Independent Multidisciplinary Science Team (IMST)

State of Oregon

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December 30, 2005

Ed Bowles
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Dear Ed,

Attached is the Independent Multidisciplinary Science Team's (IMST) review of the draft Oregon Native Fish Status Report (ONFSR; *Public Review Draft* dated August 22, 2005). We commend your agency staff for a thorough and professional review of the status of some of Oregon's native fishes. We feel the draft report is a good first step in providing for the conservation of native fish, and sets the stage for your agency's ongoing efforts to do so. We recognize that a wealth of science information has contributed to the preparation of the ONFSR, and commend the State for its ongoing commitment to collection of the kind and quality of data that make these kinds of analyses possible. That said, we acknowledge that there are some significant data gaps in the ONFSR, and emphasize the importance of continuing to accumulate needed information to ensure scientific credibility in future assessments.

Our comments are mostly directed at strengthening the rationale behind the methods, inferences, and assumptions used by the ONFSR's authors, such that both the process and the quality of the data are transparent to the reader. We have also pointed out places where we feel the level of uncertainty should be more explicitly addressed, and the implications of those uncertainties to the report's conclusions should be more clearly disclosed. In addition, we point out some of the difficulties that arise from using the same analytical framework for species for which there are considerable data (i.e., Coastal coho salmon) and those where data are very limited.

We hope your staff finds these comments constructive, and helpful in crafting a more scientifically sound document. Feel free to contact us if you have questions about our review.

Sincerely

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IMST Review of ODFW's Oregon Native Fish Status Report: *Public Review Draft August 22, 2005*

Independent Multidisciplinary Science Team
Oregon Plan for Salmon and Watersheds

December 30, 2005

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Introduction

This document constitutes the Independent Multidisciplinary Science Team's (IMST) scientific review of the Oregon Department of Fish and Wildlife's (ODFW) draft Oregon Native Fish Status Report (ONFSR; *Public Review Draft* dated August 22, 2005). In a letter from Ed Bowles, ODFW Fish Division Administrator, dated November 4, 2005, the IMST was asked to address four specific review questions:

1. Were the data that ODFW used for each assessment appropriate for the analysis performed?
2. Were wrong conclusions drawn from any of the assessments?
3. Are there better assessment methods for any of the criteria than was used?
4. Did ODFW interpret the intent of the criteria in a biologically appropriate manner?

We commend the ODFW for its thorough and professional review of the status of some of Oregon's native freshwater and anadromous fishes. We feel the ONFSR constitutes a serious effort aimed at accurately portraying the complex issues and relative risks that are likely to affect efforts to sustain native fish in Oregon. We view the ONFSR as a necessary first step in conservation of native fish, and that it will provide a good foundation for ODFW's ongoing efforts to identify needed future actions, including additional surveys to fill in information gaps, development of conservation plans, and management activities.

Our comments are intended to be constructive and to help enhance the final draft, especially by pointing out places where the rationale behind classifications of risk status, and the level of confidence in the data or professional judgments used in the assessment, need to be clarified. Our comments should in no way be taken to imply that the IMST is not cognizant of the wealth of information that has gone into the preparation of the ONFSR, or feels that data have been grossly misinterpreted.

In general, we feel the ONFSR would be significantly strengthened by explicitly stating the rationale behind the methodology, inferences, and assumptions used by ODFW in each risk assessment, such that the assessment process and level of confidence in the data used in the assessments are made transparent to the reader. Many of our questions and critiques revolve around this issue. Also, pertinent references, including personal communications, should be cited to support assumptions, claims, and conclusions. If references are not available, the rationale or basis supporting the assumptions, claims, and conclusions should be provided. In the absence of rationale for assumptions and citations of supporting information, there is no way for a reader to judge the accuracy of risk classification, the associated uncertainties and their implications, or potential inadvertent bias. When data are limited or statistical power is low, the resulting risk judgment may have a high probability of being incorrect, and we feel this uncertainty should be somehow disclosed. We ourselves were unsure of the accuracy of the risk assessment for Species Management Units (SMUs) other than coho salmon, due variously to lack of data and supporting evidence, uncertainty about the quality of data, or level of confidence in assumptions, depending on the species.

In our review below, we first present general comments on the six interim criteria. These are followed by general comments on data shortcomings, SMUs and populations, editing, and final thoughts. Detailed comments on criteria, SMUs and populations, and editorial matters can be found in Appendices I–III attached to our review. We address ODFW's four specific questions within the framework of our review. All comments are based upon review of the printed hard copies of Volumes I and II that we produced from .PDF files provided by ODFW to the IMST. Our comments are NOT presented in order of importance.

General Comments on Criteria

In our review of the ONFSR, we tried to keep in mind that this is an interim effort meant to provide ODFW with a quick assessment of the near-term (5–10 years) risks to native fish in order to prioritize conservation planning efforts. That being the case, we did not review the ONFSR from the same standpoint as if it had been an assessment of risk over a longer term, where a higher standard of rigor would be required. We recommend ODFW view our comments not only from the standpoint of revising the draft ONFSR, but also to identify problems with the criteria and data that will likely affect future assessments and conservation planning efforts.

Also, we recognize that the criteria are a "given" for this assessment, and not subject to change. While we do not comment extensively on the criteria themselves (as per ODFW request), the problems we see with them inevitably color our views of the ONFSR's conclusions. We hope the ONFSR will be strengthened by the suggestions we make to overcome some of these problems. In 2002, the IMST reviewed the source document for the interim criteria, the Native Fish Conservation Policy (NFCP)[1]; our comments regarding the criteria are still relevant today.

We find it somewhat problematic that the NFCP and the ONFSR (Volumes I and II) use different wording for the criteria. We feel ODFW should ensure that this lack of strict unification of criteria does not lead to applications that do not adhere to the NFCP. In addition, it would be helpful if the ONFSR addressed the implications of what amounts to double- and triple-weighting of some criteria in several SMUs, which arises from using some criteria as surrogates for others where data are sparse or missing.

In general, we agree that the criteria incorporate appropriate factors of importance for risk assessment: existing populations, distribution, abundance, productivity, reproductive independence, and hybridization. Maximum individual size and number of expected size/age classes observed have been found to be useful criteria by others (e.g., Mebane et al. 2003; Hughes et al. 2004) in assessing status, and we recommend they be considered as additional criteria for the ONFSR.

[1]Letters dated Jan 16, 2002 and March 14, 2002 to Ed Bowles, ODFW Fish Division and letter dated November 7, 2002 to Lindsay Ball, ODFW Director, are available at www.fsl.orst.edu/imst/reviewproj.html

We see difficulties in the ONFSR where the thresholds or cutoff points used may not be appropriate for all species across the board (see Appendices I and II for specific examples) and end up being adjusted, weighted, combined, or otherwise modified, resulting in inconsistent application. While this may be necessary due to lack of data, we do feel the potential implications to confidence in the assessment's conclusions should be discussed. The current criteria thresholds appear to be salmonid-centric, and not necessarily appropriate for species other than anadromous Pacific salmon and steelhead.

It would be helpful if the ONFSR could more clearly discuss how the difference between determining "at risk" and "not at risk" impinges on how the criteria are used. Metrics used and threshold decision points are most likely different between analyses conducted to establish risk to species as opposed to those with the objective of establishing lack of risk. This would be analogous to differences between listing and delisting decisions under the federal Endangered Species Act. It would also strengthen the analyses if the risk assessment model employed (the criteria and methods for drawing conclusions from the criteria) could be validated, for example by using the model on native Oregon fish species known to be "not at risk". Species such as redside shiner, largescale sucker, or speckled dace could be run through the risk assessment process as a positive control, to see if risk determined by the six ODFW criteria would yield similar conclusions.

Data Shortcomings

The paucity of recent data needed for a sound risk assessment is of concern, both to IMST and ODFW. Successful conservation of Oregon's native fishes requires up-to-date information to enable accurate risk assessment. Indeed, "pass/fail" trends in the most recent five-year period are an essential component in five of the six interim criteria in the NFCP, but in many cases, the most recent data presented are over five years old. As an overall comment, we feel the implications of the data limitations need to be clearly spelled out in the ONFSR. In addition, it would increase understanding of the data if graphs, where possible, included confidence intervals or ranges for data points or trend lines so that the reader can distinguish whether the changes in the trends are meaningful or rendered ambiguous by the variation in estimates.

We find no problem in applying the criteria for coho salmon. However applying the criteria across all SMUs seems problematic where data are missing. Our January 16, 2002 technical review of the NFCP pointed out that lack of available data on abundance, distribution, productivity, connectivity, life history strategies, and habitat requirements of native fishes impair risk determination. We thus acknowledge the difficulty faced by ODFW in attempting to ascribe risk for SMUs for which there are few or no data. Explicit admissions of data limitations for each population, and the implications of the resulting uncertainty to risk determination, should be provided to allow the reader to evaluate data and risk assessment rigor.

