



Reviews of Life-Cycle Model for Coho in the Klamath River

**James R. Hatten
John W. Beeman
Patrick J. Connolly
Matthew G. Mesa
Alec G. Maule**

**U. S. Geological Survey, WFRC
Columbia River Research Laboratory
5501A Cook-Underwood Rd.
Cook, WA 98605
509-538-2299
FAX 509-538-2843**

July 6, 2007

General Comments

Overall, we believe that Cramer Fish Sciences (CFS) has provided a global framework to address specific salmonid population questions in the Klamath River. Without such a framework, one cannot begin to grapple with the complex flow-related issues facing resource managers in the Klamath. We are supportive of the open review process that USBR has provided collaborators and interested parties in the Klamath River Basin and encourage them to continue to do so. We think that the value of these documents is in identifying specifically where data are lacking and pointing to research needed to provide those data. To that end, it would be very helpful if CFS provided a conceptual diagram of the entire modeling process that includes all models and their respective inputs (e.g., hydrographs, temperature, flow, salmonid counts or estimates) and their expected outputs (e.g., coho population estimates). Without a good conceptual model of the entire modeling process, it is difficult to know how all of the pieces (i.e., technical memorandums) fit together and whether the overall modeling architecture is sound. As is, we found ourselves reviewing the individual technical memorandums without a solid grasp on the big picture, thus we were unsure if our evaluations are as insightful as they could have been. It is also unclear to us how the model outputs will be used in the decision making process. While this was not specifically our task to evaluate, it could be important in how we view the technical merits of each memorandum and how they all fit together.

In short, we are unclear whether CFS is using the best available science to answer critical questions because of a lack of clarity in the big picture aspect of this project. A decision support system (DSS) that explicitly shows how and where the model outputs are used and what the outcome (i.e., decision) might be would go a long way in alleviating our concerns. A DSS is currently being used by the USGS and USBR in the Yakima River Basin of Washington: (http://mercury.ornl.gov/metadata/nbii/html/nbii/www.nbii.gov_metadata_mdata_USGS_FORT_usgs_brd_Fort_p_98A2500.html), the Yellowstone River (http://smig.usgs.gov/SMIG/features_0398/habitat.html), and in the Delaware River Basin: <http://www.delawariverfoundation.org/images/USGS%20Study%20Draft.pdf>. A DSS could be tailored for the Klamath Basin to accept multiple model outputs for scenario testing (e.g., what hydrographs produce the best results for multiple salmonid life-stages, and what criteria will be used for decision making). Until more clarification is provided, we think that using the model output to direct water management decisions is premature. At a minimum, measures of model stability and output variance or confidence, plus some effort at validation, will be needed to adequately apply the model results to quantitative questions.

Summary of Reviews

We have completed our review of the Klamath Coho Integrated Modeling Framework Technical Memorandum Series (memorandums 5, 6, 7 and 8) and summarize our reviews below. Each technical memorandum addressed a specific portion of the project's goal, which was to "Develop a predictive tool, based on best available science, to compare probable responses of coho salmon populations to a broad range of management actions, including different flow regimes downriver of IGD". We reviewed the flow and temperature memorandum first since the outputs from these models are used by CFS to address salmonid population questions in Technical Memorandums 5, 6 and 8. More detailed reviews can be found in Appendices A – E.

Technical Memorandum #7 (review: Appendix A) provides excellent information about flow and temperature regimes in the Klamath River and we have no major concerns about the calibration and validation of the models. We do wonder, however, why CFS did not utilize any of the detailed 2-D hydrodynamic modeling information that was provided by Utah State University (USU; Hardy et al. 2006 citation in Appendix A). Two-dimensional hydrodynamic flow modeling data are difficult and expensive to produce and could be integrated (i.e., nested) in the 1-D flow modeling network. Given that USU linked their spatially explicit hydrodynamic models to habitat preferences of steelhead, Chinook and coho salmon this omission is difficult to understand. Before such data are ignored, it might be prudent to contract an independent review of the Hardy report. Currently, we think there is a place for spatially explicit (i.e., 2-D) hydrodynamic and habitat modeling under the CFS modeling framework because they would fill in some of the blank spots produced by the 150-m transects. It might also be possible to expand the Hardy sites in the future given the advances in water penetrating lidar, which was used effectively to produce an 80-km long bathymetric surface for the Hanford Reach on the Columbia River (Anglin et al. 2006). It might also be possible to develop a crosswalk so the 2-D outputs could be compared to overlapping portions of the hydrodynamic and habitat models presented in Technical Memorandums 5 and 7, providing additional insight and confidence in the overall modeling effort.

