comments in margins on various issues or concerns that were raised. I have provided my commented version of the Conservation Plan to Tom Satterthwaite as part of my preliminary review and I hope that it has been of value for revising and improving the Plan. I then went through the Appendix documents in a similar fashion. I did not devote much of my review attention to critique of the many recommendations for management actions. Instead, I concentrated my review on assessment of the technical basis and support behind these recommended actions. I judged that my input would be most greatly valued and most relevant at this technical level.

In my review below, I summarize what I feel are the primary issues or concerns I have with the Conservation Plan and its Appendixes. A large number of other relatively minor issues or concerns are raised in my commented versions of the draft documents. Also, my commented version of the draft contains many statements of agreement with proposed methods, etc., that are not presented in this written review. My formal written review concentrates on areas of concern and/or suggestions for possible changes or improvements in analyses or presentations in the draft Plan.

## CONSERVATION PLAN

1. Estuaries and the importance of large size at outmigration.

The Conservation Plan at page 80 and elsewhere makes a case for the importance of estuaries for fall Chinook in general and for the smaller south coastal stocks in particular. I have long been and remain a skeptic on the "critical" importance of estuaries for fall Chinook salmon. First, the fact that survival rate of smolts seems related to size at outmigration does not immediately cause me to believe that large and productive estuaries are necessarily required to support large and productive populations of fall Chinook. Indeed, the fact that outmigration in many coastal Chinook populations seems stretched out over many months, during which mean fish size increases, suggests to me that there is probably a life history "window of opportunity" trade-off in these fish: it is possible that survival conditions are good for early-migrating smaller juveniles in enough years so that there may be strong population advantage to this strategy. More small smolts entering the ocean at what may prove to be a favorable time may sometimes generate greater numbers of returning adults than a smaller number of larger smolts entering later in the summer or fall, even though the survival rate per individual of the smaller smolts may be lower. Second, many large river systems with robust fall Chinook populations, e.g. the Rogue River, do not have well developed estuaries, as the Plan acknowledges. What distinguishes the small coastal populations from those in larger systems? Finally, Volks recent work in the Salmon River estuary is for a north-migrating fall Chinook stock type in an estuary that has been the focus of extensive restoration and enhancement work. I take no issue with a contention that fall Chinook salmon will take advantage of good conditions for estuary survival and growth when they are available, but I remain a skeptic concerning the requirement of well-developed estuaries for robust fall Chinook populations. If freshwater rearing conditions are favorable and generate high quality smolts, then I think that is usually enough. Finally, the Plan recommends estuary habitat actions that are designed to decrease phosphorous and nitrogen that enter estuaries and to favor Chinook salmon. Perhaps a better objective might be to "restore naturally-functioning estuaries", recognizing that there are other important organisms present in estuaries that might be adversely affected by changes in delivery of phosphorous and nitrogen. Of course, my area of specialty is not estuaries, so my comments on this topic must be taken with many grains of salt!

*<u>ODFW response:</u> ODFW agrees that large and productive estuaries are necessarily required to support large and productive populations of CHF. The conservation plan conveyed that the*

*estuary of the Rogue River provides habitat for few juvenile CHF because juvenile CHF are able to rear to smolt size in the Rogue River. However, the conservation plan also conveyed clear evidence that a high proportion of juvenile CHF in coastal basins, other than those in the Rogue River Basin, enter the estuaries at lengths less than 9 cm. Repeated ODFW analyses of CHF scales from these basins clearly shows that adult survivors entered the ocean as juveniles at sizes greater than 9 cm in length. Thus, small juveniles must grow in estuaries in order to survive to reach the adult life history stage and small estuary sizes, coupled with evidence of marginal water quality in the estuaries, leads one to infer that these factors likely limit CHF production for the non-Rogue populations.*

*The conservation plan conveys that CHF populations in the small coastal basins are robust as evidenced by the very low estimates of extinction probabilities during the next 100 years. However, the conservation plan also conveys that in order to have a greater chance of ensuring attainment and maintenance of desired status of the populations, that more quality rearing habitat is needed in streams and estuaries for small juvenile CHF produced in the coastal stratum of the Species Management Unit.*

2. Water temperature effects

At page 50 and elsewhere, there is a discussion of effects of water temperature on growth and size at ocean entrance of juvenile fall Chinook salmon from coastal and Rogue management unit populations. In my commented version of the report I make some fairly detailed remarks on this subject. But the take-home message of these remarks is: beware of application of Brett's work on thermal tolerances of BC Chinook as they may provide little guidance for Chinook residing in streams where summer rearing temperatures may greatly exceed those in more northern areas. Also, there may be substantial inter-stock variation in thermal adaptations of Chinook within the same river basin (see recent terrific work on adult "thermal loads" that can be tolerated by different Fraser River sockeye stocks prior to successful spawning: Science 332:109-112, 1 April 2011, Eliason et al). In the lower American River, a tributary to the Sacramento, juvenile fall Chinook salmon rear in water temperatures at which, according to Brett's work, they should be dead and yet they are in superb physiological condition. These comments are NOT intended to imply that high water temperatures are desirable for rearing and/or spawning of Chinook salmon! Instead, they argue for some limited laboratory studies on juveniles from some south coastal fall Chinook stocks to determine if there is evidence for local thermal adaptation when compared to Brett's findings. This would make for some nice MS theses.

*<u>ODFW response:</u> ODFW agrees that generally accepted estimates of thermal tolerances for juvenile Chinook salmon are likely not reflective of local adaptations for NP CHF native to southern Oregon streams. ODFW retrieved and assessed recently published research of thermal tolerances of juvenile Chinook salmon in the southern portion of their native range. This information was incorporated into the conservation plan.*

4. “Harvest Rates vs Age-Specific Exploitation Rates

When I read through the Conservation Plan, I was concerned by the use of “over-brood harvest rates” as compared to age-specific “annual exploitation rates” in the context of a more complicated age-structured analysis. From an annual fishery management perspective, ocean fisheries generate age-specific ocean fishery exploitation rates. Any specified over-brood harvest rate could result from a very large number of combinations of age-specific annual exploitation rates in ocean and freshwater fisheries. Maturity schedules of individual stocks then determine how these annual age-specific ocean and freshwater fishery exploitation rates get converted to over-brood harvest rates (fraction of cohort that is removed by fishing over its lifespan). This subject is considered in more detail in my review of the Appendixes (methods used to fit stock-recruitment models). The text of the Conservation Plan should be revised, as appropriate, to make sure that there is consistent and correct usage of the terms “harvest rate” (reserve for over-brood removals) and “exploitation rates” (reserve for annual age-specific removals in fisheries).

*ODFW response: ODFW agrees with this comment. The conservation plan was modified to convey the explicit difference between the two ways that harvest rates are characterized.*

5. Inconsistencies in reporting and interpretation of freshwater CWT recoveries

I found inconsistencies in use of counts of CWTs found in stream surveys as indicators of straying rates or proportions of hatchery fish. In some instances, only counts of observed CWTs were noted, whereas in other instances (e.g. in Appendixes) observed CWT counts were always scaled up according to the fraction of releases that received CWTs. Reported observations of CWTs should always be consistently scaled up by marking fractions so that they do not misleadingly underestimate the numbers of hatchery fish corresponding to the observed CWT counts.

*ODFW response: ODFW agrees with this comment. The conservation plan was modified to convey that all assessments of straying rates, or the proportions of hatchery fish among spawners, were based on brood year expansions based on the proportion of the release groups that were CWT marked.*

6. Effects of fishing on recruitment

At page 65 and elsewhere, the Plan makes statements like: ”This finding indicates that mortality related to fishing likely affected NP CHF recruitment in the Chetco population area only during those return years preceded by low survival rates of age 1-2 NP CHF in the ocean.” I disagree with this statement as written and suspect that what was intended was more like: “….mortality related to fishing *likely had a substantially adverse effect on* NP CHF recruitment….”From a theoretical perspective, fishing has a very definite effect on recruitment via reduction in the potential number of spawners. At very low levels of exploitation, the Ricker curve predicts that fishing can actually increase recruitment. Not exceeding MSY harvest rates is a good thing, but does not imply that fishing has not affected recruitment. That would require a comparison of equilibrium unexploited

recruitment with that under MSY fishing strategies. My objection to this statement might be addressed by some modest changes in language. Or perhaps it would be better to drop this kind of statement and instead save the idea for the management recommendations which call for more restrictive ocean fishing regulations in years when apparent survival from smolt to ocean age 2 has been very low and "normally acceptable" levels of fishing might further reduce what are already predicted to be very poor returns of spawners. That is routine practice of the PFMC.

*ODFW response: ODFW agrees with this comment. The conservation plan was modified as per the recommendation.*

7. Errors in variables issues.

There are many plots and fits of data that have probably very large but unknown errors in both Y and X, e.g. proportion of Pistol River and Winchuck River smolts outmigrating during June vs total smolt abundance (Figs 41, 42). When both attributes have very large errors, this generates quite a mess for application of standard least-squares regression methods which formally require that either X is known without error or that measurement + process variation in Y given X dwarfs measurement error on X. Also, in the particular case noted in this paragraph, the estimated proportion migrating in June is based on some of the same data incorporated in the X variable, and Elk River data are somehow also incorporated. Many complicated statistical issues are raised (e.g., see some of the literature on "errors in variables"). It's tough to know what to make of these relationships without having a more detailed understanding of the nature of the values plotted on the graphs.

*ODFW response: ODFW agrees with the primary point of this comment. There are multiple assumptions associated with regression analysis. One of these assumptions is that independent variables are measured without error, a virtually impossible requirement when including biological data as independent variables. During development of the draft conservation plan, ODFW considered other analytical options including correlation analysis and non-parametric analyses and concluded that the benefits of employing regression analysis out-weighted the benefits of the other potential analytical approaches. The underlying assumption associated with the application of the regression analyses is that the estimation error of the independent variable is relatively small as compared to the error associated with the estimation of the dependent variable. In relation to this assumption, additional information was added to the conservation plan.*

*However, ODFW also concluded that there is value in reporting, and showing, regression lines and associated confidence intervals. Inclusion of these analytical results and allied data plots, affords plan readers the chance to assess the validity of the postulated relationships; and this approach has been recommended by other reviewers of ODFW fish conservation plans.*

8. Measures of comparative population productivity.

The discussion of population productivity (p. 105, p. 57) was a bit confusing to me. To me, the very best single measure of population productivity would be something like the estimated Ricker stock-recruitment model productivity parameter (typically called "alpha") which measures the recruits per spawner at low population size under average environmental conditions. (See Parken, McNicol and Irvine 2006, their Table 2, for a comparative listing of calculated alpha parameters for various Chinook stocks.) The Plan appears to consider using average values of recruits/spawner as an indicator of productivity for different populations, but these values would be expected to vary substantially due to inter-annual variation in ocean and freshwater conditions for survival and due to among stock variation in stock status (i.e., spawner abundance compared to, say, stock-specific spawner abundance for MSY). As shown in Hankin and Healey (1986) and elsewhere, given alpha and conditional maturation probabilities, one can calculate MSY ocean exploitation rates and over-brood harvest rates and one even can construct a "full model" that also has river exploitation rates. So long as these calculated MSY harvest rates are not less than those that are currently faced by Chinook, then the stock-specific alpha parameters could be regarded as demonstrating "adequate productivity". If the estimated alpha were not large enough to support these levels of fishing, then increased productivity would be needed for population survival OR fishing levels would need to be reduced. Figure 57, which compares recruits/spawner across populations, reflects what must be a complicated mixture of ocean and freshwater environmental effects that may not be shared by populations, as well as differences in relative stock status among populations. (More on this topic is considered under the stock-recruitment section, Appendixes.)

