Independent Multidisciplinary Science Team (IMST)

State of Oregon

Stan Gregory
Robert M. Hughes
Nancy Molina
Carl Schreck
Rich Shepard
Carlton Yee

c/o
Oregon State University
Department of Forest Science
321 Richardson Hall
Corvallis OR 97331-5752

March 18, 2005

Michael Carrier
Natural Resources Policy Advisor
Governor's Natural Resource Office
900 Court St. NE
Salem, OR 97301-4047

Dear Mr. Carrier:

In your December 17, 2004 and February 1, 2005 letters to the Independent Multidisciplinary Science Team (IMST), you requested that the IMST provide a review of the State's draft Oregon Coastal Coho Assessment (Coho Assessment) as it relates to the viability of coastal coho, the effectiveness of the State's conservation efforts (occurring primarily under the Oregon Plan for Salmon and Watersheds (Oregon Plan)), and NOAA Fisheries' final listing determination under the federal Endangered Species Act (ESA). You asked the IMST to provide feedback on "whether the scientific approach and analyses are credible and consistent with accepted scientific standards, and whether the assumptions and uncertainties are reasonable and characterized accurately". You further asked IMST to provide suggestions for improvements to the draft assessment.

We have completed our review of the draft *Viability Criteria and Status Assessment of Oregon Coastal Coho* (*Viability Analysis*), the draft *Policy to Evaluate Conservation Efforts: The Certainty That The Conservation Efforts Will Be Effective* (Part 3B; *PECE Certainty Analysis*), and the draft *Synthesis of Viability Analysis and Evaluation of Conservation Efforts* (*Synthesis*). This letter along with attachments contains our assessment of the approach and analyses, as well as suggestions along the lines you requested. We would like to emphasize that we have not taken a position on what the listing status of coastal coho should be under the ESA; we believe that to be, in part, a policy question, and outside the scope of IMST's role as neutral science advisors to the State. We have confined our review to matters of scientific rigor, adequacy of evidence to support assertions, and clear logic track from information to conclusions.

In general, we found the *Viability Assessment* to be generally holistic and robust, and to constitute a thorough review of the viability criteria and status of the Oregon Coastal Coho Evolutionary Significant Unit (ESU). While the *PECE Certainty Analysis* had a good discussion of the State's policies and actions for salmonid protection under the Oregon Plan, we felt some of the statements about effectiveness were insufficiently supported by data, and lacked sufficient examples of successes and limitations. We recognize the authors of the documents were given a short time in which to complete their

work, and commend them for the high quality of the report they produced under those circumstances.

Our review of the Coho Assessment occurred under a tight timeline, and this has some implications that we want to make sure are clearly understood. First, we did not have sufficient time to perform any outside reviews of literature that might have been pertinent to the Coho Assessment. We also did not have time to do our own review or analyses of the data used in the Coho Assessment, and few of our members had the time to review the supporting agency technical reports. And we have had very little time as a team for discussion of our comments and for reaching consensus. That said, we feel we have adequately identified any significant problems in the assessment, and are in agreement as a team on the major points expressed in our review document.

In your letter, you acknowledge that the State is actually trying to accomplish dual goals with the Coho Assessment – those related to the implementation of the Oregon Plan, and those related to the ESA listing process. We appreciate the challenge this presented to the report's authors, because it is clear that sometimes the two efforts pose different questions (especially with regard to the timeframe of consideration), and to attempt to cover divergent issues in one analytical framework would appear to be quite difficult.

We commend the State's Coho Assessment authors for the particular strengths we observed in their reports:

- The use of the major criteria (abundance, distribution, productivity, persistence, and diversity) on which the *Viability Assessment* is based – we agree they are scientifically appropriate, and are consistent with criteria used by the NOAA Fisheries' Oregon Coast Workgroup of the Oregon/Northern California Coast Technical Recovery Team (TRT).

- The overall rigor, integration of disciplines and variables, and blending of modeling approaches with use of observed data;

- The clarity of presentation and generally logical development of conclusions from data and analyses presented;

- The readability of the documents, especially in making highly technical discussions understandable;

- The inclusion of the *Viability Assessment*'s section VIII – Synthesis and Additional Discussion, which we found to be especially helpful, and which significantly enhanced the report's credibility. The uncertainties and risks presented in this section are very well-described, and they provide essential context to the overall interpretation of the report's conclusions.

Finally, we feel the State should be applauded for implementing the monitoring and data collection that provided the foundation for the Coho Assessment, without which the analyses to support the Oregon Plan and the ESA listing process could not be performed.

As you requested, in addition to the strengths described above, we identified points we felt need to be strengthened in the assessment. These points are summarized briefly in this letter, and are developed more thoroughly in the attached review document. We wish to emphasize that even though we identified areas of needed improvement, we feel that overall the *Viability Assessment* represents a major advance in the use of scientific tools to address population viability questions for ESA listing consideration. In addition, we were informed about additional data and analyses that were not fully described in the draft report during our discussions with the authors. Some of our comments would not apply if that additional information had been included (and we hope it will be in subsequent drafts). Therefore, we emphasize that these comments apply only to the specific content of the drafts that were available to us at the time of our review.

- All three documents we reviewed would benefit from a more balanced presentation of the information, analyses, conclusions, and uncertainties. After the conclusions are presented, the reports tend to focus solely on the evidence and outcomes that support those conclusions. Uncertainties are largely dismissed, especially those related to the ability of the State to respond rapidly to possible future declines in coastal coho status. The conclusions may remain the same, but the State should clearly describe uncertainties that may affect future trajectories in these fish and the landscapes that support them.

- It would be helpful if the Coho Assessment clearly indicated the State's determination of the status of coastal coho salmon along the continuum from endangered to threatened to minimally viable to recovered to healthy and robust. IMST recognizes that the NOAA Fisheries listing decision requires only a categorical determination and not a determination of where the stocks generally fall along such a continuum. However, a clearer indication of the relative status of the fish would provide an important perspective for both the listing decision under ESA and the development of a conservation plan for coastal coho salmon under the Oregon Plan.

- In places, it is difficult to determine whether the authors are speculating or stating facts, and the support of text through citations is uneven.

- The *Viability Assessment* relies on the survival of coastal coho salmon through recent periods of low ocean productivity, the relationship between the production of pre-harvest adults per spawner at different densities (recruits/spawner), and analysis of risk of extinction based on the Ricker model. The report would be strengthened by a more balanced discussion of the strengths and weaknesses of those important lines of evidence.

- The Coho Assessment's conclusions also rest heavily on a new "low abundance paradigm". This perspective on coastal coho salmon has not been thoroughly reviewed or tested. The State should be cautious about making decisions that are based heavily on the new "low abundance paradigm" and should seek additional intensive review.

- The State's assessment of risk of extinction or persistence is based on a single model of population dynamics—the Ricker model. Several major types of population models are available and have been applied to coastal coho salmon. The TRT for Oregon coastal coho salmon is exploring several types of models. We encourage the State to incorporate the outcomes of these models prior to its final report to NOAA Fisheries, if possible.

- We have questions about the determination of population structure used in the *Viability Assessment*, especially the categorization and analytical treatment of dependent and independent populations. We suggest the report be revised to include more discussion of the rationale for the determinations of ESU, strata and population structure, and the implications for the report's conclusions if those determinations prove to be incorrect.

- We feel the Coho Assessment, as a whole, takes an unduly static view of the Oregon coastal landscape, and would be strengthened by inclusion of more future scenarios that integrate the potential consequences of multiple factors that affect fish populations (including ocean conditions, drought, fires, overall watershed conditions brought about by human population increases and land use change, and stream habitat conditions), and assess what would happen if individual trends detrimental to fish converged. We feel the report was overly dismissive of the likelihood that such scenarios might actually occur over the long term.

- The *PECE Certainty Analysis* lists many exemplary policies and plans that the State is implementing to insure coho salmon rehabilitation. However, in very few cases was there supporting documentation of the likelihood of successful implementation or effectiveness. Also, possible time lags between policy or plan adoption and biological response seem not to have been factored into the analyses. A holistic approach to these issues that includes uncertainties related to political, demographic, economic, land use, and other non-biological factors, would considerably strengthen the State's assessment.

- The *Synthesis* seems to combine elements of a true synthesis (of the viability assessment and predicted effectiveness of conservation practices) with lengthy restatement and summarization of material from the other two documents. Separation of the summary and synthesis components would make for a much more understandable product.

As a general conclusion, the IMST believes that the assumptions and analyses underpinning the State's Coho Assessment are valid. We commend the authors for their excellent synthesis of the relevant information, and creative approach to their problem. We were pleased to have had the opportunity to review the report, and hope our comments and suggestions are useful. We would be happy to answer any questions that our comments raise.

Sincerely,

Nancy Molina
IMST Co-Chair

Carl Schreck
IMST Co-Chair

encl.

cc with enclosures:
Rep. Karen Minnis, Speaker of the House
Sen. Peter Courtney, Senate President
Jim Myron, GNRO
Tom Byler, OWEB
Ed Bowles, ODFW
Bruce McIntosh, ODFW
Rep. Bob Jenson, Water Committee
IMST

IMST Review of State of Oregon's Draft Oregon Coastal Coho Assessment Documents: Viability Assessment, PECE Certainty Analysis, and Synthesis

Independent Multidisciplinary Science Team
Oregon Plan for Salmon and Watersheds

March 18, 2005

Members of IMST
Stan Gregory, Department of Fisheries and Wildlife, Oregon State University
Robert M. Hughes, Department of Fisheries and Wildlife, Oregon State University
Nancy Molina, USDI Bureau of Land Management
Carl Schreck, USGS Biological Resources Division
Richard Shepard, Applied Ecosystem Services, Inc.
Carlton Yee, Yee Forestry Associates

Citation: Independent Multidisciplinary Science Team. 2005. IMST review of State of Oregon's draft Oregon Coastal Coho Assessment documents: Viability assessment, PECE certainty analysis, and synthesis. Oregon Watershed Enhancement Board, Salem, Oregon.

This page left blank intentionally

TABLE OF CONTENTS

INTRODUCTION……...……..1

TECHNICAL COMMENTS

Viability Assessment……..…….2

PECE Certainty Analysis……...……21

Synthesis…….....27

OVERALL CONCLUSIONS…….....32

This page left blank intentionally

INTRODUCTION

In this review, IMST provides technical comments on three draft documents produced by the State of Oregon for the Oregon Coastal Coho Assessment:

- **Viability Assessment** – *Viability Criteria and Status Assessment of Oregon Coastal Coho*. December 20, 2004 Draft
- **PECE Certainty Analysis** – *Policy for the Evaluation of Conservation Efforts: The Certainty That the Conservations Effort Will Be Effective* [Part 3B]. January 31, 2005 Draft
- **Synthesis** – *Synthesis of Viability Analysis and Evaluation of Conservation Efforts*. January 31, 2005 Draft

These comments represent IMST's response to a request from the Oregon Governor's Natural Resource Office to

> "...review these documents and provide feedback on whether the scientific approach and analysis are credible and consistent with accepted scientific standards, and whether the assumptions and uncertainties are reasonable and characterized accurately. Also, please provide any suggestions on ways we can improve our analysis with the available data."[1]

For the *Viability Assessment*, we were asked to specifically address the five primary components of the analysis[2]:

1. Determination of the ESU, strata and population structure;
2. Description of attributes used to define viability and assess fish status;
3. Development of specific criteria for each attribute used to define population viability;
4. Development of specific criteria for strata and ESU viability based on roll-up of population criteria; and,
5. Assessment of current status of coastal coho relative to population, strata and ESU viability criteria based on the key attributes described in (2).