The confidence one has regarding the final assignment of risk is closely tied to the quality of data used in the analyses and the number of extant populations within an SMU. It is difficult to have confidence in conclusions from the analyses where one sees numerous "assumed" metric values (i.e., asterisks) in the summary tables. Without a randomized study design, questions arise about conclusions where the analysis involves extrapolating data from one population to an adjacent population with no data, or presuming status of several populations from one or few populations within an SMU for which data exist. We feel that extrapolated or "assumed" information should be more clearly identified as such, and again encourage full disclosure of consequences to the final risk determination where there is high uncertainty in the data.

It appears that the SMUs can be categorized into at least four different types based on data quality and status certainty:

1) those for which there are ample data and number of populations so that the criteria, as written, are appropriate (e.g., coastal coho salmon),
2) those for which there are some solid data but there are too few populations to allow an 80% rule to be appropriate (we are unaware if any of the assessed SMUs would fall into this category),
3) those that include a large number of populations but for which there are insufficient quantitative data to allow assessment of the criteria as written (e.g., redband and cutthroat trouts), and
4) those that have a small number of populations and for which there are insufficient data (e.g., sturgeon and lamprey).

We suggest that a different format for summarizing the analyses for each of these different "types" of SMUs be considered, to reduce confusion regarding the soundness of the final risk ascribed by readers who only examine Volume I of the ONFSR. In addition, some kind of ranking of the confidence in data quality (including professional judgment or knowledge, and anecdotal information) and assumptions used in the risk assessment (even as simple as "high" , "moderate", or "low") would allow the reader to better evaluate the quality of the risk assessment. Simply stating that something is "not known" or "has not been examined" would be much more clear and useful than "is expected", "likely", "it is presumed that", "it is assumed that", "…is believed that", "was not considered", or "has not been identified".

Information-based decisions require sound scientific data, not assumptions. ODFW's ability to make future decisions about fish populations would be greatly strengthened if the data shortfalls identified in the ONFSR were used to design a monitoring program to obtain the critical data over the next decade. This is entirely feasible if biologically relevant monitoring and management units are sufficiently large. We suggest ODFW consider a proposed random probabilistic sampling design described by Dent et al. (2005) that incorporates major basins and ecoregions into the design and could an adequate number of sites could be monitored quantitatively through state and federal collaboration.

SMUs and Populations

A clear framework for delineation of SMUs is important, both because the ability to accurately assess risk status of a fish taxon rests on it, and because it provides the basis for implementation of management plans and monitoring. There are significant differences in the strength of classifications used to recognize the SMUs of the different species in this analysis. The ONFSR would benefit from clear explanations of how each SMU and population was determined. SMU and population delineations currently appear inconsistent, and driven by the amount of available data. If so, this should be described, and the consequences to the conclusions of the assessment should be disclosed. One can have considerable confidence in the classification of salmonid populations where genetic and life history data abound, but quite little confidence in the classification of species such as sturgeon and lamprey for which there are few data. The strength of SMU classifications for other species evaluated fall somewhere in between these two extremes.

Some questions that could be addressed in the ONFSR include: Why is it assumed that some populations are representative of an SMU? If the abundance of some populations is representative of other populations within an SMU, are they the same population? In addition the identification and classification of populations within an SMU as "independent", "potentially independent", and "dependent" should be considered for all species and SMUs, as was done for coastal coho, to the extent the data allow.

Given that the bases of the SMUs are populations, it is imperative that the word *population* be precisely defined. The definition of *population* given in the "glossary" section of Volume I is weak, and does not differ in any great measure from the common definition of *species* used in the biological species concept (see Mayr 1964). However *population* is defined, this definition must be applied consistently across the SMUs, and the basis for any departure needs to be justified. For example, when comparing an SMU between species (e.g., mid Columbia fall Chinook vs. mid Columbia steelhead, and Snake fall Chinook vs. Snake summer steelhead), why are there different population designations? Refer also to the specific comments on the "Distribution" criterion in Appendix I and also to specific SMUs in Appendix II for additional examples.

Editorial Comments

As currently ordered, the species range from anadromous Pacific salmon and steelhead trout to non-salmonids (sturgeon and lamprey), then back to salmonids (redband trout, bull trout, cutthroat trout), followed by non-salmonids (chub and dace). We recommend that the order of the species in ONSFR Volumes I and II be arranged so that all salmonids occur before the non-salmonids. This ordering scheme would effectively range the available knowledge base from high (anadromous Pacific salmon and steelhead) to low (non-salmonids). It would also make sense to place coho as the first species (most information known). It makes no taxonomic sense to separate rainbow/steelhead so widely. The interior steelhead is the same subspecies as the redband trout, *Oncorhynchus mykiss gairdneri* (see page 81 in Behnke 2002) and should be considered in neighboring

sections. Also, rainbow trout and coastal steelhead are the same species and should be dealt with sequentially.

The draft document would benefit from a thorough, professional editorial review. The presence of numerous editorial errors and inconsistencies detracts from the quality of information presented in both Volumes I and II. There are inconsistencies in style and format between the SMUs, a likely result of multiple authors. Some specific examples of editorial issues can be found in Appendix III.

In some situations, the "pass/fail" indication was not in agreement between Volumes I and II (see Appendices for examples). Often criteria were stated to have passed in Volume I when the information in Volume II was either unclear or incomplete or indicated that a population should have failed. Often there is a decision to "fail" a population for a given criterion if there was a lack of information (see anadromous salmon and steelhead SMUs), yet this was not always the case (e.g., sturgeon).

We suspect that some readers will focus mainly on the figures and tables, and so we encourage the authors to make them as informative as possible. The tables presenting "pass" or "fail" judgments in Volume II would be strengthened by inclusion of information that would help the reader understand which criteria, modifications, and data actually went into the judgment. In many cases, table and figure legends could be more explicit and descriptive of the information presented in the table so that a lay reader would be able to understand the nature of the information being presented. All of the types of data represented need to be mentioned and defined in the titles, and definitions should be consistent. Information presented in the tables should coincide with the column headings, and where possible, should include data instead of qualitative information. For example, several tables have a mix of numbers and statements or sentences within a column that calls for quantitative information. Many of the tables had large font sizes, rendering the tables hard to read. Some of the summary tables appropriately use footnotes to indicate that the content is based on assumptions while similar tables for other SMUs do not.

Page numbering is incorrect in some places (watch for duplicate pages). In addition, page numbers do not always conform between the table of contents and the text. The text of Volume II would be easier to read if the paragraphs were indented and an additional line space was included between paragraphs or a larger space between paragraphs was used.

We suggest that ODFW consider merging Volumes I and II into a single document, combining all of the information (summaries, more in-depth analysis, supporting graphics, etc.) for each SMU from both volumes into one place, and eliminating redundancies and inconsistencies. This would make it much easier for readers to delve into the rationale behind the report's conclusions, depending on the level of interest. In the event that Volumes I and II are merged, ODFW might consider producing a glossy brief summary of the ONFSR's conclusions for wide public distribution, akin to the Oregon Plan biennial report for coastal coho salmon (OWEB 2005).

Final Thoughts

Most of the species considered in the ONFSR occur outside of Oregon. It would be beneficial for determining potential causes of population or SMU declines in Oregon if comparable information were available from neighboring states in the Pacific Northwest. Unfortunately, as far as we are aware, Oregon is the only state that has attempted classification of the status of its fishes. Similar efforts in neighboring states could benefit all parties by providing the basis for comparisons on a regional, range-wide scale. Public and private entities alike, concerned with documenting the status of native fishes, will be able to use the ONFSR as a model and blueprint for processes and format. In light of the above, we encourage ODFW to seek ways to circulate the ONFSR widely. This could involve targeting professional audiences through presentations on the process and the final product at local, regional, and national meetings (e.g., Oregon Chapter of the American Fisheries Society (AFS), Western Division of the AFS, and the National AFS meetings), through articles in national journals such as *Fisheries,* or in regional newsletters like the AFS Western Division's *Tributary*. In addition, we believe there would be considerable science education value in making printed versions of this information widely available at locations visited by the public, including hatcheries, regional and district ODFW headquarters, and various state and federal natural resource offices, wildlife refuges, dam visitor centers, and the State Capitol.

While we commend ODFW staff for undertaking such an ambitious analysis, we also urge the State to seek the means to expand the scope of scientific analyses so that the status of ALL of Oregon's fishes, including marine species, can be estimated at least qualitatively. It is clear that the accurate status of most of Oregon's fish species remains to be determined. In fact, the ONFSR does not mention that the assessment focuses on those species believed to be most at risk. We are not proposing that there should be more sensitive species, but an analysis that included all fish species would help alleviate the fear that some species are being overlooked and not receiving the protective attention they need. For example, eastern Oregon populations of lamprey and two Klamath sucker species (which are at risk and have an abundance of data upon which to base an assessment) may deserve inclusion.

We hope that ODFW further analyzes the results of the ONFSR. For example, perhaps a landscape analysis can be attempted to identify whether certain basins or ecoregions contain at-risk SMUs of several species, such that a more regional management plan could be targeted at benefiting them all. Among multiple not-at-risk SMUs, it is possible that multiple populations "fail" the criteria in the same geographic area. This could lead to either taxonomic reclassification of the SMUs or call attention to particularly troubling subregions or subbasins.

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APPENDIX I
Specific Comments on Criteria

We recommend the ONFSR more clearly discuss the rationale for application of criteria to SMUs containing fewer than five populations, in which criteria based on 80% of populations is illogical.

