Klamath River Chinook Salmon Production Models
Summary of Stakeholder Review and Compilation of Comments Received

## Introduction

Under the Klamath Hydroelectric Settlement Agreement (KHSA) the Secretary of the Interior is required to make a determination (Secretarial Determination) by March 31, 2012 as to whether 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.

A complex Fall Chinook Salmon Life Cycle Production Model is under development and was originally intended to be used to help inform the Secretarial Determination. However, in January of 2011it was determined that the model would likely not be completed in time to meet the aggressive schedule that was established to meet the timeframes required under the KHSA. As a result of the action, the modeling team shifted their efforts towards development of alternative population models that were less complex in nature yet could still meet the needs of the Secretarial Determination process. This resulted in the development of two alternative Chinook salmon population models, one based on stock-recruitment relationships specific to the Klamath River basin and the other based on a log-linear modeling approach using geographic based parameters. Throughout the development of these two models the modeling team provided numerous opportunities for stakeholder interaction and review. The purpose of this document is to summarize the stakeholder participation, assemble comments that were received, and provide a written response to those comments.

The decision to move forward with development of the two alternative models was shared with the Technical Coordinating Committee (TCC), comprised of the signatories to the KHSA, during their meeting held on February 23, 2011, in Klamath Falls. Following this announcement subsequent opportunities were provided to various stakeholders for direct interaction with the model team on April 15$^{th}$, April 25$^{th}$, April 27$^{th}$, and June 15$^{th}$ of 2011. Comments that were received by the model team along with the responses that have been developed are provided in the following table.

**Responses to comments received on the Chinook Salmon Production Model Draft Reports for the Klamath River: "Forecasting the response of Klamath Basin Chinook populations to dam removal and restoration of anadromy versus no action" by N. Hendrix; and, "Using model selection and model averaging to predict the response of Chinook salmon to dam removal" by S. T. Lindley and H. Davis.**

| Number | Date | Comment Author | Affiliation | Model, by Author | Comment | Response |
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| 1 | 4/13/2011 | Kevin Malone | PacifiCorps | Hendrix | PacifiCorp requests that all assumptions for the proposed single stock model be clearly documented and provided to PacifiCorp when available. | List of assumptions added to methods section. |
| 2 | 4/13/2011 | Kevin Malone | PacifiCorps | Hendrix: Lindley and Davis | Use of Parken/Leirmann to Model Upper Klamath River Basin Streams: PacifiCorp has concerns regarding the use of the Parken/Leirmann methodology for modeling Chinook fish production upstream of Keno Dam (i.e., Upper Klamath River). The primary concern is that the Upper Klamath River has habitat conditions substantially different than the habitat conditions in the streams used to develop the Parken/Leirmann model. The model is therefore being applied to an area with habitat conditions that were not considered in developing the model. The lack of similarity between habitat conditions in the Upper Klamath River and the basins relied upon to develop the Parken/Leirmann model may lead to modeling results may not represent the fish production capability of the Upper Klamath River for the reasons presented below. As Parken notes: “Predictions should be carefully examined before implementation because the model may poorly represent other natural or anthropogenic habitat conditions, thus predictive errors will increase as stocks depart from average habitat conditions or depart from average productivities of the modeled stocks.” The Upper Klamath basin differs from the basins used to develop the Parken/Leirmann model in regards to:<br>a. Latitude- The Klamath basin is located further south than the streams modeled.<br>b. Elevation- Upper basin stream habitat is at elevations greater than 4,000 ft; compared to the modeled ocean type streams which were low elevation coastal streams (< 1,000 ft).<br>c. Water Temperature regime- Stream temperatures vary dramatically from stream to stream in the upper Klamath River. In some streams, such as the Sprague River, water temperatures are currently such that neither stream-type nor ocean-type life histories will likely be as productive as streams used in model development. In other streams, such as the Williamson, productivity may be much higher.<br>d. Flow regime (i.e. water yield)- Water yield is likely substantially lower for the upper Klamath River than for coastal streams such as the Skagit, Chehalis and Harrison Rivers used for modeling ocean type life history. As Parken states: “The habitat model may overestimate Smsy and Srep in watersheds with lower | While this concern may be true of Parken (2006) the Liermann et al. (2010) effort uses a broader range of watersheds that encompass a broader range of watershed sizes and physical conditions. In addition, the Liermann et al. (2010) approach uses a method that explicitly incorporates the uncertainty in prediction. |

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| 3 | 4/13/2011 | Kevin Malone | PacifiCorps | Hendrix: Lindley and Davis | Lack of Productivity Effect Lack of Productivity Effect<br>In the proposed model, it appears that habitat quality has little, if any effect on population productivity. This seems counterintuitive and brings into question the model's usefulness outside of the area modeled originally by Parker/Leirmann. If habitat quality has no effect on fish production does that mean the estimates produced by the model represent historical (1800's) conditions as well as current conditions? If it doesn't, is that evidence that habitat quality needs to be accounted for in the modeling? Or, does that mean that streams where the model is applied must be similar in capacity and productivity as the streams used in model development?<br><br>We also note that spawning escapement data for the Upper Columbia River spring run fish after 1969 was removed from the model as the authors concluded that hydro development had reduced population productivity. This seems to be an indication that the authors determined that productivity is influenced by habitat quality and must be accounted for when applying the model. Note that the authors could just as well have used the 1969-2000 data to develop the model, which would have been more reflective of the time period data for other streams were collected. It appears that the authors of the model made a conscious choice to exclude a stream based on a lower productivity value; indicating that the model is not reflective of all habitat conditions present in the Pacific Northwest region, much less the complex habitat conditions of the Upper Klamath basin. | |
| 4 | 4/13/2011 | Kevin Malone | PacifiCorps | Hendrix: Lindley and Davis | Modeling KBRA Actions: The proposed modeling approach suggests that KBRA actions will improve population productivity - the question is from what level? Is population productivity assumed to be improved above the "average" level calculated using Leirmann, et al (2010)? Either habitat quality (e.g. as affected by KBRA) is irrelevant as the Leirmann approach assumes (i.e. habitat quality is measured by watershed size) or it affects productivity. If the latter is true, does the proposed model assume that the Klamath population will perform better (have higher productivity) than the average Northwest/British Columbia Chinook population as the KBRA is implemented? Is the Klamath River expected to have better productivity than the Harrison River in British Columbia? Might it not be equally, or more, likely that KBRA measures, if fully implemented, may bring Klamath population productivity closer to the average productivity performance of salmon-bearing rivers? | Implementation of KBRA in the EDRRA model assumes that the conditions in the Klamath River will improve over the 50 year time period of the model. This process was modeled by removing the chance for low productivity in later years of the time series. The EDRRA model does not assume that the Klamath will be better than the Harrison, which is better than average. Instead it was assumed that production from the Klamath River at the beginning of the time series could range from the worst to the best. In future years, the likelihood that the Klamath would act like the worst rivers diminishes such that by the end of the 50 year time period, the Klamath is expected to have production better than the lower 25%. |