Due to time constraints, we did not perform an outside review of scientific literature that might have been pertinent to assessing the status of coastal coho salmon. We make comments only on the three documents listed above; providing comments on the many other technical reports produced in the Oregon Coastal Coho Assessment was not possible in the time allotted.

We summarize our findings in an "Overall Conclusions" section at the end of this review.

[1] February 1, 2005 letter to IMST from Michael Carrier, Governor's Natural Resource Policy Director.
[2] December 14, 2005 letter to IMST from Michael Carrier, Governor's Natural Resource Policy Director.

TECHNICAL COMMENTS

IMST Review of Part I: *Viability Criteria and Status Assessment of Oregon Coastal Coho* [December 20, 2004 Draft][3]

These are the Independent Multidisciplinary Science Team's (IMST) comments on the draft report, *Part I: Viability Criteria and Status Assessment of Oregon Coastal Coho* [December 20, 2004 Draft] (*Viability Assessment*). We applaud the State of Oregon for synthesizing available biological data on coastal coho salmon. We recognize the tremendous effort that went into producing this assessment, and the short time frame the authors had to work under. The Oregon Plan for Salmon and Watersheds' (Oregon Plan) exceptional monitoring program provided the State with much more empirical information than is usually available to assess a species' status. The wealth of data provided a unique opportunity for the State to produce a status assessment that is more quantitatively rigorous than the norm.

In this review, we focus on major scientific issues that could affect the credibility of the *Viability Assessment*'s conclusions. We have not taken a position on what the listing status of coastal coho should be under the federal Endangered Species Act (ESA); we believe that to be, in part, a policy question, and outside the scope of IMST's role as neutral science advisors to the State. Our comments are intended to help the State produce a rigorous scientific assessment of the status of coastal coho salmon.

Overall, we commend the authors for the rigor of many of the analyses, and for the data-richness of the assessment. We find that the viability analysis represents careful integration of disciplines and variables. While the majority of our review focuses on places where the draft could be strengthened, this imbalance of text devoted to criticism is not intended to reflect the merit of the viability analysis. Rather, we acknowledge that we are commenting on a draft assessment and intend for our critical review to help the authors focus efforts to make the final assessment as strong as possible.

Our review starts with a section of general comments, followed by suggestions on six major thematic areas:

1. Use of the "low abundance paradigm" and assumptions about ocean conditions
2. Population structure
3. Population viability analysis
4. Range of conditions included in the assessment

[3] In subsequent drafts, the Viability Assessment was labeled Part 2. Please note that page numbers in this document refer to the December 20, 2004 draft.

5. Modeling and data analyses
6. Thresholds and decision "cut-offs".

The authors will find a list of additional comments on technical details, as well as suggestions of improving presentation of results following the main body of this review.

General Comments

Note: We were informed about additional data and analyses that were not fully described in this report during our discussions with Oregon Plan agency staff. Some of our comments would not apply if that additional information had been included (and we hope it will be in subsequent drafts). Therefore, we emphasize that these comments apply only to the specific content of the December 20, 2004 draft.

Major strengths of the report

The following list exemplifies elements in the report that we thought were particularly well done:

- In general, we believe that the major criteria on which the *Viability Assessment* is based—abundance, distribution, productivity, persistence, and diversity—are scientifically appropriate. These criteria are consistent with criteria used by the NOAA Fisheries Oregon Coast Workgroup of the Oregon/Northern California Coast Technical Recovery Team (TRT), further strengthening the basis for assessment.

- We like that the authors blend modeling with use of observed data.

- The document has clarity of presentation and logical development of conclusions from data and analyses presented; it is very readable, and makes the highly technical discussions understandable.

- The inclusion of Section VIII – Synthesis and Additional Discussion, was especially helpful, and significantly enhanced the report's credibility. The uncertainties and risks presented in this section are very well-described, and they provide essential context to the overall interpretation of the report's conclusions.

General concerns about the report

- The assessment does not clearly indicate the State's final determination of the status of coastal coho salmon along the continuum from "endangered" to "threatened" to "minimally viable" to "recovered" to "healthy and robust". IMST recognizes that listing decisions under the federal ESA require only a categorical determination and do not require assessment of where an evolutionary significant unit (ESU) generally falls along such a continuum. However, this assessment of the status of coastal coho salmon also serves both the people of Oregon and the agencies dedicated to implementing the Oregon Plan. In our discussions, Oregon Department of Fisheries

and Wildlife (ODFW) staff stated that their analyses indicated that the populations met the minimum criteria for being considered viable but clearly are not fully recovered. These are important conclusions about viability that are currently lacking in this report. A more clear indication of the relative status of the fish would provide important perspective for both the upcoming decisions under ESA and the development of a long-term conservation plan for coho salmon under the Oregon Plan.

- The report relies heavily on the survival of coho salmon through recent periods of low ocean productivity, the relationship between the production of offspring per adult at different densities (recruits/spawner), and analysis of risk of extinction based on the Ricker model. The report would be strengthened by a more balanced discussion of the strengths and weaknesses of those important lines of evidence.

- Frequently, it is difficult to determine whether the authors are speculating or stating facts. The text should clearly differentiate between the two types of statements, and provided supporting information, references, or rationale where appropriate. Likewise, the support of text through citations is uneven. A thorough review of the document should be done with this in mind, and citations included where possible.

- The State's rationale would be strengthened by a more thorough discussion of the potential for the major types of errors in scientific inference or conclusions. In general, the assessment focuses on the risks of drawing an incorrect conclusion when the overall hypothesis or assertion is correct (Type I error). The assessment should also devote substantial attention to the risk of drawing an incorrect conclusion when the overall hypothesis is incorrect (Type II error).

Topic 1. Use of the "low abundance paradigm" and assumptions about ocean conditions

The section "Revising the Low Abundance Paradigm for Coastal Coho" (pp. 12–14), attempts to refute the idea that,

> "...most coho populations, if faced with such poor ocean conditions and low abundance, would invariably decline. Further, it was thought that if such conditions existed for a sufficient period of time this decline would result in the extirpation of most populations" (p.12).

This section then hypothesizes that a "new equilibrium" for coastal coho populations was achieved during the 1990–1997 period of poor ocean conditions. The assessment places major emphasis on this untested paradigm and it greatly influences the conclusions of the report.

We are not convinced that that the resulting assumptions about the "low abundance paradigm" are sufficiently supported scientifically to conclude that a "new equilibrium" was attained during the seven-year "trough" in ocean conditions. Also, the performance

of coho salmon in Oregon's coastal basins during the seven-year "trough" is not sufficiently understood to conclude that the populations would survive under similar ocean conditions in the future, and hence are not threatened or endangered. Further evidence to support these key assumptions of the assessment would significantly strengthen the report's conclusions. Below are some particular issues we feel the State should address.

- We disagree that the data show "a period of relative stability" (p. 12) in the 1990s. One could just as readily argue that that the 7-year period of low smolt survival is not evidence of stability. It only appears low because of the low range; certainly, the variability is low when compared against deviation from the mean. There simply were never any good years during this period, which was similar to what occurred in 1977–1984.

- On p. 14, second paragraph, the report indicates that a new "equilibrium" is reached under a period of poor ocean survival. Visually, this appears to be so, but that "equilibrium" is extremely close to zero, or extinction. If one were to plot the lower prediction interval for each population rather than the mean for R/S, would it fall below 1 for the majority of the years? We agree that the following statement appears reasonable: "it is the degree of random variations in the recruitment process, not an inevitable decline that is the reason for this increased risk" (p.14).

- In the data presented, it appears populations reach a low level for four years, but that level is extremely close to zero, or extinction. There is no scientific evidence in the report that these are "equilibrium" conditions and that the population is behaving differently than at high abundance. The recruitment curve is a property of the populations across a range of conditions and do not necessarily indicate a new equilibrium. IMST recommends that the State drop use of the term "equilibrium"; few ecologists currently support the concept because apparent equilibria only appear so for a short time. There are substantial problems with the interpretation of the recruit/spawner curve and its interpretation, which are discussed under Topic 3 (Population Viability Analysis) below. If and how these problems could affect the overall conclusions regarding a delisting decision should be addressed.

- Do we know that a 7-year period is sufficient to prove the previous paradigm was wrong? What is the probability that we will have a considerably longer period of poor ocean survival? And, if that happened, what would be the results in terms of population viability? Do we have any understanding regarding how "poor" the ocean can become and for how long? A balanced assessment would explicitly discuss these questions and assumptions.

- The assumption that ocean conditions and marine survival are cycling within the patterns observed over the last 43 years (pp. 8–9) is questionable. The data presented do not support the degree of regularity assumed. The Pacific Northwest has experienced relatively minor climate variation during the last 40 years (Ron Neilson, U.S. Forest Service, personal communication) in comparison to previous patterns

(based on tree ring analysis) and future projections. Future and past climates may exhibit periods of even greater variation than patterns observed over the last 43 years. If longer term paleo-oceanographic data indicate otherwise, they should be added to the model runs. Also, the plots shown assume a homogeneous ocean with all 17 coastal coho populations equally mixed, which we question. Data on the asynchrony of coho populations that we have seen in earlier presentations is lacking in this report. That asynchrony helps buffer the ESU from wholesale extirpation and should be added.

- At the January 2005 IMST meeting, the IMST was informed that the State had conducted sensitivity analyses to test alternate assumptions about the length of poor ocean conditions/marine survival, among other variables. The report should state for which variables this was done, the ranges in conditions examined, and the conclusions. In addition, we have questions about whether taking a 43-year period and repeating it with truncation at 100 years is statistically defensible, given the way the results are applied to the conclusions. Consulting a statistician with expertise in modeling autocorrelation in marine survival may be helpful.

In summary, we feel that the State should be cautious about suggesting that 1) the salmon populations from 1990–1997 indicate that a new equilibrium was attained and 2) the survival of coho salmon through the period from 1990–1997 indicates that the risks to these stocks are less than previously thought. The assertions about the low abundance paradigm are not adequately supported by other scientific literature. Future monitoring and periods of poor ocean condition may provide additional information that supports the hypothesis, but the current evidence as presented seems insufficient. Topic 5 on modeling and data analysis contains additional concerns regarding the analyses supporting the paradigm shift.

Topic 2 – Population structure

We have questions about the classification of populations used in the report, especially the categorization and analytical treatment of dependent and independent populations. We suggest the report be revised to include more discussion of how and why the determinations of ESU, strata, and population structure were made and the implications for the report's conclusions if population structure is different from those determinations.

