Review of Klamath River Fall Chinook salmon production model and final report.

An External Independent Peer Review Report

Produced by

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Produced for

The Center for Independent Experts (CIE)

Executive Summary:

This review was conducted mainly to identify the strengths and weaknesses of recent opinions concerning the impacts of the Klamath River dam removal plans on the future state of the supported fish populations and the aquatic ecosystem. The review focused primarily on the data, assumptions, and analytical methods used to determine the potential for salmon stock rebuilding, as described in various reports submitted by the CIE to the author. This revealed that considerable efforts were made to compile information from multiple sources on past and current conditions of the habitat and supported aquatic populations. Pertinent data sets were then used with ancillary information on the expected conditions of the habitat after dam removal to make predictions on the future state of the ecosystem with or without dams. The results of the various forecasts made are encouraging in that they all support the notion that the dynamics of this aquatic ecosystem would substantially improve with the removal of the four dams in the lower reaches. My major conclusions concerning the six major issues to focus on, according to the Terms of Reference (*ToR*), are as follows;

ToR 1: Evaluation and recommendations of data quality: Overall, the amount and quality of the data used is very good, and is considered sufficient for modelling and forecasting purposes. More data on the actual dam removal procedures would have been desirable.

ToR 2: Evaluation of strengths and weaknesses of, and recommendations to improve analytic methodologies. Most procedures used appear to be well established and scientifically sound. A few additional details would have been useful on some procedures used for numerical simulations, and a few forecasted trends based on deterministic calculations might have been more informative if they had accounted for stochastic variation and plausible error distributions.

ToR 3: Evaluation of and recommendations to improve model assumptions, estimates, and characterization of uncertainty. Some very sophisticated biological models were used to account for past conditions, the system dynamics, and uncertainties in both parameter values and future conditions. It might be desirable to adjust the structure of the most complex model used to focus less attention on long term trends under highly uncertain future conditions that may or may not prevail 20+ years from now, and more attention on the plausible range of impacts due to dam removal and stock rehabilitation activities that have not yet even begun.

ToR 4: Determine whether the science reviewed is considered to be the best scientific information available. I cannot confirm categorically if it is the best [scientific information] available, but I would not hesitate to state that it appears to be to a large extent "some of the best scientific information available".

ToR 5: Recommendations for further improvements. Forecast shortcomings are often due to data deficiencies. There are considerable data on past conditions but these may not be representative of future conditions. Sufficient resources should be used to properly plan and execute the stock rebuilding operations for the period following dam removal, and monitor the success of such operations using the best methods available. The empirical results should be used to periodically verify/adjust the biological model parameters and the underlying hypotheses.

ToR 6: Brief description on panel review proceedings highlighting pertinent discussions, issues, effectiveness, and recommendations. Based on a cursory review of the above, apart from the relatively small issues highlighted above, there do not appear to be obvious or major gaps, omissions or errors in the panel review proceedings.

Background :

After decades of controversy and litigation over water management for fisheries, wildlife and agricultural irrigation, key stakeholders within the in the Klamath River Basin sat down together over the course of several years and negotiated two landmark agreements, the Klamath Hydroelectric Settlement Agreement (KHSA) and the Klamath Basin Restoration Agreement (KBRA). Together, these two agreements have the potential to dramatically change the way water is managed for beneficial uses within the Klamath River Basin. Signatories and/or supporters of the agreements include many Parties comprised of three tribal governments, several fishing organizations, irrigation districts, environmental groups, hydroelectric power companies and several local, state, and federal agencies.

The KHSA and KBRA were signed by the Parties during February 2010. Under the KHSA the Secretary of the Interior is required to make a determination by March 31, 2012 as to whether the removal of the four downstream-most dams on the Klamath River that are owned by PacifiCorp 1) will advance restoration of the salmonid fisheries of the basin, and 2) is in the public interest, which includes, but is not limited to, consideration of the potential impacts on affected local communities and tribes. If the Secretary determines not to proceed with facilities removal, the KHSA terminates unless the Parties can agree to a remedy for the issues leading to the negative determination. Prior to the adoption or public release of such a determination, the Secretary would notify the Parties of the tentative determination and its basis. The Parties would consider whether to amend the KHSA in a manner that would permit the Secretary to make an Affirmative Determination.

Concurrently with the signing of the KHSA, the same Parties, with the exception of the two federal parties and PacifiCorp, signed the KBRA. The KBRA includes interrelated plans and programs intended to benefit fisheries throughout the basin, water and power users in the Upper Klamath Basin, counties, tribes, and basin communities. The KBRA brings many parties together to support one another's efforts to restore fisheries in the Klamath River Basin and provide for sustainable agricultural communities. Implementation of the KBRA is intended to accomplish the following:

1. Restore and sustain natural fish production and provide for full participation in ocean and river harvest opportunities of these fish;
2. Establish reliable water and power supplies for agricultural uses, communities, and National Wildlife Refuges (NWRs); and,
3. Contribute to public welfare and sustainability of all communities through reliable water supply; affordable electricity; programs to offset potential property tax losses and address economic development issues in counties; and efforts to support tribal fishing and long-term economic self-sufficiency.

Two key provisions of the KBRA require the establishment of (i) a Fisheries Program that includes development of basin wide fisheries reintroduction, monitoring and restoration plans; and, (ii) Water Resources Program that includes calls for establishment of water diversion limitations to irrigators within the Klamath Project in exchange for increased predictability of seasonal water deliveries and includes development of affordable power supplies. The additional water made available through savings created by implementation of water conservation measures, development of additional water storage facilities, and reductions to irrigation will be managed under an Environmental Water Program whose purpose is to use this additional water for the benefit of fishery and aquatic resources.

The Secretary of the Interior, in cooperation with the Secretary of Commerce and other Federal agencies, will (i) use existing studies and other appropriate data, including those in the FERC record for this project; (ii) conduct further appropriate studies, including but not limited to an analysis of sediment content and quantity; (iii) undertake related environmental compliance actions, including environmental review under NEPA; and (iv) take other appropriate actions as necessary to determine whether to proceed with facilities removal.

Two fish production models were developed to provide Chinook salmon population estimates to assist in the economic analysis that is currently underway for the Secretarial Determination. This analysis, along with several others, will be used by the Secretary as he contemplates on the determination as to whether or not the removal of the four lower most dams on the Klamath River, along with those actions described in the KBRA, should proceed.