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| 5 | 4/13/2011 | Kevin Malone | PacifiCorps | Hendrix: Lindley and Davis | Influence of Harrison River on Model Outcomes: Parken (2006) notes that the Harrison River (a large watershed- 7,611 km 2) had a large influence on model results and when this river was omitted from the model there was a large predictive error. The author noted that the model could be improved with the addition of more watersheds of sizes greater than 4,500 km2. Model predictions for a large watershed are therefore heavily influenced by the data for the Harrison River. This is important as the Williamson River has a watershed size of over 7,800 km2. If this system is modeled as a single population inhabiting all system tributaries then results should be similar to those for the Harrison River. If tributaries are modeled separately then another problem may occur (see below). The Harrison River is one of, if not the best, Chinook producing stream in all of Canada. It is considered the crown jewel of British Columbian salmon resources and was just named a salmon stronghold in Canada1. The "White Chinook" of the Harrison River spawn in approximately 16.5 km of river below Harrison Lake. The Harrison (and Frazer) River fall-run stock group exhibits an ocean-type life history, but is unusual in that upon emergence from the gravel the fry migrate immediately to the estuary where they rear for three to six weeks before moving offshore (instead of staying 60 to 150 days in freshwater as is typical of most stocks with an ocean-type life history2.) The Harrison River is a low elevation (< 300 ft) coastal stream of British Columbia with excellent water quality. This is in contrast to the Upper Klamath, which is at a high elevation of 4,000 ft, has high stream temperatures, poor water quality (in a large portion of the basin, but not all) and where juveniles must migrate over 240 miles to the ocean and through a large lake which is impassable in portions of the year, and through Lake Ewauna, which has very poor water quality (dissolved oxygen) during critical times of the year. | There are additional large rivers added to the Liermann et al. (2010) effort that attempt to address this limitation relative tothe uniqueness of the Harrison River. The size of the Klamath River in the upper basin was assumed to be 5200 km2 and for the lower basin 1790 km2. |
| 6 | 4/13/2011 | Kevin Malone | PacifiCorps | Hendrix: Lindley and Davis | Identifying and Defining Stock Units: Parken notes: " …that the accuracy of the model depends on accurate identification of stock units, since the sum of Smsy predictions for watershed components will exceed the prediction for the entire watershed."<br><br>Each stock used to develop the habitat model consisted of a single stock unit corresponding to a group of fish in a watershed with common migration times, spawning area, spawning times, exploitation times, exploitation history, survival, age structure and correlated spawning abundances.<br>It is PacifiCorp's understanding that a single population will be modeled for the upper Klamath River Basin. If this is the case then we would like to see documentation of the rationale for this approach; especially for the Williamson River. The Upper Klamath system consists of three streams (Williamson, Sprague, Sycan) each with substantially different water temperatures, flows and habitat conditions. | In contrast to Parken et al. (2006), the Liermann et al. (2010) models the Ricker function as a function of Sueq (unfished equilibrium population size). Adding Sueq values from watershed components may be more robust to this over-prediction error; however, we did not test this assumption. In teh EDRRA model, we assumed that there would be a mix of stream-type and ocean-type Chinook in the Upper Klamath system and that this mix of the two types would vary annually. Given the complexities of specific conditions of each stream, we were unable to include such detail in this model. |
| 7 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 2. A list of the major assumptions in the modeling effort should be presented in the paper upfront (e.g. assumed harvest, assumptions about ongoing hatchery production, etc.). A discussion of the implications that these assumptions have on model outcomes should be presented as well. See document Salmon Technical Team 2005 report (it is included in the list of references) for an example. | List of assumptions added to methods section. |