Volume II frequently uses the lead-in sentence, "For this report…" which may convey the impression that the assessments and criteria are not standard. Is this really the intent?

For most populations, a qualitative assessment of limiting factors should be included (flow and channel alterations, water rights, land use, etc). Currently the document focuses too much on fish biology and omits key environmental factors, such as landscape conditions and chemical, physical, and biological habitat.

The word "historically" is used throughout the document in application of the criteria, but the time frame that the authors are referring to is unclear. It would be helpful to have a short discussion about what the authors mean by their use of the term, and also a bit of rationale as to why they feel that particular timeframe is appropriate (for example, given known climatic fluctuations, evolutionary history of fish, etc.).

EXISTING POPULATIONS

- The "Existence" criterion seems to contain a problem where the populations within an SMU have vastly different numbers of individuals. For an SMU to pass this criterion, 80% of all historical populations within an SMU must exist and not be at risk of extinction. Suppose 99.9% of the individuals of an SMU existed in one fifth-order stream population and are "at risk", and the remaining 0.1% existed in 50 first-order stream populations and are "not at risk", for an extreme example. In this example, if each of the small streams receives the same weight as the one large stream, this criterion would be ranked "pass". We suggest that statistical distribution of population size and importance may need to be considered in application of the 80% rule for "Existence" in certain instances.
- Ignoring extinct populations in an SMU seems scientifically problematic. We believe extinct populations in an SMU should be factored into the risk assessment like any other population, in contrast to focusing on exclusively on extant populations (see page 5, Table 2 of Vol. II and also comments on Snake fall Chinook and mid Columbia fall Chinook below).

DISTRIBUTION

- Volume II clearly acknowledges that detailed survey data on distribution exist only for coastal coho (page 8). That implies that there are insufficient data to support use of this criterion for the other species. Presumed access to historical areas is an inadequate surrogate for distribution because distribution into marginal habitats is often driven by high abundance in core habitats.

- It is not clear how estimates of historical distribution can be considered conservative (see sentence 6 of paragraph 2, page 8 of Vol. II) and yet overestimate habitat use at the same time (see last sentence on page 8 of Vol. II).
- The methodology reasons that "Habitat lost in smaller streams…is not likely to account for 50% of any population, and thus does not appreciably alter assessment outcomes derived using data at the 1:1000K scale" (e.g., see lines 4–6, page 9 and lines 4–6, "Habitat Use Distribution" section, page 16; page 45 and throughout Vol. II). Where is the evidence and what is the logic to support the contention that habitat lost in smaller streams is *not likely* to account for 50% of any population?
- The habitat use distribution criterion is based on current and historically accessible areas; why not on actual habitat use (e.g., page 16 and throughout Vol. II)? For example, there is some suggestion that the habitat usage criterion does not adequately express reality (see last paragraph in the "Habitat use distribution", page 280 and elsewhere). In some areas it is not clear how habitat use was determined (e.g., see last sentence, page 113; last 3 lines of page 187).
- The methodology indicates that the data on habitat use distribution do not reflect changes in habitat usage resulting from changes in habitat quality or availability of estuarine habitat (e.g., first sentence of 2nd paragraph, page 16 and throughout Vol. II). Why not? Should the assessment of habitat use distribution be considered as a snapshot, and if so, how representative is that snapshot? Will it change with season? How useful is the habitat use designation, and could it be improved by including available habitat quality data, including water quality and quality of the riparian zone (see DEQ, ODF, EMAP riparian habitat metrics such as canopy density and complexity)? The questions we pose here could be addressed up front in a few brief sentences.
- In some cases, the methodology indicates an absence of records for particular fish using a particular habitat (see lines 3–4, 3rd paragraph, page 73 of Vol. II). Did these claims take information from Native Americans into consideration? What is the possibility of indicating a lack of habitat use when fish really do use the habitat (Type II error; i.e., how confident are you about your claim)?
- Criteria for examining habitat use do not consider type of use (e.g., migratory corridor, rearing, or overwintering habitat; see paragraph 2, page 157 of Vol. II). This is a serious shortcoming, because any one can be limiting.
- Whereas influences of major dams on habitat use distribution are included in the judgment of a pass/fail of the habitat use distribution criterion (see 2nd to last sentence, page 107 of Vol. II), there is no quantification of specific minor dams and culverts. Numerous, unidentified barriers or minor dams could collectively impede access to substantial areas of habitat. The methodology acknowledges that it is not able to account for habitat area lost in smaller streams because of barriers (see third bullet, above), but assumes that this habitat area is negligible; how is that known?

- Regarding major dams, it would be helpful for the reader if ODFW: 1) included dams on all population maps, and 2) identified the dams in such a way that the reader can distinguish dams with fish passage from dams without fish passage (upstream and downstream).
- How can accessible reaches occur above inaccessible reaches (e.g., Ecola Creek on map, page 27; Umpqua River on map, page 40; map on page 229 and throughout Vol. II)?
- Pages 149–154 are redundant with pages 142–146 in Vol. II).
- In the redband trout and bull trout sections, can frequency of connectivity (typically used in habitat use distribution criterion) be better defined (e.g., year-round, seasonally, or ~ every 10 years)? Which is adequate? Also, specifically how were 10 km and 10% (redband trout) and 10 km and 50% (bull trout and cutthroat trout) of habitat determined? Ten km and 10% of habitat (not in agreement with "Habitat use distribution" criterion) were often interchangeably used, and it is not clear on what basis this interchange occurred. Nor is it clear that 10 km equates with 50% of historical habitat for bull trout or cutthroat trout.
- The use of 10 km of stream occupied as the "minimum habitat necessary to support the abundance of a viable population" for redband trout and other species should be supported by data, citations, and/or rationale used to develop this "professional judgment". We are not convinced that 10 km is sufficiently long enough to be used as the decision point for the species where it is applied.
- Why does the map for the Applegate population of the Rogue winter steelhead (page 249 of Vol. II) differ from mid-Rogue coho (page 185 of Vol. II)? This is one example of what appears to be an ongoing discontinuity in the way populations are defined (see comments on SMUs and Populations below).
- Why does the map of the Walla Walla population of the mid Columbia fall Chinook SMU terminate at the Washington state border (page 78 of Vol. II), whereas the map for the Walla Walla population of the mid Columbia summer steelhead expands beyond the Washington state border (page 308 of Vol. II)?

ABUNDANCE

- Many (but not all) tables fail to list the ages and lengths in the "Abundance" section of Vol. II. If these data are not specifically stated, the ONFSR should explain whether the sampling includes juveniles and adults, or one or the other. This is especially important in the absence of a size range criterion.
- We found ambiguities in the presentation of data for the abundance criterion in Vol. I. For example, do the trend lines or bar graphs depict an average of all populations (see page 21 and throughout Vol. I)? Sometimes abundance data for hatchery and naturally-produced fish are combined when it would be more informative to show these data as separate trend lines (page 37 of Vol. I).
- There are numerous instances in the ONFSR where there is a lack of strict adherence to the NFCP's criteria. One example would be the abundance criterion,

which was not always accurately assessed with respect to the 25% of historical population levels (e.g., compare the 1st sentences of the "Abundance" section on pages 368 and 371 with the criterion at the front of Vol. II; there are other incidences of this inconsistency throughout this document). Some discussion of the reasons for and implications of this would be helpful.

- There appears to be some double-weighting of criteria in several SMUs, where one criterion substitutes for another because of missing data. One example is the first sentence of page 10 (Vol. II), where it appears that the ONFSR equates habitat with abundance (see also duplication of abundance and productivity criteria for redband trout on page 379 and distribution and abundance criteria for bull trout on page 397). Elsewhere, there appears to be a triple-weighting of criteria. For example, see coastal cutthroat trout ("Existing populations", "Habitat use distribution", and "Abundance" on page 443). Are there implications of this that should be discussed?
- In the last sentence of paragraph 2, page 10, Vol. II indicates "Periods shorter than 10 years were not used." However, the document frequently uses periods shorter than 10 years (e.g., see sturgeon, lamprey, redband trout, bull trout, cutthroat trout, chub, and dace sections).
- Should populations fail the abundance criterion when hatchery fish comprise 50% or more of spawners (see 1st sentence of last paragraph, page 10 of Vol. II)? This is not specifically stipulated in the NFCP nor is it stated in Vol. I.
- Abundance appears to be overestimated for anadromous Pacific salmon and steelhead because "peak counts" were the primary data source used (e.g., see 1st sentence, page 17, Table 8 on page 18, and elsewhere in Vol. II). Whereas there is some discussion of potential biases of peak count sampling for individual populations ("Population details", lines 8–10, page 61 of Vol. II), it would be more useful to provide some discussion of potential biases in the abundance section of each SMU. Again, it would be helpful if the presented data were accompanied with some designation of the quality or the confidence in the data. For the aforementioned example, which surveys were not conducted at the proper time of the spawning season? Which years were abnormal flow years?
- Are the "dead fish" (see caption of Table 8 and elsewhere in Vol. II) spawned out individuals or individuals whose deaths originated from unnatural causes?
- Where did the 5% value for marine survival come from (page 157 of Vol. II)?
- The authors should spell out "OCN", following the first usage (same page).
- The variation in the number of spawning adults necessary to prevent genetic drift or inbreeding is unclear within redband trout (different criteria for Upper Klamath Basin redband in comparison with other redband SMUs) and amongst redband trout, cutthroat trout, and bull trout. Whereas the determination of adequate numbers of bull trout adults relies upon the simulations of Rieman and Allendorf (2001), references for the much smaller number of adults for redband trout and cutthroat trout are old texts that, judging from their titles, do not directly address

redband trout or cutthroat trout biology. The variation in numbers of adults cited as being essential to maintaining genetic integrity is not clearly reasoned, and is vastly different amongst species. For example, the numbers of adults cited as necessary for Upper Klamath Basin redband trout and Coyote Lake Lahontan cutthroat trout seem particularly low in comparison to bull trout.