The NMFS Office of Science and Technology coordinates and manages a contract providing external expertise through the Center for Independent Experts (CIE) to conduct independent peer reviews of NMFS scientific projects. Each CIE reviewer is contracted to deliver an independent peer review report to be approved by the CIE Steering Committee. As a selected CIE reviewer, the present report summarizes my findings in accordance with the Statement of Work (SoW), Terms of Reference (ToR), and the report format specified by the CIE.

Description of the reviewer role and review activities:

The reviewer is requested to conduct an independent peer review of the NMFS project reports as specified in the Statement of Work (SOW) and Terms of Reference (ToR). The reviewer must complete the review according to required format and content as described in Annex 1 (given below):

1. The CIE independent report shall be prefaced with an Executive Summary providing a concise summary of the findings and recommendations, and specify whether the science reviewed is the best scientific information available.
2. The main body of the reviewer report shall consist of a Background, Description of the Individual Reviewer’s Role in the Review Activities, Summary of Findings for each ToR in which the weaknesses and strengths are described, and Conclusions and Recommendations in accordance with the ToRs.
3. The reviewer report shall include an Appendix 1 containing a bibliography of materials provided for the review, and an Appendix 2 containing copy of the CIE SoW.

The CIE reviewer must also complete the independent peer review addressing each ToR as described in Annex 2 (given below):

1. Evaluation and recommendations of data quality.
2. Evaluation of strengths and weaknesses of, and recommendations to improve analytic methodologies.
3. Evaluation of and recommendations to improve model assumptions, estimates, and characterization of uncertainty.
4. Determine whether the science reviewed is considered to be the best scientific information available.
5. Recommendations for further improvements.
6. Brief description on panel review proceedings highlighting pertinent discussions, issues, effectiveness, and recommendations.

The CIE reviewer must complete the following chronological list of tasks a timely manner as specified in the SoW, namely;

1. Conduct necessary pre-review preparations, including the review of background material and reports provided by the NMFS Project Contact in advance of the peer review.
2. Conduct an independent peer review in accordance with the ToRs (**Annex 2**).
3. No later than 2 June 2011, each CIE reviewer shall submit an independent peer review report addressed to the "Center for Independent Experts," and sent to Mr. Manoj Shivlani, CIE Lead Coordinator, via email to shivlanim@bellsouth.net, and CIE Regional Coordinator, via email to David Die ddie@rsmas.miami.edu. Each CIE report shall be written using the format and content requirements specified in Annex 1, and address each ToR in **Annex 2**.

It should be noted that prior to accepting the present contract, the present CIE reviewer informed the CIE Lead Coordinator (Mr. Manoj Shivlani) that due to other work obligations, it might not be possible for me to complete the review requested by June 2nd 2011. Subsequently, on May 21st 2011, the CIE Lead Coordinator extended the deadline, and authorized me by e-mail to send the report "over the weekend of the 10th ", i.e., before June 12, 2011.

Summary of Findings:

Document Reviews: Major comments by report category and chronological order

Some issues that raised concern were identified when reviewing the various reports. These include perceived computation problems, information gaps, tenuous or unsupported hypotheses, statements requiring clarifications, and possibly missed comments. The main issues are described below since they form the basis for some of the opinions provided later on.

Background material

A: Historical Evidence... (Hamilton et al., 2005).

Review provides good background information, and historical observations helps determine habitat production potential in the absence of alternatives. No problems to report.

B: Compilation of possible impacts on fish populations and habitat... (Hetrick et al., 2009).

1. P.16: “Earlier snowmelt and peak flows may also cause juveniles to reach the estuary earlier and at a reduced size, thereby decreasing their probability of survival”. A similar statement is made by the authors on P. 99. Such statements should be reconsidered. There are system-specific optimum smolt sizes and time for ocean entry, as shown by Bilton et al. (1982, 1984) and many others since then. The right combination is hypothesized to be partly a function of ocean conditions that are also changing, with recent ocean survival rates much lower for some British Columbia-to-California stocks than during the 1980s, perhaps because the optimal time/size combination is changing. This hypothesis could explain why some stocks/species with relatively small smolt sizes have recently exhibited higher ocean survival rates than historical rates, by contrast to others with larger smolt sizes (see recent Pacific Salmon Commission reports). To determine if this holds true for the Klamath River stocks as well, some experimental releases from Klamath River hatcheries could be done, as other investigators did in the past.

2. P.22: Table 1-2. The spreadsheet numbers show some components of the model, but the caption does not say if these are deterministic calculations or the results of stochastic simulations conducted with specialized third party MS Excel add-in applications (such as Crystal Ball, distributed by Oracle Corp.). Many scientists would prefer modeling impacts using the later approach because the distribution of forecasted trends reflect the combined effects of some key parameters each characterized by known or plausible error distributions. It is difficult to determine if this was done without actually reading reports describing the model, but judging from the figures and equations given in Section IV, it seems that the predicted trends are for specific combinations of key variable values within certain ranges; if so, this implies that they are considered to be equally likely. Some clarifications would help readers determine if additional model modifications are essential or just beneficial.

3. P.58: Figure 11-3: The line showing the predictions fit the data fairly well, but I doubt there is sound justification for using a third degree polynomial. It mainly serves to provide a slightly better fit to the values for Nov. 28, largely influenced by one value for 2001. A simpler model would likely provide a similar fit, be more parsimonious, and easier to interpret.

4. P.100: The authors draw attention to the use of the SALMOD model for assessment purposes, which is a component of the SIAM model (P. 91). The former is structured to account for the effects of certain environmental variables (mostly physical) on fish production, but does not [seem to] include potential species interactions, including predator impacts which could be important if salmon predators re-colonize new habitats faster. This is not a criticism per se since no model is perfect. However, if this issue is considered to be of negligible significance in the present context, the authors should

state so and explain why; but if not, the authors might include some comments as to the need for follow-up analyses using alternative ecosystem models (such as ECOPATH, ECOSYM, etc.), so as to help determine if predator build-ups could potentially delay the salmon stock-rebuilding schedule. The scientific literature on such issues is extensive, but the readers can refer to Walters and Martell (2004) for a summary of relevant issues and investigative methods.

C: Synthesis of the effects of two management scenarios… (Hamilton et al., 2010).