*ODFW response: ODFW agrees that the productivity section of the conservation plan was in need of revision in order to increase clarity. This section of the plan was modified accordingly. ODFW considered the concept of using stock-recruitment model estimates of brood harvest rates at MSY, coupled with current estimates of brood harvest rates, as possible productivity criteria. This possibility will be included in the options that need to be researched in order to resolve this matter for all of the native conservation plans scheduled to be completed by ODFW.*

9. Conservation Criteria

Although I did not have any strong objections to the proposed conservation criteria, when I first read through the Conservation Plan I found that I did not understand what was meant by "forecasted to reach conservation criteria", though I suspected it meant that forecasts of abundance (or other measures) were sufficiently low to trigger conservation concerns, presumably based on pre-established levels for individual populations. I did a search through the main report and found no definition of conservation criteria – these seem to be developed only in the Appendixes. I think that it is very important to have this topic explicitly defined/discussed in the text of the main body of the report.(Note that "Conservation criteria" is not listed under Appendix A definitions either.) Also, I don't particularly like the idea of "reaching" a conservation criterion. Reaching usually implies an upward movement toward an objective. I prefer usage like "projected not to achieve a conservation criterion" or "projected to approach a conservation criteria" or "projected to be near a conservation criterion" or "projected to fall below" (my favorite). My comments here reflect an

objection to definition and usage and not to the conceptual argument for designating stock status measures that serve as indications of potential problems when and if they are not achieved.

*ODFW response: ODFW modified verbiage in the conservation plan to address the points raised within the comment.*

10. Harvest Actions

I believe that the PFMC typically makes two different kinds of management forecasts of abundance: (a) pre-season abundance and projected numbers that would return to spawn in the absence of ocean fisheries, and (b) projected catches and FW returns given alternative fishing seasons/regulations. Actions refer to "During years when freshwater returns are forecasted to reach conservation criteria" and it is unclear just what is meant by this, especially as some call for additional ocean harvest restrictions whereas others instead focus on (implicit) reduction in FW fisheries. E.g., Actions 3.2, 3.3, Management Strategy 5.3. I suspect that what is intended is "forecast to fall below conservation criteria given projected ocean fishing seasons/regulations", but in any event the ambiguity needs to be cleared up.

*ODFW response: ODFW modified verbiage in the conservation plan to address the points raised within the comment.*

11. Management Strategy 5.4.

The text states "Changes in broodstock composition for the Chetco CHF program are warranted to compensate for the selective harvest of older CHF in the ocean fisheries." Ocean fisheries shift age composition of spawners toward younger aged spawners in hatcheries and on natural spawning grounds. Completely random mating in a hatchery may lead to additional unintentional selection for earlier age at maturity *in the absence of any fishing.* Adopting a mating policy that requires that females are always mated with males that are at least their length should pretty much eliminate such unintentional selection. This would not normally be termed a change in broodstock composition. Instead it would be termed a change in mating practices. (See Hankin et al. 2009.)

*ODFW response: ODFW modified verbiage in the conservation plan to address the points raised within the comment. ODFW will also consider this comment when crafting spawning protocols for CHF broodstock.*

12. Spawning escapement conservation criteria.

This is a very interesting issue that caused me much pondering and I still am uncertain just how I feel about it! Instead of using 0.5 SMSY, or SMSY itself, or some other multiple of MSY, as a conservation criterion for spawning escapement (e.g., always manage to ensure that the population does not drop below half of the estimated number of spawners needed for MSY), the Plan instead uses a bootstrap procedure to generate alternative, plausible estimates of SMSY, and sets the conservation criterion at 0.5 SMSY*, where SMSY* = $75^{th}$ percentile of the bootstrap estimates of

SMSY (Table 41, see also **APPENDIX F)**.

Because SMSY* will typically be larger than SMSY, sometimes substantially so, use of 0.5 SMSY* leads to an increased spawning escapement conservation criterion that may, in principle, exceed MSY. Although I suppose this would only happen when existing stock-recruitment data were uninformative (i.e., did not generate accurate estimate of SMSY), it seems odd to adopt a conservation criterion which could theoretically exceed estimated SMSY!

Before the Plan adopts this conservation criterion I think that additional support should be presented in the form of (a) formal journal publications that propose this procedure, or (b) simulation analyses which somehow address the value and logic of setting the conservation criterion at 0.5 SMSY*as opposed to some other criterion. (Personally, I kind of like the sound of 0.75 SMSY as an option.)It is beyond the scope of this review to suggest how these simulation analyses might be carried out, but I think it would be quite a chore. Perhaps the Plan should instead consider a criterion like "spawner abundances falls below MSY level for three successive brood years" which I believe (?) would be similar to historic versions of the PFMC's salmon plan (I could be wrong). (Note that whatever levels are finally selected will need to be adjusted by the revised stock –recruitment analyses.) .

*ODFW response: ODFW considered this comment at length. The 0.5 $S_{MSY}$ criterion was chosen because of the scientific review received during development of the National Standard 1 (§ 600.310) within Subpart D of the federal Magnuson-Stevens Act and the application of the standard for management of salmon populations by NMFS in the area of the Pacific Ocean covered by PFMC. With the significant uncertainty in $S_{MSY}$ estimates, ODFW concluded that it was prudent to use a greater spawner abundance to trigger conservation action because the actual value of $S_{MSY}$ may be greater than the point estimate. For this reason, ODFW chose the upper 75$^{th}$ percentile of $S_{MSY}$ as the critical abundance of spawners that triggers conservation action.*

13. Lower Rogue Conservation Criteria: hatchery fish

The Plan proposed a 15% hatchery fish conservation criterion for Lower Rogue natural spawners. This criterion seems a bit extreme to me IF it is true that a very large fraction (say, 50%) of Indian Creek Hatchery broodstock are wild fish. The criterion seems especially extreme when compared to 10% hatchery fish conservation criteria for streams without hatcheries (Hunter, Pistol). 20%, as for Chetco, might be better. If the Indian Creek facility cannot reliably bring a large fraction of natural spawners into the hatchery program, then perhaps the 15% level is a good one.

*ODFW response: The advisory committee and ODFW considered this matter during the process of formulating the relevant conservation criterion. As the desired status criterion calls for no more that 10% hatchery fish among adult CHF that spawn naturally, and it is always uncertain as to whether NP CHF can be collected for broodstock, it was judged best if the conservation status criterion calls for no more that 15% hatchery fish among adult CHF that spawn naturally.*

14. Research Needs (passages in quotes are taken from the Conservation Plan):

A. "Productivity criteria should be developed for all independent NP CHF populations and these criteria will need to be useful in a timely manner. Such criteria cannot presently be developed for two primary reasons: (1) recruitment is needed to estimate productivity, yet is currently estimated by a variety of methods employed by fishery scientists and (2) current methods to estimate the productivity of a single NP CHF brood are not available until six years after parents spawn."

As previously noted, I continue to believe that the best measure of "productivity" is something like the Ricker alpha parameter, recruits/spawner at low population size. Ricker alpha parameter estimates have been used to compare productivities across populations although care must be taken to ensure that recruits are measured in the same manner for all populations.(See Parken et al. reference previously cited.)

*ODFW response: ODFW agrees with this comment. This portion of conservation plan was modified as per the recommendation.*

B. "The propriety of methods employed to estimate spawner escapement should be tested with the application of more conventional methods, such as a mark-recapture experiment, to estimate salmon abundance. Other methods, such as a mean count of spawners (Holt and Cox 2008), may prove to be more accurate. This item is of lower priority because management goals (desired status criteria and conservation status criteria) are based on (scaled to) the estimation methods employed in this conservation plan." I found myself wishing that I had more time to ponder this assertion. I believe that the answer to the question of whether evaluation of spawner escapement estimates should have high or low priority depends on whether the estimation methods used to generate the indexes of abundance used in developing this plan produce reliable indexes of abundance. If not, then I would give this research need a very high priority rather than low priority. Indeed, I would argue it is important to carry out some mark-recapture experiments in several of the smaller coastal streams – to see if these estimates are or are not highly correlated with those produced by the methods that were used in this plan. This issue is also central issue for management application of conservation criteria – is it appropriate to trigger a management action due to falling below a conservation criterion when the evidence is very poor (i.e., when estimates of spawning escapement have extremely large errors)? (See my concluding comments concerning Forecasting methods.)

*ODFW response: ODFW agrees that assessment of the spawner escapement methods devised for the conservation plan is a high priority, along with all of the other research needs identified for the coastal populations. Clear delineation of priorities is important because ODFW judged that the availability of potential funds is very limited and the agency must allocate efforts to find funds accordingly. All of the other four research needs, identified as higher priorities, relate to the generation of estimates that were derived from even less reliable (or absent) data. At least for the spawning escapement estimates, there is information that indicates these estimates are reasonable (Lower Rogue estimates are highly correlated with escapement estimates for other Rogue*

*populations, Chetco estimates for 1995-96 line up fairly well with radio-tag estimates generated for those years, and Chetco estimates of pre-fishing stock size at age 3 are correlated to the analogous metrics for Klamath CHF).*

C. I'd like to see some improved estimates of spawning escapement for the Applegate and Illinois river stocks. These might be mark-recapture estimates of spawning escapement for the Applegate and Illinois river populations, based on tag-recovery of live fish or on tag-recovery of fresh carcasses. An alternative and possibly more cost-effective approach would be to use genetic methods (GSI, probably relying on SNPs) to estimate stock contribution based on Huntley Park passage data, but feasibility would depend on ability to genetically differentiate stocks. One advantage of the GSI approach is that it would be possible to mine historic collections of scales if they have been taken and archived. Based on recent work on Klamath Chinook, I speculate that the Illinois and Applegate stocks are well differentiated from other Rogue populations.