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| 8 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 3. A list of the factors that are important to the Klamath but not considered in modeling should also be presented (e.g. habitat quality, water quality, etc.). | There are many factors that fit into this category. The authors have attempted to be explicit about what is incorporated in the EDRRA model so that readers can infer what was not considered. |
| 9 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 4. How should the modeling results be interpreted? | The modeling results should be interpreted as the relative perfomance of conducting the Dam Removal Alternative to conducting the No Action Alternative. |
| 10 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | a. Does one look at the median values presented or the confidence intervals around these estimates? You note that the confidence intervals (CIs) overlap substantially - does this mean that there is no statistically significant difference in the estimates for the two scenarios? | One should look at the relative difference between paired NAA and DRA runs (i.e., percentage increase or decrease in performance of DRA relative to NAA). |
| 11 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | b. At what confidence level do the CIs not overlap? | Model output revised so this comment no longer applies. |
| 12 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | c. Should the numbers be considered quantitative estimates of production or qualitative? | Model output revised so this comment no longer applies. |
| 13 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | A section needs to be added to the report clearly describing the “correct” interpretation of results in terms the public can understand. | Addressed by providing results as relative improvement of DRA versus NAA. |
| 14 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 5. The CIs are huge for most outputs. Tribal harvest (NAA 2012-2020) for example ranges from 919-373,000. The megatable shows that the maximum tribal harvest from 1982-2003 was 56,476 (1996) with a median of ~15,000. Why is the confidence interval so large and what does it mean in layman’s terms? How will this be interpreted in the EIS? | The future production of Chinook in the Klamath Basin, even under NAA, is highly uncertain due to large variations in stock recruitment dynamics. |
| 15 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 6. In the figure below you will see that estimates of harvest (in-river) predicted by the model are substantially higher than what has been observed historically. On page 17 of the document a rationale is provided for why this likely occurs. How does having perfect knowledge of starting ocean populations affect conclusions regarding fishery shutdowns, achieving spawning escapement targets, etc. between the alternatives? It would appear from the data in the figure that model results should be considered qualitative, rather than quantitative in nature, and therefore analyses should be presented as a percentage change. (See the original memo attached for figure). | The percentage change is now provided. |
| 16 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 7. The paper notes that having perfect information on ocean abundance has a large effect on model outcomes. Was the uncertainty associated with this parameter included in the analysis? If so – how, if not-why not? | Fishery management uncertainty was not included in the EDRRA model due to the complexities of modeling the uncertainty in annual age structure composition, setting of harvest rates by the council, and the precision with which quotas could be fulfilled. |

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| 17 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 8. Was the Iron Gate to Keno reach and Upper Klamath River considered two separate populations and the Liermann method applied accordingly? It appears that this was the case and then the 2 population data were combined into a single production function. Is this correct? | Iron Gate to Keno was assumed to be part of the production from the Lower Klamath Basin, which includes production from existing sources (Bogus, Scott, Shasta, Trinity, etc.), whereas the Upper Klamath Basin was composed of the tributaries to Upper Klamath Lake. A unique production function was used for the Lower Basin and a distinct production function was used for the Upper Basin. |
| 18 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 9. It appears that there is an assumption that habitat quality for the Klamath River reaches is similar to those watersheds used by Liermann and Parken. Is this correct? If so, why is this assumption appropriate given the findings of Lindley in his Klamath analysis (Lindley and Davis 2011 in prep)? | In the EDRRA model, the Klamath River was assumed to have productivity that spanning the range of salmon producing streams analyzed in Liermann et al. (2010). The method used in Lindley and Davis 2011 was different from that applied in Liermann et al. (2010) in several respects: 1) the watersheds analyzed were different; 2) Lindley and Davis utilized an equilibrium approach rather than a model incorporating population dynamics and 3) to be consistent with other watersheds, Lindley and Davis had to use different rules for specifying watershed size. |
| 19 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 10. In calculating watershed size for the upper basin, how far up in the watershed was fish spawning assumed? In other words, were any areas eliminated from the area calculation due to stream size or blockages? The draft paper notes the following: The assumptions in Lindley and Davis (In prep) differ than those made here with respect to accessibility to portions of the watershed and the spatial structure of Chinook populations once they become established; therefore calculations using parameters in Liermann et al (2010) are not directly comparable between the two works. A table describing how model assumptions vary between the two analyses and their effects on outcomes would be helpful. Also, why would the two approaches be different? The Parken paper clearly describes the steps to be used in this type of analyses. | Assumptions for calculating the watershed size in the EDRRA model are provided, and areas were eliminated from the area calculation due to barriers to migration. |
| 20 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 11. The Liermann approach could have been used to model the entire lower river basin (below Iron Gate dam and Lewiston Dam); why was this analysis not performed? | The Liermann et al. (2010) analysis would provide results that spanned the range of all streams, yet there was information to allow an analysis similar to Liermann et al. (2010) on data specific to the Lower Klamath Basin. Analyzing site-specific data will provide better estimates, and this result was shown in Figure 4 of Liermann et al (2010). |

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| 21 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 12. The model should be run based on population structure identified for Chinook in the Klamath River. If the results of this analysis matched the observed data for these areas, then this would give us more confidence as to the applicability of the Liermann approach to the Klamath. | We did not calculate an estimate using the Liermann et al. (2010) model for the Lower Klamath Basin. Still, agreement with the Lower Basin may not transfer to the Upper Basin due to the unique habitat and likelihood for mixed ocean and stream type Chinook there. |
| 22 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 13. The Iron Gate to Keno reach was defined based solely on physical structures and not population biology or environmental factors. As Parken notes: " …that the accuracy of the model depends on accurate identification of stock units, since the sum of Smsy predictions for watershed components will exceed the prediction for the entire watershed." How do the results change if the analysis assumes that all reaches upstream of Iron Gate is considered a single population based on the Liermann approach? | We assumed that the Iron Gate to Keno reach would be colonized by Chinook from the existing stocks in the Lower Klamath Basin, thus primarily ocean type Chinook. We assumed the Upper Klamath Basin will be a mix of stream and ocean type Chinook. Given the evaluation of results of DRA relative to NAA, results should be robust to these assumptions. |
| 23 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 14. There are 190 miles of mainstem habitat below Iron Gate dam that have a median spawning escapement (1990-2008) of 3,472 fish (18 fish per mile); with more than 30 percent being of hatchery origin in some years (Megatable). The model shows that the entire watershed (tributaries and mainstem) from Iron Gate Dam to Keno Dam (38 miles of mainstem habitat), produces an un-fished equilibrium run size of 23,000 (~605 fish per mile). From this, it appears that habitat upstream of Iron Gate Dam is assumed to be much more productive than what has been observed downstream? Does your analysis support this conclusion on the greater productivity expected in upstream habitat? And if so, why would this be the case? Again, running the Liermann model for all areas below Iron Gate dam would help determine the applicability of the approach used in this paper. | The estimates are not comparable: the median spawning escapement from 1990 to 2008 occurred in the presence of a fishery which was being managed on a watershed scale for Smsy of the watershed, whereas the estimate of 23,000 is the long-term median escapement that would be expected in the absence of a fishery. |
| 24 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 15. How is fall Chinook hatchery production accounted for in the model? Are strays from Iron Gate Hatchery removed from the natural spawning population after 2029? | Hatchery production is incorporated through annual hatchery releases and their survival to age 3 in the ocean. Straying of hatchery fish in the Lower Klamath Basin was not incorporated explicitly into the EDRRA model. Assumptions regarding hatchery/natural production in the Lower Basin was consistent with the assumptions in STT (2005). |
| 25 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 16. The Salmon Technical Team 2005 report notes that in development of their model a major assumption is: a. Hatchery/Natural dynamics. Estimates of spawning stock and recruitment are representative of a natural stock that can be considered independent of hatchery influences. Does this assumption hold for the Klamath River? Data collected in the Trinity River indicate that on average approximately 40 percent of the natural spawners are of hatchery origin, with hatchery influence in Bogus Creek at 40 percent, Shasta River at 0-30 percent, and the mainstem Klamath at 0-35 percent (all numbers approximate).<br>Iron Gate Hatchery origin fish contribute over 5,000 adult Chinook in most years to the natural population. How do these inputs affect the assumed production function, and how does the function change when they are eliminated with the closure of hatchery as is anticipated to occur in 2029? | See above response. The EDRRA model assumes that the closing of the Iron Gate Hatchery does not affect natural production in 2029 or thereafter. |