1. Overall, a well-written report covering a wide range of potential impacts (past and future) of pertinent factors (hydrology, limnology, geomorphology, temperature, sediment transport, fauna/flora composition, habitat conditions, pathogens, pollutants, etc.) on the condition of fish populations of the Klamath River and its tributaries. The biotic and abiotic factors examined are directly linked to the construction, operation, and possible removal of the dams in the future.

2. P.16: the authors note that the major focus is on comparing [anticipated] conditions under two management scenarios [with and without dams]. However, the procedures used to physically removing the dams may themselves have a large impact on the conditions of the natural aquatic resources occupying this system, their recovery rate, etc. This issue is noted by the authors in the Executive Summary (P. 6), by stating that "more planning and environmental analysis would be needed to determine the exact methods for dam removal and …" Still, the report would benefit from the inclusion of some details on options considered for the removal of such large structures, what operations would likely be involved (blasting, concrete debris removal, etc.), what these [actions, operations] could potentially cause in terms of additional impacts not identified in the report (if any) or extend the range of conditions considered for numerical simulations.

3. P.21: "Replicating the natural hydrologic regime under which the Fish Species evolved likely represents the best flow regime to conserve and recover Klamath River anadromous fish stocks…" Restoring the system to its past state could be beneficial in many respects, but it is unsure that identical conditions would expedite the recovery process *per se*, i.e., enough to favour the rebuilding of fish populations, especially in light of the anticipated changes in climatic conditions. Additional human interventions and controls might be required, including perhaps, the creation of habitat conditions that are more suitable than historical ones.

4. P.22: "In simulations, we assumed that this 30,000 AF was available". What if the land owners refuse to sell their water rights? The authors might want to run another simulation for comparative purposes with a less optimistic scenario.

5. All sections: The authors should exercise caution is using the catch word 'adaptive management' when qualifying this large scale experiment (dam removal). According to

Walters (1986), the term usually applies to cases involving the deliberate manipulation of natural systems to gain further understanding of their dynamics. These are usually 'repeatable' experiments (disturbances), not single interventions like a once-in-a-lifetime removal of dams that likely won't be rebuilt in the near future simply for hypothesis testing purposes. The term is more appropriately used by Hetrick et al. (2009) when referring to repeatable experiments involving in-season adjustments of flows from reservoirs/dams to determine ideal conditions.

6. P.48: "lower harvest rates…could contribute to the… and re-establishment of populations into areas where they have been extirpated". It is doubtful that you can re-establish extirpated populations that had unique phonotypic and genetic attributes. Best to re-phrase such as 'allow for the progressive re-colonization of areas historically occupied by salmon'.

7. P.49: "Dam removal would result in an increase in habitat and likely re-establish coho salmon above IGD in a short period of time". In this context, short is relative. It is doubtful that anyone can state categorically that it will take 5 years, 10 years, 20 years, or more. Same hold true for the statement in the Executive Summary (P. 8) that "fall-run chinook are expected to recover within 5 years of dam removal". This is wishful thinking at best, and these passages should be re-worded. Equally important is that inferences about stock rebuilding schedules do not appear to account for potential species interactions, and particularly the impacts of fresh water predators of juvenile salmon. The authors comment about possibly modifying hatchery supplementation programs and limiting/reducing harvest rates, but no mention of possibly using predator controls to speed up stock rebuilding (e.g., as currently done in Cultus Lake, BC, Canada, to expedite the recovery of its endangered sockeye salmon stock). At a minimum, even qualitative statements on this issue would make the report more complete.

8. P.109-110: There is increasing support for the notion that ongoing climate changes will have various types of impacts on the North American continent. However, it is difficult to predict the type and magnitude of future changes in the Klamath estuary induced by anticipated changes in climatic conditions. These may simply amount to progressive changes in habitat conditions and supported ecological communities, with repercussions that cannot be forecasted with certainty. Despite the references cited, I would hesitate to make bold statements about future tide levels, [O2] levels, salinity, sea levels, food availability, etc. In fact, dam removal may have major impacts on estuarine conditions, plus compensate for or increase the effects of changing oceanic conditions.

9. P.113: "The positive gains… depend on a number of incalculable elements at a larger ecosystem scale". "Large-scale dam removal is a relatively new concept and there is limited experience and literature to draw upon to predict how the ecosystem will respond". Excellent points about empirical evidence, which should be stressed more strongly in this scientific report. An old colleague (C.S Holling, mentioned on p.114)

would certainly support the restoration of natural variation in environmental conditions to maintain ecosystem resiliency and diversity.

Reference material

D: Klamath river fall Chinook stock-recruit analysis.. (SST, 2005).

1. P.iii: The authors used survival of hatchery releases as a surrogate for those of wild stocks (likely by necessity). Ideally, one should have hatchery/wild CWT releases for comparative purposes, at a minimum to determine if hatchery releases typically exhibit lower/greater survival, ocean distribution and exploitation rates. It would also be desirable to have a map indicating the location of the hatchery production facilities and the location of major impediments to their migration on the way out or on the way back as adults (turbines, spillways, fishways, etc.). The material reads as if two hatchery stocks were used; Trinity and Iron Gate, both in California.

2. P.iii: For correlations between river flow measures and production, the authors used cohort reconstructions methods (fingerling released to age 2 survival). These are outdated methods, and typically used when more age classes are involved. I would rather see the equation used than a simple reference to a cohort reconstruction.

3. P.2-3: All assumptions noted are untenable. They are simply used as justifications used for model simplification purposes. Assumptions about non-stationarity should be investigated given regime shifts. The Central Limit Theorem allows the use of a normal distribution to capture the combined effects of several variables, but requires that such variables be identically distributed, which does not hold in many contexts. Measurement error can be considerable, so are differences between natural and hatchery stock behavior/migration/survival, and using information from stock aggregates to make inferences about the demographic traits of some populations using a given watershed. The authors should at least acknowledge this.

4. P.11: The authors acknowledge the importance of accounting for adult straying (if any), and straying of natural stocks should be equivalent for the S/R relations of hatchery stocks to apply to natural stocks. For some salmon stocks occupying adjacent systems, straying rates of hatchery and wild types can vary considerably, and can be largely influenced by flow conditions (Labelle 1992).

5. P.11: As noted by the authors, it would have been desirable to conduct two S/R analyses given the systems are not close to each other and may be subject to different impacts during their downstream or upstream migrations.