*ODFW response: ODFW agrees that monitoring of individual populations is preferable to the alternative of monitoring aggregated populations. Funds needed for such an effort are significant (as for monitoring CHF spawning escapement in the Klamath River Basin). The impetus for funding such surveys in the Rogue River Basin is low because of the lack of (1) any federal treaty obligations and (2) monitoring of ESA listed fish populations are of greater priority for fishery management agencies. However, if classification of genetic samples becomes more cost effective, and NP CHF from different populations can be differentiated, then it would be possible to estimate freshwater escapement of individual populations through genetic assignment of fish sampled at Huntley Park. This approach was incorporated as a research need in the revised draft of the conservation plan.*

15. Evaluating effectiveness of management actions

Because stock status seems quite good for almost all the populations considered in this report, especially aggregate Rogue fall Chinook, I caution that it may be very difficult to assess the "effectiveness" of management strategies and specific actions. It's hard or impossible to further improve a very positive situation. That is, if habitat improvements or other actions resulted in "no change" in stock status, when stock status is good, then it would be impossible to conclude (through a measure like recruitment) that these habitat improvements had been "effective". An alternative management action for which it might be possible, at least theoretically, to evaluate effectiveness would be curtailment of fishing when populations were projected to fall below conservation criteria.

*ODFW response: ODFW agrees that it is difficult to rigorously evaluate the efficacy of specific management strategies and management actions. Assessment of the collective effects of implemented management will be completed annually by comparing current and desired status within annual reports to be completed for the SMU. Consequently, the proposed evaluation of individual management strategies was deleted from the conservation plan.*

16. “Degree of Certainty”

Use of the term “certainty” should be reconsidered throughout the report. Either one is certain or one is not certain. If one is uncertain, there are “degrees of uncertainty” which can be quantified using statistical methods. The term “degree of certainty” does not make any sense to me. If I am certain that the run size of fall Chinook in the Rogue is 105,435 fish this year, then the associated 95% CI would be 105,435 – 105,435, i.e. there is no associated uncertainty. Certainty typically results only from a complete census with no enumeration errors at any stage in the census process.

*ODFW response: ODFW modified verbiage in the conservation plan to address the points raised within the comment.*

APPENDIXES

1. Cohort Analysis Run Reconstructions

The basic cohort reconstruction equation (see p. 17), as typeset, is incorrect, and needs to be corrected in the final version of the Conservation Plan. Calculations were apparently carried out correctly.

*ODFW response: ODFW modified the equation and verbiage in the conservation plan to address the points raised within the comment.*

2. Use of Huntley Park Index to generate estimate of spawning escapement

I did not fully understand the Huntley Park spawning escapement “calibration” methods on a first reading on the Plan documents and I had a long phone conversation with Tom Satterthwaite to discuss this topic. After this conversation, I feel much better about the methods that have been used. Use of the mark-recapture estimators, from the two years of presumably decent application (relative high flow, low pre-spawning mortality), for calibration is an obvious device but is severely limited by the very low number of years for comparison and may also be faulted for subjective exclusion of data (i.e., the decision not to use the year that produced an extremely high mark-recapture estimate of abundance). The comparison of the Huntley Park Index with the Gold Ray Dam counts would make much better sense if the logic and details of the comparison were presented in the following fashion: (a) we assume that the Gold Ray Dam counts provide an accurate estimate of upper Rogue fall Chinook escapement; (b) tagging studies have shown that virtually all fall Chinook that reach Gold Ray dam have passed the Huntley Park location by August ___; (c) Therefore, a comparison of a modified Huntley Park Index, calculated through the period July 15-August 5 (?), with the Gold Ray Dam counts should provide an estimate of the ratio of upper Rogue escapement to a Huntley Park index that should reflect that portion of the Rogue fall Chinook run that is destined to pass Gold ray Dam; and, finally (d) we assume that the Huntley Park index for the remainder of the passage time period (August 6 – whenever) represents the same fraction of the spawning escapement in other populations (e.g., Applegate, Illinois). I would not,

however, make too much of the coincidental fact that the two approaches both lead to a conclusion that, on average, the Huntley Park Index is something like 40% of the unknown true total escapement above Huntley Park. That could be just a wild coincidence or it could be meaningful agreement between the two.

*ODFW response:* *ODFW modified verbiage in the conservation plan to address the points raised within the comment.*

3. Adjustment of spawning survey data for habitat index.

Stream "gradient" does not seem like a good variable to include in a four variable habitat quality/quantity "product" because it has a preferred range (e.g., 2-4%, say) rather than implying better or more habitat as gradient increases (i.e., one expects a non-linear response of abundance to gradient). I suggest that additional analyses be carried out excluding this variable, if possible. Alternatively, the reader should be convinced that (a) the range of considered gradients are all within those judged suitable for Chinook (due to "pre-filtering of Chinook habitat"), and that (b) over the range of gradients in the data set actually used, it is reasonable to conjecture that a steeper gradient leads to better spawning habitat.

*ODFW response:* *ODFW agrees with this comment. The referenced analysis was modified, as was related verbiage in the conservation plan.*

4. Presentation of spawning surveys analyses

I thought that the methods used to deal with some very less than optimal spawning survey data were very creative and I do not find major fault with them (with the exception of the use of stream gradient in the habitat index and the previously stated observation that these data may be "pushed too far"). I do feel, however, that the (a) "scaling" of survey locations could be better explained (comparisons made across different years because not all survey locations used in all years within individual streams); and (b) simple equations might better express just how the scaling, peak counts and GIS habitat data were used to generate estimates of spawning escapement. For example, I "think" that the following equation approximately captures what was typically done:

$$Escape = 2.82 \sum_{i=1}^{n} L_i \sum_{j=1}^{k_i} \frac{PC_{ij}}{k_i} Y_{ij}(X_{ij})$$

Where:

2.82 = fixed adjustment factor to convert from peak counts to total spawners (reference);

n = number of tributaries with different mean peak spawner densities (i = 1,2,..,n)

$k_i$ = number of locations surveyed in tributary I;

$PC_{ij}$ = Observed Peak Count/Mile in surveyed location j in tributary i;

$Y_{ij}(X_{ij})$ = Regression prediction of Peak Count scaling as a function of habitat index for survey location j in tributary i;

$L_i$ = number of miles of spawning habitat that is available in tributary i.

The above can't be quite right because in many years peak counts are not made in all survey locations and sometimes there are no surveys at all in particular tributaries, but hopefully it provides some guidance for how the actual calculation might be expressed so as to remove ambiguity concerning how it is actually made. I think it may be "close" to what was actually done, however, and it is evident that the actual calculation equation is pretty complicated. An accurate text description of these calculations would be very difficult to construct, but appropriate equations would be unambiguous.

*ODFW response:* *As per the previous comment, ODFW modified the referenced analysis.*

5. S-R analyses.

This is the only area where I feel very strongly that it is important for the reported analyses to be revisited and for the previous conclusions to be revised in view of corrected analyses.

Stock-recruitment fits for Chinook salmon are typically done with "recruits" measured by either (a) ocean abundance at age 3, prior to fishing (N_3 in the cohort reconstruction) OR (b) "adult equivalents" (numbers of adults that would have returned to spawn over the brood given the apparent conditional age-specific maturation probabilities that brood exhibited and assumed values for ocean natural survival rates, assuming no ocean fishing).

For option (a), there is a solution for MSY ocean fishery exploitation rate (in a no terminal fishery model) that can be calculated from formulas presented in Hankin and Healey (1986), which also requires knowledge of maturation probabilities (assumed time-invariant) and ocean natural survival rates. It is also possible to construct a more complicated model (presented but not analyzed in Hankin and Healey) that includes both ocean and river exploitation rates and to then find that combination of exploitation rates that produces a conditional MSY given a harvest sharing agreement constraint (e.g., 50% of total harvest must get to Indian net fishery). For option (b) (see PFMC Klamath S-R analysis), the solution for MSY "harvest rate" is interpreted as the fraction of the adult equivalent spawners that are harvested over a brood's complete adult lifespan, without specification of where the harvest takes place. Many possible sets of ocean exploitation rates and freshwater exploitation rates might achieve this same MSY "harvest rate", so a sharing agreement would be needed to specify corresponding appropriate exploitation rates in ocean vs terminal fisheries that would achieve the MSY harvest rate.

For either approach, the Ricker alpha parameter, adjusted upwards for multiplicative error structure (see Klamath S-R analysis), provides a useful measure of productivity. The productivity parameter expressed in terms of age 3 ocean abundance will, of course, be larger than the estimated parameter when recruits are instead expressed as adult equivalents.

In the analyses presented in the draft Conservation Plan, recruits have been defined as the sum of ocean and freshwater catches plus spawning escapement attributable to a given brood year. This definition should give recruitment values that are highly correlated with but less than the age 3 ocean abundance (because they do not include fish that die due to natural causes in the ocean between ages 3 and 5), but may not give values similar to the "adult equivalents" because the recruits reflect the specific pattern of exploitation that was experienced by a given brood year's progeny. The typical adult equivalent calculation of recruitment starts with estimated age 3 ocean abundance and then adjusts it for age-specific maturation probabilities and ocean natural survivals (assuming no ocean fishing). Age-specific maturation probabilities are brood-year-specific in these calculations. The formulas that have been used in the Conservation Plan to calculate MSY harvest rate and spawning escapement are based on the Klamath S-R analysis, which assumed that recruits were measured as adult equivalents, and are therefore not appropriate for application to the recruitment metric that was generated for the Plan. My hunch is that the MSY harvest rate and SMSY values that have been generated are too high because the recruitment is too high relative to the adult equivalent metric (note that ocean catches include immature fish whereas adult equivalents consist only of mature fish), but I would have to check that out to be certain.

My recommendation is that these calculations be revisited using sets of brood-year-specific conditional age-specific maturation probabilities that can be calculated from the cohort reconstructions and the same natural ocean mortality (survival) rates that were used in the cohort reconstructions. Take the estimated age 3 ocean abundances, convert them to adult equivalents, and then fit the S-R models (Ricker or Beverton-Holt) in a manner similar to that described in the Klamath S-R analysis.

*ODFW response:* *ODFW agrees with this comment. Recruit estimates were modified to reflect the reviewer's option (b) as outlined within this comment and all other relevant analyses were also revised.*

6. PVA analyses

I am not an expert on PVA analyses and so have limited my comments on this topic. There are some aspects of the analyses which give me cause for concern, however, and these have briefly been noted by comments in the Appendixes. I suspect that it might be worthwhile to have a PVA expert review these analyses, without reviewing all of the other issues with which the Plan is concerned.

At the same time, I wish to express how impressed I am that the Plan made attempts to carry out PVAs for all of these populations, in addition to all of the other work that was done to prepare the draft Plan and the Appendixes. Wow!