- <u>ESU determination.</u> We recognize that this report is addressing ESA listing issues and hence must consider the ESU for coastal coho as defined by NOAA Fisheries. It would be beneficial if the report provided some insight into the confidence the State has that the NOAA Fisheries classification of populations is correct. We believe that the State has a responsibility under the Oregon Plan to consider the science behind the classification of populations into ESUs and the potential consequences to Oregon's coho resources if the federal classification is incorrect. For example, what if in actuality Oregon's coastal coho actually represented two ESUs, such as the Lake stratum and those more northerly? In that case the populations at risk in the latter could get closer to the 50% criterion (6 vs. 9, Table 4 if one removes the Floras/Sixes

population as well). Also, that 2 (or 50 %) of the independent populations must pass all of the population criteria (p. 41) for the ESU to be considered not at risk appears reasonable regarding an ESA listing decision. Is that level of ESU robustness sufficient for objectives of the Oregon Plan?

- Strata determination. Strata are said to "represent clusters of populations that share ecological or geographic similarities" (p.6). The report provides no citations or evidence to elaborate on what these ecological or geographic similarities might be. ODFW staff indicated at the February 2005 IMST meeting that this comes from NOAA Fisheries. A reference to some federal document would be helpful.

- Population determination**.** The rationale presented on pp. 4–6 for distinct coho populations is insufficient. The largest systems are deemed independent, the smallest are deemed dependent, and the intermediate basins are considered potentially independent (i.e., dependent) because of the likelihood of straying from independent populations. If there are rigorous criteria supporting the classification, they should be presented; if rigorous criteria do not exist, then the consequences of a misclassification should be presented. Using this same logic, within the large (independent) basins/populations, there are subbasins, some of which are major coho producers (independent) and some of which are not (dependent). These subbasins or subpopulations may be related simply to size, but more likely to habitat extent and quality, especially spawning, rearing and overwintering quality and their proximity to each other. For example, in the Siuslaw Basin, Knowles Creek produces 19% of the coho smolts but represents only 2% of the surveyed drainage. Similarly, most coho juveniles in Knowles Creek are restricted to a small portion of that drainage (Dewberry, *in review*). Similar concentrations were observed in other basins (Dewberry, *in review*). Other salmonid species also selected specific areas with ideal flows and temperatures out of 10s of km of available habitat (Fausch et al. 2002). Assuming such patterns exist in the other independent coho populations, each one probably truly consists of independent and dependent subpopulations. And if the hotspots, anchor habitats, or independent subpopulations in the "independent" populations are eliminated or impaired, that entire independent population may collapse. Likewise, if more of those hotspots for spawning, rearing, and overwintering were rehabilitated, both the subpopulations and the populations would become increasingly independent—regardless of size.

  In a similar vein, the size of the Upper Umpqua independent population relative to the others raises some questions. Is this the result of a single (independent) population in the South Umpqua and a single dependent North Umpqua population, two dependent populations that together appear independent, or a larger number of dependent and independent populations, as mentioned during the February 2005 IMST meeting?

  Likewise, the small size of the small dependent coastal stream populations raises some questions. If their sizes represent the size of dependent basins/populations, then presumably the current independent basins should be subdivided into similar-sized systems to determine which catchments within those basins are independent versus

dependent. Another concern with deeming the small populations dependent is that they can be overlooked. In fact they are likely to possess genes that merit conservation. Scudder (1989) discussed the importance of fish in marginal habitats for the genetic diversity of the population. IMST agrees that relatively little is known about the population implications of small populations with high straying rates for the viability of a salmonid stock across an ESU. The assessment does not note this major uncertainty and does not discuss the consequences for the State's conclusions if these populations are important for the survival of the stock.

While we agree that the basin/population model is a useful construct, we feel that the assumption that it is reality, and not simply an artifact of the observation scale, may cause the State's analysis to be flawed. The State should critically question the consequences for their conclusions if independent and dependent populations are incorrectly classified or spatially identified. If there are quantitative or historical genetic, morphological, and life history characteristics that differentiate the populations, they should be included in the final report. If basins are the only grounds for dividing the ESU, the authors should explicitly state this.

Topic 3. Population viability analysis

3.1 General comments on approach to viability analysis

- The shape of the Viability Model Recruitment Curve used in the model is generically exemplified in Figure 3. This is a critical relationship and is cited repeatedly throughout the assessment as proof that coho salmon are less vulnerable to extinction than species without this relationship. The relationship depicts the production of recruits per adult spawner as a function of spawner abundance. Readers must understand that this is not a measure of the total production of salmon but the ratio of offspring that return to freshwater to spawn produced per parent. When coho populations are low, the number of offspring produced is still extremely low even though the ratio of offspring per spawner is very high.

  Another issue related to coho productivity is the report's conclusion that depensation is not occurring at low spawner abundances. We understand from ODFW's comments at our January 2005 meeting in Salem that the data in Figure 4 showing the lowest proportion of full seeding for two of the three populations depicted might reflect aberrant conditions. However, these few data points appear to be the only information that is available to judge a whether depensation is occurring or not. We feel the inference of no depensation is weakened by the sparseness of data and contrary data points. The *Viability Assessment* needs to more explicitly explain whether or not depensation was accounted for in the models, and the limitations of the data used to build the models. The State indicated that at low population abundance levels the model in some way incorporated the potential for depensation. If this is correct, then a more thorough an explanation regarding how this was accomplished should be provided.

- The *Viability Assessment* also needs to more explicitly explain that a population could go extinct during a model run. This is explained in the mid paragraph on p. 30 starting with "The QET [quasi-extinction threshold] level for each population…." This approach to extinction makes sense, and ODFW's contentions regarding the setting of QET levels for each population and not using a "knife edge transition" (later in that paragraph) are also reasonable.

- Historical abundance and life history diversity are two population attributes that are relevant to population viability but are missing from the discussion (pp. 6–7). Historical abundance is certainly an important criterion for assessing rehabilitation, but it is also important for placing current viability in context. It indicates how much a population can change but still persist. Loss of summer-resident coho from the lake populations (due to non-native species) is one example of this. Climate change, coupled with non-native species may eliminate the winter residents and lower Umpqua population**.** Ultimately, life history (e.g., timing of smoltification or adult return) and natural history (e.g., disease-resistance ability) diversity is more important than genetic variability because (1) the life- and natural-history phenotypes are a product of both genetics and the environment and (2) there are many ways of measuring genetic diversity, with many of the measures of genetic diversity having no relationship to fitness (ability to persist in a variable environment) while these phenotypes are directly related to fitness. Do populations have wide or narrow spawning and smolting ranges? Do they include estuarine and freshwater forms? Do they persist/rear in headwaters only or in lower reaches in rivers and near sea level also? Are adult and smolt size distributions shifting to lower ranges and medians? Compressions of life history expressions are early signals of population stress. We think these attributes are scientifically defensible, unambiguously measured, with readily available data, easily understood, and yield consistent results.

- We believe habitat was not adequately factored into the analysis. Ignoring habitat quantity, quality, and distribution within the drainage basin appears to ignore the determination of limiting factors. Given the amount of ODFW habitat monitoring, this sort of subbasin-scale assessment should be included. The *ODFW Habitat Report* (ODFW 2005) provided as monitoring background for the assessment clearly indicates that habitat quality across the ESU is lower than would be expected from reference sites:

  > "Streams within the coho ESU are pool rich, but structurally simple. The amount of pool habitat is high within all monitoring areas in the ESU, although the amount of slow water and off-channel habitat is limited. Compared to conditions in streams with minimal human disturbance, amounts of large wood are low in all monitoring areas. In addition, amounts of fine sediment are higher than reference conditions in three of the four monitoring areas. The lack of large wood and relatively high amount of fine sediment was evident across all land use types. The only exception was that the levels of fine sediment were comparable to reference conditions on public land." (ODFW 2005, p. 10)

The *ODFW Habitat Report* (ODFW 2005) also indicated that monitoring data indicate that recent restoration practices have not significantly altered these conditions across the ESU:

> "Across the ESU as a whole, we are unable to detect an overall improvement in levels of large wood due to restoration efforts. The number of restored stream miles (451) relative to the number of miles in our habitat sampling universe (5,553), and our relatively small yearly sample sizes contribute to our inability to detect a trend signal from habitat restoration, even though site specific monitoring of a subsample of restoration sites show significant increases in large wood." (ODFW 2005, p. 11)

Based on minimally disturbed reference sites, Hughes et al. (2004) estimated that 45% of the Coast Range stream length was impaired, and low Index of Biotic Integrity scores were associated with increased fine sediments and road density, and decreased riparian condition and concealment. These data on measured habitat conditions in the Coast Range indicate that habitat degradation has been significant, and habitat problems take time be corrected by restoration activities.

- Both the extent and quality of habitats should be linked with extinction risk. As with humans, it is not the <u>size</u> of the building or city in which we live that determines life span, but the <u>quality</u> of those environments. Possibly, the reason that extinction risk is insensitive to S_{max} is that S_{max} is an insensitive habitat indicator that needs replacing with or inclusion of S_{qual} (quality of habitat). Recent research in landscape and conservation ecology indicates that all habitats do not have equal value to the survival of a species or population (Torgersen et al. 1999; Baxter and Hauer 2000; Fausch et al. 2002).

- Regarding the discussion on p. 55, we agree that some sort of spatial/geographical analysis of distribution is useful. However, the approach taken assumes all stream lengths are of the same quality, and we know they are not. The randomly collected data should be used to build better models for stratifying "anchor habitats" for spawning and rearing. Furthermore, we believe that it could be important to consider the distribution of numbers of spawners across the habitats within a population's range when describing viability criteria. Does the State know if a population of the same size that is evenly distributed across its range is more, equal, or less viable than one with a more patchy distribution?

- Additional comments on modeling of population viability are provided in Topics 5 and 6.

3.2 Risk determination

The document should be explicit about the variation that is encompassed in the timeframe within which the determination of risk is made. In other words, the judgments of passing or failing the five biological criteria (Table 4, p.42) are made for the for the next 100 years without consideration of Oregon's coastal human demographic effects. The State should consider and least mention that the environment will likely be quite different over the next 100 years because of further human impact and that considerable, broad landscape changes such a global warming and urbanization of the Oregon Coast will occur. What consequence might these have on the analysis of risk relative to delisting and other goals of the Oregon Plan?

Dependent populations are small and probably more directly affected by land use change than populations in the larger basins, and as a consequence are likely to be more vulnerable to local extirpation. As such, they serve as very valuable sentinel systems for the independent populations. We feel the Viability Assessment would be strengthened by a discussion of uncertainty that exists regarding the potentially important role that these small populations may play in the ESU. The report indicates that the status of the stocks is not sensitive to habitat conditions, an assumption that we find to be contradictory to a large body of research on Pacific salmon (Nehlsen et al. 1991; Oosterhout et al. *in press*).