6. P.11: The authors claim that the spawning stock size for maximum recruitment is low for a basin of this size, relative to the habitat based model estimates. The authors should state how the accessible portion of the watershed compares to the range of others used

for assessment purposes. The authors also note that estimation of survival rate of fingerlings to age-2 were conducted via a cohort analysis. It is hard to see how/why a cohort analysis would be done for returns consisting of so few age groups. As stated above, it would be preferable to use an equation such as survival = sum {age 1 escapement and catches (if any) + age 2 escapement and catches (if any)} divided by fingerlings released. It seems that one should account for catches and escapement of Chinooks of ocean age 3+, adjusted for losses of age 2+ fish. The accounting procedure used is somewhat unclear.

7. P.13: This statement is not clear. Watershed size for 5^{th} order stream or higher below impassable barriers is about 17,000 km^2. Note that the Parken model (check) does not include 5^{th} order stream or more for stream type chinook, and on first reading, it is unclear if the impassable barriers noted include dams or only natural barriers above existing dams. And then on P.15 the authors note that the Klamath River watershed actually covers about 34,000 km^2. Which is it?

8. P.16: What are naturally spawning chinook? Those that return to the hatchery + those that stray + those that move past dams via fishways?

9. P.17: The authors rightfully note that “Consideration remains on whether the results from stock-recruitment analysis can reliably predict future production… over the long run”. This is an important point.

10. P.17: What is “natural spawning escapement.” Are these hatchery releases that return back to the hatchery? Also, the authors note that spawning escapements averaged about 97,000 in the 1960s. This could be true, but it does not necessarily mean that the lower value of S_{my} is in error.

E: Habitat-based methods to estimate escapement goals... (Parken et al 2006).

1. This is an innovative investigation with results that can provide guidance in data-poor contexts. It also provides several useful recommendations for follow-up studies. These are identified.

2. The abstract confirms as above that the model overestimates the productive capacity of stocks with relatively small spawning areas as noted in SST (2005). This emphasizes the need for more data from small streams.

3. P.7: The authors note that the life history type factor has a larger effect than geographic region (latitude not mentioned here). However, are both factors correlated? Life history types appear to be strongly linked with broad geographical regions (stream types mostly up north, ocean types mostly south).

4. P.6 The authors note that the effects of error-in-variables should be accounted for in future investigations. An important issue for S/R analyses (Hilborn and Walters, 1992).

5. P.11: It is noted that this model may not be applicable to introduced, non-sustainable stocks. Presumably, this includes hatchery supplemented stocks or those that are or rebuilding naturally or via enhancement after a dam removal.

6. P.20: The authors recommend that the reliability of the habitat-based model should be assessed against new stocks as additional stock-recruitment data becomes available.

E: Using accessible watershed size to predict management parameters... (Liermann et al., 2010)

1. This paper is basically an extension of the one cited above, and uses watershed size as a proxy of productivity or the unfished equilibrium population size (E) and that at the maximum sustainable yield level (S_{msy}). However, the authors use a well established integrated modeling approach to simultaneously account for uncertainty in parameter estimates and additional data sources, and assess the suitability of alternative model structures. Some issues that may merit further attention are given below.

2. P.41: The authors use log-log plots to establish a linear relation of S_{msy} or E versus watershed size. Not ideal. Often the use of a non-linear relation is preferable.

3. P.46: The spawner-recruit figures used to estimate the S/R parameters for the Skagit River (Fig. 3, upper right) are too clustered to get reliable estimates for the Ricker model. Basically, the range in spawner levels is too narrow (as recognized by the authors on P.47). Furthermore, for both cases illustrated, it is doubtful that historical figures (catch & recruitment data since the 1970's) are all reliable. In fact, the spawner-recruit data for these two stocks, and perhaps some of the other 23 listed in Table 1 (p. 3) may be subject to such uncertainty that the S/R parameter estimates are likely biased to some degree because of the well-known 'error in variable' effects (Hilborn and Walters, p.234, 288). Those biases tend to make stocks look more productive than they actually are (estimate of slope at low stock size is steeper than it could be in relatity). The authors note that spawner-observation error is not accounted for by their model, but cite a 2008 PSC report on P. 49 that indicate this problem 'was unlikely to introduce large biases that would be of concern to managers'. This may be true for some populations used in their analysis (10 of them), but I doubt this is the case for all. Also, the so-called management needs referred to do not necessarily match the intended use of the results of this investigation (i.e., the potential productivity of rebuilding populations after dam removals). In light of this observation, and in the present context, it might be desirable to re-evaluate the relation between watershed areas and the productivity parameters using only the most reliable updated data series that also show a large range of spawning levels. This could also include other populations not used so far but that are more representative of those in the region of interest (as note by the authors on P. 49).

4. The algebraic steps used to re-parameterize the basic models should be given, and those used to formulate alternative ones are lacking. Also, it is difficult to relate the values of centred log-transformed parameters to those of non-transformed values reported in the literature. In some cases, there is little or no justification for choices of non-uniform priors (authors often simply cite the 'textbook' approach from Gelman et al.).

5. P.20: The authors recommend that the reliability of the habitat-based model should be assessed against new stocks as additional stock-recruitment data becomes available. This is an important point, particularly in the present context, given that there is time to do so before the start of the short- and long-terms potential impacts that may occur during the next 50 years or so.

Model Reports

F: Using model selection and model averaging to predict … (Lindley and Davis 2011).

1. While the authors used established statistical methods to assess the influence of some habitat variables on escapements levels to various systems, this 'draft report' would likely not be approved in its present form by a major scientific journal. It currently reads as a summary of work-in-progress, in part because of uncertainties and perceived shortcomings of the data sets used, assumptions lacking support, procedures lacking details and justification, and model limitations. The major deficiencies are as follows.

2. P.5: What do the escapement numbers represent? Those that would have been obtained in the absence of catches in approach waters? Why average [or median] abundances? Spawners returning in some years might have experienced unusually low survival since early life, so these should not [ideally] be used. Some investigators argue best to use only peak escapements over a given period as an indicator of the actual habitat capacity observed when environmental conditions are stable and fishing losses are minimal. Obviously, additional justification is required.