*ODFW response:* *Comment noted.*

7. Forecasting and Management Response to Stocks Falling Below Conservation Criteria

I believe that the proposed methods for forecasting spawning escapement of age 3-6 Rogue River aggregate NP fall Chinook salmon have substantial merit, and it seems that that these same kinds of methods have useful application for lower Rogue fall Chinook. For the Rogue River aggregate fall Chinook population, the historic God Ray Dam counts, along with the concurrent Huntley Park

seining data, provide a very strong method whereby estimates of spawning escapement can be pinned down with reasonable accuracy. The assumption that Rogue and Klamath fall Chinook share similar exploitation histories in ocean fisheries seems reasonable and should lead to decent calculations of projected spawning escapements from a sum of age-specific sibling regressions (age i+1 ocean abundance against age I escapement for all appropriate brood years). For several reasons, however, I have mixed feelings concerning whether or not it is appropriate to force regressions through the origin. First, this is always a dangerous strategy if data suggest strongly otherwise. Second, the effect of forcing the regression lines through the origin on predictions of y given x may be modest except when stock size is very low, so it is important to compare the conservation criteria with the scale of the x variable (to see how critical it is to force the prediction of y to be zero when x equals zero). Finally, the reported $R^2$ values for regressions fit through the origin are very misleading and present an exaggerated and overly optimistic notion of how these forecasting procedures might work. There is no unambiguous measure of goodness-of-fit for a linear regression forced through the origin. Therefore, I'd like to see (a) regressions fit with intercepts, with meaningful $R^2$ reported, and (b) the conservation criterion indicated on the fitted plot. I checked the Rogue aggregate conservation criterion and it appears to be 22,000 spawning adults. It would be interesting to compare predicted escapements (from summed sibling regressions) for zero intercept and for non-zero intercept regressions given an observed spawning escapement of 22,000 adults. I do not have strong objections to forcing the Rogue regressions through the origin so long as there is a strong argument made for this device.

For a number of reasons, however, I cannot give my positive support to use of the forecasting models for the other (smaller) coastal stocks (Chetco, Winchuck, Hunter, Pistol, etc.) of NP fall Chinook salmon. First, as noted above, the reported $R^2$ values for the fitted zero intercept models are misleadingly large. (I suspect that in many cases the correlations between Y and X are very low, perhaps non-significant.) Second, although I agree completely with the author of the Conservation Plan that the estimates of spawning escapement for the small coastal populations are unreliable and often seriously biased, I do not share the author's optimism that averaging escapements for these populations over two or three years will generate a substantially improved estimate of average escapement. Below I suggest why this result is not obvious to me.

For independent unbiased estimates of escapement in each of several years, each with an estimated variance, the average of two or more such adjacent estimates should give an unbiased estimate of mean abundance over the two or more years for which the average is calculated. The variance of this estimate of the mean is equal to $(1/n)^2$ times the sum of the estimated variances associated with each estimate. Thus, if one were to assume, for the sake of argument, that estimates for individual years all had the same identical variances, then the mean of two adjacent years would have a variance equal to ¼ times the sum of the two identical variances, or ½ times the variance of the original estimates. For three years, the variance would be equal to 1/9 times the sum, or 1/9 times 3 times the original year-specific variance, or 1/3 of the original variance for a single year. This logic leads to a conclusion that errors of estimation of the mean escapement over several adjacent years may be substantially less than that for spawning escapement in a particular year.

The conclusion noted in the above paragraph, however, applies to unbiased and independent estimates, however. For seriously biased estimates of annual escapement, which the author notes is very likely given the very substantial interannual variation in viewing conditions for visual surveys and the correspondingly low number of surveys made in particular years, the simple average of two

or more adjacent years does not produce an unbiased estimate of average abundance and there is no reason to conclude that this kind of averaging produces an improved estimate of a running mean that can be trusted for forecasting purposes. If, for example, viewing conditions were poor in several adjacent years and estimated escapements therefore had significant negative bias in all those years, then it would not be true that the average of these three estimates would somehow be an "improvement" over the estimates for a single year. For biased estimators, the appropriate measure of uncertainty is mean square error ($bias^2$ + variance) and, frankly, I am uncertain just how mean square error is propagated over an average of several biased estimates. I've never seen any nice result for a situation like that and I suspect that there is not any nice simple result as for the case of independent unbiased estimates. If bias could be assumed consistently small, then the results for independent and unbiased estimates could possibly trusted, but that is clearly not the case for estimates of spawning escapement for the non-Rogue coastal populations. Indeed, one might also argue that it would be very unlikely for adjacent spawning survey-based estimates to be independent of one another. Very often wet years will follow one another and dry years will follow one another, implying that bias (positive or negative) is unlikely to be independent across adjacent years even though actual collected data certainly would be.

For the above reasons, I do not believe that it would be wise to attempt to try to use preseason forecasts of spawning escapement of the non-Rogue coastal stocks for the purpose of regulating freshwater harvest during years when forecasts suggest spawning escapements would fall below conservation criteria unless fisheries were curtailed. I just don't think that these forecasts would be sufficiently reliable to justify this kind of active management. Instead, I believe that a better basis for curtailing freshwater fisheries for these coastal stocks might be the average of two or more spawning escapements over a series of, say, 3-5 years, during which at least two years had at least, say, 8 spawning surveys accomplished. If the average of all these spawning surveys which had at least 8 spawning surveys were to fall below the conservation criterion (threshold), then fisheries could be shut down in the following year(s) until the threshold was again being consistently exceeded.

Finally, I note that the 95% confidence bounds indicated on the graphs displayed in the forecasting Appendix are bounds around the *expected value* of y given x. For forecasting, I believe that a more relevant 95% confidence bounds would be for the predicted value of an *individual value* of y given x. If such bounds (which would be huge) were plotted on these figures, especially for the smaller stocks, they would further support my contention that forecasting for these smaller stocks is not wise.

*<u>ODFW response:</u> ODFW considered these comments at length. There are multiple good points raised within the review comment. Potential changes to harvest was not appropriately conveyed in the tables of Appendix K. Those estimates were generated under the assumption that the freshwater fisheries would be closed under predicted conservation shortfalls (worst case scenario). Instead, with pre-season forecasts, fishery managers can judge the appropriate means by which to make the appropriate changes to harvest regulations. In some instances, the resultant management changes will be negligible. The Appendix tables were modified to better convey this more-probable outcome.*

*As recommended, regressions of hindcasted predictions of annual spawner abundances on "observed" spawner abundances were instead developed without driving the regressions through the origins. The revised regressions were then embedded in the conservation plan. These*

*regressions were significant ($P < 0.05$) for all of the populations, with the exception of the Hunter Creek and Pistol River populations.*

*However, ODFW concluded that active management of the small coastal populations results in more effective conservation coverage as compared to the more passive type of management scenario proposed in the review comments. There is a possibility of two successive brood failures and ODFW concluded that conservation criteria, that cover two or three years, has a greater surety of quickly addressing population declines and also accelerates the restoration rate of natural production. An allied important point is that the multiple year coverage by conservation criteria incorporates "observed" estimates of spawner escapements from either one (year i-1) or two previous years (years i-1 and i-2). Incorporation of a single pre-season forecast (for year i) will account for only one-third or one-half of the constituent estimates that will be used to judge whether a spawning escapement (year i) is forecasted to drop below the relevant conservation criterion.*

# 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**


**Independent Multidisciplinary Science Team** **Members**

Oregon Plan for Salmon and Watersheds

http –//www.fsl.orst.edu/imst

Robert M. Hughes

Nancy Molina

Clint Shock

Vic Kaczynski

Carl Schreck

J. Alan Yeakley

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**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.

**TABLE OF CONTENTS**

# 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.

*ODFW response: ODFW considered all available information in relation to this matter, and conveyed that information on pages 6-7 in the technical draft of the plan. That draft defines CHF as those Chinook salmon that enter freshwater and pass Huntley Park after July 15th (not August 15th). Population metrics and allied management criteria cover all of the Rogue CHF populations as an aggregate. ODFW (1992), as referenced in the draft plan, provides the rationale associated with the choice of a fixed date (July 15) by which to differentiate CHF and CHS among those Chinook salmon that pass Huntley Park. It is unclear what plan feature is responsible for causing IMST uncertainty related to this matter but ODFW made some clarification edits to the draft plan. ODFW does agree that the time of freshwater entry can vary to some degree among years and that this temporal variation will affect estimates of CHF passage at Huntley Park.*

*Use of genetic monitoring might have some utility in relation to attempting to differentiate CHF originating from the mainstem of the Rogue River as compared to those originating from the*

*major tributary streams and a relevant edit was added to the research needs section of the plan. However, as conveyed in the 2005 presentation referenced in the above IMST comment, it was not possible to differentiate, based on genetic assessments, those wild CHS that passed Gold Ray Dam over a period of three months. This lack of difference suggests that fine-line differentiation of wild CHF originating from the mainstem Rogue populations will be problematic; at least with classification methods that are currently available to genetic scientists.*

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.

*ODFW response: ODFW agrees that the draft CHF conservation plan should be modified to clearly address this matter, rather than solely depending on an executive summary to do so. There was extensive consideration of this matter by both the public advisory committee and by ODFW, and the related options are conveyed in the technical draft of the plan in Table 39 on page 110. Both of the suites of management strategies, that received support from members of the public advisory committee and from ODFW, clearly state that restoration of wild CHS production is of greater priority as compared to the maintenance of wild CHF production in the Rogue River Basin. As such, there is integration between the CHS and CHF conservation plans. To help clarify this matter, ODFW crafted and included a new section near the front of the conservation plan.*

## 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).

*ODFW response: The draft plan proposes management criteria that cover pooled populations of wild CHF in the Rogue River Basin for one primary reason. There is no monitoring of the individual populations in the basin. Consequently, it was not possible to craft quantitative management criteria for those populations because population status, in relation to any management criteria, can only be assessed through monitoring. Conservation plans can be amended. Should funding for additional monitoring become available, then resultant data will be analyzed and quantitative management criteria can be developed for an individual population (as in the case of the Lower Rogue population). ODFW agrees that monitoring of all independent populations of native fish is advisable, and a relevant addition was made to the monitoring section of the draft plan.*

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.

*ODFW response: Table 1 on page 8 refers to documented life history differences among the mainstem Rogue populations. These life history differences have a strong genetic basis within Chinook salmon as conveyed in the paragraph that follows the table. The draft was modified to convey that the documented differences in life history have a strong genetic basis; an indicator of substantial differences among the mainstem populations.*

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.

*ODFW response: These populations are not monitored because there are no funds to cover the degree of survey effort needed to appropriately characterize population status. A relevant addition was made to the monitoring section of the plan.*

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?