- How can the abundance criterion be assessed when proportions of hatchery fish are unknown (e.g., "Hood" population, page 60, and line 3 of page 61 and elsewhere in Vol. II)?

PRODUCTIVITY

- The "Productivity" criterion, as interpreted and displayed, is confusing and often unsupportive of the "pass" determination. In several instances (e.g., see coastal fall Chinook), the recruits per spawner is deemed to have passed, when the bar graphs do not clearly indicate that the 1.2 recruits per spawner threshold has been met in three of the most recent five years "when total spawner abundance is less than the average abundance of naturally produced spawners over the past 30 years".
- We believe that a threshold of spawner abundance should be defined and included in the "Productivity" criterion for each SMU. As currently stated, the "Productivity" criterion only assesses recruits per spawner, and productivity is determined to have passed if recruits per spawner is greater than or equal to 1.2. One notable example is the Winchuck population of Rogue fall Chinook. Should this population be considered to be productive even though it is decreasing?
- More information on spawner abundance estimates would be useful for the reader to gauge the quality of the data that is evaluated and presented. In addition, this would allow the reader to judge the appropriateness of the conclusions. For example, how many spawner estimates were conducted, and where were they conducted (e.g., 1st sentence, page 18 and elsewhere in Vol. II)?
- Using the slopes of curves and the variation in the data as metrics would improve the robustness of this criterion.
- For some populations, ODFW acknowledges a paucity of data and an inability to distinguish hatchery from wild fish (last sentence, page 12 of Vol. II). However, as noted in the abundance criterion above and the reproductive independence criterion below, assumptions are made (see "Sandy late-run" population, last sentence, paragraph 2, page 63; see also pages 65–69 of Vol. II — lack of empirical data) and it is not clear on what basis these assumptions were made or whether they are appropriate.
- Data presented for the productivity criterion were often ambiguous in Vol. I. For example, it is not clear whether presented data span the last 5 years (e.g., see page 23 and throughout Vol. I).
- Age composition: Briefly explain the statistical basis for determining a sample size cutoff of N = 50 (see line 6 of page 18, page 46, and elsewhere in Vol. II)?

How was this determined and how does this compare with aggregate age composition from all years? The number of individuals that must be sampled from a population to reflect conclusions about productivity should be based on a consideration of the age structure of the population, as well as age of first maturity and whether or not the fish spawn annually. This is a particularly true for long-lived species and those whose population structure was not pyramidal. The sampling design necessary to reflect biological productivity is very species-specific.

REPRODUCTIVE INDEPENDENCE

- What data were used to denote a "high incidence" of hatchery fish (line 7, page 13 of Vol. II)? What is the evidence to support the contention that > 10% of hatchery spawners would equate with a lack of reproductive independence (e.g., why not > 12% or even > 20%)?
- Data presented for the reproductive independence criterion were often ambiguous in Vol. I. For example, do the bar graphs depict an average of all populations (see page 21 and throughout Vol. I)?
- In some circumstances, conclusions are drawn when data are lacking or of poor quality (e.g., lines 2–5, paragraph 1, last sentence of paragraph 2 on page 19 and elsewhere in Vol. II). In the example on page 19 of Vol. II, assessments for many populations were "based largely on hatchery release levels", yet there is an inherent inability to distinguish between hatchery and wild fish. The methodology assumes that mark rates of hatchery adults equal the mark rates of juveniles, yet there is no reason to believe that this actually occurs. For example, if 50% of juveniles are marked and 50% of these marked juveniles died upon ocean entry, mark rates of returning adults would only be 25%. How do these difficulties in the data and interpretations affect the overall conclusions?
- It is not always clear which analyses were conducted to determine spawner fractions (e.g., see lines 2–4 of page 159 and elsewhere in Vol. II).

HYBRIDIZATION

- What is the quantitative definition of "rare" or "non-existent" hybrids (see page 397 and throughout Vol. II)?
- This is not a relevant criterion for most SMUs, and its inclusion biases risk assessments towards a determination of "not at risk".
- Often hybrids are mentioned when it is questionable whether hybrids can be accurately and consistently identified (e.g., refer to redband trout, bull trout, and cutthroat trout sections in Vol. II). In addition, there are several instances in which there is evidence that another species has been repeatedly stocked within an SMU, but it is not considered to hybridize with the natural redband trout or natural bull trout (i.e., allopatric — upstream stocking or inhabitance of non-native fish vs. downstream occupancy of native fish). Are there any radiotelemetry data to support the contention that there is no movement between stocked rainbow or

cutthroat trout and redband trout, or between stocked brook trout and bull trout, especially when habitat quality deteriorates (i.e., habitat and hence species become condensed) or improves (high flow improves connectivity)? Salmonids are known to stray, particularly when hatchery fish have been reared at a location different from the stocking location (Quinn1993; Hayes and Carmichael 2002).

Literature Cited in Appendix I

Hayes, M.C. and Carmichael, R.W. 2002. Salmon restoration in the Umatilla River: a study of straying and risk containment. Fisheries. 27: 10–19.

Quinn, T.P. 1993. A review of homing and straying of wild and hatchery produced salmon. Fisheries Research. 18: 29–44.

APPENDIX II
Comments on Specific SMUs and Populations

Please note: These comments are not exhaustive, and the problems we highlight in this section for specific species and SMUs often recur in other places in the ONFSR.

FALL CHINOOK

Coastal Fall Chinook — What is the basis for passing the Umpqua population on abundance trends and assessment outcomes of other populations (Table 9, page 19, Vol. II)?

Determination of abundance for the Yachats population is dependent upon harvest, yet the reader does not know the extent of harvest effort. It should be clearly stated whether or not harvest efforts were constant (page 36, Vol. II).

Lower Columbia Fall Chinook — What exactly is meant by "induced thermal marks" (4th to last sentence, paragraph 5, page 62 of Vol. II)? Also, it is not clear what math was used to combine the two percentages garnered from CWT's in carcasses and thermal marks in otoliths of unmarked fish to correct the redd count of 117 down to 26 (next sentence).

Mid Columbia Fall Chinook — "Where more than 20% of historical populations have become extinct, an SMU is considered potentially at risk…" (see sentences 1–3 of paragraph 2 on page 7; see also figure at bottom of page 74 of Vol. II). It would seem that the lack of existence alone should place the mid Columbia fall Chinook "at risk" versus "potentially at risk" (see comments on "Existence" criterion in Appendix I).

Snake Fall Chinook — The maps in Vol. I should show extant and extinct populations. For example, page 26 (Vol. II) depicts Snake fall Chinook from northern to southern OR as one SMU. Although there are extant populations below Hells Canyon Dam, the populations above the dam are all extinct. Extinct populations should be reflected in the assessment, perhaps in this case by changing the risk status from "potentially at risk" to "at risk". Extinct populations in an SMU should be considered along with extant populations (see page 5, Table 2 of Vol. I; also see "Existence" criterion above).

Klamath Fall Chinook —Why is there no review for Klamath fall Chinook? This would be easy.

SPRING CHINOOK

Rogue Spring Chinook — The last sentence in the "Abundance" section (page 97 of Vol. II) indicates that harvest estimates for 2003 and 2004 were not available. Why aren't

these data available? Also, what is the basis for the assumption that the harvest rate of naturally produced fish during 2003 and 2004 is the same as the average of wild fish harvest during 2001-2002?

What is the basis or evidence for the assumption that 5% of adult spring Chinook returning to Cole Rivers Hatchery were naturally spawning hatchery fish (2nd sentence in "Productivity" section, page 97 of Vol. II)? What is the supporting evidence for using the average age composition for 1974–1994 to represent that of 1994–2003 (2nd to last sentence, page 97)?

The Rogue spring Chinook SMU appears biased (page 32 of Vol. I); the SMU does not depict as many basins as Rogue fall Chinook (page 20).

Willamette Spring Chinook — Page 107 (1st sentence of 2nd paragraph under "Habitat use distribution" of Vol. II) indicates that the assessment did not account for lost access to habitat above several dams because of the presence of trap and haul programs. In the next sentence, the authors state that "habitat above those barriers was classified as inaccessible". What is the basis for the assumption that there is no habitat loss above dams because those areas are inaccessible? Is the implicit assumption that dams are part of the historical habitat or that trap and haul programs effectively mitigate for dams (assuming that dams are not part of the historical habitat)? What is the basis for the assumption that dams are part of the historical habitat or that the trap and haul program is effectively mitigating for the obstruction of the dam? The trap and haul programs would not seem to be a sustainable option; therefore, habitat above dams should probably be classified as lost access, or at least dependent on long-term implementation of artificial measures.