3. P.7: The structure of Eq. 1 is somewhat unconventional. Log-Linear models are often expressed differently (see Hilborn and Walters 1992, p.127 for textbook examples). Proper mathematical notation would usually identify vectors in bold (here habitat quantities and qualities). The authors should also distinguish (in a table if need be) the variables that are continuous and categorical (if any).

4. P.7-8: "To reduce the number of variables…. we dropped mean January temperature and maximum elevation, as these were strongly correlated with August air temperature and mean elevation…" It is common practice to eliminate one or each pair of variables that are substantially positively or negatively correlated before estimating statistical influence of the remaining variables and determine the final model structure (often the most parsimonious of several with similar fits). The authors should specify how variable

filtering was done, what criteria were used to determine the rejection threshold correlation levels, and the justification for removing each one of the correlated variables.

5. The authors seemingly allow for only linear effects in the model structures evaluated. The spatio-temporal patterns of fish abundance are often non-linear functions of some variables; salmon often exhibit ‘preferences’ for habitats with certain temperatures, with spawning activity or survival greater under some conditions than others. To account for this, investigators can include polynomial functions (such as quadratics) in log-linear fisheries models (see Maunder and Punt 2004 examples). The authors should test for these as well when evaluating the best model structure(s).

6. P.9: “We therefore present results of models based on spring-run…”. While the authors describe some results, they omit typical diagnostic features used to determine if the model fits the data well. At a minimum, these generally include plots of predicted vs. observed values or trends, residual scatter trends, etc.

7. P.11-12: The authors use Eq. 3 to correct for an ‘inferred harvest rate” to eventually compare their estimates to those of Liermann et al. (2010), and then note that this cannot account for all the difference in results between the two models. There is no reason to consider this overly simplistic correction factor is as scientifically credible method to adjust results, with exploitation rates and survival rates possibly very different than when William Ricker used this adjustment in another context. Consequently, the differences are not considered to be surprising, and the question remains as to which model (this one or Liermann et al.) is more suitable in the present context.

8. P.19-26: All tables and figure captions lack details on what the column heading refers to.

G: Forecasting the response of Klamath Basin Chinook populations... (Hendrix 2011).

1. A very sophisticated, integrated Bayesian model partly built upon the results of other models reported above (SST 2005, Liermann et al., 2010). It is used to determine plausible recovery patterns prior to and after dam removal, based on a [very] large number of assumptions [and priors] that cannot all be evaluated in-depth given review time constraints, uncertainties about future conditions, and the availability of replicate samples in past studies. Such facts, coupled with the relatively long forecasting period (2012-2061), make it difficult (if not impossible) to determine on first reading how reliable the forecasts are. The inclusion of ‘pseudo-code’ in an Appendix greatly helps the reader to visualize how the components are linked, and the author should be commended for doing so, as it is not commonly done by other investigators describing simulation models. Additional comments are:

2. P.5: Eq. 5. Why not use commonly used terms like L to define likelihoods?

3. The results of Leirmann et al. (2010) are relied upon, but this means that errors or shortcomings of their analysis or results could [possibly] have negative repercussions on the reliability of the forecasts obtained with this model. This should be noted.

4. P.11: The author makes assumptions about the re-introduction of chinook in some tributaries past 2019 from hatchery fry outplants. The success of such activities (if any) is highly uncertain, and may even be detrimental (e.g. disease transfer/propagation). Later on (P.15), the author assumes plausible ocean harvest rates authorized by the regional PFMC in the future, including the potential implementation of F-based control rules (P.17). Granted, future socio-economic benefits should be taken into account when making decisions about projects of this size, but there is so much uncertainty about such future conditions of this system that some reviewers might wonder if such long term forecasts are scientifically defensible at this point in time. At a minimum, the author should acknowledge this [somehow/somewhere].

5. P. 18-19: The author focuses attention on (i) the use of models based on Bayesian or Frequentist approaches, and (ii) comparing the results of various models used to predict impacts in the Klamath River that are not based on the same set of assumptions. Issue (i) may never be resolved to the likes of all academics. However, issue (ii) can be addressed by revising the assumptions periodically based on empirical investigations and updating the assumptions of various models as needed. In fact, Walters and Martell (2004) recommend trying alternative modeling approaches followed by verification/comparison/experimentation to explain differences between the model predictions, and identify the most appropriate model.

Conclusions and Recommendations:

Considerable efforts were made to compile information from multiple sources on the past and current conditions of the habitat and supported aquatic populations. Pertinent data sets were then used with information on the expected conditions of the habitat after dam removal to make predictions on the future state of the ecosystem with or without dams. The results of the various forecasts made are encouraging in that they all support the notion that the dynamics of this aquatic ecosystem would substantially improve with the removal of the four dams in the lower reaches. Given the importance and magnitude of the project contemplated (dam removal), there is a genuine need to make sure that the assumptions, data and methods used for simulation and prediction purposes are sound. Even when sophisticated methods are applied, there is always room for improvement and a need to verify the accuracy of predictions (or forecasts) periodically to make adjustments (if need be) to the assessment methods used. Miscellaneous suggestions are given above on possible ways of improving the reports and assessments results presented. What follows are general opinions that stem from my review, for each of the 6 issues listed in the Terms of Reference (ToR).

ToR 1: Evaluation and recommendations of data quality.

Considerable efforts were obviously made to compile information from multiple sources on the past and current conditions of the Klamath River system, the supported aquatic populations, and the habitat conditions throughout the catchment area. Background information was then used in conjunction with data on the anticipated conditions of the system after dam removal, so as to make predictions about the future state of this system with or without dams. Overall, this amounts to a broad and extensive data set, that is considered to be of very good quality, and sufficient for modelling and forecasting purposes. As noted earlier, it could have been helpful to have more information on the actual dam removal procedures being considered to also account for the potential impacts of such activities.

ToR 2: Evaluation of strengths and weaknesses of, and recommendations to improve analytic methodologies.

In general, the analytic procedures used appear to be well established and scientifically sound, as evidence by the fact that may were published in scientific journals, or rely on methods that have been. In terms of improvements, it should be noted that the large report published by Hetrick et al. (2009) contains numerous passages indicating that numerical simulations were conducted to assess the effects of expected changes on habitat conditions. The simulations referred to were presumably conducted by limnologists, hydrologist, engineers and etc., but no details are given on the simulation models used for such purposes (only references). Perhaps this was considered necessary because too many types were used or because they are too complex to be easily described in the reports provided. In the future, if such simulation models are not identified, it would be desirable to at least provide statements that indicate well established scientific or engineering methods were relied upon.