Technical Memorandum #5 (reviews: Appendices B and C) describes the formulation of a method to estimate juvenile coho salmon carrying capacity of the Klamath River and tributaries downstream from Iron Gate Dam. The “habitat-based capacity model” used is largely based on the Habitat Limiting Factors Model (HLFM), and it incorporates scalar functions on several important physical attributes, including habitat type, stream temperature, velocity, depth, cover, and flow. When watershed-specific and empirical data were lacking, the authors did an admirable job of using relevant data and professional knowledge to fill in the gaps. The underlying premise of the model is that summer rearing capacity is the factor limiting juvenile coho salmon abundance. As it stands now, we have serious concerns as to whether the model will be adequate for the stated objective for numerous reasons. For instance, we think there may be severe consequences with how the model uses wetted width. For most habitats, except for alcoves, beaver ponds, and backwaters, there is a severe scale down between an active width of 9.9 m and 10.0 m. For example, trench pools within a stream segment that is 9.9 m gets a value of 1.79 coho salmon parr/m², but for the same habitat in a stream 10.0 m wide gets a value of 0.21 coho salmon parr/m², an 88% decrease. It is not likely that coho salmon sense a difference of 0.1 m change in width. The model also gives high weight to alcoves, beaver ponds and backwaters, but it is not clear how the authors estimated the quantity of these habitats when survey data were lacking. Another concern is that coho salmon appear to have been modeled in a vacuum, with no scalar or discussion about the limitations represented by interspecific interactions with other fish in the system (native and introduced). We think a scalar function should be introduced to account for the competition and predation represented by other fish species.

As it stands now, the output from Technical Memorandum #5 appears most useful as one to illustrate life cycle concepts rather than one to precisely predict the outcomes of various management strategies. This is due to the reliance on out-of-basin data to populate the various

portions of the models, the model complexity and many assumptions, and the lack of a measure of variability in the output. The use of data from other basins was necessary to enable the work to be done in the specified timeframe and the authors appear to have used the best available information, but there appears to be insufficient data from within the Klamath River Basin with which to populate such a model and therefore certain assumptions had to be made to complete the task. The HLF model did provide an answer, i.e., the number of parr and smolts representing capacity of the Klamath Basin under current conditions, but we question the validity of the model without further testing. The authors did provide an empirically-based comparison with Scott Subbasin, and we would like to see additional comparisons. One way to insure some reality and usefulness of the model is to run other models (e.g., SHIRAZ, EDT) and compare the results basin wide or within selected subbasins. If there is agreement among these models, then we would have more confidence in the habitat-based capacity model. If not, then managers should be very concerned about the appropriateness and usefulness of the model.

The goal of Tech Memorandum #6 (review: Appendix D) is to describe how disease mortality will be included in the overall life-cycle model that will be a “predictive tool” to inform management actions. We commend CFS for selecting the lead author of this technical memorandum, as she is perhaps the most knowledgeable researcher working on disease issues in the Klamath Basin at this time. In general, the authors conclude that modeling for disease related mortality is only needed for juvenile coho salmon that emigrate through the reach between Iron Gate Dam and the mouth of the Scott River (Reach 1). The authors frequently, and rightfully, point out the limitations of available data and the need for additional work to validate their assumptions. However, we have a couple of issues with this memorandum. The general premise is to apply a scalar to decrease survival of juveniles upstream from approximately the Scott River to account for the effects of disease. We believe the similarity of the occurrence of the host and the survival results from 2006 are likely a coincidence rather than expression of a causal mechanism because yearling coho salmon spent much of their time between rkm 309 and rkm 234, the time from exposure to death from *C. shasta* is long enough that much of the mortality that might have occurred during the coho salmon survival study in 2006 would likely be attributed to reaches downstream from the Scott River, the prevalence of *C. shasta* and subsequent mortality of coho salmon in 2006 trials were quite low, and the timeframe of the coho salmon survival study did not overlap with the timeframe of the summer period the life cycle model seeks to represent. For these reasons, we think a disease scalar, if justified, should not be limited to the single reach as described..