The lower extinction probabilities for the lake populations (on p. 53) indicate the importance of high quality over-wintering habitat (large pools). Bustard and Narver (1975), Tschaplinski and Hartman (1983), Bisson and Nielson (1983), Phillips (1986), and others have documented the importance of these habitats to coho. We suggest that the authors add a discussion of the role that this limiting factor plays in the effectiveness of habitat rehabilitation efforts, along with reducing shallow warm summer habitat.

3.3 Timeframe for viability and recovery

The assessment does not clearly state the timeframes that are used for determination of viability and the biological basis for those timeframes. The increase observed since 1997 is short and spans less than two generations of coho. In 1999, IMST convened a workshop "*Defining and Evaluating Recovery of OCN* [Oregon Coastal Native] *Coho Salmon Stocks: Implications for Rebuilding Stocks under the Oregon Plan*" with regional experts (IMST 1999). The participants discussed timeframes that are necessary to detect significant trends in coho salmon populations:

> "Concerns were expressed that these criteria do not span sufficient time to indicate real population trends. Alternative requirements could be an absolute minimum criterion for recovery of OCN coho stocks of greater than 1:1 spawner-to-spawner replacement for each brood year over at least three brood cycles and a recovery to a minimum population size before harvest impacts can exceed 10–13%. Another alternative would be at least two consecutive generations of recovery (spawning recruits/parental adult of >1.5) until seeding above 50% of high quality habitat is achieved.

> Workshop participants agreed that the timeframe for population responses is a critical aspect of measuring recovery of salmon stocks and requires careful evaluation." (IMST 1999, p. 9)

The three brood cycles represent 9 years for coho salmon, which have a 3-year life history. The timeframe used by the state in this assessment would not meet the criteria recommended in the workshop on recovery of coastal coho salmon.

Topic 4 – Range of conditions included in the assessment

The report would be strengthened by inclusion of more future scenarios that integrate the potential consequences of multiple factors that affect fish populations (including ocean conditions, drought, fires, watershed conditions, human population increases, land use change, and stream habitat conditions), and then assess what would happen if individual trends detrimental to fish converged. We feel the report was overly dismissive of the likelihood that such scenarios might actually occur over the long term. We feel these changes have a reasonable probability of occurrence.

- The State's assumptions about future landscape and environmental change seem overly static to us. We suggest the authors take advantage of the Coastal Landscape Analysis Modeling Study (CLAMS) database[4], which provides not only a robust and spatially explicit repository of current land use and land cover types, but also allows for scientifically valid assessment of historical and future landscape scenarios. Assuming a static, non-changing landscape in the central Oregon coast subregion for the next century flies in the face of the virtual certainties of population growth and land use change over the next century.

- Ludwig (1999) concluded that catastrophic events have substantial effects on the analysis of the risk of extinction. Regarding the discussion on pp. 54–55, we disagree that the probability of catastrophic flood, drought, tsunami, or fire affecting fish populations across the ESU is minor when considered over 100 years; we think the probability of such events having population impacts could actually be significant. In addition, anthropogenic influences such as intensified logging, damming, water diversions, disease introduction, and land use change related to human population growth ESU-wide over the next 100 years could be equally or more significant. The probability of such chronic and catastrophic changes affecting populations ESU-wide may be low, but they should not be dismissed as trivial or minor. In our opinion, such factors should be considered as important unpredictable or stochastic events in the analysis of the risk of extinction. The modeling of the persistence criteria should further address the possibility of these "catastrophes", possibly by additional stochastic modeling and by incorporating habitat degradation into the models.

  It would be very useful to document the recovery potential of salmonids following adverse circumstances, e.g., fire and flood. We briefly tried to locate relevant publications for salmonids but failed. Matthews (1998), however, provides several

[4] Information about the CLAMS database is available on the web at http://www.fsl.orst.edu/clams/

examples of the recuperative powers of warm-water fish assemblages following severe floods and droughts.

- We feel the lack of assessment of a plausible "worst case" scenario for the near future (5–20 yr) is a significant omission in the analysis. The discussion on pp. 43–55 would benefit significantly from analyses combining some or all of the following: poor ocean conditions, poor rainfall conditions, continued degradation of high quality habitats, continued or increased hatchery releases, continued global warming, and unusual ocean patterns. Simulation modeling over a 99-year period will not explicitly identify such dynamics, and the outcomes may simply be one or two simulations with substantial extinctions. In a risk of extinction analysis without such a scenario, this would only be seen as a small fraction of a percent increase in extinction probability. However, such events would have severe impacts at this time and are relevant for considering the status of coho. Overall, we find that additional analyses with differing assumptions would greatly strengthen the persistence modeling.

Topic 5 – Modeling and data analysis

The assessment would benefit from a clear statement and explanation of model assumptions, including those that affect model behavior, and those that are most influenced by few and variable data. In addition, the report should clearly address the probability of committing Type I and II errors and what the consequences of such errors would be to the report's conclusions. This report assumes that the representation in the models is accurate and the data incorporate all sources of uncertainty and variance. If this is not correct, what is the risk of making decisions that are detrimental to fish? The assessment would be strengthened if these questions were addressed.

The choice of a model to assess population viability (or "persistence") is an important technical consideration. The assessment only used one model, the Ricker model, to estimate future populations, persistence, and risk of extinction. The Ricker model used in this assessment is a stock-recruitment model.

The Ricker model is based on the relationship:

$$R = S\, e^{(a + BS)}$$

where R = the total number of adults (pre-harvest) produced from the spawners 3 years previously, S = number of spawners, e = base of natural logarithms, a = parameter for intrinsic population growth rate, and B = parameter inversely proportional to maximum capacity to produce recruits.

The Ricker model has been shown to overestimate survival for Pacific salmon in comparison with other models (Sabo et al. 2004). We suggest that the authors compare results from the modified Ricker model with those from other models. The conclusions would be stronger if they were supported by the output of other models

(such as those used by Nickelson and Lawson 1998, McElhany and Payne 2004, or Wainwright (unpublished), used by the TRT). Nickelson and Lawson (1998), for example, incorporate habitat information with a population simulation based on individuals. Other approaches (e.g., Bayesian models) are available and being used to assess coastal coho. In comparing multiple models, it would be important to explain the differences in extinction probabilities in light of model assumptions, variables, and approximation to ecological reality. The IMST encourages the State to work closely with the TRT to examine the results of the several models being applied.

- The analysis of the Ricker model and the sensitivity of risk of extinction to changes in the model parameters are extremely important, but sensitivity to changes in model parameters can be misinterpreted. The authors of the *Viability Assessment* altered the beta value in the Ricker equation — a parameter that sets the upper limit of the recruitment curve — and evaluated the change in risk of extinction.

  The assessment describes beta as a parameter "inversely proportional to habitat capacity". The sensitivity analysis altered the value for beta and determined the change in the risk of extinction. Unfortunately, the authors described this analysis as changing the *habitat capacity*. The scientists conducting the assessment clearly understood that they were changing the recruitment limit in the model and were not changing a direct measure of the quality of the habitat. However, the assessment does not describe this clearly , and it would very easy to interpret their description as an analysis of changing the quality of stream habitat (e.g., frequency of pools, depth of pools, amount of large wood, percent of fine sediments. As we comment in our attached review of the *Synthesis* document, this distinction needs to be made very clear to avoid misinterpretation. The assessment concludes that the risk of extinction resulting from the Ricker model estimates was relatively insensitive to changes in the beta value (reduced capacity by 50–80%). Unfortunately they expressed that conclusion as

  > "Therefore, the assumptions about future habitat capacity are not as important to the accuracy of extinction risk predictions, especially considering the improbability of eliminating 50% to 80% of the remaining coho habitat in the near future– as this is the range of change necessary to elicit substantial changes in the risk of extinction. However, as a caveat, it should be stated that if habitat that is lost under a "what if" scenario is disproportionately of high quality then the analysis should be redirected to look at the associated changes to life cycle survival." (p.52)

  Changing beta in the Ricker model is not the same as changing habitat by 50–80%. The assessment is misleading and the statement could give policy-makers and the public a skewed picture of the amount of habitat conservation and restoration that is needed to recover coho salmon.

One difficulty with risk analyses is that the certainty of prediction decreases as a population approaches extinction. Variance in the recruits per spawner increases sharply at low spawner abundances. As a result, the uncertainty at low numbers would be greater and this needs to be acknowledged.

- Given the fact that this document breaks new ground in the arena of fish population viability analysis, we urge caution in interpreting results and in "explaining away" any apparent discrepancies in the data. Consideration of potential differences in the results between the approach taken in this report and more traditional methods would provide important context for interpreting results.

- It was appropriate, as explained to IMST at our January 2005 meeting in Salem, that the State ran its model and sub-models numerous times, changing variables to cover a wide array of potentially realistic situations. However, the document does not explain that this was done. The report would be greatly strengthened by explaining for which variables this was done, the ranges in conditions examined, and the conclusions. This would also help define where further such analyses should be conducted.

- We provide a detailed list of comments intended to improve analyses and presentation of data after the main body of this review.

Topic 6 – Use of thresholds and decision "cut-offs"

The cover letter accompanying the document states that the *Viability Assessment* "will also be used to inform NOAA Fisheries' final listing determination for Oregon Coast Coho under the federal Endangered Species Act"[5]. However, the assessment does not define threatened, endangered, or viable. The authors state, "for the entire ESU to be classified as not at risk, and therefore a candidate for de-listing, all 5 strata must pass a conservation risk threshold" (p.41). It would be helpful if the assessment linked the conservation risk thresholds in their analysis to the management categories (endangered, threatened, viable), and gave some indication of where the analyses place current and near-future coastal coho populations. The assessment should also explicitly qualify cases where biological information informing these categories is qualitative or reliant on professional judgment. This issue is relevant for both population-level and ESU-level criteria.

We found unevenness in the rigor of the rationales for some criteria and thresholds used in the analysis. In general, the report would be strengthened by explicitly stating and justifying the rationale for selection of particular cut-off and decision points, and by a more nuanced treatment of values that fall near thresholds. Even though some of the categories may be widely used by other agencies concerned with listing decisions, ODFW's obligation is also to meet objectives of the Oregon Plan and category definitions accepted by others may not meet the State's needs under the Oregon Plan. A discussion clarifying these issues would be helpful. Below are some detailed suggestions for addressing this need.

[5] December 14, 2005 letter to IMST from Michael Carrier, Governor's Natural Resource Policy Director.

6.1 Population scale criteria

- Criteria 1 and 2. Productivity at low spawner density. When defining a measurement for R/S (p. 21 top), it should be stated that shaker or bycatch mortality is included in the estimated mortality for fishing mortality. Further, the assessment should explicitly state that the R/S value of 1.1 used (p. 22 top) is based on a marine harvest (including bycatch) of 8%. Alternative estimates of bycatch mortality should be provided and discussed.

  The logic for the choice of 5 spawners per mile (and 15 spawners per mile for the expanded data set (second paragraph, p. 22) needs to be given.

  On p. 22 it is stated that four fish per mile are "thought" to represent a critically low spawner level. Is this "thought" or known? If the former, what consequences to the model outcome would there be if this value was really higher? Sharr et al. 2000 (p. 22), the basis for the number "four", is not in the reference section.