Some of the analytic procedures reported by Hetrick et al., (2009) consist of MS Excel spreadsheet models that seemingly serve to make deterministic calculations for given combinations of parameter values within certain ranges. As noted above, some scientists would prefer conducting stochastic calculations instead, so that the distribution of the forecasted trends reflect the combined effects of key parameters each characterized by known or plausible error distributions.

ToR 3: Evaluation of and recommendations to improve model assumptions, estimates, and characterization of uncertainty.

In the absence of time series on salmon spawning levels and corresponding recruitment levels, several models were used to make inferences about the potential productivity of these populations using simple habitat attributes (watershed area), demographic traits (life type), and other factors. The biological models relied upon in the present context range from simplistic ones using little data and few assumptions, to sophisticated hierarchical, integrated, Bayesian models (like those of Liermann 2010, Hendrix 2011) that attempt to make best use of multiple data sets, prior information and hypothesized states.

The most simple biological models reviewed (SST 2005, Parken et al. 2006) are overly simplistic for impact assessment purposes. The model of Lindley and Davis (2011) has several shortcomings that need to be addressed before it is relied upon to predict equilibrium capacities, and future developments should help identify other major habitat determinants of productive capacity. The hierarchical, integrated, Bayesian statistical models are the best suited for making predictions about the future states of populations under certain environmental conditions. They typically include several sub-models (or functions) that can differ in complexity, and most rely on a multiple assumptions to characterize, limit and keep track of the uncertainty levels associated with the variables of interest. Such models are increasingly being relied upon for fisheries stock assessment and population modelling (see Maunder 2004, Maunder et al. 2006 for examples). On this basis, using the more sophisticated models of Liermann (2010) and Hendrix (2011) would seem preferable for impact assessment and forecasting purposes.

Hendrix's model is considerably more complex and relies on many more assumptions than that of Liermann et al. The Hendrix model uses some of the results of the Liermann's et al. model, which is itself a derivation of the more simple models. Consequently, it takes into account accumulated knowledge, and can be used to forecast population states for 50+ years. On this basis, the Hendrix model seems as the best of those reviewed for impact assessment purposes in the present context, even if the draft report describing its features has not yet been subject to a second [and more] intensive peer review as would be conducted by a major scientific journal.

It is a well known fact that model complexity is not synonymous with model performance (Wilborn and Walters 1992), so despite the level of sophistication, the Hendrix model does not necessarily yield more reliable estimates than simpler alternatives. Another major issue highlighted earlier is that the model includes several tenuous assumptions about the future, including the success of future fry outplants, ocean survival rates, allowable harvests on adult returns, and so forth. In general, scientists are thought to use the most parsimonious model sufficient for the task at hand. On this basis, and in light of the above comments, one might consider simplifying the Hendrix model to initially focus forecasts on say the first 10 years, eliminate or reduce the number of hypotheses for which there is no support currently (such as ocean conditions and allowable harvests given hypothesized future productivities (in 12+ years). The model can be updated in due time given the specifics of additional plans (as noted by Hendrix 2011, p.19 re[:]? "how anadromy would be restored"), additional information on habitat conditions after initial impacts, updates and refinements of the various sub-models used, and additional empirical observations to support the various model hypotheses.

ToR 4: Determine whether the science reviewed is considered to be the best scientific information available.

Based on a review of the reports provided, a large number of in-depth investigations were obviously conducted to gather a considerable amount of information on a wide range of topics covering historical, current and potential future states of the Klamath River system and supported communities. Some of these data have been subject to repeated analyses using several methods commonly used in science and engineering, including some of the latest methods used for forecasting purposes. On this basis, I cannot confirm that the science reviewed is "the best scientific information available", but I would not hesitate to state that it appears to be to a large extent "some of the best scientific information available". That qualifier is based on the fact that there is always room for improvement, but one has to bear in mind that there is rarely (if ever) unlimited resources to address every conceivable issue and all unavoidable uncertainties about the future.

ToR 5: Recommendations for further improvements.

Model deficiencies are often due to data deficiencies (Hilborn and Walters 1992). While we clearly have considerable data on past conditions, these are by no means representative of future conditions. So one major recommendation for further improvement would be to ensure that sufficient resources are used to properly plan and execute the stock rebuilding operations for the period following dam removal. Pacific salmon (and other species) do have the ability to naturally recolonize new [or marginal] habitats (in part via 'straying'), but this does not always occur rapidly, in part because salmon tend to home back to their stream of origin. In the present context, there may be a considerable amount of new habitat planners that would hope to re-seed as rapidly as possible to expedite the recovery process. This could (or will likely) involve extensive stocking of hatchery-reared progeny from suitable donor stocks. Ideally, these fish should all be subject to coded-wire tagging to determine their subsequent colonization success, survival rates, migration and exploitation patterns, and homing/straying rates scientifically sound juvenile production and escapement monitoring programs that can be expensive and labour intensive. There should be adequate controls, wild/hatchery comparisons, replicated experiments for comparative purposes, and the use of proven methodologies for outplanting and monitoring purposes (volational releases of outplants to allow for sufficient imprinting, PIT tagging, acoustic tracking, etc.). The empirical results should be used to periodically verify/adjust the biological model parameters and the underlying hypotheses.

ToR 6: Brief description on panel review proceedings highlighting pertinent discussions, issues, effectiveness, and recommendations.

The documents outlining the KBRA and KHSA agreements amount to a considerable amount of information that, to a large extent, concerns legal rights and obligations (contractual or otherwise). Much of this exceeds the scope of expertise of the present reviewer, so only a cursory review of these two documents was conducted. Furthermore, and for the same reasons, no attempt was made at finding and evaluating information or reports on issues relating to the weighting of ecological, social and economic benefits to determine if dam removal is in the best interest of the community at large were not evaluated. Only the document entitled "Summary of the Klamath Basin Agreements" was reviewed in detail, and the pertinent comments that relate to the review results presented above are as follows:

P.2-3: Phase I plan. The initial Fisheries Restoration and Fisheries Reintroduction plans focus on the first 10 years and "near-term investigations", which supports my view that the biological model forecasts made should first focus on the initial part of the 50 year period.