At 43% accessibility, the Upper Willamette population of Willamette spring Chinook should be noted as a "Fail" (Table 47, page 107 of Vol. II).

The "pass/fail" indication was not in agreement between Volumes I and II. Page 108 of Vol. II indicates that the Molalla population failed the "Productivity" criterion. However, the summary table in Vol. I (page 36) indicates an incomplete data set for productivity of the Molalla population.

Mid Columbia Spring Chinook — What is the basis for the assumption that hatchery fractions in 2002–2004 were equivalent to the average hatchery fractions of 1997–2001 (last sentence, page 118 of Vol. II)?

What is the basis for the assumption that age compositions in 2002–2004 were equivalent to the average hatchery fractions of the previous five years (last sentence, 1st paragraph, page 119 of Vol. II)?

How were hatchery and wild fish distinguished in the John Day population (last sentence, "Reproductive independence" section, page 119 of Vol. II)?

Snake Spring Chinook — Should this SMU be the "*lower* Snake spring Chinook" to distinguish it from the "*upper* Snake spring Chinook" SMU?

What is the basis for the assumption that the 2004 age composition of Snake spring Chinook was equivalent to the average age composition of the previous five years (last sentence of "Productivity" section, page 130 of Vol. II)?

Coastal Spring Chinook — Abundance estimates were noted to have been "somewhat sporadically monitored" (see top of page 84 of Vol. II). What exactly is meant by "sporadically monitored"? It would be useful to provide explicit information.

The methodology for estimating productivity of coastal spring Chinook appears to have two significant problems. First, the authors use data from other criteria (for which there is an inability to distinguish wild progeny from hatchery — see "Reproductive Independence" in the same section). Second, in the absence of other data the authors substitute age composition for Rogue spring Chinook, even though they acknowledge important differences in life histories between Rogue spring Chinook and northern Oregon coast spring Chinook.

COHO

Coastal Coho — It is not clear how populations were delineated in this SMU (see 2nd line, page 156 of Vol. II). What is meant by the sentence, "Of the 67 populations, 19 are identified as *independent* or *potentially* independent" (italics ours)? It would be useful to cite the ODFW coho report. The caption for Table 63 states that "Italicized populations are dependent populations"; presumably the authors are referring to the other 48 populations, which are not (and should be) listed in the table. What is specifically meant by the column header "Description" in Table 63?

How was the methodology for determining habitat occupancy established (2nd to last sentence, paragraph 1 in "Habitat use distribution" section, page 156 of Vol. II)?

The footnotes to Table 65 (page 158 of Vol. II) indicate that the assessment of the "Floras" population is based on the outcome of the "Sixes" population, yet there is no estimate of full seeding for the latter.

It would be useful to include a brief discussion about naturalized or stray coho that occasionally occur above Willamette Falls.

Lower Columbia Coho — On the last 3 lines of paragraph 3, page 187 of Vol. II, the authors suggest likely distribution patterns in relation to flow. Are there any data available on distribution patterns in relation to flow such that this information could be discussed with some certainty?

STEELHEAD

Redband trout are the same subspecies as interior steelhead (*Oncorhynchus mykiss gairdneri*; see page 81 in Behnke 2002); as such, the status review of each should be considered in adjacent sections of the ONFSR. The fact that resident and anadromous forms can hybridize and that each form can be derived from the others deserves more attention.

Coastal Winter Steelhead — The only abundance trends with > 30 years of data are for the North Umpqua (2nd sentence, page 206 of Vol. II). This makes conclusions regarding the other populations tenuous.

What is the basis for the assumption that the remaining 21 populations of coastal winter steelhead passed the productivity criterion, based on the results of only 2 populations (last sentence, 1st paragraph, page 207 of Vol. II)?

What is the basis for the assumption that the remaining 22 populations of coastal winter steelhead passed the abundance criterion, based upon assessments of only 1 population (lines 8–9 and 14–15, page 206 of Vol. II)?

Rogue Winter Steelhead — What evidence or logic is there to support the contention that the abundance and productivity data for the mainstem Rogue and Applegate populations are representative of the remaining six populations (page 239 of Vol. II)?

Lower Columbia Winter Steelhead — It is not clear what the basis is for comparing some SMUs to other SMUs as a means to gauge criteria such as abundance (e.g., comparing Lower Columbia River steelhead to Coastal steelhead, page 253 of Vol. II).

What is the basis for the assumption that the Clatskanie population passed the "Reproductive Independence" criterion (Page 252 of Vol. II)? No reason is provided in the discussion of this population (page 254).

Willamette Winter Steelhead — What is the basis for the assumption that the remaining 5 populations of Willamette winter steelhead passed the abundance criterion, based upon assessments of only 4 populations (last 2 sentences of "Abundance" section, pages 266–267 of Vol. II)?

What is the basis for the assumption that four populations pass the productivity criterion, despite "very limited data" ("Productivity" section, lines 5–6, page 267 of Vol. II)?

CHUM

Coastal Chum — Is it not likely that the populations in this SMU were grossly overfished nearly to extinction before these data sets started (Table 131, page 327, Vol. II)? There should be some discussion of this possibility in Vol. II.

SOCKEYE

Snake Sockeye — There is some discussion of the extinct Wallowa population (page 354 of Vol. II). What about kokanee in Wallowa Lake? Some brief discussion about these fish would be informative.

STURGEON

Southern & Northern Green Sturgeon — We suggest that the discussion of this species in Vol. II would benefit from a more thorough analysis of the criteria based on the life history characteristics of the species. There is also a strong need for citation of references and data to back up a number of the qualitative claims and assumptions. Criteria based on factors such as "the last five years" are likely inappropriate for species with individuals that live 100 years. It is also a meaningless metric when fish do not reproduce until 10–20 years of age. Also, there is no mention of spawning frequency. Such a discussion should include an analysis of the consequences if sturgeon populations do not reproduce each year under natural conditions. Likely such long-lived fishes have extreme "boom or bust" years in reproduction and population age structure is not pyramidal. If very little is known about historical and current abundance of green sturgeon, how can the abundance criterion be assessed (sentence 2, "Abundance" section, page 364 of Vol. II)? In some cases, the methodology notes that "there is no indication that abundance of green sturgeon in Oregon is currently less than historic abundance…" (see lines 7–10, page 365). What is the accuracy of this claim if current abundance really is less than the historical abundance (Type II error; i.e., how confident are you in your claim)?

Is the availability of spawning habitat known well enough that ODFW can estimate the area of available spawning habitat (page 365 of Vol. II)?

For the sentence beginning on line 13 (2nd paragraph of the "Productivity" section, page 365 of Vol. II), there is no mention of SMU — is it the northern or southern SMU or both? The sentence beginning on the next line should mention the dammed, historically-used rivers as well.

White Sturgeon — We suggest that the discussion of this species in Vol. II would benefit from a more thorough analysis of the criteria based on the life history characteristics of the species. There is also a strong need for citation of references and data to back up a number of the qualitative claims and assumptions. Criteria based on factors such

as “the last five years” are likely inappropriate for species with individuals that live 100 years. It is also a meaningless metric when fish do not reproduce until 10–20 years of age. Also, there is no mention of spawning frequency. Such a discussion should include an analysis of the consequences if sturgeon populations do not reproduce each year under natural conditions. Perhaps such long-lived fishes have extreme “boom or bust” years in reproduction and population age structure is not pyramidal. In addition, effects of the slot fishery needs to be considered; are there sufficient numbers of these “juveniles” surviving to become broodstock? What is the effect of “bycatch” of the “oversized” fish on the reproductive capacity of the populations? If populations are indeed increasing in abundance as stated, then why was the management practice of “trawl and haul” undertaken for the lower Columbia? This program is not mentioned and it would seem to be important information to consider with regards to the abundance criterion. Without this program, would the Bonneville reservoir population pass the abundance criterion? For criteria regarding abundance and productivity, what confidence limits would there be for abundance estimates of young age groups (0-age through age 5+ for example)? Indeed, without a good understanding of recruitment, how can ODFW assess the productivity (lines 1–2, page 370 of Vol. II)? Is there anything known about spillway downstream passage of juveniles at Columbia River dams? Is there any likelihood of this?

Information regarding historical and current abundance estimates for white sturgeon has problems similar to those that we noted for green sturgeon. The interim risk assessment indicates that white sturgeon for Oregon have no special status, whereas Idaho considers this fish to be a species of concern (middle of page 367 of Vol. II). How can the status of white sturgeon be assessed (see Table 139) when historical abundance levels are “relatively unknown” (line 5, page 368)?

Vol. II indicates a poor understanding of historical habitat use, yet “beliefs” are set forth with no supporting evidence or logic (see last 2 sentences on page 367 of Vol. II).