*ODFW response: Populations of fall Chinook salmon in the Rogue Species Management Unit are included in the Southern Oregon - Northern California ESU. This ESU is not listed under the Endangered Species Act. Consequently, there will be no effort to develop a federal recovery plan. The Native Fish Conservation Policy of the state of Oregon guides the development of the conservation plans for populations of fish produced in Oregon. At the present time, there are no fall Chinook salmon populations in the Klamath River Basin of Oregon.*

## 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.

*ODFW response: The draft plan proposes management criteria that cover pooled populations of wild CHF in the Rogue River Basin for one primary reason. There is no monitoring of the individual populations in the basin (Lower Rogue population excepted) and ODFW was not able to craft quantitative management criteria for populations that are not being monitored. Should monitoring be established in later years, then the resultant data will be analyzed and quantitative management criteria can be developed for an individual population (as in the case of the Lower Rogue population). A relevant addition was made to the monitoring section of the draft plan.*

*There is no point in attempting to craft interim management criteria for two reasons: (1) as referenced in other IMST comments, there are no comprehensive habitat surveys that cover any of the NP CHF population areas and (2) there is no habitat-based model that is appropriate to apply to NP CHF populations in the Rogue SMU; whose life history strategies differ significantly as compared to NP CH populations in most of the Pacific Northwest.*

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? *ODFW response: It was not possible to estimate the statistical confidence associated with the referenced sampling other than to compare two different methods by which fish abundance estimates could be generated. As detailed in*

*Appendix Table C-2, the resultant two estimates compare favorably.* 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. The purpose of the management alternatives is to outline potential management strategies that will either attain, or maintain, desired status. 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.

*ODFW response: The purpose of the conservation plan, as stated in Oregon's Native Fish Conservation Policy, is to attain desired status. Thus, ODFW concluded that the efficacy of management strategies, and allied management actions, is directly reflected in whether or not desired status has been attained.*

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. *ODFW response: There is no such assumption in the plan. The draft plan presented the only available information.* Also, explain why temperatures at Agnes are sufficient for reflecting thermal conditions in the upstream basin. *ODFW response: As stated in header paragraph, the purpose is to ensure that releases from Lost Creek and Applegate lakes directly interface with NP CHF management objectives. As conveyed in the technical draft of the plan, the Agness gage serves that purpose.* Explain why it is sufficient to monitor shade once every 10 years, etc. *ODFW response: Trees grow slowly, so a ten year periodicity in surveys seemed reasonable.* 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? *ODFW response: ODFW agrees with this comment and modified the draft plan accordingly.* 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). *ODFW response: In the opinion of ODFW, direct monitoring of NP CHF populations appropriately covers the issues raised in the above two paragraphs, given the monitoring funds that are currently available and given the prognosis for obtaining additional funding. Monitoring of adult CHF abundance and ocean survival rates of CWT-marked hatchery fish act as indicators of habitat capacity in freshwater and the ocean, respectively.*

## 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). *ODFW response: ODFW has a differing opinion on this matter. When freshwater returns of NP CHF decline, the primary causative factor (freshwater conditions or oceanic conditions) can be determined based on the monitored ocean survival rates of CWT-marked hatchery fish.*

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. *ODFW response: ODFW is hesitant to explore*

*this approach because no analogous approach can be found in the scientific literature, as directly related to the type of data in question.*

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? *ODFW response: Yes, because the confidence intervals reflect the uncertainly associated with the regression lines, not the individual data points.* 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. *ODFW response: ODFW considered this matter but is not comfortable with making such guesses.*

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. *ODFW response: ODFW agrees with this comment and modified the draft plan accordingly.*

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. *ODFW response: ODFW considers the CHF hatchery programs in the SMU to be very effective in providing additional harvest opportunities for Oregonians with an associated minor risk of negative impacts on wild fish. Hatchery adult CHF that escape harvest, home to the Indian Creek facility at high rates. As conveyed in the technical draft of the plan, hatchery fish account for only about 4% of the natural spawners in the Lower Rogue population area. The recreational fishery that targets these fish primarily occurs near the mouth of Indian Creek, so allied interception rates on NP CHF are low. Natural spawning by hatchery fish, and the interception of wild fish within the recreational fishery, is of greater concern in the Chetco River. As a result, the technical draft of the plan called for establishment of a collection facility for returning adults of hatchery origin. This action will result in a fishery scenario similar to that for the hatchery program in the Lower Rogue.*

## 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?

*ODFW response: ODFW is somewhat uncertain about these comments. The limiting factor assessments interface directly with the guidance outlined in the Native Fish Conservation Policy. This policy requires "an assessment of the primary factors causing the gap between current and desired status". The limiting factors assessments included in the plan accomplish that requirement and are based on available information. ODFW is not aware of any information that shows the aforementioned list of use impacts to have significant effects on the ability to attain (or maintain) formulated desired status criteria described in the technical draft of the plan. ODFW understands that these types of uses have been shown to have impacts on depressed salmonid populations listed under the Endangered Species Act (CHF in the Rogue SMU are not listed under ESA). However, recovery plan goals for ESA listed species differ significantly as compared to conservation plan goals that are designed to meet the requirements of Oregon's Native Fish Conservation Policy.*

## 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.

*ODFW response: The conservation plan does not include any provisions to increase harvest during years of high abundance of adult NP CHF, except in the late-season, state managed, ocean fishery near the mouth of the Chetco and Winchuck rivers. However, there are provisions to ensure baseline spawning escapements of NP CHF during years of low adult abundance (conservation criteria) through greater restrictions within all of the fisheries (ocean and freshwater, commercial and recreational).*

*ODFW is aware that salmon carcasses are an important source of nutrients that enhance the productivity of coastal water bodies, especially in nutrient poor systems such as found in many Alaskan streams. As described in the draft plan, there is good evidence that the growth rates of juvenile NP CHF are density dependent within the coastal population areas of the Rogue SMU and that growth limitation in streams and estuaries reduce survival rates of potential recruits. As a result, there were multiple action items crafted for the purpose of enhancing stream productivity without producing water quality problems (i.e. riparian zone protection). It may be that without harvest, CHF recruitment would increase for the coastal populations but then the social, cultural, and economic benefits of consumptive fisheries would be foregone. These societal benefits were considered during development of desired status criteria, using stock-recruitment relationships as guidance for the identification of optimal spawning escapements.*

*In contrast to the coastal populations, there is good evidence, as described in the technical draft of the plan that juvenile NP CHF grow at very fast rates while resident in productive streams of the Rogue River. There also is good evidence, as described in the plan, that spawning escapements of NP CHF now exceed those of about 100 years ago as a result of increased accessibility to additional habitat and improvements in water temperature during the critical summer months. As a result of the aforementioned factors, ODFW concluded that actions to increase nutrient inputs, through harvest curtailments, would not likely increase NP CHF recruitment in the Rogue stratum of the SMU.*

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. *ODFW response: ODFW*

*considered this suggestion at length and concluded that incorporation of survival rates for CWT-marked hatchery fish as covariates within the stock-recruitment relationships calculated for each population, effectively acts as a proxy for variable ocean survival rates and produces the most effective means by which to identify primary limiting factors during the freshwater life history stages of NP CHF in the Rogue SMU. Identification of primary limiting factors in freshwater leads to better management because freshwater environmental factors can be more effectively managed as compared to oceanic factors, which are primarily beyond the influence of ODFW fishery managers. ODFW looked into the possibility of multiple recruitment curves for individual data sets, but was unable to identify any objective means as to how to appropriately split the data sets into the two or more groups needed for the recommended analyses.*

## 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. *ODFW response: ODFW believes that the results of the persistence assessments, included in the technical draft of the plan, show otherwise in that all extinction probability estimates were less than 1% over a 100 year period.* 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.). *ODFW response: ODFW structured the Rogue CHF planning approach as a two stage process. The first stage was designed to (1) identify desired status, (2) assess primary limiting factors, (3) determine whether the primary limiting factors affect the attainment or maintenance of desired status, and (4) determine whether the populations would remain viable for the foreseeable future. ODFW also concluded that it was most appropriate to include only primary fishery interest groups in the first stage of the planning process because these groups have the greatest stake in the identification of desired status criteria.*

*Had the limiting factors assessments indicated that land and water use been primary factors that limited either attainment or maintenance of the chosen desired status elements, then ODFW would have notified the appropriate interests of the results and would have offered to involve those parties in the second stage of plan development. In retrospect, this was a good call by ODFW. The public advisory committee met 22 times before completing work on a desired status statement. To expect the involvement of non-fishery interest groups, during such a lengthy process, would have resulted in a disservice to participants. ODFW reviewed the action items*

*listed in the first draft of the conservation plan and distributed that draft to those primary entities who be affected by the action items. Finally, the general public, and all other interested parties, are afforded the chance to review the public draft of the conservation during the public review process.*

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.

*ODFW response: ODFW understands, and agrees with, the IMST perspective on this matter in relation to the development of recovery plans for species listed under the Endangered Species Act. However, ODFW conservation plans serve a distinctly different purpose, as described by the Native Fish Conservation Policy. The conservation plan for NP CHF in the Rogue SMU was crafted following guidance outlined in the policy, with the associated purpose of developing structure which ODFW will follow while managing NP CHF populations in the Rogue SMU (there is currently no management plan for this group of fish).*

# 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. *ODFW response: This important topic is directly addressed on pages 23-24 and in Table 8 of the technical draft of the plan. Results in this section clearly show, in the opinion of ODFW, that NP CHF populations in the Rogue co-vary demonstrably; despite significant differences in the methods employed to sample the various populations.* 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? *ODFW response: Harvest rates of CHF in the Rogue River are negatively related to the number of CHF that return to freshwater and this type of negative relationship is typical for Chinook freshwater fisheries. Thus, passage estimates at Huntley Park provide a better estimate of CHF abundance and the two methods developed to estimate CHF abundance produce very similar results (as described in APPENDIX C). Consequently, there is no need to use estimates of freshwater harvest as a proxy for CHF returns in the Rogue River.* 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? *ODFW response: Given the previous response, there are no implications related to the relevant conclusions in the plan because the CHF*

*passage estimates at Huntley Park are the best estimate of NP CHF abundance in the Rogue River.*

- 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. *ODFW response: The federal/state recovery plans, reviewed by IMST, are fundamentally different than the CHF conservation plan for the Rogue SMU. The federal recovery plans set goals for fish recovery so that the ESU is a viable unit of natural populations. The recovery goals are primarily based in habitat restoration, which generally takes a long period of time. In contrast, the CHF conservation plan sets goals that are based on desired status; which is based on ecological, economic, and cultural considerations (as per ODFW policy - described in the draft conservation plan). These considerations resulted in desired status criteria that are much shorter in temporal length. It is also important to note that the independent CHF populations in the Rogue SMU are all viable, some of the major populations are significantly more abundant as compared to historical levels, and that CHF habitat in the SMU has been less impacted as compared to ESUs listed under the federal Endangered Species Act.*