P.3: Screening Program. "The reintroduction program will prevent reintroduced salmon and other aquatic species from entering irrigation diversions". It is a well-known fact that coho salmon, in particular, make extensive use of beaver ponds, slews, and holding water, and side channels for holding/rearing purposes, even during dry spells. In fact, when floods were common (in the Lower Fraser River and Columbia River valleys for instance), many juvenile salmon species (including coho) used these, and were commonly found on agricultural grounds when waters receded. Not being able to escape was, obviously, a mortality factor, but likely provided substantial benefits to juveniles that did escape. In fact, many biologists would argue that the availability of these shallow and even temporary holding areas was essential to maximize the productivity of some salmon stocks. Consequently, some areas adjacent to the main Klamath River channels and tributaries that historically were irrigated naturally via floods (not via diversions), may consist of key habitats required for the successful re-establishment of some salmon stocks. The main point is: efforts should be made to ensure that the land classified as suitable for ‘artificial irrigation’ via diversions should not include areas historically subject to flooding. This issue is possibly addressed [implicitly or explicitly] in on P.3 under the heading “Additional Water for Fish”, and in other sections that follow (as in P.10).

Apart from the relatively small issues highlighted above, there do not appear to be obvious or major gaps, omissions or errors in the panel review proceedings.

Literature Cited

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Maunder, M. 2004. Population viability analysis based on combining Bayesian, integrated and hierarchical analyses. Acta Eocologica. 26 (2004) 85-94.

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Parken, C. K., R. E. McNicol, and J. R. Irvine. 2006. Habitat-based methods to estimate escapement goals for data limited Chinook salmon stocks in British Columbia, 2004. Research Document 2006/083, Fisheries and Oceans Canada. URL http://www.dfo-mpo.gc.ca/csas/.

STT (Salmon Technical Team). 2005. Klamath River fall Chinook stock-recruitment analysis. Prepared by Salmon Technical Team, Pacific Fishery Management Council.

Walters, C.J. 1986. Adaptive management of natural resources. Macmillan Publ. Co. New York, N.Y. 374 p.

Walters, C.J., and S.J.D. Martell. 2004. Fisheries ecology and management. Princeton University Press. Princeton, New Jersey, USA. 399 p.

Appendix 1

Copies of main scientific reports provided by the CIE for this review (As noted in the previous Literature Cited section)

Reference material: Liermann et al. (2010), Parken et al. (2006) and SST (2005).

Klamath background material: Hamilton et al. (2005, 2010) and Hetrick et al. (2009).

Model Reports: Hendrix, N. (2011), and Lindley and Davis (2011).

Copies of Klamath Settlement Agreements also provided

Summary of the Klamath Basin Settlement Agreements. 2010.

Klamath Basin Restoration Agreement for the Sustainability of Public and Trust Resources and affected Communities (KBRA). February 18, 2010.

Klamath Hydroelectric Settlement Agreement (KHSA). February 18, 2010.

Copies of additional memorandums concerning document reviews

Hefner (2010). Expert Review of the Document entitled: Compilation of Information to inform USFWS Principals on the Potential Effects of the Proposed Klamath Basin Restoration Agreement (Draft 11) on fish and Fish Habitat Conditions in the Klamath Basin, with Emphasis on Fall Chinook Salmon. PBS&J Memorandum addressed to J. Hamilton, USFWS. Nov. 15, 2010.

Appendix 2

Statement of Work for Dr. Marc Labelle
External Independent Peer Review by the Center for Independent Experts
Klamath River Fall Chinook salmon production model and final report
(May 2011)

Scope of Work and CIE Process: The National Marine Fisheries Service's (NMFS) Office of Science and Technology coordinates and manages a contract providing external expertise through the Center for Independent Experts (CIE) to conduct independent peer reviews of NMFS scientific projects. The Statement of Work (SoW) described herein was established by the NMFS Project Contact and Contracting Officer's Technical Representative (COTR), and reviewed by CIE for compliance with their policy for providing independent expertise that can provide impartial and independent peer review without conflicts of interest. CIE reviewers are selected by the CIE Steering Committee and CIE Coordination Team to conduct the independent peer review of NMFS science in compliance the predetermined Terms of Reference (ToRs) of the peer review. Each CIE reviewer is contracted to deliver an independent peer review report to be approved by the CIE Steering Committee and the report is to be formatted with content requirements as specified in **Annex 1**. This SoW describes the work tasks and deliverables of the CIE reviewer for conducting an independent peer review of the following NMFS project. Further information on the CIE process can be obtained from www.ciereviews.org.

Project Description: The United States, the States of California and Oregon, the Klamath, Karuk, and Yurok Tribes, Klamath Project Water Users, and other Klamath River Basin stakeholders negotiated the Klamath Basin Restoration Agreement (KBRA) and the Klamath Hydroelectric Settlement Agreement (KHSA), thereby proposing the largest dam removal restoration action in US history. In 2012 it is anticipated that a determination will be made by the Secretary of the Interior, in consultation with the Secretary of Commerce regarding removal of four hydroelectric dams on the Klamath. A benefit-cost (BC) analysis is needed to inform this determination. The BC analysis will compare two alternatives: (1) dam removal and implementation of the KBRA; and (2) current conditions projected into the future. To inform the BC analysis and environmental compliance documents, two Klamath River Chinook fish production models (Option A and B) has been developed. Option A is capable of providing annual forecasts of stage specific abundances under the two alternatives over a 50 year time period. A written technical report will be completed and available for the CIE review on 16 May 2011 including: the assumptions incorporated into the fish production model, mathematical equations used to define reproduction, growth, and mortality for all phases of the fish production model, and definition of model coefficients described based on how they were derived. This model and report will inform a landmark federal action with a recent litigious history. The results of this model have large potential implications on the economy of California and Oregon, commercial, tribal and recreational fisheries in California and Oregon, and tribal and public trust resources. The Terms of Reference (ToRs) of the peer review are attached in **Annex 2**.

Requirements for CIE Reviewers: Three CIE reviewers shall conduct an impartial and independent peer review in accordance with the SoW and ToRs herein. CIE reviewers shall possess a combination of expertise with working knowledge and recent experience in the application of fish production modeling, Bayesian methodologies, hydrology, climatology, river restoration, and Pacific salmon life history. Each

CIE reviewer's duties shall not exceed a maximum of 10 days to complete all work tasks of the peer review described herein.