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| 26 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 17. Are Iron Gate Hatchery fall Chinook included in the ocean harvest and river harvest components from 2012-2020? 2021-2029? After 2029? | In the NAAI Iron Gate Hatchery fall are included in the harvest in all years provided the escapement floor is met. In the DRA alternative, production of smolts stops in 2029 thus this is the last cohort that is included in the fishery. |
| 27 | 6/13/2011 | Kevin Malone | PacifiCorps | Hendrix | 18. Iron Gate Hatchery produces approximately 40,000 fish per brood year. We would expect to see a drop in adult production starting in 2029 as hatchery production is eliminated. Does this occur in modeling? | Yes |
| 28 | 6/15/2011 | George Kautsky | Hoopa Tribe | Hendrix | Verbal recommendation provided during WebEx: Should incorporate new Fishery Control Rule (Ammendment 16) recently approved PFMC. | Incorporated |
| 29 | 6/15/2011 | Larry Dunsmoor | Klamath Tribes | Lindley and Davis | 1. Wood and Williamson rivers have large, cold springs as their primary water sources. Using watershed area:precipitation as a metric tracking habitat is unlikely to reflect these systems properly. In addition, and perhaps more significantly, thermal regimes in these systems will not reflect August air temperatures in the same way that the Sprague will - it seems likely that use of this metric will lead to underestimates of production. | In subsequent work, we looked at the air temperature issue in two ways: allowing for a quadratic effect (highest abundance at intermediate temps) and an interaction between temperature and baseflow (the idea being that spring or groundwater dominated systems should be less sensitive to air temperature and therefore have a different slope. Models with these terms do receive moderate support from the data (but aren't among the very best models), and the baseflow-temperature interaction is in the expected direction (negative, ie, higher baseflow leads to a shallower abundance-temp slope). The quadratic term, however, is not in the direction expected, rather, the data suggest (rather weakly) the possibility that abundance is higher at very low and very high temps. This doesn't make much sense and we assume this is a spurious relationship. Overall, these effects are relatively weak in magnitude and not clearly significant, so we feel they can be ignored at this stage. |
| 31 | 6/16/2011 | Walter G. Duffy | USGS; HSU | Hendrix | Many of the statistical techniques described are beyond my expertise. I am, however, familiar with the KRHM and the fishery modeling methods. Overall, I think this is a good report. The population projections made under dam removal and no-alternative seem reasonable, and there is at least some biological/social interpretation of model forecasts. I offer a couple of points that I think could strengthen the report. | Comment is appreciated (Hampton) |
| 32 | 6/16/2011 | Walter G. Duffy | USGS; HSU | Hendrix | The analysis relies on KRHM amendment 16. This amendment should be defined for the reader and, if it is out of date (e.g. Kautsky's Comments) the present version should be used. | The present version of the fishery control rule, which is an update to ammendment 16 was used for the report dated 9/19/11. |