Classifying white sturgeon populations by dam reaches is inappropriate (page 367 of Vol. II). Dams are not historical features, and it is not clear that sturgeon are able to pass these barriers effectively; therefore, it seems that > 50% of historically-used habitat is no longer available. If one assumes that 30 years ago constitutes a historical assessment (NFCP), then some dams will pass this criterion. However, it seems possible that some individuals of this long-lived species, alive today, would have been present in the Columbia prior to impoundment. The reasoning given for population delineation is contradictory to the first six lines in the “Abundance” section (page 368 of Vol. II).

Is less documentation of upstream passage a result of methodology or actual biology (“Distribution”, page 89, Vol. I)?

What was the basis for using the average abundance and not the median abundance (line 7 of “Abundance” section, page 368 of Vol. II)?

Specifically where were the few hatchery white sturgeon released (sentence 2, page 372 of Vol. II)?

LAMPREY

There is a strong need for citation of references and data to back up a number of the qualitative claims and assumptions for these species.

There could be losses of lamprey from other sources besides smallmouth bass depredations ("Productivity" section on pages 372 and 375 of Vol. II). Documentation of mortality from other predatory sources needs to be added. The number of lamprey found in smallmouth bass diets seems relatively small and from the information presented, it is not clear whether losses to smallmouth bass are significant. If this is the case, it should be simply stated. However, while lamprey may comprise a small proportion of the diet of bass, bass are abundant in some rivers and their effect on lamprey populations may not be trivial.

Pacific Lamprey — We agree that there are almost no data upon which to base separation of the species into populations. The few data that exist suggest that there is no fidelity to natal streams. There is also no genetic data allowing taxonomic classification below the species level. However, there is also no convincing data that ESU's or gene conservation groups below the species level do **not** exist. Without evidence of homing behavior (and hence a distinct population structure), populations would seem to be arbitrary. Is it possible to use sea lamprey (*Petromyzon marinus*) biology as a surrogate for estimating homing behavior or population structure ("Existing populations" section, page 370)? We recognize the difficulty with trying to prove a negative, but the difficulty for this species rests with the almost complete lack of data. We suggest that criteria used for assessment of risk for this species needs to take into account the consequences of doing an analysis based on one large population in the species, if indeed there was some structuring along population lines.

Another important unknown for lamprey is the age(s) at which they reproduce. There are no data on how many years Pacific lamprey may spend at sea before retuning to freshwater. It is not unlikely that some fish may be 10 years old before reproducing. How this could affect risk needs to be considered. In addition, the potential that population age structure is not pyramidal but represents "boom or bust" reproductive strategies needs to be considered. If no data are available to assess productivity for lamprey, how can this criterion be assessed ("Productivity" section, page 372 of Vol. II)?

How can habitat use and distribution be assessed if it is extremely difficult or perhaps even impossible to identify ammocoetes to species? How is it known (or what sort of logic was used to determine) that distribution is "somewhat reduced" in certain

reaches if there is difficulty identifying species ("Distribution" section, page 370 of Vol. II)?

Are the "anecdotal observations" of local ODFW biologists ("Abundance" section, page 371 of Vol. II) really observations or guesses? If they are observations, this sentence would benefit from a citation of appropriate ODFW annual reports or "professional judgment". If the latter is used, some notation of the degree of confidence (for example, high, medium, or low) in the judgment would be useful. Also, the abundance section would benefit from the presentation of the recent dam counts. And, is this a species for which historical information from native Americans might be available?

Western Brook Lamprey — Without being able to quantitatively assess abundance, how can the abundance criterion be assessed at all? What sort of evidence is available to support the claim that western brook lamprey pass the criterion ("Abundance" section, page 375 of Vol. II)? If no data are available to assess productivity for lamprey, how can this criterion be assessed ("Productivity" section, page 375)?

The "Habitat" section (page 374 of Vol. II) notes that, "…it is believed that they occupy more than half of their historic habitat." What is this belief based on?

REDBAND TROUT

It would be more logical if the discussion of redband trout, being the same subspecies as interior steelhead (*Oncorhynchus mykiss gairdneri*; see page 81 in Behnke 2002), were placed next to the section reviewing that SMU. The fact that resident and anadromous forms can hybridize and that each form can be derived from the others deserves more attention.

The criteria used to assess the Upper Klamath Basin redband trout differ from other redband trout SMUs. Criteria used in all SMUs should be carefully compared to ensure consistency. In instances where the authors are unable to consistently apply the criteria across SMUs due to lack of data (or other reasons), these extenuating circumstances should be clearly identified, and the authors should discuss how the data, analyses, and conclusions resulting from the new criteria should be interpreted with respect to the "old" criteria (e.g., state quality and confidence of assessments).

What is the rationale for basing redband SMUs on the major pluvial lake basins (top of page 377 of Vol. II)?

The document states that "current distribution and abundance is treated as an indication of past population trend". What logic and supporting evidence is there for this assumption (bottom of page 379 of Vol. II)?

Samples of < 10 and especially < 5 per year seem low (Table 148, page 379 and throughout Vol. II). In addition, often times only < 5 years of data are available. Mean density estimates compiled from limited yearly sampling data, compared over only a few years, are used as a surrogate criterion for lack of historical abundance data (previous 30 years). How confident is ODFW that limited sampling accurately depicts redband abundance? Over what time frame is the mean density of a population sufficient to serve as a surrogate for SMUs of redband trout ("Abundance" section, page 378)? What is the basis for using mean density and not median density? The mean subject to outliers more than the median. The abundance data seem particularly problematic in that the surveys are conducted only during high water years (Malheur redband trout, 2nd sentence of page 380 of Vol. II). It seems imperative to also collect data for low water years in order to adequately understand variability in abundance. In other redband SMUs, it is not clear whether the surveys were conducted exclusively in high water years.

For which years are the abundance estimates and assessments in the tables (Table 164 and elsewhere in Vol. II)?

Aren't depletion-removal methods a poor estimator for abundance (top of page 379 of Vol. II)?

What proportion of sites were randomly selected (2nd to last sentence prior to "Productivity" section on page 379 of Vol. II)?

Is there any concern over interbreeding between historically stocked hatchery rainbow trout (*Oncorhynchus mykiss*) and native redband trout (*O. mykiss gairdneri*)? Is this best classified as hybridization or reproductive independence? Would it affect the expression of the life history characteristics of the redband trout? It seems plausible that rainbow trout stocked in the past could maintain rainbow trout characteristics (life history, temperature tolerance, etc.) if they are not completely sympatric with redband trout. This underscores the need for more research, including morphological, genetic, distribution, movement, and life history work.

Warner Lakes Redband Trout — In Volume I, the interim assessment notes that this SMU is "potentially at risk" in the section heading when it should be noted as "at risk" (page 99 of Vol. I).

Catlow Valley Redband Trout — If the presence of redband trout is "uncertain" in Guano Creek (page 377 of Vol. II), how can it be judged to pass the "Existence" criterion?

Chewaucan Redband Trout — Trapping and hauling fish is not a sustainable fix to the problem of distribution (lines 2–5 of page 378 of Vol. II), so is it accurate to include the upstream distribution area for fish trapped downstream in determination of habitat use?

Is the 100 m sample site for abundance estimates random, and thus representative of the population (2nd line of page 379 of Vol. II)?

Malheur Redband Trout — What confidence does ODFW have in assuming that the densities and abundance in the Silvies population reflect the Coffeepot Creek population (2nd paragraph of page 379 of Vol. II)?

BULL TROUT

Criteria were often applied inconsistently among SMUs. In instances where this happens, due to lack of data (or other reasons), these extenuating circumstances should be clearly identified, and the authors should discuss how the data, analyses, and conclusions resulting from the new criteria should be interpreted with respect to the "old" criteria (e.g., state quality and confidence of assessments).

Year to year variability and lack of historical data could make the "Productivity" criterion subjective (middle of page 397 of Vol. II). Also, how many years are needed to discern whether a population is stable or in flux? Isn't a trend in a very small, "at risk" stock more difficult to detect than a trend in an abundant, "not at risk" stock?

Timing of sampling should be noted in other bull trout SMUs, akin to the Hells Canyon SMU (see bottom of page 449 of Vol. II).

Is the information presented in Table 189 (and elsewhere in Vol. II) anecdotal? What are the sources of the empirical data (snorkel surveys, reference literature)?

How can the percentage of historical distribution be 100% (Table 199, page 426 of Vol. II) for several populations within the John Day SMU if distribution is highly fragmented and limited (see top of page 425)? The Umatilla bull trout SMU appears to have a similar discrepancy (see Table 204 and bottom of page 431).

In the "Hybridization" sections for Willamette bull trout (page 410 of Vol. II) and John Day bull trout (page 429) the ONFSR states "In cases where little or no information is available, and bull trout and brook trout are sympatric, the ONFSR assumes hybrids are common". Apparently there is information to the contrary for the McKenzie population (Willamette SMU), as it states that "hybrids have never been observed during routine monitoring activities", and this population passes the criteria. The Upper John Day population similarly notes that "hybrids are uncommon" (Do the authors mean "rare"?), and this population also passes the criterion. How extensive were the monitoring activities and is the average biologist able to distinguish hybrids from pure bull trout (see specific comments above on "Hybridization" above)? What is the possibility of indicating an absence of hybridization when fish really do use the habitat but the sampling regime or the ability to identify hybrids is not sensitive enough (Type II error; i.e., how precise or confident is your claim)?