- 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. *ODFW response: Verbiage such as "recommends", "requests", "supports", and "initiates" need to be employed when the relevant action items are not directly covered by ODFW statutory guidelines. As such, ODFW is unable to dictate, and can only encourage, whether the actions will be undertaken. The other verbiage ("develops" and "manage" relate to those actions covered by ODFW statutory guidelines and can directly be accomplished by ODFW.* Will there be follow up? *ODFW response: Yes, through the completion of annual reports, which are described in the draft plan. The annual reports are intended to function as a feedback loop so that interested parties can track plan implementation.* What happens if the USACE ignores the requests *(ODFW response: ODFW can only continue to recommend action.),* how extensive and intensive is the "support", and how sufficient is the "development" and "support programs" in terms of staff and funding, etc.? *ODFW response: Current ODFW resources allow for the completion of all action items embedded in the alternative management strategies that currently receive support from ODFW and advisory committee members. In the event ODFW has to reduce programs to meet funding limitations, then it is likely that some action items will not be accomplished as proposed.*

- 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). *ODFW response: Given that (1) hatchery fish account for less than 2% of the adult CHF that spawn naturally in the SMU, (2) there are no proposals to increase hatchery production and (3) current guidance in the scientific literature indicates that current hatchery programs do not present significant risk for NP*

*CHF in the Rogue SMU, ODFW concluded that there is minimal risk of hatchery fish becoming a problem in future years.* 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. *ODFW response: ODFW agrees that the technical draft did not clearly convey the estimation methods related to this comment and the relevant portions of the conservation plan were modified accordingly.*

- 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. *ODFW response: There are several instream aggregate mining sites located in the SMU and there is aggregate mining that occurs in the floodplains, but neither activity appears to be a primary factor that limits CHF production in the SMU. Potential impacts to CHF production are covered in the plan under Action Item 1.14 (Rogue Stratum) and Action Item 1.2 (Coastal Stratum). Consequently, no edits were made. There is minimal commercial instream gold mining in the SMU and permitted use is limited to summer. Recreational mining occurs in the Rogue River and Chetco River basins, but it primarily occurs outside of NP CHF habitat. Within NP CHF habitat, recreational mining tends to focus on the use of suction dredges around bedrock areas. The California ban on suction dredging will terminate in 2016. As a result, ODFW does not expect any long-term increase in recreational dredging in Oregon. Regardless, any potential impacts to CHF production are covered in the plan under Action Item 1.14 (Rogue Stratum) and Action Item 1.2 (Coastal Stratum).*

- 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. *ODFW response: ODFW is unable to publish the numerous historical agency reports within scientific journals. However, the agency made considerable effort to generate and provide digital copies of many Rogue related reports to the Hannon Library Digital Collections at Southern Oregon University. Those reports can be accessed in the Bioregion collection at the following website:* http://soda.sou.edu/

## 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.

*ODFW response: In response to these comments, ODFW completed a thorough editing of the draft plan.*

# 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. *ODFW response: ODFW agrees with this comment and the relevant portion of the conservation plan was modified accordingly.* How does removal affect ODFW's classification of spring and fall Chinook salmon? *ODFW response: This matter was addressed later in the technical draft of the plan.*
- 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. *ODFW response: ODFW agrees with this comment and the relevant portion of the conservation plan was modified accordingly.*
- 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. *ODFW response: ODFW agrees the information in the table needed clarification and made changes to the draft plan.*

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. *ODFW response: There are numerous cases where adjacent salmonid populations have been found in have distinct genetic differences. However, ODFW acknowledges the problematic nature of the conclusions. The referenced section of the draft was modified to convey the relevant research project that was presented later in the plan. Until genetic surveys can be completed, ODFW concluded it was prudent to assume that the coastal CHF populations differed from each other.*

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. *ODFW response: The purpose of the section was solely to provide historical information. The topic in question was addressed in a primary (later) section of the technical draft of the plan.*

Page 12, Table 5 – Provide data indicating that the listed flow manipulations had overall non-detrimental or positive effects on Chinook salmon. *ODFW response: The purpose of section was solely to provide historical information. The topic in question was addressed in a primary (later) section of the technical draft of the plan.*

- Page 15, Figure 2 – Given that Gold Ray Dam was removed, provide justification for separating Upper Rogue and Middle Rogue populations at that location. *ODFW response: ODFW agrees with this comment and the relevant portion of the conservation plan was modified accordingly.*
- 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). *ODFW response: ODFW agrees with this comment and the relevant portion of the conservation plan was modified accordingly.*
- Page 16, Figure 3 – Given that these are numbers are estimated, provide the confidence intervals around the estimates. *ODFW response: ODFW was unable to generate confidence bounds for the estimates and the relevant portion of the conservation plan was modified accordingly.*

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. *ODFW response: Given that it is not possible to visually differentiate late-spawning CHS from early-spawning CHF, nor is it possible to differentiate juvenile CHS and CHF, it will be very difficult to monitor the CHF population in the Upper Rogue.* 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. *ODFW response: ODFW remains uncertain about this comment. The confidence intervals presented reflect parameters associated with the regression equation rather than individual data points.*

- 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. *ODFW response: ODFW agrees with this comment and the relevant portion of the conservation plan was modified accordingly.*

- 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? *ODFW response: ODFW agrees with this comment and the relevant portion of the conservation plan was modified accordingly.*

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. *ODFW response: ODFW is uncertain about this comment. The confidence intervals presented reflect parameters associated with the regression equation rather than individual data points.*

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"? *ODFW response: ODFW is unable to quantitatively assess the assumption. If such an assessment could have been made, it would have been included in the draft plan.*

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. *ODFW response: ODFW remains uncertain about this comment. The confidence intervals presented reflect parameters associated with the regression equation rather than individual data points.*

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. *ODFW response: As conveyed in the technical draft, there is good evidence of co-variation of NP CHF populations in the Rogue SMU. Consequently, there is evidence that monitoring of the aggregate populations provides some conservation coverage to all of the NP CHF Rogue populations. However, ODFW agrees that monitoring of all independent populations of native fish is advisable, and a relevant addition was made to the monitoring section of the draft plan.*

Page 22 – Given the importance of the Applegate population, it certainly would be useful if the adult monitoring could be resumed. *ODFW response: ODFW agrees that monitoring of all independent populations of native fish is advisable, and a relevant addition was made to the monitoring section of the draft plan.*

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). *ODFW response: ODFW remains uncertain about this comment. The confidence intervals presented reflect parameters associated with the regression equation rather than individual data points. ODFW considers the r2 statistic of 0.48 to be significant, given the inherent variability of disparate types of sampling in fishery science.*

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. *ODFW response: As conveyed in the technical draft, there is good evidence of co-variation of NP CHF populations in the Rogue SMU. Consequently, there is evidence that monitoring of the aggregate populations provides some conservation coverage to all of the NP CHF Rogue populations. However, ODFW agrees that monitoring of all independent populations of native fish is advisable, and a relevant addition was made to the monitoring section of the draft plan.* Where did the other 2 tags originate? *ODFW response: ODFW edited the conservation plan to address this question.*

- Page 28 – These numbers do not compute. How can 7 tags for 20–500 fish mean that 49% are hatchery fish? *ODFW response: ODFW agrees that the technical draft did not clearly convey the estimation methods related to this comment and the relevant portions of the conservation plan were modified accordingly.*

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. *ODFW response: An edit was made in relation to this comment.* 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. *ODFW response: An edit was made in relation to this comment.* It would be useful to add physical habitat structure (e.g. large woody debris, substrate size) *ODFW response: an edit was made in relation to this comment* 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? *ODFW response: This critical issue was addressed in a later section of the technical draft.*

Page 43 – Is spawning habitat patchy or continuous? *ODFW response: ODFW is unable to answer this question because no reliable habitat surveys have been conducted.* How would the distribution of spawning habitat affect estimation of capacity? *ODFW response: ODFW is unable to answer this question.*

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? *ODFW response: The draft plan presented the only data available and this data conveys some sense as to the abundance of gravel in the basin.*

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. *ODFW response: An edit was made in relation to this comment.*

Page 45, 1st paragraph – What is the cumulative impact of the known 20-plus push up dams on the system? *ODFW response:* ODFW *was unable to locate or develop any such information.* What is the estimate of illegally constructed dams and diversions? *ODFW response: ODFW was unable to locate any source for the information needed to address this comment.* And what potential impacts might they have on the system? *ODFW response: ODFW is uncertain about this comment. The relevant paragraph seems to directly address the comment.*

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. *ODFW response: ODFW is uncertain about this comment. The succeeding paragraph seems to directly address the comment.*

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. *ODFW response: ODFW was unable to locate any source for the information needed to address this comment.* It seems irrelevant that the former are outweighed by the increased habitat area provided by the latter. *ODFW response: ODFW concluded that the matter is relevant because of the marked increase in NP CHF habitat volume and habitat quality.*

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. *ODFW response: ODFW has a differing opinion on this matter for the primary reasons outlined in the technical draft: (1) sampling documented that few juvenile NP CHF rear in the Rogue River estuary and (2) sampling documented that juvenile NP CHF rear to smolt size in the mainstem of the Rogue River.*

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 *ODFW response: ODEQ sampling results indicate otherwise; as referenced in the first paragraph of the section on page 49 of the technical draft.* 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? *ODFW response: Not specifically within the Rogue River Basin. However, there have been recent papers published on this topic (Klamath and Sacramento CHF). This portion of the draft plan was significantly modified.*

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. *ODFW response: An edit was made in relation to this comment.* Also consider stream width, air temperature and stream flow. *ODFW response: These factors were incorporated into the ODEQ modeling effort (included in edit).* Retention of streambank trees provides more than shade: e.g., increased bank stability, allochthonous inputs, potential LWD, and perhaps floodplain sediment deposition. *ODFW response: The referenced section specifically pertained to water quality. The other benefits were addressed in other sections of the plan and an edit was made in relation to this comment.*

Page 50 – Some discussion of pre-spawning mortality at temperatures below 19º C would be useful. *ODFW response: An edit was made in relation to this comment.*

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? *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: There is likely no consequence because water temperature appears to be the primary factor that affects the distribution of these species in the primary areas inhabited by juvenile NP CHF in the Rogue River Basin.*

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. *ODFW response: An edit was made in relation to this comment.*

Page 54, 2nd paragraph – There is no inference on how avian predation of fall Chinook changed after the introduction of redside shiners. *ODFW response: An edit was made in relation to this comment.* The intent is left to the imagination of the reader. Was the illegal introduction of redside shiners intentional or an inadvertent mistake? *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: This is a fundamental issue that biologists have been considering for more than 30 years. As such, this type of analysis is beyond the scope of the CHF conservation plan.*

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? *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: The purpose of these historical (1980s) simulations was to determine the sensitivity of varied inputs only in relation to the effects of freshwater factors on NP CHF production. In relation to this specific purpose, ODFW concluded that the model output exhibited relatively high precision as the coefficients of variation were all less than 20%.*

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. *ODFW response: ODFW agrees there is not a strong correlation between the two variables, but the statistically valuable relationship provides important information when coupled with other sources of information that are germane to the issue in question.*