- Criterion 4. Within population distribution. The logic behind using 50% occupancy of spawning habitat in Criterion 4 (p. 34) should be given. In addition, it would be useful to examine the effects of running the model with higher and lower occupancy percentages.

  In the discussion on p. 34, even though the report included citations supporting a spawner density of 4 per mile, we still question if this density is sufficient to represent occupancy. This is at best 2 pairs per mile and could easily be only one functioning pair. Given that there is no abundance criterion in this assessment, we believe a higher density should be considered. Four fish in 1600 m with a mean spawning stream width of 5 m would yield 4 fish in 320 wetted channel widths or 1 fish per 80 channel widths, which in fish assemblage analyses constitutes rarity. How does this number compare with the number needed for full seeding? We suggest that the authors provide more support for the 4 fish per mile criterion (i.e., with a figure).

- Criterion 5. Within population diversity. Selection of maintaining at least 95% of a population's heterozygosity appears reasonable (p. 35). We support the calculations of age at maturity of 2.94 for the stream and 2.78 for the lake populations. And, the calculations of N_e appear reasonable. The work leading to $N_e = 0.53N$ to account for unequal reproductive success is well-done. That the ultimate calculation for the reduction of $N_e = 0.212$ N is within the ranges reported by others (bottom of p. 37) is remarkable and strongly suggest accuracy. Arriving at an $N_b = 600$ (p. 39) also appears reasonable.

6.2 ESU scale criteria

- Number of populations/strata. That 2 or 50% of the independent populations must pass all of the population criteria (p. 41) for the ESU to be considered not at risk

appears reasonable regarding an ESA listing decision, but again, no scientific justification is given.

- Probabilities associated with color categories. With regard to the summary of population criteria and evaluation thresholds, we have difficulty with lack of justification for the probabilities associated with the red, yellow, and green categories. It is not clear to us where the values came from, or if they are unduly conservative or liberal. Some discussion of the basis for these values would help them seem less arbitrary.

Additional comments on technical details, and suggestions for improving data/results presentation

Effects of marine survival

pp. 8–9. We suggest the analysis presented on these pages should be revised to include a discussion of hooking mortality rates, from sport and commercial fisheries. Either here or in the subsequent report, available literature on hooking mortality should be reviewed and summarized. There is a real possibility that the estimates of hooking mortality are low because of delayed mortality resulting from exhaustion of the returned fish making them easier prey.

Density dependent recruitment – resilience at low spawner abundance

p. 10. Figure 2. On this page, it would be helpful to indicate whether these data are from the Columbia or the Coast. Figure 2a (in both the high and low points, and also the medians) shows a declining trend from 1964 to 1995. Explanation in the text of what ocean and weather conditions were associated with this trend, versus the increasing trends from 1959–1964 and 1995–2003, would be useful. We suggest that the authors consider the likely consequences of repeating that declining trend for another 30 years at current low population levels in the model sensitivity analysis (see also our related comments under Topic 2). We further suggest adding a second curve to the graph showing recruits or coho abundance.

p. 11. Figure 3. We suggest adding a second y-axis indicating total recruits. Without some indication of actual recruits there would seem to be little reason to be interested in abundance. The State will need to consider this when dealing with rehabilitation. Since the Tenmile population is not typical of the Coast, a display of the patterns in the isolines from a plot of R/S versus S for selected dependent and independent populations, coded by year and population on the same graph (or a family of graphs), may be revealing.

p. 11. In the Figure 4 caption, an explanation is needed for the natural and anthropogenic factors that are associated with the high and low points along the curve for the Alsea and Lobster data. Outliers and isolines have the potential for teaching us much about ever-changing limiting factors.

Revising the low abundance paradigm for coastal coho

pp. 12–13. The State uses a non-parametric analysis of relative spawner abundance versus recruits per spawner (R/S) data from 1990–1996 (Figure 6) to support a revised paradigm. In this analysis:

- The reasons for analytical choices (such as the reason for log transforming R/S ratios) should be more clearly documented. The analytic approach may be sound, but it needs more explanation for the reader to understand *why* it is sound.
- Figure 6 would be more informative if it were depicted in terms of actual abundance. There are only two years where the R/S average is above replacement, a small number that could result in a false sense of security. It would be nice to know how close to zero spawner abundance was in those years. And, if the spawner abundances of some of the higher abundances (spawner abundance ranks 5–7) were in actuality quite similar to those of abundance category 2, that would lower the confidence in the conclusion based on Figure 6 concerning population resilience, amongst others.
- The report states that the R/S values were "symmetrically distributed" (p. 13). How do we know this? Perhaps it only appears symmetrical when using an artificial scale. Perhaps the actual difference between the lowest two groups is not 1 but 0.5 or 1.5? In addition, of what importance would it be if the distribution was indeed asymmetrical?
- p. 13. We feel the reader would like to be able to identify the 17 individual basins in Figure 6, which could be accomplished by using symbols or letters instead of circles. We are interested in the consistency of responses among these basins during good and bad ocean conditions, and suspect that certain basins do relatively better in poor years than the others, and different basins do relatively better than the others in good years. Assuming there are these differences, they should be explained in the text.
- p. 13. The kind of "population" used in the analysis (second line p. 13) should be stated, like "independent", assuming that that is what was done.

pp. 16–18. A graph of hatchery spawners versus wild recruits, by basin and also % marine survival versus miles occupied would be useful.

Criterion 3 – Long-term persistence

p. 25. We suggest that the modified Ricker curve (Figure 7) reflect what Figure 3 reflects to make it more realistic (assuming Figure 3 is representative of coastal basins); that is, recruits do not increase exponentially below 5000 spawners and then level off above 5000 spawners. Presumably this would reflect changes in the B term of the model as well.

p. 28. Regarding the stochastic simulation, describe the independent checks conducted to assure that the computer program macro performed as assumed.

p. 42. We suggest the authors provide numerical criteria under each heading in Table 4 so that the reader can easily compare them against results.

p. 53. We suggest adding a plot showing the effects of hatchery coho on wild coho.

pp. 43–55. What is the statistical power (probability of Type II error) in the model discussed in these pages? We assume that error variances were calculated and B can be estimated from them.

p. 43–55. In the stochastic simulation of population abundance, is it known that “arand” is normally distributed? At low population abundances, would it not make sense that the lower end of the variation associated with intrinsic growth rate would be zero? If so, then some other distribution should be used for “a”.

Literature cited

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

Bisson, P.A., and Nielson, J.L. 1983. Winter habitat utilization by salmonids in Pacific Northwest streams: the role of channel morphology and woody debris. Weyerhauser, Tacoma, Washington.

Bustard, D.R., and D.W. Narver. 1975. Aspects of winter ecology of juvenile coho salmon (Oncorhynchus kisutch) and steelhead trout (Salmo gairdneri). Journal of the Fisheries Research Board of Canada 32: 667–680.

Dewberry, T.C. In review. Do salmon anchor habitats exist? Part 2, evidence from the Nooksack Basin, Washington, Siuslaw Basin, Oregon, and Napa Basin, California. Canadian Journal of Fisheries and Aquatic Sciences.

Fausch, K.D., Torgersen, C.E., Baxter, C.V., and Li, H.W. 2002. Landscapes to riverscapes: bridging the gap between research and conservation of stream fishes. BioScience 52: 483–498.

Hughes, R.M., Howlin, S., and Kaufmann, P.R. 2004. A biointegrity index (IBI) for coldwater streams of western Oregon and Washington. Transactions of the American Fisheries Society 133: 1497–1515.

Independent Multidisciplinary Science Team (IMST). 1999. Defining and evaluating recovery of OCN Coho Salmon stocks: implications for rebuilding stocks under the Oregon Plan for Salmon and Watersheds. Technical Report 1999-2. Governor’s Natural Resources Office, Salem, OR.

Ludwig, D. 1999. Is it meaningful to estimate a probability of extinction? Ecology 80: 298–310.

Matthews, W. J. 1998. Patterns in freshwater fish ecology. Kluwer, Norwell, MA.

McElhany, P., and Payne, J. 2004. Draft user manual for SPAZ version 1.0.0 alpha. Salmon population analysis. NOAA Fisheries Northwest Fisheries Science Center, Seattle, WA.

Nehlsen, W., Williams, J.E., and Lichatowich, J.A. 1991. Pacific salmon at the crossroads: stocks at risk from California, Oregon, Idaho and Washington. Fisheries 16: 4–21.

Nickelson, T.E., and Lawson, P.W. 1998. Population viability of coho salmon, Oncorhynchus kisutch, in Oregon coastal basins: application of a habitat-based life cycle model. Canadian Journal of Fisheries and Aquatic Sciences 55: 2383–2392.

Oosterhout, G.R., Huntington, C., Nickelson, T.E., and Lawson, P.W. In press. A stochastic model investigation of the potential benefits of a conservation hatchery program for supplementing Oregon Coast coho salmon (Oncorhynchus kisutch). Canadian Journal of Fisheries and Aquatic Sciences.

Oregon Department of Fish and Wildlife. 2005. ODFW Habitat Report. The status and trend of physical habitat in coho bearing streams in the Oregon Coast Coho Evolutionary Significant Unit. January 31, 2005 Public Review Draft. Oregon Department of Fish and Wildlife, Salem, OR.

Phillips, S.H. 1986. Winter ecology of juvenile coho salmon (Oncorhynchus kisutch) in modified and unmodified sections of a coastal stream. Masters thesis, Oregon State University, Corvallis.

Sabo, J.L., Holmes, E.E., and Kareiva, P. 2004. Efficacy if simple viability models in ecological risk assessment: does density dependence matter? Ecology 85: 328–341.

Scudder, G.G.E. 1989. The adaptive significance of marginal populations: a general perspective. Pp. 180–185 in Levings, C.D., Holtby, L.B., and Henerson, M.A., Editors. Proceedings of the National Workshop on Effects of Habitat Alteration on Salmonid Stocks. Canadian Special Publication of Fisheries and Aquatic Sciences 105.

Torgersen, C.E., Price, D.M., Li, H.W., and McIntosh, B.A.. 1999. Multiscale thermal refugia and stream habitat associations of chinook salmon in northeastern Oregon. Ecological Applications 9: 301–319.

Tschaplinski, P.J., and Hartman, G.F. 1983. Winter distribution of juvenile coho salmon (Oncorhynchus kisutch) before and after logging in Carnation Creek, BC, and some implications for overwintering survival. Canadian Journal of Fisheries and Aquatic Sciences 40: 452–461.

IMST Review of the State of Oregon's Oregon Coastal Coho Assessment, Part 3B: *Policy for the Evaluation of Conservation Efforts: The Certainty That the Conservations Effort Will Be Effective* [January 31, 2005 Draft]

This section contains the Independent Multidisciplinary Science Team's (IMST) review of the State of Oregon's draft *Oregon Coastal Coho Assessment Part 3B: Policy for the Evaluation of Conservation Efforts: The Certainty That the Conservations Effort Will Be Effective* [January 31, 2005 Draft] (*PECE Certainty Analysis*).