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| 33 | 6/16/2011 | Walter G. Duffy | USGS; HSU | Hendrix | I question how accurate the shift in the productivity distribution, as illustrated in figure 3, really is. It seems that we lack the information on fish response to restoration needed to rely on such assumptions. However, in fairness this assumption does not appear to have a strong influence on model results (figure 8). | The result of habitat restoration through KBRA actions are largely unknown. As a result, it is difficult to prescribe a method for modeling the changes in productivity due to KBRA restoration. In the EDRRA model, the magnitude of shift in the productivity distribution was assumed to remove 25% of the lower tail of the distribution by 2061 (e.g., Figure 3). This assumption is open for criticism. The second portion of the comment is also true; the uncertainty in productivity due to stock-recruitment dynamics is greater than the shift in productivity due to habitat restoration. |
| 34 | 6/16/2011 | Walter G. Duffy | USGS; HSU | Hendrix | The forecast for escapement to the project reach and upper tributaries could be strengthened if an estimated of the unfished equilibrium population size for the basin below IGD were presented for comparison. | Comparison added |
| 35 | 6/16/2011 | Walter G. Duffy | USGS; HSU | Lindley and Davis | I am not very comfortable with the regression approach taken here. Assumptions underpinning this regression technique would, I think, be 1) that the variables selected are correlated (+ or -) with salmon populations and 2) data sets used are representative of salmon populations in the Klamath. Variables - the finding that August air temperature is positively correlated with salmon abundance, and that baseflow is negatively correlated with spring stocks and positively correlated with spring + fall stocks raise my concern that these variables are "statistically" correlated but may have limited biological significance. | |
| 36 | 6/16/2011 | Walter G. Duffy | USGS; HSU | Lindley and Davis | The data sets selected could also influence the model results. Some effort should be made to demonstrate the "outlier" populations were appropriately considered. For example, the fall population at the very top of figure 2 could be pulling model forecasts up, similarly the spring population on the right side of the same figure could have a strong influence on model forecasts. | |
| 37 | 6/16/2011 | Walter G. Duffy | USGS; HSU | Lindley and Davis | Beyond statistical techniques, I'm a little uncomfortable with modeling post-harvest abundance. It would seem to me that we should considered the response of salmon to dam removal or the no-alternative. Their response to fishing pressure would seem to be a separate question. Having said that, the escapement estimates projected using this model corrected for harvest are not dissimilar to those from model "A". | |
| 38 | 6/17/2011 | Dave Hillemeier | Yurok Tribe | Lindley and Davis | Overall, the paper seems to present a reasonable approach for roughly estimating the type (spring or fall) and abundance of Chinook salmon that would be expected to populate areas above Iron Gate Dam (IGD). The multi-variate statistics used are beyond my expertise; however I do have some comments regarding the data that the model was based upon and it's potential relevance to the Klamath Basin. | |
| 39 | 6/17/2011 | Dave Hillemeier | Yurok Tribe | Lindley and Davis | Spring Versus Fall Run Chinook: While habitat variables indicate that spring chinook may have been the dominant run in the Upper Basin, the empirical evidence of Fortune et al.(1966) and Hamilton et al. (2005) provide ample evidence that the Upper Basin also supported fall chinook. Therefore, it is reasonable to conclude that both races could once again inhabit the Upper Basin, even though spring chinook may be the most abundant. | |

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| 40 | 6/17/2011 | Dave Hillemeier | Yurok Tribe | Lindley and Davis | Upper Columbia Basin: Figure 1 indicates that many (roughly 1/3) of the populations that the model is based upon are located in the Upper Columbia River Basin - I assume these are primarily spring chinook populations. Given the mortality that fish migrating to/from the Upper Columbia experience as juveniles and as adults (migrating through the myriad of dams in the Columbia River – with a mortality rate associate with each dam for each life stage), it seems that these basin escapements may be less than what could be expected in the Upper Klamath. Therefore, inclusion of these basins may result in an under-estimation of the capacity potential of the Upper Klamath. | |
| 41 | 6/17/2011 | Dave Hillemeier | Yurok Tribe | Lindley and Davis | *Upper Klamath Lake:* A unique characteristic of the Upper Klamath Basin is the presence of Upper Klamath Lake (UKL), which has two factors worth considering: 1) relatively warm in the areas without spring influence during the summer months, and 2) extremely high productivity. While warm water during the summer/early fall may influence salmon behavior (i.e. cause them to use thermal refugia) and survival during portions of the year, it is unlikely that such conditions will affect the migration of spring chinook. Spring chinook adults and juveniles would likely migrate through the lake during the spring months, before the lake warms. A portion of the spring chinook juveniles may leave the Upper Basin as sub-yearling (during the late fall), however this would likely happen after the lake had already cooled. The high productivity of Upper Klamath Lake would likley result in phenomenal growth rates for the portion of the population that over-winters in the lake. It is also possible that a portion of the population would over-summer in spring influenced portions of the lake, taking advantage of the thermal refugia while also accessing abundant food resources of the lake. It is unlikely that many (if any) of the populations contained in the data set that the model was based upon are exposed to such productive conditions. Therefore, the model may be under-estimating the productivity and resultant escapement potential of the Upper Klamath Basin. | |
| 42 | 6/17/2011 | Dave Hillemeier | Yurok Tribe | Lindley and Davis | Ground Water: While warmer August (and January, although my understanding is it wasn't used in the final model) air temperatures significantly explained variation in the model, it is unclear how well this can be related to the Upper Klamath Basin (UKB). The geology of the UKB is somewhat unique; resulting in much groundwater in some areas. The groundwater influence results in potentially cooler summer water temperatures and warmer winter water temperatures. It is unclear how this somewhat unique thermal regime relates to the basins in the data set that the model was based upon. | |
| 43 | 6/17/2011 | Larry Dunsmoor | Klamath Tribes | Hendrix | Page 3, 1st paragraph, 1st line: should also cite the report by Butler et al. 2010. The Use of Archaeological Fish Remains to Establish Predevelopment Salmonid Biogeography in the Upper Klamath Basin. | Citation added |
| 44 | 6/17/2011 | Larry Dunsmoor | Klamath Tribes | Hendrix | Figures 14-17: I suggest changing the Y axis scale – do not use a log scale. These are the topics of greatest interest to readers, and use of the log scale diminishes the ability to easily discern the magnitudes differences between the alternatives. | Figures modified in updated report to reflect relative difference. |
| 45 | 6/17/2011 | Larry Dunsmoor | Klamath Tribes | Hendrix | Figure 18: I assume the horizontal dashed line is at 0.5 – should describe in figure caption. The narrative on pg 17 (1st sentence in 1st full paragraph) does not accurately describe this figure, particularly during the 2012-2020 period. | Figures modified in updated report to reflect relative difference. |