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. *ODFW response: The importance of these referenced factors varies annually in relation to CHF population sizes, water yield, and climatic conditions. Thus, it is difficult to further prioritize their relative importance because, in any given year, any of the factors could be a number 1, or number 5, priority.*

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. *ODFW response: An edit was made to cross-reference the primary conclusion related to this matter.*

Page 68, Table 24 – If the estimates are professional opinion, a likely range can be estimated. *ODFW response: ODFW is unsure how this recommendation can be accomplished in a defensible manner.*

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. *ODFW response: Edits were made in relation to this comment.*

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. *ODFW response: ODFW only performed a correlation analysis on this data set (and other allied data sets) for two reasons (1) there were only five data points and (2) one of the data points (Chetco) overwhelmingly influences quantification of the relationship. For the same reasons, ODFW chose not to try and fit any regression lines to the data.*

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..." *ODFW response: An edit was made in relation to this comment.*

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? *ODFW response: An edit was made in relation to this comment.* Which ones of those have proven increases in temperature? *ODFW response: An edit was made in relation to this comment.*

Page 76, 1st paragraph – Less shade and warmer stream temperatures are also associated with increased stream width. *ODFW response: An edit was made in relation to this comment.*

Page 77, 1st paragraph, lines 3–5 – Why have these changes affected microhabitat? *ODFW response: An edit was made in relation to this comment.*

Page 77, 2nd paragraph, line 7 – Words are missing. Edit to read "Based on a score... is rated from 10..." *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: An edit was made in relation to this comment.*

Page 80 – Might the observed relationship between higher densities of juveniles and earlier downstream migration suggest density dependent food or habitat limitation? *ODFW response: An edit was made in relation to this comment.*

Page 82, *Competitors* section – Indicate whether or not hatchery fish compete with juvenile Chinook salmon. *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: In freshwater CH fisheries, fish abundance affects harvest rate and thus harvest rate is 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? *ODFW response: An edit was made in relation to this comment.*

Page 99, Figure 55 – The graphed relationship only explains 19% of the variability, indicating a weak relationship. *ODFW response: ODFW agrees there is not a strong correlation between the two variable, but the statistically value relationship provides important information when coupled with other sources of information that are germane to the issue in question.*

Page 100 – The biological status section is quite vague, particularly from the fourth paragraph starting with "Abundance of adult…." *ODFW response: ODFW reviewed this section and was unable to develop any improved verbiage.*

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. *ODFW response: Available genetic assessments (pages 7-8 in the*

*technical draft) indicate that the Upper Rogue and Applegate populations differ from each other, and that the Upper Rogue population is likely unique. ODFW agrees that more detailed genetic assessments are needed, as called for in the research needs section of the technical draft.*

Page 101 – Table 32 would benefit from more explanation. Where does the number 55,000 as a criterion come from and why? *ODFW response: An edit was made in relation to this comment.* What scientific basis is there for the percentages used in the other criteria? *ODFW response: No guidance in the scientific literature could be found, other than in relation to the percentage of hatchery fish among spawners. As such, criteria were developed as a "best shot" assessment by the advisory committee and ODFW.*

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. *ODFW response: This comment was made in relation to the temporal coverage of the desired status criteria, and would be directly relevant if the desired status criteria were to also serve as evaluation criteria. However, that is not a purpose of the desired status criteria. These criteria instead serve (per Oregon's Native Fish Conservation Policy) as a societal statement of fishery resource status. Evaluations of population status will, as outlined in the technical draft of the plan, occur every 15 years. This period of time provides the opportunity to complete a thorough assessment. Determination of survival rates of standardized releases of marked hatchery fish is critical to this need, because variable rates of ocean survival must be factored out of the recruitment of wild CHF in order to better quantify the status of wild CHF. Population models presented in the technical draft of the plan clearly show this need, in the opinion of ODFW.*

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. *ODFW response: The most recent guidance in the scientific literature (discussed on page 91 of the technical draft) indicates that the draft desired status criterion is appropriate for Chetco CHF.*

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. *ODFW response: ODFW concluded that it was not appropriate to generate*

*desired status criteria when there are no means by which generate such criteria and there are no means by which to assess population status.*

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. *ODFW response: The population models presented in the technical draft produced direct estimates of productivity (alpha parameters), and variable ocean conditions are at least partially accounted for with the use of survival rates of marked hatchery fish as a proxy variable. But there are primary problems with interpretation of the estimated alpha parameters: (1) methods to calculate recruits are not standardized in the scientific literature, (2) there are no recommendations in the scientific literature for alpha parameter values for healthy populations, and (3) the alpha parameters will change over time as new methods are devised to estimate recruits - invalidating adopted criteria. These issues make the crafting of population productivity difficult (although called for in the Native Fish Conservation Policy). As a result, research is needed on this matter and was listed as a research need in the technical draft of the plan.*

Page 104, 3rd paragraph – The Rogue aggregate of 950 Chinook salmon needs further explanation here than that it is for five populations. *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: ODFW has a different opinion on this matter. Variations in ocean conditions are directly accounted for, in the population models, with the inclusion of survival rates for standardized releases of marked hatchery fish. Use of this metric as a proxy for ocean conditions, permitted better quantification of the effects of fishing mortality and freshwater environmental conditions - factors that can be directly influenced through resource management.*

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. *ODFW response: The rationale behind the choice of desired status criteria is described in the desired status section of the draft plan.* Also, should not judgment of compliance be based on lowest 95% confidence interval, not the mean? *ODFW response: There are no compliance implications (Oregon's Native Fish Conservation Policy) associated with the non-attainment of desired status.* Also, why is a 95% confidence interval appropriate rather than a more conservative confidence interval? *ODFW response: The 95% confidence interval provides some insight as to the dispersion of data around the mean. There are no management implications.*

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? *ODFW response: There are no compliance implications (Oregon's Native Fish Conservation Policy) associated with the non-attainment of desired status.* It is more than 10% out of compliance. If so, are more protective or enhancing measures warranted? *ODFW response: There are no compliance implications (Oregon's Native Fish Conservation Policy) associated with the non-attainment of desired status.*

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). *ODFW response: The most recent guidance in the scientific literature (discussed on page 91 of the technical draft) indicates that the proposed desired status criterion is appropriate for Chetco CHF.*

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). *ODFW response: This action is directed at a primary limiting factor for NP CHF; which is water temperature during the summer. TMDLs completed by ODEQ indicate that shading of streams results in lower water temperatures during summer. Maintenance and restoration of riparian zones not only increases stream shading, but also increases the benefits mentioned in the comment.*

Page 113, Action 1.13 and Page 119, Action 1.5 – What is considered mature riparian vegetation on a bar? *ODFW response: Any vegetation that grows naturally.* By definition, bars lack trees. *ODFW response: Some vegetation grows on gravel bars as exemplified by willow and cottonwood growing on gravel bars.* Are vegetated islands the desired future status of these areas? *ODFW response: Not necessarily.* Are these expected to increase side channels? *ODFW response: No, there is no such expectation.*

Page 120, Action 1.14 – How does ODFW propose to enhance primary and secondary production, yet decrease nutrient flux into estuaries (Action 1.15)? *ODFW response: Maintenance and restoration of riparian zones increase primary and secondary production in streams through the increased input of allocthonous material. Nutrient loads in estuaries can be decreased by improved management of areas adjacent to estuaries (i.e. better fencing and cattle management) and through improved sewage treatment (i.e. providing better sewer services to homes within urban growth boundaries).*

Page 121, Action 1.17 – Why not breach tidal blockages in estuaries? *ODFW response: This possibility was considered during the planning process and two issues were identified related to this matter: (1) increased inflowing ocean water is high in nitrogen and phosphorous and could possibly result in degraded water quality in estuaries during low stream flow periods and (2) sand bars can quickly become established again due to tidal action and strong northwest winds that typify the southern Oregon coast during summer.*

Page 124, Management Strategy 6.5 – Why eliminate a natural cascade? *ODFW response: The action is designed to increase the production of NP CHF. Such an action is allowed under state statute (ORS 509.630). An edit was made to clarify this point.*

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. *ODFW response: ODFW has a different opinion on this matter. Survival rate estimates of hatchery fish are included in the population models as covariates. This parameter serves as a proxy for variable conditions of ocean productivity and thus better characterizes the underlying stock-recruitment relationship for each of the populations.*

Page 127, Table 42, item 3. Specify what is <5%. *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: ODFW and the advisory committee concluded differently in relation to this matter.*

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. *ODFW response: ODFW and the advisory committee concluded differently in relation to this matter.*

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? *ODFW response: The draft plan proposes management criteria that cover pooled populations of wild CHF in the Rogue River Basin for one primary reason. There is no monitoring of the individual populations in the basin. Consequently, it was not possible to craft quantitative*

*management criteria for those populations because population status, in relation to any management criteria, can only be assessed through monitoring. Conservation plans can be amended. Should funding for additional monitoring become available, then resultant data will be analyzed and quantitatively management criteria can be developed for an individual population (as in the case of the Lower Rogue population). ODFW agrees that monitoring of all independent populations of native fish is advisable, and a relevant addition was made to the monitoring section of the draft plan.*

Page 130, "Weekly Monitoring" – It is advisable and reasonable to monitor flows and temperatures continuously, rather than weekly. *ODFW response: An edit was made in relation to this comment.* 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. *ODFW response: The purpose of these monitoring proposals is to provide periodic snap-shot assessments of important habitat conditions rather than appropriate data for trend analyses.* 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. *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: Recommendations are specific to given sets of conditions for two reasons: (1) monitoring of CHF prespawning mortality is not needed during years of average or high flows during summer (ODFW 1992 and experience during succeeding years) and (2) monitoring of ramping rates is not needed unless there are rapid decreases in reservoir outflows (ODFW 2000 and experience during succeeding years).*

Page 131, Item 4 – The ODFW habitat protocol is for wadeable streams; see Peck *et al.* (In Press) for methods for nonwadeable rivers. *ODFW response: An edit was made in relation to this comment.*

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. *ODFW response: ODFW concluded that annual variations (substantial) in CHF maturity rates negate the utility of this proposed approach.*

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. *ODFW response: ODFW agrees that such surveys would provide very useful information in relation to physical features of CHF habitat. However, as outlined in the limiting factors section of the technical draft of the plan, direct linkage to the primary factors that (could) limit attainment of desired status is not readily evident. With monitoring funds in very short supply, ODFW concluded that it was best not to include the recommended surveys as a monitoring need. If the plan was to state such a need, then stakeholders and others would conclude that ODFW is obligated to try and identify funding for such an effort.*

Page 131, "Coastal Stratum" Item 1 – It seems wise to conduct such evaluations annually, not just every 15 years. *ODFW response: ODFW has a different opinion on this matter. The substantive variation and uncertainty in recruitment estimates makes it impossible to appropriately interpret the results on an annual basis and results in wasted effort. However, it is possible to annually monitor and report SMU status, as proposed within the draft. This approach makes more effective use of limited ODFW resources. No edit made.*