In this review, we focus on scientific aspects of the *PECE Certainty Analysis* which need to be strengthened. We have not taken a position on what the listing status of coastal coho should be under the federal Endangered Species Act (ESA); we believe that to be, in part, a policy question, and outside the scope of IMST's role as neutral science advisors to the State. Our comments are intended to help the State produce a rigorous scientific assessment of the status of coastal coho salmon.

The State, in the Oregon Plan for Salmon and Watersheds (Oregon Plan), has made extraordinary strides in moving towards conservation of fish in a way that balances the needs of humans and the environment. The responsible agencies deserve great credit for the significant progress they have made in 1) monitoring, 2) development and implementation of plans and regulations, 3) working with stakeholders, and 4) collaborating with sister agencies. We particularly commend the State for:

- Good use of "newer" scientific information on headwater streams, debris flow routing, and sources of large wood and other structural elements. This is an excellent example of adaptive use of new information;
- Commitment to monitoring; and
- Commitment to good information management, including shared databases and consistent data standards and definitions.

From our discussions with Oregon Plan agency staff, we understand the focus of the *PECE Certainty Analysis* is to make the State's case that conservation measures under the Oregon Plan will be sufficient and effective to provide necessary protection for coastal coho. Our review focuses on weaknesses we observed in the assertions and rationale in the *PECE Certainty Analysis*, and areas where those assertions need to be supported or clarified. Our comments do not reflect criticism of the overall Oregon Plan framework or of the State's efforts to implement the plan. We acknowledge the State's successes, and hope that our comments are viewed as constructive.

Clarity of intent. The overall intent of the *PECE Certainty Analysis* isn't clearly stated in the document. We feel it would be helpful to state in the introduction that this is not a rigorous risk analysis, but rather a qualitative presentation of information to help NOAA Fisheries determine the likely effectiveness of the State's conservation efforts under the

Oregon Plan. Clear definitions of "certainty and uncertainty", "viable", "threatened", and "endangered" should be included in the introduction to help the reader interpret the information provided.

Balance. The *PECE Certainty Analysis* comes across more as an argument in favor of the decision not to list rather than a balanced analysis of the uncertainty and effectiveness of conservation efforts. The guidance on the PECE process in the *Federal Register* clearly indicates that the responsible authorities will evaluate both the certainty and the effectiveness of conservation efforts. A technical assessment of conservation efforts would be expected to document and provide scientific evidence of effectiveness. The current *PECE Certainty Analysis* emphasizes the positive aspects of the State's efforts, but it does not present an objective evaluation of scientific evidence for either the effectiveness of actions or the certainty of their implementation. A more balanced assessment of the strengths and weaknesses based on available evidence would strengthen the evaluation of conservation efforts.

Unsupported assertions. There are numerous instances in the text where assertions are made that are not supported by data within the *PECE Certainty Analysis.* Some of the assertions are likely to be supported by material in the individual technical reports prepared by State agencies as part of the larger Oregon Plan Coastal Coho Project[6]; however, citations and/or documentation of logic is lacking. The IMST feels documentation is especially needed to support the statement in the summary that "Oregon concludes that current regulatory and non-regulatory programs that address forest management, water quality, water quantity, and restoration have stopped the decline in habitat quality and quantity and are likely to improve habitat conditions in the foreseeable future" (p. 30; also see additional comments on habitat in following sections).

Specific instances where additional support is needed are too numerous to list here. Individual IMST members have offered to provide marked up drafts to the authors if requested.

Discrepancies between supporting technical reports and PECE Certainty Analysis conclusions. There are discrepancies between the information on habitat quality in the individual technical reports and statements in the *PECE Certainty Analysis*. As we point out in the previous section, the *PECE Certainty Analysis* states that programs "have stopped the decline in habitat quality" (p. 30). The data reported in the *ODFW Habitat Report* (ODFW 2005) do not necessarily support this conclusion. In fact, the *ODFW Habitat Report* indicates that "Our analysis indicates no consistent trend in instream habitat conditions in ESU streams from 1998 to 2003. These results are consistent with a study by Thom and Jones (1999) who assessed change in habitat from 1993–99 for all Oregon coastal streams" (ODFW 2005, p.10).

In addition, the *PECE Certainty Analysis* acknowledges the degraded condition of stream habitat and considers that to be a need for future improvement, but not a major limit to

[6] Individual technical reports are available on the web at http://nrimp.dfw.state.or.us/OregonPlan/. Retrieved March 9, 2005.

the survival of the species. This is primarily based on the sensitivity analysis of a parameter in the Ricker model that is not a direct measure of habitat quality. The *Viability Assessment*[7] uses the term "habitat capacity" when referring to the beta parameter in the Ricker model. These distinctions are not stated clearly in the evaluation of certainty, and the potential for misunderstanding and possible error is substantial. Most readers will interpret this as the quality of the habitat (e.g., pools, large wood, sediment, water temperature, etc.), but our discussions with staff make it clear that this is not what the assessment means by the term "habitat capacity". This needs to be explained much more clearly.

Circularity of viability/habitat discussion. We find circularity in the argument that habitat must be adequate since viability was determined by the assessment to be not at risk. On p. 8 of the *PECE Certainty Analysis,* the authors state "Given that the ESU is judged to be biologically viable, we conclude that habitat quantity and quality is sufficient to maintain a viable ESU through adverse ocean and freshwater conditions similar or slightly more adverse than the most recent period of poor survival conditions (1980s through late 1990s)." The circular reasoning can be framed as: *the ESU is viable; hence the habitat must be adequate. Therefore, the habitat must be adequate because the ESU is viable*. This can lead to a false sense of security in determining if the ESU could be de-listed. A more accurate statement could be: *the ESU might be viable, in spite of the fact that the habitat is quite marginal.*

Certainty regarding conclusions of Viability Assessment. The *PECE Certainty Analysis* and *Synthesis*[8] accept the *Viability Assessment's* conclusion of coastal coho viability without any reservations. We feel this overall assessment of effectiveness of conservation practices needs to address uncertainties that flow from the modeling of population viability. These uncertainties include measurement error, prediction uncertainty (difference between modeled response and true response), model uncertainty (how certain are you that the mathematical models chosen reflect reality) and natural stochastic variation (Steel et al. 2003), and a thorough evaluation of Type II error. Please refer to our attached review of the *Viability Assessment* for our specific concerns about the State's approach to analyzing population viability. The *PECE Certainty Analysis* reads as though the State is convinced that the ESU is not at risk of extinction and builds a case to support that contention. It would have been more convincing if the approach in the *PECE Certainty Analysis* and *Synthesis* documents had included a discussion of consequences to the report's conclusions of the uncertainties as described above. Then, if the results of the analyses suggested otherwise, there would be no hint of unintentional bias in any conclusion.

Evaluating uncertainty quantitatively. This report includes little quantitative analysis of uncertainty. We found the addition of the section "Sensitivity to Measurement Error and Analytical Assumptions" to the *Viability Assessment* (pp. 43–55) an extremely

---

[7] State of Oregon. Part I: Viability Criteria and Status Assessment of Oregon Coastal Coho [December 20, 2004 Draft]. All references to the Viability Assessment in this review are made to this draft version.

[8] State of Oregon's Oregon. Coastal Coho Assessment. Part 1: Synthesis of Viability Analysis and Evaluation of Conservation Efforts [January 31, 2005 Draft]

effective way to lay out areas of uncertainty and their potential effects on conclusions. We suggest a similar effort be made to address other sources of uncertainty in the *PECE Certainty Analysis*.

Unwarranted certainty regarding implementation and effectiveness of policies and practices. The *PECE Certainty Analysis* assumes a fairly high degree of certainty with regard to (1) the State's ongoing ability to implement/enforce regulatory and conservation plans and policies; (2) the effectiveness of those plans and policies (i.e., that they will achieve the intended goals); and (3) that adequate monitoring will occur to identify if 1 and/or 2 are not occurring. The IMST feels that the State should present evidence for certainty where it exists. Where uncertainty is present, it needs to be identified and the potential consequences of that uncertainty on the assessment's conclusions should be discussed.

The following are specific areas where the analysis of uncertainty could be improved. Please note that these comments in no way reflect pessimism on the part of IMST about the effectiveness of the State's conservation practices or the State's ability to implement them. We are simply pointing out the scientific need for the report to consider all sources of uncertainty that may affect the State's conclusions.

- Uncertainties related to program funding and policy implementation. The State confidently assumes the policies and activities mandated by the Oregon Plan will be fully implemented and funded, even though the document admits that there is uncertainty about this. Since 1997, IMST has made many scientific recommendations to State agencies to improve implementation of the Oregon Plan. Agency responses to these recommendations[9] consistently document funding, policy, and legal constraints that limit Oregon Plan implementation. The *PECE Certainty Analysis* would benefit from some summary of information on the track record of the various agencies in implementing conservation policies, and assessment of scenarios that disclose possible effects of lower funding levels, a reduction in skills and workforce in the State agencies, and lessened political and fiscal support for the Oregon Plan.

- Consistency of efforts on private lands. The report assumes that conservation practices (both voluntary and required) on private lands will be consistently implemented. We feel that there are likely to be uncertainties regarding compliance that should be considered explicitly in the analysis.

We recommend that the authors read and seriously consider McEvoy (1986), who described how efforts to implement management activities can fail despite the best intentions because of undue optimism and lack of accountability.

Assertions about the State's ability to respond to and mitigate declines. A key component of the State's logic is the assertion that risk factors will not deteriorate so quickly that the State would not be able to detect population declines and respond

[9] Agency responses to IMST recommendations are included on the IMST web site http://www.fsl.orst.edu/imst/reports.html.

effectively with mitigation. As we note in our attached review of the coastal coho project *Synthesis*, ODFW scientists indicated to IMST that Lower Columbia River coho salmon changed status rapidly and were extirpated from several basins in a short period of time (M. Chilcote, ODFW, personal communication) Evidence that changes will be gradual and the State will have time to respond is not reported, nor are there specific descriptions of the responses that would be measured, the drivers that could be altered by management decisions, and the timeframe that would be required for the habitat or the species to respond.

The *PECE Certainty Analysis* does not present information on the likely effectiveness and timeliness of mitigative actions. Uncertainty about the effectiveness of many commonly-used aquatic and riparian restoration strategies (Roni et al. 2002) should be considered in the State's analysis. As we described in our attached review of the Synthesis, even when appropriate strategies are implemented, change may not be observed for years to decades. Including some discussion of the time frame within which conservation efforts are expected to be effective would strengthen the analysis of certainty.

The sensitivity analysis of instream habitat and riparian variables shown in Figure 4[10] is a strong part of the report. We feel that the addition of information on the time over which monitoring would need to occur in order to detect change, and time needed for mitigations to be implemented, would be very helpful. Also, the addition of units for the habitat variables would help interpretation of this table. Overall, the State's argument about its ability to respond to and mitigate declines would be more persuasive if it evaluated the ability of the State's monitoring program to detect both (1) declines in populations and (2) changes in risk factors over appropriate scales of time and space.