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| 46 | 6/17/2011 | Larry Dunsmoor | Klamath Tribes | Hendrix | Page 19, 2nd paragraph, last sentence: Describing *C. shasta* as being "deadly" to Chinook is a bit over the top. While the parasite clearly causes problems for Chinook during certain periods under present conditions, the two species have co-existed for a very long time. Describing the relationship as "deadly" implies that Chinook are doomed. | Statement revised |
| 47 | 6/17/2011 | Larry Dunsmoor | Klamath Tribes | Hendrix | Page 19, 3rd paragraph, first sentence: Another place to cite the Butler et al report (see first comment). | Citation added |
| 48 | 6/17/2011 | Larry Dunsmoor | Klamath Tribes | Hendrix | Page 19, 3rd paragraph, last sentence: Two points here. First, the decision was to provide passage for anadromous fish, not just salmon. Second, the decision was made by NMFS and USFWS – these agencies exercised their mandatory conditioning authority under the Federal Power Act to require fish passage as a condition of whatever FERC license is issued for the Project. | Statement revised |
| 49 | 6/20/2011 | Joe Polos | USFWS | Lindley and Davis | 1. In section 2.1, the authors state that 77 populations were used. While the populations are identified in Figure 1, it would be helpful to have the populations identified in a tabular form with the years that escapement data for each population and the status of the populations (e.g ESA listed, depressed, etc.). | |
| 50 | 6/20/2011 | Joe Polos | USFWS | Lindley and Davis | 2. Section 3.1 and Table 3, it should be noted that the data that were used to develop the population estimates for the upper basin watershed are primarily based on that of depressed stocks (at least I'm assuming this based on the map in Figure 1 and the status of most spring Chinook salmon populations). | |
| 51 | 6/20/2011 | Joe Polos | USFWS | Lindley and Davis | 3. In the 1st paragraph of the discussion in the description of life history differences between ocean-type and stream-type Chinook salmon, one of the differences identified is the extent of their marine migration with stream-type making "extensive offshore migrations in the open oceans" and ocean-type "remaining in coastal waters". I haven't reviewed the CWT recovery data from Trinity River Hatchery and Iron Gate Hatchery in many years but it is my recollection that both fingerling and yearling (ocean-type and stream-type, respectively) releases had very similar distribution, and mostly a coastal distribution. I'm not sure if or how this would influence the modeling exercises conducted for this report but there is an extensive CWT recovery database could be queried to evaluate this if it is important to this analysis. | Chinook salmon in the Klamath River all are ocean type, even though some reside in freshwater for a year or more. See Waples et al 2004. Prolonged freshwater residence as juveniles and adults will tend to reduce population productivity, since less time is spent in the richer marine environment. True stream-type chinook (e.g, interor Columbia springers) are much less vulnerable to ocean fisheries than Klamath chinook. This is an issue for comparing escapement data among populations that experience substantially different harvest rates. |
| 52 | 6/20/2011 | Joe Polos | USFWS | Lindley and Davis | 4. In the 1st paragraph of the discussion the term "more productive" is used in describing ocean-type Chinook salmon but it is unclear what this means. It would be helpful to clarify this. Is it more productive in terms of age 3 recruits? Contribution to harvest? Contribution to escapement? Or some combination? | Chinook salmon in the Klamath River all are ocean type, even though some reside in freshwater for a year or more. See Waples et al 2004. Prolonged freshwater residence as juveniles and adults will tend to reduce population productivity, since less time is spent in the richer marine environment. True stream-type chinook (e.g, interor Columbia springers) are much less vulnerable to ocean fisheries than Klamath chinook. This is an issue for comparing escapement data among populations that experience substantially different harvest rates. |

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| 53 | 6/20/2011 | Joe Polos | USFWS | Hendrix | 1. In section 2.2.2.1, it is stated that production from the ULK component of the production is modeled as a mixture of stream and ocean type life histories but it is unclear if these differences were incorporated into the harvest rate model. I'm assuming that yearling CWT data would be appropriate for stream-type UKL production as fingerling CWT data are used for ocean-type Chinook salmon. | The KHRM fishery model was developed for Chinook that are currently in the Klamath Basin, which are predominantly ocean type Chinook. The EDRRA model did not alter the KHRM model to reflect a potentially higher proportion of stream type Chinook. |
| 54 | 6/20/2011 | Joe Polos | USFWS | Hendrix | 2. A general concern is that the data used for the stock-recruit analysis and subsequent harvest modeling is based on current and past conditions when freshwater habitats have been compromised (e.g,: low flows in the Trinity in the late 1970s through the early 1990s). While I think the method for modifying alpha in the Ricker S-R relationship is a good approach, the document should acknowledge that the data is based on a population and watershed condition that have been degraded and that the relationship in the data may be different under a restored system. | Added to the list of assumptions in the EDRRA model. |
| 55 | 6/20/2011 | Joe Polos | USFWS | Hendrix | 3. While the document states that the fishery management would continue under "fishery control rule A16" I think that an additional modeling exercise addressing increased spawning capacity should be carried out. During the conference call/Webex on June 14, some additional information was presented on how the fishery rule was modeled and some discussion on this topic (the graphic of the fishery rule was very helpful and should be included in the document) with some corrections identified during this discussion. While it is impossible to know exactly how fishery management would change under the DRA, conducting some modeling, assuming modifications to the fishery rule, would be helpful to the individuals that will use this information while conducting the economic analyses. Possibly increasing the spawning escapement goal in proportion to the anticipated increase in spawning capacity under the DRA. Or possibly a series of model runs with a range of increases (10%, 25%, 50%) to provide the individuals that will conduct the economic analysis a range of values to evaluate. I'm sure the modelers that are involved in both of these reports would have more appropriate and elegant ways to account for this. | While such modeling to understand how to optimize the fishery is beyond the scope of this report, the authors are performing a study to evaluate how the fishery control rule might be altered to meet certain fishery objectives, such as escapement at or above Smsy in both the upper and lower basin. |
| 56 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix | Based on the information provided on the June 15, 2011 conference call with the fish production modeling team, PacifiCorp has the following comments on the draft Lindley and Hendrix modeling papers. These comments are in addition to those previously submitted prior to the conference call by PacifiCorp (dated June 13, 2011). | |
| 57 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix | 1) The conference call reinforced PacifiCorp's previous comment that the primary assumptions used in the analysis should be clearly outlined and the effect these assumptions have on modeling results should also be described. The underlying support for the assumptions should also be provided. For example, it appears that a key assumption in the modeling approaches is that upper Klamath habitat (and Keno reach) is assumed to be just as productive (with similar carrying capacity) as those streams used in Liermann and Parken for a mixed stock. Is this correct? If so, what information was used to support this assumption? | |