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. *ODFW response: ODFW is unable to publish the numerous historical agency reports within scientific journals. However, the agency made considerable effort to generate and provide digital copies of many Rogue related reports to the Hannon Library Digital Collections at Southern Oregon University. Those reports can be accessed in the Bioregion collection at the following website:* http://soda.sou.edu/

Page 156 – How good are the spawner counts in tributaries of the lower Rogue for estimating spawner abundance of the entire Lower Rogue population? *ODFW response: This topic is addressed in the draft plan and in later in APPENDIX D. No edit made.*

Page 162, 1st paragraph – Prespawn mortality of 2% is not high, but elsewhere in the text prespawn mortality is described as being high. *ODFW response: An edit was made in relation to this comment.*

Page 162–163 – The estimates of spawning escapements have many assumptions and limited data. This potentially introduces errors in subsequent applications. *ODFW response: True, however, there is no other data that could be used to improve the estimates so the options are very limited. No edit made.*

Page 166 – Spawning escapements for the Lower Rogue and Coastal populations appear to be over- estimates. *ODFW response: Do not understand comment. How was this conclusion reached? No edit made.*

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. *ODFW response: Edits made throughout draft in relation to this matter.*

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. *ODFW response: Point noted but it should be also noted that neither of these two potential actions were embedded in management alternatives preferred by either ODFW or by any member of the public advisory committee. Consequently, neither action will be undertaken as a result of plan implementation. No edit made.*

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. *ODFW response: Point noted and it should be also noted this action is embedded in both management alternatives preferred by either ODFW or by any member of the public advisory committee. Consequently, this action will be undertaken as a result of plan implementation. No edit made.*

Page 227 – The reason for Item A12c is not clear. *ODFW response: An edit was made in relation to this comment.*

Page 227, Items A12e and A12f – Can the pinnipeds be trapped and moved elsewhere? *ODFW response: Yes.* Has relocation proved effective elsewhere? *ODFW response: No. Edit made.*

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. *ODFW response: Point noted but it should be also noted that this potential actions is not embedded in management alternatives preferred by either ODFW or by any member of the public advisory committee. Consequently, the action will be not undertaken as a result of plan implementation. No edit made.*

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). *ODFW response: An edit was made in relation to this comment.*

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## ODEQ comments - Technical and Co-Manager Review Draft of July 8, 2011

ODFW notes in parentheses

[HT1]: Define primary and secondary

(re: classification of CHF spawning habitat in the Rogue River Basin)

*ODFW response: The table title states "Classification base on……. generalized ODFW observations". Title appears to address comment so no edit appears warranted.*

[HT2]: Since the time these surveys were complete a number of land use edits and hydromodification has occurred, including gravel removal, dam construction, and bank stabilization which impact gravel recruitment.

(re: 1944 gravel surveys)

*ODFW response: The paragraph states "the only source of information originates from….surveys conducted in July-September of 1944. It goes without saying that development has affected habitat so no edit appears warranted.*

[HT3]: Applegate dam has minimized the amount of gravel transported through the system and gravel extraction is present in the watershed.

(re: gravel not likely a limiting factor)

*ODFW response: The paragraph states "gravel abundance is not likely a primary factor that limits NP CHF production…….. with the possible exception of localized areas". Two relevant items: (1) the draft calls for maintenance of spawning gravel below Applegate Dam and (2) it has not been shown that gravel extraction outside of the channel has affected the amount of gravel in the channel. As a result, no edit appears warranted.*

[HT4]: Four means – Bank Stabilization impacts gravel availability.

(re: gravel recruitment blocked at spots, i.e. riprap)

*ODFW response: The paragraph states "gravel volumes in streams can diminish if recruitment rates are insufficient to compensate for the amount of gravel extracted from the stream channel". Bank stabilization affects rates of gravel recruitment so the plan verbiage appears to address comment and no edit appears warranted.*

[HT5]: Pushup dams in the Little Butte and Bear Creek tributaries are plentiful with extensive irrigation canals impacting tributary streams.

(re: influence on CHF spawning distribution)

*ODFW response: Tributaries of Little Butte Creek and Bear Creek are not used by spawning CHF so no edit appears warranted.*

[HT6]: Gravel extraction operations need to align with gravel recruitment. A sediment transport study would provide the necessary information to quantify how much gravel is entering the system.

(re: minimizing impacts on CHF spawning habitat)

*ODFW response: No similar comment was received from DSL. The lack of a sediment budget does not preclude reduction of the risk based on the actions outlined in the sentence. As a result, no edit appears warranted.*

[H7]: Does Fish Lake impact stream flow?

(re: unregulated stream flow in Little Butte Creek)

*ODFW response: An edit was made in relation to this comment.*

[HT8]: Gravel recruitment from the upper watershed has been eliminated due to the construction of Applegate Dam.

(re: relationship to the flow-based section)

*ODFW response: The point of this comment is unclear because the subject of the paragraph is the increase in juvenile CHF habitat in the Rogue River Basin. Applegate Dam increased CHF distribution and production regardless of whether the dam is blocking gravel recruitment. As a result, no edit appears warranted.*

[HT9]: Development along the Rogue River and tributaries has resulted in channel alterations.

(re: relationship to the flow-based section)

*ODFW response: The point of this comment is unclear because the subject of the paragraph is the increase in juvenile CHF habitat in the Rogue River Basin. As the Rogue River flows primarily in a constrained channel and is the primary rearing location for juvenile CHF, it remains unclear how development has impacted the amount of rearing habitat. As a result, no edit appears warranted.*

Comment [HT10]: Describe the second area defined by Ratti.

(re: estuary habitat classification)

*ODFW response: An edit was made in relation to this comment.*

Comment [HT11]: Unclear why temperature is the only limiting factor for adult CHF. Applegate is listed for dissolved oxygen levels and limited by flow and habitat modification. Temperature, dissolved oxygen, and fine sediment have been identified as pollutant stressors that affect fish and other aquatic life throughout the basin. Meyers, 2011. Rogue Basin: Integrated Watershed Approach, Status Report and Action Plan 2011 to 2015. Oregon Department of Environmental Quality, Medford.

(re: quality of freshwater habitat)

*ODFW response: The conservation plan is required (by policy and ORS) to identify the primary factors that likely limit the abundance and diversity of the target fish populations (in this case CHF in southern Oregon). This comment appropriately covers known stressors in a general sense over the entirety of fish habitat in southern Oregon, but that does not mean that those stressors are primary factors that limit CHF production and life history. It's generally known that different species of salmonids have different water quality needs. What is not well known is that different populations (same species) within the same basin may have different water quality needs. As a result, no edit appears warranted.*

Comment [HT12]: In some portions of the Rogue dissolved oxygen, nutrients, and pH have been identified as stressors. (Meyers, 2011)

(re: quality of freshwater habitat)

*ODFW response: See response to [HT11} comment.*

Comment [HT13]: As stated in the previous paragraph, juvenile NP CHF have migrated downstream and entered the Rogue River during the summer months. Temperatures in the mainstem Rogue rarely reach 26C except for the lower reach near the mouth. (Rogue Basin TMDL, 2008. Oregon Department of Environmental Quality)
http://www.deq.state.or.us/wq/TMDLs/rogue.htm

(re: conveyed water temperature)

*ODFW response: An edit was made in relation to this comment.*

Comment [h14]: USACE dams are located in the upper watershed and would not impact peak flow events resulting from urban stromwater runoff.

(re: effect of dams on peak flows)

*ODFW response: The USACE dams in the Rogue River Basin are operated to attenuate peak flows. As a result of dam operations, peak flows in downstream areas are reduced during periods when large volumes of urban stormwater enter the Rogue River. As a result, no edit appears warranted.*

Comment [HT15]: Unclear why temperature is the only limiting factor for adult CHF. Applegate is listed for dissolved oxygen levels and limited by flow and habitat modification. Temperature, dissolved oxygen, and fine sediment have been identified as pollutant stressors that affect fish and other aquatic life throughout the basin. Meyers, 2011. Rogue Basin: Integrated Watershed Approach, Status Report and Action Plan 2011 to 2015. Oregon Department of Environmental Quality, Medford.

(re: emphasis on water temperature effects on adult CHF)

*ODFW response: See response to [HT11} comment.*

Comment [prb16]: USGS Sediment transport study reports available for Hunter and Chetco. How does gravel harvest effect gravel supply? Chetco report indicates gravel harvest levels have exceeded recruitment

(re: gravel not likely a limiting factor)

*ODFW response: Gravel harvest in the Chetco River and Hunter Creek occurs primarily downstream of CHF spawning, but there could be some impact on CHF fry production in localized areas. However, the quality and quantity of rearing habitat appears to be the primary factor that limits CHF production in the coastal basins, gravel availability is probably not a primary limiting factor.*

Comment [prb17]: Water is also available for appropriation during high flow (winter) and for residential uses. Exempted uses such as stock watering can fluxuate.

(re: water availability)

*ODFW response: The point of this comment is unclear because the subject of the table is water availability during summer, which is the critical period of rearing for juvenile CHF. As a result, no edit appears warranted.*

Comment [prb18]: Unstable gravels resulting from gravel harvest during years when flows result in early lower river Chinook spawning.

(re: gravel not likely a limiting factor)

*ODFW response: ODFW is not aware of any data directly related to this matter and the comment runs somewhat counter to the views of the local ODFW biologists. A request was sent to determine if DEQ has such data.*

Comment [prb19]: State of Oregon WQ standard for DO is +8mg/L DO and pH standard is 6.5 – 8.5.

(re: revise standards)

*ODFW response:* *The table was revised, as per DEQ recommendation.*

Comment [prb20]: Field DO Pistol River site is 5mg/l, median TP 0.04 in 2008, 2009 see LASAR. These numbers should be rechecked/updated. Let me know if you want me to do it.

(re: LASAR values need to be checked)

*ODFW response:* *The table was revised, as per DEQ recommendation.*

Comment [prb21]: We monitor a seasonally estuarine ambient site @ Pistol River

(re: DEQ sampling)

*ODFW response:* *Point noted.*

Comment [prb22]: Yes !! Larger dataset not included in this doc. supports this

(re: data sets available)

*ODFW response:* *As outlined in the Evaluation section of the draft, ODFW needs to quantify and characterize juvenile CHF rearing habitat in the non-Rogue estuaries during spring and summer (the draft calls for such an evaluation). This goal requires balanced sampling (spatial, temporal, and vertical components). A request was sent to determine if DEQ has such data for any estuary in Curry County.*

Comment [prb23]: Appendix H is not available for review - it there reference to FPA and AWQMP's? I would be interested in looking @ App H.

(re: convey Appendix H).

*ODFW response:* *A copy of Appendix H was sent, as requested.*