We also do not think CFS adequately addressed the question of disease-related, pre-spawning mortality in adult coho salmon. In September 2002 there was a huge die-off of adults—primarily Chinook salmon but some coho salmon as well. Adult coho salmon spawning peaks later in the fall, but with unknown future thermal inputs and flow regulations, adult impacts should not be discounted. Despite all of the author-identified limitations in the available data, it appears that the development of a predictive model will proceed. We believe that this is premature and that the value of this modeling will be to identify data gaps and specific research to fill those. We are aware that some of that research is ongoing and recommend that modeling disease effects on coho salmon in the Klamath River be put on hold until more data specific to the species and area of concern are completed.

Technical Memorandum #8 (review: Appendix E) simulates the effects of temperature and flow on adult and juvenile coho salmon as they migrate through the Klamath River, with most of the anticipated effects occurring on the juvenile life stages. We had numerous concerns with data borrowing, assumptions, and validity of the output from the model. For example, the authors state that they have no data on pre-spawn mortality for coho salmon as a result of high water temperatures, so they used data from studies on Chinook. The authors also describe how temperature and pre-spawn mortality will be analyzed by using the highest mean weekly temperature. We think that focused laboratory experiments could shed light on how static temperature exposures might impact coho salmon in the Klamath River. Furthermore, a study that monitors the movements and distribution of upstream migrating adults that have been implanted with temperature-sensitive radio tags would be extremely useful. Overall, there was little output data presented in Tech Memo #8. We think it would be useful to provide a figure that shows the ultimate relations between number of adults in, number of adults that survive to spawning, number of resulting eggs, and number of resulting fry for the expected range of the adult coho salmon run based on historical run sizes. Our biggest concern is the difficulty of validating the results of the model in the wild. The memo points to the need for more information and shows the gaps in our knowledge and where the uncertainties lie. Currently, we question how meaningful output from the model will be and suggest a combination of lab experiments and bioenergetics to examine some of the effects of temperature and flow on adult and juvenile coho salmon.

Appendix A

Review of Technical Memorandum #7—Temperature and flow dynamics of the Klamath River

Reviewer: James R. Hatten

Objectives of this part of the model

From p. 3: The primary objective is to “Develop a predictive tool, based on best available science, to compare probable responses of coho salmon populations to a broad range of management actions, including different flow regimes downriver of IGD.”

From p. 4: “The purpose of this memorandum is to describe the range of temperature and flow conditions that might be reasonable to expect at various points along the Klamath River and provide a basis for dividing the mainstem into reaches with uniform summer temperature and flow conditions within the coho salmon life-cycle model. Model output and presentation of data is used to lay the groundwork for supporting the coho salmon life-cycle model. Also presented is an introduction to the flow and temperature model for readers unfamiliar with basin and recent refinements in temperature model calibration.

General comment on successfulness of meeting the objectives

The technical memorandum was prepared by two experienced hydrologists and temperature modelers in the Klamath Basin (Leon Basdekas and Michael Deas), of Watercourse Engineering, Inc. This is a data rich technical memorandum, with large quantities of temporal validation data that span several years. The authors used two RMA models to model temperature and flow from Iron Gate (RM 190) down to Turwar (RM 5). The RMA models were applied in one dimension to represent longitudinal variations in temperature and flow, hourly, at 150 m intervals. The authors did a good job of using time series and longitudinal profile plots to visually present temperature and flow data, and they recently modified their models to incorporate new information and comments obtained from the USGS. They also did a good job of describing flow and temperature variability both longitudinally and seasonally, dividing the river into six reaches that have uniform summer temperature and flow conditions. From a 1-D perspective, the two RMA models appear to be well calibrated and the validation tests produced good results, thus the authors were successful at meeting some of their objectives.