Location of Peer Review: Each CIE reviewer shall conduct an independent peer review as a desk review, therefore no travel is required.

Statement of Tasks: Each CIE reviewers shall complete the following tasks in accordance with the SoW and Schedule of Milestones and Deliverables herein.

Prior to the Peer Review: Upon completion of the CIE reviewer selection by the CIE Steering Committee, the CIE shall provide the CIE reviewer information (full name, title, affiliation, country, address, email) to the COTR, who forwards this information to the NMFS Project Contact no later the date specified in the Schedule of Milestones and Deliverables. The CIE is responsible for providing the SoW and ToRs to the CIE reviewers. The NMFS Project Contact is responsible for providing the CIE reviewers with the background documents, reports, and other pertinent information. Any changes to the SoW or ToRs must be made through the COTR prior to the commencement of the peer review.

Pre-review Background Documents: Two weeks before the peer review, the NMFS Project Contact will send (by electronic mail or make available at an FTP site) to the CIE reviewers the necessary background information and reports for the peer review. In the case where the documents need to be mailed, the NMFS Project Contact will consult with the CIE Lead Coordinator on where to send documents. CIE reviewers are responsible only for the pre-review documents that are delivered to the reviewer in accordance to the SoW scheduled deadlines specified herein. The CIE reviewers shall read all documents in preparation for the peer review.

Desk Review: Each CIE reviewer shall conduct the independent peer review in accordance with the SoW and ToRs, and shall not serve in any other role unless specified herein. **Modifications to the SoW and ToRs must not be made during the peer review, and any SoW or ToRs modifications prior to the peer review shall be approved by the COTR and CIE Lead Coordinator.** The CIE Lead Coordinator can contact the Project Contact to confirm any peer review arrangements.

Contract Deliverables - Independent CIE Peer Review Reports: Each CIE reviewer shall complete an independent peer review report in accordance with the SoW. Each CIE reviewer shall complete the independent peer review according to required format and content as described in Annex 1. Each CIE reviewer shall complete the independent peer review addressing each ToR as described in Annex 2.

Specific Tasks for CIE Reviewers: The following chronological list of tasks shall be completed by each CIE reviewer in a timely manner as specified in the **Schedule of Milestones and Deliverables**.

Conduct necessary pre-review preparations, including the review of background material and reports provided by the NMFS Project Contact in advance of the peer review. Conduct an independent peer review in accordance with the ToRs (**Annex 2**). No later than 2 June 2011, each CIE reviewer shall submit an independent peer review report addressed to the "Center for Independent Experts," and sent to Mr. Manoj Shivlani, CIE Lead Coordinator, via email to shivlanim@bellsouth.net, and CIE Regional Coordinator, via email to David Die ddie@rsmas.miami.edu. Each CIE report shall be written using the format and content requirements specified in Annex 1, and address each ToR in **Annex 2**.

Schedule of Milestones and Deliverables: CIE shall complete the tasks and deliverables described in this SoW in accordance with the following schedule.

9 May 2011	CIE sends reviewer contact information to the COTR, who then sends this to the NMFS Project Contact
16 May 2011	NMFS Project Contact sends the CIE Reviewers the report and background documents
16-30 May 2011	Each reviewer conducts an independent peer review as a desk review
2 June 2011	CIE reviewers submit draft CIE independent peer review reports to the CIE Lead Coordinator and CIE Regional Coordinator
16 June 2011	CIE submits the CIE independent peer review reports to the COTR
20 June 2011	The COTR distributes the final CIE reports to the NMFS Project Contact and regional Center Director

Modifications to the Statement of Work: Requests to modify this SoW must be approved by the Contracting Officer at least 15 working days prior to making any permanent substitutions. The Contracting Officer will notify the COTR within 10 working days after receipt of all required information of the decision on substitutions. The COTR can approve changes to the milestone dates, list of pre-review documents, and ToRs within the SoW as long as the role and ability of the CIE reviewers to complete the deliverable in accordance with the SoW is not adversely impacted. The SoW and ToRs shall not be changed once the peer review has begun.

Acceptance of Deliverables: Upon review and acceptance of the CIE independent peer review reports by the CIE Lead Coordinator, Regional Coordinator, and Steering Committee, these reports shall be sent to the COTR for final approval as contract deliverables based on compliance with the SoW and ToRs. As specified in the Schedule of Milestones and Deliverables, the CIE shall send via e-mail the contract deliverables (CIE independent peer review reports) to the COTR (William Michaels, via William.Michaels@noaa.gov).

Applicable Performance Standards: The contract is successfully completed when the COTR provides final approval of the contract deliverables. The acceptance of the contract deliverables shall be based on three performance standards:
(1) each CIE report shall completed with the format and content in accordance with **Annex 1**,
(2) each CIE report shall address each ToR as specified in **Annex 2**,
(3) the CIE reports shall be delivered in a timely manner as specified in the schedule of milestones and deliverables.

Distribution of Approved Deliverables: Upon acceptance by the COTR, the CIE Lead Coordinator shall send via e-mail the final CIE reports in *.PDF format to the COTR. The COTR will distribute the CIE reports to the NMFS Project Contact and Center Director.

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Annex 1: Format and Contents of CIE Independent Peer Review Report

Klamath River Fall Chinook salmon production model and final report

1. The CIE independent report shall be prefaced with an Executive Summary providing a concise summary of the findings and recommendations, and specify whether the science reviewed is the best scientific information available.
2. The main body of the reviewer report shall consist of a Background, Description of the Individual Reviewer's Role in the Review Activities, Summary of Findings for each ToR in which the weaknesses and strengths are described, and Conclusions and Recommendations in accordance with the ToRs.
3. The reviewer report shall include the following appendices:
   Appendix 1: Bibliography of materials provided for review
   Appendix 2: A copy of the CIE Statement of Work

Annex 2: Tentative terms of Reference for the Peer Review

Klamath River Fall Chinook salmon production model and final report

1. Evaluation and recommendations of data quality.
2. Evaluation of strengths and weaknesses of, and recommendations to improve analytic methodologies.
3. Evaluation of and recommendations to improve model assumptions, estimates, and characterization of uncertainty.
4. Determine whether the science reviewed is considered to be the best scientific information available.
5. Recommendations for further improvements.
6. Brief description on panel review proceedings highlighting pertinent discussions, issues, effectiveness, and recommendations.