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| 58 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix | 2) It is also unclear as to how the total watershed area was calculated from Table 4 in the Hendrix paper. How was the upstream extent of fish utilization determined? It appears that its simply based on watershed size…the Parken paper notes that stream order and human/natural barriers need to be accounted for and basin size altered accordingly. For example, the Iron Gate to Keno reach has several fish barriers. Barriers exist on Jenny Creek at RM 1.0, Fall Creek RM 1.0, Shovel Creek about RM 4, Spencer Creek RM 8 and Scotch and Camp Creek. Has the watershed size been adjusted for these barriers? | For the Iron Gate to Keno reach, the areas were summed and were consistent with the values used in Lindley and Davis. In the Lindley and Davis Model their was no adjustment made for barriers under the assumption that comprehensive barrier information on barrier locations was lacking. |
| 59 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix | 3) The Hendrix report would be much more easily understood if the figures on fish production were not presented using a log scale (e.g., Figure 13). Additionally, having finer resolution on both scales for Figure 12 (and others) would make differences more clear. An appendix with the tables the data for the figures were generated from would also be helpful. | Figures modified in updated report to reflect relative difference. |
| 60 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix | 4) Although it is understood that this is meant to be a simplified analysis compared to the previous efforts, it should be made clear what the results are being used for. It is PacifiCorp's understanding that the analysis is being used by the economic team to describe benefits (in fish production,) on a year by year basis until 2061; so when benefits accrue is important. The assumption that adult returns for the period 2019-2032 "were at or above the unfished equilibrium population size" means that benefits accrue almost immediately for all areas upstream of Keno Dam. Is this true? | True, escapement to the upper basin was modeled as reaching capacity after the initiation of the active reintroduction. The economic team utilizing the results of the EDRRA model are aware of the simplified modeling of the reintroduction. |
| 61 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix | 5) The Hendrix paper notes that the agencies are expected to release hatchery fish (fry) to Upper Klamath to fully seed this habitat. Using the fry to total adult survival rate (~0.14%) observed in the lower Klamath River in the 1980's for wild fall Chinook tagged in the Shasta River and Bogus Creek, a total of ~34 million fry would need to be released annually to produce 34,350 adults (as estimated in the paper for the upper Klamath) To produce this number of fry would require approximately 20,000 adult salmon for hatchery broodstock. Is this the type of effort that is being assumed in the Fish Reintroduction Plan? | Thank you for making the values explicit. The EDRRA model assumes that fry are released such that the habitat will be fully utilized by adults. Initially, a level of effort would be required to meet the stocking goals entirely by hatchery production. As adults returned and were able to produce naturally, the hatchery production would decrease. |
| 62 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix | 6) The most dramatic difference between the no action and the dam removal alternatives seems to be in the lower bounds of ocean harvest benefits, and thus less frequent curtailment of fisheries due to low escapement. These results are more a reflection of the assumed Pacific Council management policy than the alternatives. The outcome that is not clearly stated in the Hendrix paper is that under this policy, escapement needs for upper Klamath River habitat might be unmet in some years (Figure 13, LB DRA 95% line). How this outcome impacts reintroduction efforts needs to be explained. Again, the outcome would be clearer if data were not presented using a log scale. | The updated EDRAA model includes an updated fishery control rule to ammendment 16 that does not curtail fishing at the same threshold. Still, there are some years in which the Upper Klamath Basin escapement will be below Smsy given the new fishery control rule. |
| 63 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix | 7) A better way to model the ocean fishery would be to assume a fixed (MSY) exploitation rate. The assumption that spawning escapement to the basin will not need to increase to seed available habitat should be revisited. | Evaluation of fishery management is outside of the scope of this report; however, the modeling team is currently evaluating different fishery control rules under DRA. |

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| 64 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix | 8) Using the current harvest rules for the analysis may not be realistic given the size and scale of the action alternative. To restore natural production to the basin, it seems logical that harvest policy would be altered to ensure that escapement levels are substantially greater than what has occurred historically to maximize the success of the reintroduction effort. Additionally, given the potential fitness concerns of hatchery fish used for reintroduction it is highly unlikely that upper basin fish could withstand the relatively high harvest rates observed historically. During the conference call it was assumed that there was no change in fitness. What happens to the results if fitness is assumed low and the start of the reintroduction efforts but gradually improves? | Changes in fitness were not incorporated in the EDRRA model. This is a good suggestion for future efforts to model the reintroduction process in a more detailed fashion assuming that the shift in fitness could be quantified. |
| 65 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix | 9) The Hendrix paper does not account for the fact that it will likely take several generations for fish to colonize and adapt to the expanded habitat. In order for the population, derived from hatchery fish, to achieve the predicted productivity, significant improvement in fitness will have to occur. This will likely require reduced harvest rates and removal of the genetic influence of the hatchery over a period of several decades. During this period, it may be that selective fisheries for hatchery origin fish would need to be implemented. To achieve successful re-colonization, harvest rates on newly introduced fish should be curtailed. The assumption that the basin would be run as a single stock as dams are removed appears to be unlikely. The economists should be aware of the outcomes to fisheries if weak stock management occurs in the Klamath (or ocean fisheries) during the rebuilding phase. For example, implementation of such a strategy could result in a higher likelihood of fisheries closures during and after the rebuilding process unless mark selective fisheries are implemented. | The Hendrix paper assumes 10 years of active reintroduction, which would equate to several generations. The stock is currently managed as a single stock and it is unclear how the Fishery Management Council may alter their management in light of dam removal; therefore, the EDRRA model assumes that future fishery management will be consistent with current management. Although the stock may be run as a multi-stock fishery, the details of how that would be implemented have yet to be discussed. Thus, details of how the fishery will be managed in response to restoration efforts are beyond the scope of this simple analysis. |
| 66 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix; Lindley and Davis | 10) On the June 15 conference call there was discussion on the similarities on the Hendrix and Lindley results. The Lindley paper predicts that the upper basin (Iron Gate and above) produces a spawning escapement (after harvest etc.) of 3,660 fish (90% CI of 2420 to 5510) for the combined data set. The Hendrix paper estimates that the reach between Iron Gate and Keno, and the Upper Klamath tributaries produce a median unfished run size for the mixed stock of 23,000 and 34,350, respectively (combined ~57,350). The modelers thought that the two numbers were quite close if the Lindley estimate was adjusted for harvest. Given the order of magnitude difference in these numbers we cannot see how harvest can possibly account for the difference observed. Based on our knowledge of harvest rates for Columbia River stocks (and others) we expect spring Chinook harvest rates to average less than 30 percent since about 1985 and fall Chinook to be closer to 50 percent for the stocks used in the analysis. Regardless, the modelers should conduct the harvest analysis and compare estimates in both papers (i.e. produce an apple to apple comparison). This would also require that they use similar methods when estimating production using Liermann. | The unfished equilibrium population sizes calculated by Hendrix are not directly comparable to the Lindley and Davis equilibrium escapement under fishing. Instead, the Smsy values calculated under Hendrix would be a better estimate for comparison, which is what the authors were discussing in the June 15th conference call. In order to make a comparison between the two methods for harvested populations (i.e., an apples to apples comparison), the fishery dynamics in the EDRRA model would need to reflect the uncertainties in fishery management. While such an analysis would no doubt be useful, it was not included in the scope of the model. |