From a project perspective, the goal is to use the best available science to build a predictive tool for coho and Chinook salmon population modeling (we mention Chinook also because the issues will be similar), thus a critical concern is whether the 150-m spatial resolution of the 1-D models is appropriate to capture the microhabitat utilized by juvenile salmonids along the channel margins or in complex channel reaches that contain islands and split channels, or for identifying flow effects on mainstem spawning habitat. Thus, the models provide a simplified version of reality that appear suitable for answering some important population-based questions, and this is demonstrated in the other technical memorandums, but are limited for exploring direct relationships between specific flows (or hydrographs) and micro-habitat availability for specific salmonid life stages. We think the best available science is the integration of fine and coarse scale models so flow-related questions related to salmonid populations and habitat availability can be best answered.

Specific comments

1) The authors identified six reaches that characterize the temperature and flow regimes of the Klamath River. This is a good start to a complex modeling issue because they are providing a spatial construct for managers to discuss and refine the reaches if desired. These six reaches can be used as a starting point until better information is provided.

2) The authors broke the six Klamath River reaches into finer 150-m reaches for flow and temperature modeling, but numerous studies have found that juvenile salmonids rear in near-shore micro habitats that are associated with specific bed slopes, water depths and velocities (Tiffan et al. 2002, Anglin et al. 2006, Hardy et al. 2006). Micro-habitat conditions cannot be adequately characterized by 150-m resolution 1-D transects that are presented in the flow and temperature memorandum. Increasing the node spacing will do little to improve the situation since 1-D models are averaged across the lateral transects. We think it unlikely that population-based models alone will be sufficient to characterize salmonid responses to alternate hydrographs.

3) The use of 2-D hydrodynamic models would provide the spatial resolution necessary to capture the micro-habitat hydraulic conditions that salmonids utilize, thus they are better suited for the development of physical habitat response models (see Tiffan et al. 2002, Anglin et al. 2006). For example, if one alters the flow from 500 cfs to 1000 cfs, how will the velocities and depths change throughout the 150-m reaches? If only a few reaches are involved, this might not be a critical issue, but there are thousands of reaches so small changes in habitat can have a cumulative effect on salmonid populations. Incorporation of physical-based habitat models along selected reaches (see Hardy et al. 2006) is suggested to compliment the coarse scale 1-D approach presented by Fish Sciences.

4) The authors ignored 2-D hydrodynamic data that were used by Hardy et al. (2006) to develop and test spatially explicit habitat models for coho salmon, steelhead, and Chinook salmon. Spatially explicit habitat models have been used in numerous basins such as the Columbia (Tiffan et al. 2002, Anglin et al. 2006), Yakima and Delaware rivers (USGS projects in progress) to determine the effects of steady-state and dynamic (i.e., variable) flows on the life-stage habitats of salmonids. The spatially explicit habitat models developed by Hardy et al. (2006) are effectively nested within the six macro reaches identified by the authors, thus they can provide a hierarchical approach to modeling. The 150-m resolution 1-D transects presented in this technical memorandum form an effective outer envelope for the Klamath River, the nested 2-D models provide a foundation for populating the envelope with site-specific information. A hierarchical, nested approach to modeling would provide the USBR with the best information possible for assessing flow effects by increasing our understanding of micro-habitat availability resulting from different flow regimes in the Klamath River, coupled with the population-based models.

Information needed to make the results more appropriate for use by managers

1) Fish Sciences has proposed six macro reaches, plus a network of transects spaced at 150-m intervals, to model and characterize flow and temperature in the Klamath River, thereby providing information to the coho and Chinook salmon population-based models. It is currently

unknown if the flow and temperature modeling network is appropriate to answer many of the questions posed in the other technical memorandums, thus there is considerable uncertainty about the appropriateness of this study plan. A concerted effort to validate the population-based salmonid models is necessary before we can determine with certainty if the temperature and flow modeling network is sufficient to address specific flow-related salmonid population questions. The USBR also needs to assess whether the incorporation of spatially explicit physical habitat models is useful in answering specific questions that the population-based models cannot address, such as the impacts of alternate flows on rearing or spawning habitat within critical reaches of the Klamath River. A spatially explicit modeling approach similar to the efforts currently underway in the Yakima River Basin (USGS/USBR project in progress) that utilizes a decision support system to maximize the benefits of flow management on numerous salmonid life stages within five critical floodplain reaches, coupled with a population-based model (EDT) should be examined to determine if important information is being left unaddressed by the population-based modeling approach.