Water quality. The document should point out potential uncertainties in the implementation of Total Maximum Daily Load (TMDL) allocations. For example, have any TMDL or agricultural water quality plans been in place long enough to improve water quality? And, what does the compliance record look like? There are many steps between plans and biological responses, including policies, permits, funding, technology implementation, enforcement, changes in loadings and land use, and changes in physical and chemical habitat quality (Hughes et al. 2000; Karr and Yoder 2004). The Oregon Plan monitoring data evaluated in this report are not able to detect improvements in water quality in coastal coho streams over the last 10 years (p.25). Therefore, we find the State's characterization of water quality as a "low risk level" (p. 15) to be inadequately supported by the data presented.

State's adaptive management framework. The *PECE Certainty Analysis* identified a critical aspect of resource management—adaptive management—but the discussion is extremely general and vague. The *PECE Certainty Analysis* should provide additional clarification of the State's definition and use of "adaptive management". Inclusion in Section 6 of an explicit description of the processes the State has established for implementation of adaptive management under the Oregon Plan would be helpful. The

[10] Figure 4 is actually a table and needs to be relabeled.

section currently mentions goals for adaptive management and sustainability, but it provides no specific process or decision making system. The closest it comes is the introductory paragraph on p. 29, but this is a rather incomplete description of an adaptive management process. It identifies broad areas where programmatic responses have occurred, but lacks specifics for the major elements, and does not show how they are connected. Ideally, an adaptive management system would include processes within individual agencies that are integrated among different agencies where appropriate. Additionally, an integral component of adaptive management includes effectiveness monitoring, and it is not evident in this document that the state has adequate effectiveness monitoring programs to work within an adaptive management framework. Adaptive management is also an ongoing, iterative process. IMST has previously pointed out in earlier reports that, with the exception of Oregon Department of Forestry's Forest Practice Act, state natural resource agencies do not have mechanisms in place for successful adaptive management under the Oregon Plan. In the current *PECE Certainty Analysis*, there is no explanation of how the separate actions (listed in the paragraph on p.29 as noted above) collectively form a coordinated system of goals, observations, analyses, and responses.

Literature cited

Hughes, R.M., Paulsen, S.G., and Stoddard, J.L. 2000. EMAP-Surface waters: a multiassemblage probability survey of ecological integrity in the U.S.A. Hydrobiologia 423: 429–443.

Karr, J.R., and Yoder, C.O. 2004. Biological assessment and criteria improve TMDL planning and decision making. Journal of Environmental Engineering 130: 594–604

McEvoy, A.F. 1986. The fisherman's problem: ecology and law in the California fisheries, 1850–1980. Cambridge University Press, New York.

Oregon Department of Fish and Wildlife (ODFW). 2005. ODFW Habitat Report. The status and trends of physical habitat in coho bearing streams in the Oregon Coast Coho Evolutionary Significant Unit. January 31, 2005 Public Review Draft. Oregon Department of Fish and Wildlife, Salem, OR.

Roni, P., Beechie, T.J., Bilby, R.E., Leonetti, F.E., Pollock, M.M., and Pess, G.R. 2002. A review of stream restoration techniques and a hierarchical strategy for prioritizing restoration in Pacific Northwest watersheds. North American Journal of Fisheries Management 22: 1–20.

Steel, E.A, Liermann, M.C., McElhany, P., Scholz, N.L., and Cullen, A.C. 2003. Managing uncertainty in recovery planning. Pp. 74–90 *in* Ecosystem recovery planning for listed salmon: an integrated assessment approach for salmon habitat (Beechie, T.J., Steel, E.A. Roni, P. and Quimby, E. Eds). U.S. Dept. Commer., NOAA Tech. Memo. NMFS-NWFSC-58.

IMST Review of the State of Oregon's Oregon Coastal Coho Assessment
Part 1: *Synthesis of Viability Analysis and Evaluation of Conservation Efforts*
[January 31, 2005 Draft]

This document contains the IMST's review of the State of Oregon's draft *Oregon Coastal Coho Assessment Part 1: Synthesis of Viability Analysis and Evaluation of Conservation Efforts* [January 31, 2005 Draft] (*Synthesis*). We commend the State for its efforts at bringing together available information on the status of coastal coho salmon.

In this review, we focus on scientific aspects of the *Synthesis* which need to be strengthened. We have not taken a position on what the listing status of coastal coho should be under the federal Endangered Species Act (ESA); we believe that to be, in part, a policy question, and outside the scope of IMST's role as neutral science advisors to the State. Our comments are intended to help the State produce a rigorous scientific assessment of the status of coastal coho salmon.

In previous sections, we have reviewed the two separate analyses (*Viability Assessment*[11] and *PECE Certainty Analysis*[12]) that provide a foundation for this *Synthesis*. We urge the authors to consult our comments on the *Viability Assessment* and *PECE Certainty Analysis*, particularly those regarding uncertainty and need for substantiation of assertions, because those especially apply to the *Synthesis*.

Lack of appropriate citations or supporting information. Throughout the document, there are many assertions that need supporting information, citations, or presentation of a rationale. The credibility of the document would be improved if the authors were more diligent about supporting their statements.

Synthesis of Oregon Plan information. Assessing the status of coastal coho salmon involves pulling together a large body of information. During our discussion of this document at our January meeting, we could not decide if this was intended to be an executive summary or a synthesis. It is too long for the former, but not sufficiently synthetic if the latter.

It would be helpful if synthesis and summary components were separated more clearly. In the truly "synthetic" sections (i.e., those that integrate the findings from the viability assessment with Oregon Plan activities relevant to the federal PECE policy, and make conclusions from the combination of the two), it would be most helpful to have a clear and concise chain of logic that lays out (1) evidence, (2) interpretations, (3) sources and

[11] State of Oregon. Part I: Viability Criteria and Status Assessment of Oregon Coastal Coho [December 20, 2004 Draft]

[12] State of Oregon. Oregon Coastal Coho Assessment. Part 3B: Policy for the Evaluation of Conservation Efforts: The Certainty That the Conservations Effort Will Be Effective [January 31, 2005 Draft]

implications of uncertainty, and (4) conclusions. In much of the *Synthesis*, the evidence and evaluation of uncertainty are absent.

Integration with NOAA Fisheries' Oregon Coast Workgroup of the Oregon/ Northern California Coast Technical Recovery Team (TRT) modeling. We suggest the authors consider (if possible) waiting to finalize their conclusions until the TRT completes its viability modeling work. Taking advantage of the TRT's comparison of results among different models would enhance the credibility of the State's assessment and conclusions, and would go a long way to addressing our concerns about insufficient attention to uncertainty and model inconsistencies.

Need for balanced synthesis and presentation of certainty. In general, we found the *Synthesis* document did not adequately address uncertainties about (1) the viability of coastal coho, (2) implementation and effectiveness of conservation measures, and (3) the ability of the State to detect declines in coho populations or changes in risk levels, and to mitigate in a timely fashion.

- Viability of coastal coho. The *Synthesis* appears to assume from the *Viability Assessment* that coastal coho salmon are strongly viable or "not at risk" (Viability Assessment, p. 56), when in fact the authors have said in discussions at January and February 2005 IMST meetings that the modeling results actually portrayed coho as "barely viable". This is a major distinction. Some qualifications about viability documented in the *Viability Assessment* (i.e., discussion of uncertainties in Section VIII – Synthesis and Additional Discussion) appear to not have been considered in the conclusions of the *Synthesis*.

  Also, it would be helpful if the State clearly explained the current status of the species on a continuum of conservation categories from fully recovered to viable to threatened or endangered (see comments in our review of the *Viability Assessment*).

- Implementation and effectiveness of conservation measures. The *Synthesis* further seems to assume that protective measures will be implemented and effective, that monitoring will occur as planned and will give the necessary answers, and that timely response to fix problems will occur and be successful. Actual information to support this optimistic view is not presented in the document apart from a very valuable listing of programs and regulations in the section on "Delivery Mechanisms" (p. 39). State agency staff have communicated to IMST that the State does have important information to support some of these assertions. The *Synthesis* would be more credible if this supporting material was documented. If the State is unable to provide adequate documentation, the assumptions should be reevaluated for their accuracy.

  An evaluation of the adequacy of Oregon's extant rules, regulations, and programs relative to meeting objectives of the Oregon Plan would improve confidence in the conclusions offered in the certainty analysis and synthesis. There are two science-related issues here. First, what data are available to document that implementation of Oregon Plan measures, programs, and regulations have occurred and are having the

desired effects? Current compliance, implementation, and effectiveness monitoring data from the State are essential for answering those questions. As we discussed in our report on salmon recovery in lowland ecosystems (IMST 2002), we believe that such evaluations are crucial for Oregon. To have certainty that a program, policy, or regulation is effective, biological responses must be documented whenever possible.

Second, if the rules and regulations were fully enforced and programs implemented, would they be sufficient to meet the objectives of the Oregon Plan? A report evaluating the adequacy of Washington's water quality criteria (Karr et al. 2003) indicated that some Puget Sound streams complied with water quality criteria but were in poor biological condition. This indicates that water quality criteria can be inadequate for protecting designated uses such as salmon spawning, rearing, and migration. Such contradictions occur because water quality criteria do not include all chemical constituents, physical habitat structure, and altered flow regimes.

As written, this *Synthesis* contains no assessment of the potential for departures from what is expected to occur, and what the likely consequences would be to the State's overall conclusion of certainty (p. 52, see further discussion in our evaluation of the *PECE Certainty Analysis*). Without a discussion about these uncertainties, there is no opportunity for the State to explain how responses to undesirable conditions would occur. We find this to be the greatest weakness in the *Synthesis*, and one that significantly affects the report's credibility.

- Ability of the State to detect declines and to mitigate in a timely fashion. As we document in our attached review of the *PECE Certainty Analysis*, IMST finds that there is considerable uncertainty about the State's ability to respond to and mitigate declines in coho salmon. For example, although ODFW has demonstrated the ability to reduce direct coho harvest, hatcheries remain a detrimental factor in some basins. Also, if federal and state funds or staffing are reduced because of continued budget deficits, the State may be unable to adequately monitor coho and coho physical and chemical habitat. ODFW scientists indicated that Lower Columbia River coho salmon populations changed status rapidly and they were extirpated from several basins in a short time (M. Chilcote, ODFW, personal communication). Evidence that changes will be gradual and that the State will have time to respond is not reported. Also the assurances are not compelling without specific description of the responses that would be measured, the socioeconomic drivers that could be altered by management decisions, and the timeframe that would be required for the habitat or the species to respond.

Monitoring. The section on monitoring would be improved by including more information on: 1) key questions that are being monitored (see IMST 2001), 2) goals identified in the Oregon Plan monitoring strategy, and 3) predicted future trends. Inclusion of such information would significantly strengthen the State's assertions that it will be able to detect change timely enough for mitigation.