Hells Canyon Bull Trout — Are bull trout in Idaho naturally produced ("Additional Information", page 126, Vol. I)? Is there any potential for mixing with bull trout from Oregon?

Willamette Bull Trout — Are the McKenzie fry hatchery fish and is there any evidence of natural reproduction in the Middle Fork of the Willamette (page 410 of Vol. II)?

The graphs in the figure on page 409 all have different magnitudes represented on the y-axis, which hinders useful comparisons (actually, this comment applies to several graphs and tables).

Hood River Bull Trout — What is the efficiency of the nightly snorkel counts in the figure on page 412 of Vol. II)? Assuming these counts are representative of abundance per year, which the reader does not know with any certainty, it would be informative for the reader if these counts were expanded to population size.

Deschutes Bull Trout — The literal wording of the criteria may create a dilemma for this SMU. How can the Whitewater population pass the productivity standards when it is at risk of extinction due to inbreeding? Also, what about stochastic events (page 417 of Vol. II)? Similarly, how can the Warm Springs and Shitike populations pass the abundance criterion (Table 192; same page) when they are at risk of genetic drift (see last sentence before "Productivity" section)?

The criteria used in the "Productivity" section (page 417 of Vol. II) are different from other bull trout SMUs. A time series trend of five to 10 years is used, whereas other bull trout SMUs use only five years (see page 397 and elsewhere).

The graphs in the figure on page 418 of Vol. II have different magnitudes represented on the y- and x-axes, which can preclude useful comparisons. The graphs should include confidence intervals on the trend lines so that the reader can distinguish whether the changes in the trends are meaningful or muted by the variation in estimates. Are the more recent years' data indicating a decline?

Imnaha Bull Trout — Table 218 of Vol. II: Is the 8 fish/100 m^2 low relative to the 0.2 fish/ m^2 for redband trout (threshold for high abundance)?

CUTTHROAT TROUT

Criteria were often applied inconsistently amongst SMUs. In instances where this happens, due to lack of data (or other reasons), these extenuating circumstances should be clearly identified, and the authors should discuss how the data, analyses, and conclusions resulting from the new criteria should be interpreted with respect to the "old" criteria (e.g., state quality and confidence of assessments).

In both Volumes I and II, for the cutthroat trout SMUs, even though the ONFSR is often unable to adequately address the six interim criteria, the inadequacies in data and the necessary modifications to the interim criteria that are used are clearly stated. With some exceptions, the modified criteria and data analyses appear fundamentally sound, especially in comparison with other SMUs. The authors may also want to look at recent work done by researchers at the USGS Forest and Rangeland Ecosystem Science Center regarding sampling cutthroat trout distributions and extending the statistical scope of inference (e.g., Gresswell et al. 2004; Torgersen et al. 2004)

What exactly is meant by the sentence, "While this may seem to be using one measurement to assess two criteria, the *standard used here* for existence *is much higher* than was used in the assessment of this criterion for most of the other species assessed in this report" (italics ours; page 443 of Vol. II)?

We recommend using explicit numerical criteria where possible, such as 0.2 fish/ m^2 (page 443 of Vol. II) as was done for redband trout. The authors state that for abundance, "All available data (were) not thoroughly compiled and analyzed for this assessment." (page 444 of Vol. II). Why not? How was it decided which data sets would be included or excluded?

The Captions for the figures on pages 444 and 452 of Vol. II read "at least one" cutthroat trout. The y-axis of the figure is labeled, "Sites with Cutthroat (%)". Is the percentage of sites with ≥ 1 cutthroat sufficient to deduce habitat (page 444 of Vol. II) or abundance (page 452) trends? How meaningful is a data point that includes a sample size of one fish?

Oregon Coastal Cutthroat Trout — How consistent was the fishing effort among years (page 445 of Vol. II)?

Southern Oregon Coastal Cutthroat Trout — Is the 1,000 meter reach the distance between reaches or the length of the reaches surveyed (line 10, page 451 of Vol. II)?

Westslope Cutthroat Trout — What is the basis for not evaluating the three populations of Westslope cutthroat trout in the North Fork of the John Day River (top of page 473 of Vol. II)? It is ambiguous how potential abundance for sites was determined (page 474). The "Density" column in Table 243 (page 475) is rather coarse. What is the magnitude of difference between "slightly below" and "significantly below"?

The criteria in the "Distribution" section are redundant (page 473 of Vol. II).

What is the scientific justification for the population size categories (page 474 of Vol. II)? Also, authors' determination of site potentials is ambiguous.

The "Productivity" section would benefit from inclusion of data, including trend data (if available; page 475 of Vol. II). The information in Table 244 (next page) is rather coarse; again, quantitative data would be useful.

If cutthroat populations have become so low (as a result of anthropogenic influence) that they have to interbreed (lack of conspecific spawners), is it a natural evolutionary process ("Hybridization" section, page 476 of Vol. II)?

Why is the word "barrier" placed under the column heading "Hatchery Rainbow Trout" (page 478 of Vol. II)? What does this word mean in this location?

Alvord Cutthroat Trout — The subspecies of this trout should be identified within the text of Volumes I and II. Vol. II indicates that these fish are undescribed (see 2nd to last sentence on page 462), however Behnke (2002) classifies this fish as *Oncorhynchus clarki alvordensis* (see page 221 in Behnke 2002), something that should be stated in the text.

Unlike most other cutthroat trout SMUs this SMU is missing a "Summary" section.

Coyote Lake Lahontan Cutthroat Trout — The methodology in Vol. II is more similar to other species' SMUs than to other cutthroat trout SMUs.

When was the barrier in Willow Creek installed (page 464)? What is the basis for considering this barrier a temporary feature?

Unlike most other cutthroat trout SMUs this SMU is missing a "Summary" section.

Quinn River Lahontan Cutthroat Trout — The criteria for habitat usage are redundant (pages 468 and 469 of Vol. II).

The "Productivity" criterion (page 470 of Vol. II) appears to be double-weighted, if not triple-weighted, by using distribution and abundance to assess productivity. What are the implications to the assessment?

CHUB

Borax Lake Chub — It would be helpful to include scientific name for Borax lake chub in Vol. I.

Can the decline in abundance between 1995 and 1997 be explained (page 481 of Vol. II)? Why has this "at risk" SMU not been monitored since 1997?

The productivity section indicates that most chub are less than one year of age (see page 481 of Vol. II). The underlying assumption in this section is that these fish were not reproductively mature. Is this known?

Hutton Spring Tui Chub — "Abundance" section, page 483 of Vol. II: What exactly are visual estimates, and how accurate are they? In the absence of productivity data, for

consistency throughout the document, shouldn't the "Productivity" criterion be failed for this "at risk" SMU until more information is known?

Oregon Chub — Line 3, page 485 of Vol. II: Which non-native fish species negatively impact Oregon chub? It would be informative to list these species. On the middle of page 486 (and elsewhere), are "reintroductions" the same as transplants?

What is the basis for the assumption that in at least one location per SMU, Oregon chub existed in stable populations of 500 or more fish (2nd paragraph, page 487 of Vol. II)?

What is the basis for necessitating 20 populations of at least 500 adult chub, and how does this fit with the NFCP's "80% rule" (3rd to last sentence of the 3rd paragraph, page 487 of Vol. II)? If this statement arises from the Oregon Chub Recovery Plan, then this source should be cited at the end of this sentence.

DACE

Foskett Speckled Dace — It is stated that each of the six criteria were evaluated (2nd paragraph, page 492 of Vol. II), yet the very last sentence on the same page states that the "Productivity" criterion was not evaluated.

When was the concrete trough installed ("Abundance" section, page 492 of Vol. II)?

SPECIES IN ONFSR VOLUME II, APPENDIX

Following is a listing of comments specific to the fishes presented in the appendix:

- Umpqua chub is marked as "not assessed". Are there no data from Dr. Doug Markle (Department of Fisheries and Wildlife, Oregon State University) that could be used to assess this species?
- For northern and Umpqua pikeminnows, what is specifically meant by "locally abundant" and how does this relate to historical abundance?
- How does ODFW know that peamouth are "abundant", mountain sucker and largescale sucker are "broadly distributed", and mountain whitefish are "widespread and locally abundant" if they are not specifically assessed? How do current abundance and distribution relate to historical abundance?
- If the reticulate sculpin is "widespread", is it abundant?

Literature Cited in Appendix II

Behnke, R. J. 2002. Trout and salmon of North America. Free Press, NY.

Gresswell, R.E., Bateman, D.S., Lienkaemper, G.W., and Guy, T.J. 2004. Geospatial techniques for developing a sampling frame of watersheds across a region. GIS/Spatial Analyses in Fishery and Aquatic Sciences. page 515–528. published by the Fishery and Aquatic GIS Research Group.

Torgersen, C.E., Gresswell, R.E., and Bateman, D.S. 2004. Pattern detection in stream networks: quantifying spatial variability in fish distribution. GIS/Spatial Analyses in Fishery and Aquatic Sciences. page 405–428. published by the Fishery and Aquatic GIS Research Group.