References Cited:

- Anglin, D. R., S. L. Haeseker, J. J. Skalicky, H. Schaller, K.F. Tiffan, J. R. Hatten, P. Hoffarth, J. Nugent, D. Benner, M. Yoshinaka. 2006. Effects of hydropower operations on spawning habitat, rearing habitat, and stranding/entrapment mortality of fall Chinook salmon in the Hanford Reach of the Columbia River. U. S. Fish and Wildlife Service, Columbia River Fisheries Program Office, Vancouver, WA.
- Hardy, T.B., R. C. Addley, and E. Saraeva. 2006. Evaluation of instream flow needs in the lower Klamath River, Phase II final report. Prepared by Utah Water Research Laboratory, Utah State University for the U.S. Department of the Interior. Available at <http://www.engineering.usu.edu/uwrl/inse/klamath/report.html>.
- Tiffan, K. F., R. D. Garland, and D. W. Rondorf. 2002. Quantifying flow-dependent changes in subyearling fall Chinook salmon rearing habitat using two-dimensional spatially explicit modeling. *North American Journal of Fisheries Management* 22:713-726.

Appendix B

Review of Technical Memo #5: Estimating the capacity of the Klamath Basin to rear coho salmon:

Reviewer: John Beeman

Objectives of this part of the model

The objective of this memo is to estimate juvenile coho salmon carrying capacity on the Klamath River and tributaries downstream from Iron Gate Dam for inclusion in the life-cycle model of coho salmon. In the “Memorandum Context” on page 3 of memo 5, it is stated the model is meant to be “capable of predicting coho salmon production under differing flow regimes in the Klamath River downstream from Iron Gate Dam”.

General overview

The Technical Memo #5 document describes the formulation of a method to estimate juvenile coho salmon carrying capacity of the Klamath River and tributaries downstream from Iron Gate Dam. The general methods were taken largely from the peer-reviewed fishery literature and have been used in similar contexts before. The underlying premise of the model is that summer rearing capacity is the factor limiting juvenile coho salmon abundance. This is a departure from studies in other rivers indicating winter habitat is limiting (i.e., Nickelson et al. 1992), but the authors provide support for this assumption relative to the Klamath River. If this assumption is incorrect the model will not be representative of the population in question. The work presented thus far seems to indicate this assumption is true. The work was meant to apply to conditions between July 1 and September 30 (though a slightly earlier period was mentioned at the model workshop on June 8, 2007). The authors appear to have a thorough knowledge of the relevant peer-reviewed literature, and they are currently working on life-cycle models in at least one other area of the Pacific Northwest. One of the authors is a Bureau of Reclamation employee.

As may be expected, there are many assumptions associated with the work. For example, there were sections of streams that had not been surveyed, which would prevent them from being used in the Habitat Limiting Factors Model (a model by one of the authors to estimate carrying capacity). In this case they used data from similar populations. While necessary, this is one weakness in their method, as per their Table 12 the tributary with the largest amount of surveyed length was the Salmon River at 64% and the lowest was the Shasta at 5% (not used in this section of the model). The others range from 6% to 50%. They also assumed streams of similar sizes within a model reach had similar temperature regimes, which seems reasonable. Other examples of incomplete data also exist, but in each case the authors appear to have used reasonable methods to address them. The data are largely based on areas known to be less than 21.5 C per the NOAA mask in a data set used by the authors. If this does not reflect the true situation the model output may be inaccurate. The authors have generally done a good job of justifying their choices in such instances.

Several of the people at the workshop of June 8, 2007 were concerned with the specific values of model input parameters and their relevance to coho salmon in the Klamath River. One of the chief areas of concern was the relatively high productivity of the Klamath River when compared

to other coastal streams. This could be important in selecting values for habitat suitability relative to velocity, as it was shown this is dependent on insect abundance and drift. Another was the decision by the authors to fix the reaches downstream from the Shasta River as cool-water refuge dependent, as some thought this would depend on the water year type. There were other concerns about the shape of some habitat suitability curves and the difference between those used in this work and those used by Hardy et al. (2006). The current models do not appear to use any data from Hardy et al. (2006).