Synthesis conclusion, adaptive monitoring and conservation plans. The State of Oregon concludes that:

> "the Coastal coho ESU [evolutionary significant unit] is biologically viable based on habitat conditions (including marine survival) during the recent past. The ESU retains sufficient biological productivity and is supported by sufficient habitat to be sustainable through a future period of adverse ocean conditions similar to or slightly more adverse than the most recent period of poor ocean survival (1980s and early to mid-1990s). Although the conclusion that the Coastal coho ESU is currently viable is based on the biological status of the ESU and its response to existing habitat conditions, a diverse set of laws and programs supports two additional conclusions.
>
> 1. The ESU will remain viable in the foreseeable future, as measurable deterioration in habitat has been stopped, if not reversed.
> 2. It is highly likely that monitoring will detect any significant future deterioration in viability of the ESU – or in environment or management conditions that support viability – and will allow a timely and appropriate response by state and or federal entities to conserve this ESU." (p.52)

We find:

- There is no evidence in the *Synthesis* that coastal coho salmon can survive through conditions that are "slightly more adverse than the most recent period of poor ocean survival".

- The assertion that "measurable deterioration in habitat has been stopped, if not reversed" is not supported by the *ODFW Habitat Report* developed for the Oregon Coastal Coho Project (ODFW 2005; see discussion in our review of the *PECE Certainty Analysis*).

- Fishery harvest can be changed rapidly and hatchery practices can be changed relatively quickly. However, changes in land use practices that cause habitat degradation are unlikely to produce rapid habitat improvement, because [as is clearly stated in the *ODFW Habitat Report* (ODFW 2005)], generating intact and functioning riparian areas, growing large trees that become instream large wood, and developing complex pool habitats can take multiple decades. Thus the conclusion that "monitoring will detect any significant future deterioration in viability of the ESU—or in environment or management conditions that support viability—and will allow a timely and appropriate response by state and or federal entities to conserve this ESU" is not fully supported by the State's assessment. This conclusion does not address the uncertainties that exist, and is not specific about the types of change that can be addressed quickly and the types of change that require long periods for rehabilitation.

IMST conclusions about the Synthesis. We feel that many of the conclusions reached by the State in the *Synthesis* are not sufficiently supported by the information in the documents we were asked to review. In some cases, information we know exists was not

included, and in other cases we feel there was lack of attention to uncertainties. Resolution of these two problems would go a long way towards providing support to the State's arguments. In summary, we do not intend to imply that the State's efforts to provide for conservation of coho salmon are inadequate; we simply point out that the assertions in the *Synthesis* are, in many cases, not supported by the information presented or do not provide for scenarios where implementation or effectiveness is significantly less than 100% successful.

Literature cited

Independent Multidisciplinary Science Team (IMST). 2001. 1999-2000 IMST Review of Oregon Plan Monitoring Program. http://www.fsl.orst.edu/imst/reports/.

Independent Multidisciplinary Science Team (IMST). 2002. Recovery of wild salmonids in western Oregon lowlands. Technical Report 2002-1 to the Oregon Plan for Salmon and Watersheds, Governor's Natural Resources Office, Salem, OR. http://www.fsl.orst.edu/imst/reports/.

Karr, J. R., Horner, R. R., and Horner, C. R. 2003. EPA's review of Washington's water quality criteria: an evaluation of whether Washington's criteria proposal protects stream health and designated uses. Report for National Wildlife Federation, Seattle, WA.

Oregon Department of Fish and Wildlife (ODFW). 2005. ODFW Habitat Report. The status and trends of physical habitat in coho bearing streams in the Oregon Coast Coho Evolutionary Significant Unit. January 31, 2005 Public Review Draft. Oregon Department of Fish and Wildlife, Salem, OR.

OVERALL CONCLUSIONS

At the request of the State of Oregon, the Independent Multidisciplinary Science Team (IMST) has reviewed the following three documents:

- **Viability Assessment** – "Viability Criteria and Status Assessment of Oregon Coastal Coho. December 20, 2004 Draft"
- **PECE Certainty Analysis** – "Policy for the Evaluation of Conservation Efforts: The Certainty That the Conservations Effort Will Be Effective [Part 3B]. January 31, 2005 Draft"
- **Synthesis** – "Synthesis of Viability Analysis and Evaluation of Conservation Efforts. January 31, 2005 Draft"

We have focused on major scientific issues that could affect the scientific basis of the analyses and conclusions. We have not taken a position on what the listing status of coastal coho should be under the federal Endangered Species Act (ESA); we believe that to be, in part, a policy question, and outside the scope of IMST's role as neutral science advisors to the State. Our comments are intended to help the State produce a rigorous scientific assessment of the status of coastal coho salmon.

Positive aspects of the reports

- The staff assigned to the assessment of the status of coastal coho salmon has completed a complex analyses and enormous writing task in a narrow time window. IMST commends their exhaustive efforts to create a scientifically sound assessment of the status of coastal coho salmon under intense pressure and public scrutiny.

- The Oregon Plan for Salmon and Watersheds' (Oregon Plan) exceptional monitoring program provided the State with much more empirical information than is usually available to assess either a species' status or the landscape condition of a large region. The wealth of high quality data provided a unique opportunity for the State to produce a status assessment that is far more quantitatively rigorous than the norm.

- The State of Oregon has produced a major synthesis of available biological data on coastal coho salmon. The major criteria on which the *Viability Assessment* is based—abundance, distribution, productivity, persistence, and diversity—are scientifically appropriate. These criteria are consistent with criteria used by the NOAA Fisheries' Oregon Coast Workgroup of the Oregon/Northern California Coho Salmon Technical Recovery Team (TRT), further strengthening the basis of this assessment.

  The *Viability Assessment* reflects a rigorous analysis of data, careful integration of disciplines and variables, and blending of modeling and application of observed data. It illustrates the efforts of the State to clearly present data, develop logical conclusions from data and analyses presented, and produce a readable document of highly technical information.

The efforts to describe uncertainties and risks in the *Viability Assessment*'s Section VIII (Synthesis and Additional Discussion) provide essential context to the overall interpretation of the report's conclusions.

Aspects of the reports that need to be strengthened

- The three documents we reviewed do not clearly indicate the State's final determination of the status of coastal coho salmon along the continuum from "endangered" to "threatened" to "minimally viable" to "recovered" to "healthy and robust". IMST recognizes that listing decisions under the federal ESA require only a categorical determination and do not require assessment of where an evolutionary significant unit (ESU) generally falls along such a continuum. However, this assessment of the status of coastal coho salmon also serves both the people of Oregon and the agencies dedicated to implementing the Oregon Plan. All decision makers, federal and state, would benefit from a clear assessment of the status of coho salmon along the full range described above. The State has said many times that simply recovering coastal coho to the point of delisting under ESA does not meet the goals of the Oregon Plan. This mission statement of the Oregon Plan is to "**Restore the watersheds of Oregon and to recover the fish and wildlife populations of those watersheds to productive and sustainable levels in a manner that provides substantial environmental, cultural, and economic benefits.**" In 2002, the Interim Joint Salmon Recovery Task Force (as required by HB 3002) developed a definition of recovery which includes: "**The spawning and rearing habitat will be of sufficient quality and quantity to provide natural sustainability of fish populations and associated environmental, cultural, and economic benefits**". A more clear indication of the relative status of the fish would provide important perspective for both the upcoming decisions under ESA and the development of a long-term conservation plan for coho salmon under the Oregon Plan.

- Frequently, it is difficult to determine whether the authors are speculating or stating facts. The text should clearly differentiate between the two types of statements, and provided supporting information, references, or rationale where appropriate. Likewise, the support of text through citations is uneven. A thorough review of the document should be done with this in mind, and citations included where possible.

- Some of the terms used in the documents need better definition in order to be understood. For example, "habitat capacity", when referring to the beta parameter in the Ricker model, is likely to be understood by many as the quality of the habitat (e.g., pools, large wood, sediment, water temperature, etc.), while our discussions with staff make it clear that this is not what the assessment means by the term "habitat capacity". Other terms that need to be clearly understood are "viable", "endangered", "threatened", and "uncertainty". We recognize that the State faces a major challenge in clearly conveying such complex scientific concepts to a diverse audience.

- We feel the report takes an unduly static view of the Oregon coastal landscape, and would be strengthened by inclusion of more future scenarios that integrate the

potential consequences of multiple factors that affect fish populations (including ocean conditions, drought, fires, overall watershed conditions brought about by human population increases and land use change, and stream habitat conditions), and assess what would happen if individual trends detrimental to fish converged. We feel the report was overly dismissive of the likelihood that such scenarios might actually occur over the long term

- The *Viability Assessment* relies heavily on the survival of coho salmon through recent periods of low ocean productivity, the relationship between the production of offspring per adult at different densities (recruits/spawner), and analysis of risk of extinction based on the Ricker model. The report would be strengthened by a more balanced discussion of the strengths and weaknesses of those important lines of evidence.

  The conclusions of the *Viability Assessment* depend strongly on assumptions regarding a new "low abundance paradigm". This perspective on coastal coho salmon has not been thoroughly reviewed or tested. The State should be cautious about making decisions that are based heavily on the new "low abundance paradigm" and should seek additional intensive review.

  The State's assessment of risk of extinction or persistence is based on a single model of population dynamics—the Ricker model. Several major types of population models are available and have been applied to coastal coho salmon. The TRT is exploring several types of models. We encourage the State to consider the outcomes of these models prior to its final report to NOAA Fisheries to ensure that their recommendation is rigorous and includes all available scientific information.

- The reports would benefit from a more balanced presentation of the information, analyses, conclusions, and uncertainties. After the conclusions are presented, the *PECE Certainty Analysis* and the *Synthesis* documents tend to focus solely on the evidence and outcomes that support those conclusions. Uncertainties are largely dismissed, especially those related to the ability of the State to respond rapidly to possible future declines in coho status. The actions and conclusions may remain the same, but the State should clearly describe the status of the coastal coho salmon and the certainties and uncertainties that affect future trajectories in these fish and the landscapes that support them.

- We find circularity in the argument that habitat must be adequate since viability was determined by the assessment to be not at risk. The circular reasoning can be framed as: **the ESU is viable; hence the habitat must be adequate. Therefore, the habitat must be adequate because the ESU is viable**. This can lead to a false sense of security in determining if the ESU could be de-listed. A more accurate statement could be: **the ESU might be viable, in spite of the fact that the habitat is quite marginal.**

- The *Synthesis* seems to combine elements of a true synthesis (of the viability assessment and predicted effectiveness of conservation practices) with lengthy restatement and summarization of material from the other two documents. Separation of the summary and synthesis components would make for a much more understandable product.

- We feel that many of the conclusions reached by the State in the *Synthesis* are not sufficiently supported by the information in the documents we were asked to review. In some cases, information we know exists was not included, and in other cases we feel there was lack of attention to uncertainties. Resolution of these two problems would go a long way towards providing support to the State's arguments. In summary, we do not intend to imply that the State's efforts to provide for conservation of coho salmon are inadequate; we simply point out that the assertions in the *Synthesis* are, in many cases, not supported by the information presented or do not provide for scenarios where implementation or effectiveness is significantly less than 100% successful.