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| 67 | 6/20/2011 | Kevin Malone | PacifiCorps | Hendrix; Lindley and Davis | 11) On the conference call it was suggested that the Leirmann analysis be done for all reaches below Iron Gate dam to see if the results are similar to what has been observed for natural production (minus Hatchery origin fish spawning naturally). If they are similar to observed data then this would lend credibility to the model outputs for the Iron Gate to Keno Reach. While the USGS did a preliminary analysis for some tributaries, PacifiCorp has not seen outputs for the Trinity River, Salmon River and the mainstem Klamath plus smaller tributaries (Seiler, Grider, etc). How the mainstem Klamath River is modeled would substantially affect model outputs. Are there any plans for conducting an analysis below Iron Gate dam as was suggested | Although we agree that this type of analysis could provide valuable insights that could improve our modeling effort, a lack of funding has prevented us from moving forward with developement of additional analysis at this time. |
| 68 | 6/20/2011 | Michael Belchik | Yurok Tribe | Lindley and Davis | The first sentence is erroneous in declaring that the dam removal decision hinges on a cost/benefit analysis. It specifically does not. Removal of the dams depends on a determination by the secretary that dam removal would advance the anadromous fisheries of the Klamath and is otherwise in the public interest. (take quote directly from KHSA). | |
| 69 | 6/20/2011 | Michael Belchik | Yurok Tribe | Lindley and Davis | Terms such as “only” or “modest” when referring to the size of expected runs should be eliminated from the paper. These are subjective judgments, and the perception of the value of the increase in Chinook salmon can vary quite a bit depending on one’s perspective. Suggest editing document for neutral tone. Example: last sentence of abstract “only predict escapement of …” Delete “only”. Another example: page 13: *“Our model predicts a fairly modest increase in escapement of Chinook salmon to the Klamath basin if the dams are removed.”* Suggest removing this sentence entirely; the point is made adequately subsequent. What seems modest to the authors would be an enormous change to, for example, the Klamath Tribes who have had exactly zero spring Chinook salmon return above copco dam since 1918. | |

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| 70 | 6/20/2011 | Michael Belchik | Yurok Tribe | Lindley and Davis | I also have a concern that the model correlation was based on August air temperature on the stated assumption that air and water temperatures are closely correlated (as stated on page 6, paragraph 2), when water temperatures in the upper basin are heavily influenced by groundwater contributions, depending on where temperature is taken. Would like to see discussion regarding this important concept added to the paper. | In subsequent work, we looked at the air temperature issue in two ways: allowing for a quadratic effect (highest abundance at intermediate temps) and an interaction between temperature and baseflow (the idea being that spring or groundwater dominated systems should be less sensitive to air temperature and therefore have a different slope. Models with these terms do receive moderate support from the data (but aren't among the very best models), and the baseflow-temperature interaction is in the expected direction (negative, ie, higher baseflow leads to a shallower abundance-temp slope). The quadratic term, however, is not in the direction expected, rather, the data suggest (rather weakly) the possibility that abundance is higher at very low and very high temps. This doesn't make much sense and we assume this is a spurious relationship. Overall, these effects are relatively weak in magnitude and not clearly significant, so we feel they can be ignored at this stage. |
| 71 | 6/20/2011 | Toz Soto | Karuk Tribe | Hendrix: Lindley and Davis | Thanks for the opportunity to comment on these papers. I don't have modeling expertise to provide extensive technical comments therefore my comments are limited. First, I appreciate efforts to address both stream type and ocean type chinook. The models recognize the significance of life history diversity and conservations values associated with diversity, a good thing from a conservation stand point but also a good thing from a harvest and economic stand point. Both models show a positive response under the DRA. The Lindley et al paper seems a little conservative, but still positive compared to the NAA. It likely fails to account for the uniqueness of the upper Klamath habitats (ie productive lake, volcanic landscape and spring creeks/river). The Klamath is notoriously hard to model or compare with other watersheds. Perhaps upper Basin chinook production is comparable to chinook production found on the Shasta River. Overall I feel the authors did a good job acknowledging uncertainties and came up with a defensible product. | |