APPENDIX III
Editorial Comments

The editorial comments that we present in this section are not a thorough, exhaustive assessment of Volumes I and II. We do note several editorial issues here, ranging from general considerations to more specific ones.

All of the types of data represented need to be mentioned and defined in the titles, and definitions should be consistent (e.g., What do the dashes mean in Table 163 on page 377? See also column headings in Table 140, page 371; other tables have missing or non-applicable data). Information presented in the tables should coincide with the column headings (e.g., see Table 238, page 469). Many of the tables had large font sizes, rendering the tables hard to read. These large font sizes displaced cell contents into other columns, further obfuscating the valuable information in the tables (e.g., see redband trout, bull trout, and cutthroat trout).

The text of Vol. II would be easier to read if the paragraphs were indented and an additional line space was included between paragraphs or if a wider spacing between paragraphs was used.

VOLUME I — GENERAL COMMENTS

- Data "are", not data "is"; data is a plural noun.
- SMUs, not SMU's (SMU is an acronym, not a possessive pronoun).
- On the y-axis of the graphs presented in the "Abundance" section, what is meant by an "Index %"? This should be defined.
- "Chinook" should be capitalized throughout (see Nelson et al. 2003. Fisheries. 28:38–39).
- It would be helpful to type out (i.e., not abbreviate) the six criteria in the summary tables.
- Maps for the header pages should be positioned differently, such that the text is not split (e.g., see page 17 and elsewhere in the anadromous Pacific salmon and steelhead summaries).
- Page numbers are within text on the right-hand side and some table cells are not in alignment (see page 60 and throughout).
- If the current structure of two separate volumes is retained, it would be helpful to have the order of the criteria in Vol. I match the ordering in the summary tables and adjacent explanatory page of Vol. II.
- The gray half-sphere in the "Distribution" pie chart obfuscates the presented data (see page 21 and throughout).
- Summary tables for extinct populations did not always include the existence criterion (compare page 30 to page 24).

- The pass/fail designator is not always in agreement with the assessment. For example, the 2nd sentence under "Productivity" (page 31) does not agree with the pass/fail designation for the Alsea population in the summary table on the previous page.
- Asterisks are lacking in several of the summary tables of Vols. I and II.
- Distribution maps for species such as redband trout and bull trout were somewhat confusing in comparison with the anadromous salmon and steelhead maps, which were clear.
- Specific basins and rivers are often mentioned, but it is not always clear which population these rivers or basins occur within (e.g., see Fort Rock redband trout, 1st sentence under "Additional information" and elsewhere).
- It would be helpful to include dams on maps, and a designation of whether they are passable or not.
- There are no header pages for species other than anadromous Pacific salmon and steelhead.
- Some SMUs lack summary tables (e.g., Borax lake chub), which are useful.
- Compass points only exist in some SMUs (e.g., Borax lake chub).
- Inconsistent font style and size. Compare 1st paragraph on p. 18 (*T*he coastal …) with the 1st paragraph on p. 26 (*T*his SMU …).
- Avoid overlaying graphs or tables on SMU or Oregon maps (e.g. graph overlay on Oregon map, p. 62)
- Inconsistent graph size, font and wording. Compare pp. 60, 62 and 128 (and others); Not Extinct (p. 62) vs. Exist (p. 60) to describe populations.
- Inconsistent column headings in tables. Compare pp. 130, 132 and 138 (1st letter abbreviations) with other pages.

VOLUME I — SPECIFIC COMMENTS

- In the last sentence of paragraph 1, page 1, "(e.g. SMU)" should be deleted.
- "Speckled dace" and "Native fish species" should not be capitalized (page 2).
- Specifically what is the "Oregon Basin" (page 2)?
- Should include species for sturgeon, lamprey, chub, and dace in Table 1.
- The first sentence that mentions SMUs (middle of page 5) should have a brief mention of how analogous SMUs are to ESUs.
- "Productivity" section on page 51: Is the Hood population the same as the Bonneville population?
- Fort Rock redband trout: Risk status in the initial paragraph is not in agreement with the actual listing under the SMU header (page 90).

- Western brook lamprey, map, page 92: Coastal SMU should be colored red.
- "Adfluvial" should be added to the Glossary.
- Spell out "Coded-wire tag (CWT)" at first use. Compare 3rd from last and last paragraph under *Independence – Fail* on p. 23.

VOLUME II —GENERAL COMMENTS

- "Chinook" should be capitalized throughout (see Nelson et al. 2003).
- The ordering of species and SMUs in Vol. II does not always match the ordering in Vol. I, making it difficult in some circumstances to read between the two volumes. See our earlier comments about possibly merging the two documents.
- Captions for several tables are on different pages from the actual table.
- Middle of pages 19, 100, and elsewhere: the cited personal communications should include dates, affiliations, cities, and states.
- Words such as "pluvial" and "endorheic" should be included in the Glossary of Vol. I to facilitate understanding.
- Several tables are not in chronological order (see page 287 and elsewhere).
- Page 374 (incorrectly indicated as 370 in bound hard copy) and elsewhere: "Distribution" section is noted as "Habitat use distribution" in anadromous salmon and steelhead SMUs.

VOLUME II — SPECIFIC COMMENTS

- Line 9, last paragraph, page 2: "…encountered in their native streams or lakes" (should add the underline portion).
- The caption in Table 1 (page 3) should include one or two brief sentences on how the risk designations were determined (akin to the definition on page 7 of Vol. I). This would improve reader understanding.
- Line 5, page 3: sentence states "For a complete list of Oregon native freshwater fishes, see Appendix A…" — this appendix includes some marine fish as well.
- Page 7, first sentence of the last paragraph does not make sense.
- Caption for Table 5, page 11 is awkward.
- There appears to be some inconsistency in the percentage of accessible habitat (see last line of Table 7, page 17). How can there be 0 inaccessible miles and only 96% accessibility?
- Table 11, page 45: periods occur after some of the lines, but not all.
- Table 13, page 46: 25% of the average abundance for the Winchuck population equals 4.5, not 4.

- Figure 5, page 21: The x-axis should not be longer than 52 weeks; also, the data points for the weeks do not match up with the "ticks" on the x-axis.
- No data are presented for abundance on page 68, unlike adjacent pages.
- Pages 75 – 78: Left-justified page footer labeled is "Snake Fall Chinook" when it should be "Mid Columbia Fall Chinook"
- Page 85: Table caption reads "lower Columbia spring Chinook" when it is actually "coastal spring Chinook".
- Page 112: Wrong map.
- Page 117, 125: No title of population and SMU at top of page.
- Page 124: Graphs are cut-off.
- Page 240, last sentence of first paragraph: "Anecdotal" is spelled incorrectly.
- Pages 272–273: There are no data in the tables.
- Page 280, sentence beginning on line 5 of the "Abundance" section: The meaning of this sentence is unclear.
- In Table 139 (page 369), there is an inconsistency in the quantity of numbers presented between columns three and six. What is meant by "10 est's" (cell within table)? For which five year period is the table providing information?
- Page 370 & 372: The font size of the footnote is too large.
- Some populations in Vol. II are missing headers (e.g., see redband — page 377).
- There is no table in the "Existing populations" section (page 377).
- Need a caption for Table 174 (page 389).
- Table 189 (page 413) indicates a "Figure 1"; specifically which figure is being referred to?
- "Fishes' entire range" should be "fish's entire range" (page 442).
- Figure on page 444: It is difficult to distinguish populations in the bar graph using black and white.
- Figure on page 502 is formatted differently from previous figures.
- The last sentence on page 477 refers to "Table 5"— do the authors mean to refer to Table 245?
- Page 486, last paragraph: Remove the "s" from threespine sticklebacks and redside shiners; also, the start of the sentence, "We found Oregon chub are…" is awkward.
- Appendix A, last page: Column totals are incorrect.
- References: Kostow (1995), Chilcote et al. (2005), Walter and Weber (1973), Nickelson (1998), and other references (?) are not in the "References" section.

Minckley et al. (1986) is in the references two times. Nicholas (1988) is cited as (1989) elsewhere in the text.

- References: U.S. Fish and Wildlife Service and U.S. Army Corps of Engineers are not listed in a consistent format.
- The appendix is titled "Oregon Native Freshwater Fish Species", yet the table includes marine species (e.g., Pacific herring).
- Appendix: "NFSR" should be spelled out.
- Format each reference with indentation of all but 1st line (authors).
- On the last page of Appendix A, need to add the second "i" to *C. beldingii*.

VOLUME II — CITATIONS NEEDED

- First sentence, third paragraph, page 7: ODFW should cite a reference source for these "previous determinations by ODFW".
- Last sentence of the first paragraph of "Abundance" section, page 9: Need a reference for this sentence.
- Other locations needing references include (but are not limited to):
    - Last sentence on page 14
    - First sentence, paragraph 4, page 368
    - "Distribution" section, page 377: Several sentences.

References cited in Appendix III

Nelson, J.S., Crossman, E.J., Espinosa-Perez, H., Findley, L.T., Gilbert, C.R., Lea, R.N., Williams, J.D. 2003. The "Names of Fishes" list, including recommended changes in fish names: Chinook salmon for chinook salmon, and Sander to replace Stizostedion for the sauger and walleye. Fisheries. 28(7): 38–39.