General comment on successfulness of meeting the objectives

I do not believe the model will be adequate for the stated objective. The output from the Technical Memo #5, and likely others, appears most useful as one to illustrate life cycle concepts rather than one to precisely predict the outcomes of various management strategies. This is due to the reliance on out-of-basin data to populate the various portions of the models, the model complexity and many assumptions, and the lack of a measure of variability in the output. The use of data from other basins was necessary to enable the work to be done in the specified timeframe and the authors may have used the best available information. There appears to be insufficient data from within the Klamath River Basin with which to populate such a model and therefore certain assumptions had to be made to complete the task. For example, the data used to determine the carrying capacity of various tributary habitats was from northern and central Oregon streams in the late 1980s (Nickelson et al. 1992). It was applied using the Habitat Limiting Factors Model (HFLM) by applying the mean values of coho salmon per area to tributaries of the Klamath River. Much of this process was made possible by assuming unsurveyed sections were similar to other known sections (well over 50% of most streams). This appears to be common in this field of fishery biology. When asked about the extent of extrapolation at the June 8, 2007 workshop, Tom Nickelson stated he felt good about this process compared to some of the data sets he had dealt with in the past, because it was based on stream widths rather than just lengths. Some measure of model stability and output variance or confidence will be needed to adequately apply the model results to quantitative questions.

Specific comments

It is not clear during what periods they consider a fish to be a parr and smolt. This becomes important as they have applied various equations to predict the mortality from one life stage to the other as well as through the winter. On page 9 they state they applied a 90% survival rate from winter parr to smolt, and on page 34 they describe the use of a 45% parr-to-smolt survival rate.

Information needed to make the results more appropriate for use by managers

The lack of a measure of variation in the model output will make it difficult for the end user and interested parties to have confidence in its output. The authors plan to perform sensitivity analyses and these should prove helpful in determining model sensitivity to specific variables. However, a measure of the error associated with the model output would be much more useful to the end user. This could conceptually be done through the use of several values of carrying capacity and the other input variables rather than simply the mean value. For example, Nickelson et al. (1992) indicates approximately five coho salmon were found in a backwater habitat type during spring. The 95% confidence interval range around this mean is approximately 4 to 6. Thus, if one model output was done at each of 4, 5, and 6 it would cover

the likely range of the input data, resulting in a measure of variation in the output. This could be repeated for each variable or habitat type. Repeating this many times to cover the range of input data, such as a bootstrapping method, may result in useful measures of the confidence in the model output. However, it would not address uncertainties in the assumptions made in using various out-of-basin data sources. It would help one interpret the existing memo output of 1.7 million smolts: does the data indicate 1.7 million plus or minus 2 million, or plus or minus 10,000? The answer to such questions will be paramount to its use to evaluate potential operations at Iron Gate Dam.

References

- Hardy, T.B., R. C. Addley, and E. Saraeva. 2006. Evaluation of instream flow needs in the lower Klamath River, Phase II final report. Prepared by Utah Water Research Laboratory, Utah State University for the U.S. Department of the Interior. Available at <http://www.engineering.usu.edu/uwrl/inse/klamath/report.html>.
- Nickelson, T. E., J. D. Rodgers, S. L. Johnson, and M. F. Solazzi. 1992. Seasonal changes in habitat use by juvenile coho salmon (*Oncorhynchus kisutch*) in Oregon coastal streams. *Canadian Journal of Fisheries and Aquatic Sciences* 49: 783-789.

Appendix C

Review of Technical Memorandum #5—Capacity of Klamath Basin to rear coho salmon

Reviewed by: P.J. Connolly

Objectives of this part of the model

From p. 3: The primary objective is to “Develop a predictive tool, based on best available science, to compare probable responses of coho populations to a broad range of management actions, including different flow regimes downriver of IGD.”

From p. 5: This memorandum sets out to describe the “...methods for estimating the capacity of the Klamath Basin to rear juvenile coho.”

General comment on successfulness of meeting the objectives

The “habitat-based capacity model” used is largely based on the Habitat Limiting Factors Model (HLFM), and it incorporates scalar functions on several important physical attributes, including habitat type (Table 4), stream temperature (Figure 5), velocity (Figure 12, Table 7), depth (Figure 13, Table 7), cover (Table 8), and flow (Figure 18). When watershed-specific and empirical data were lacking, the authors did an admirable job of using relevant data and professional knowledge to fill in the gaps.

I particularly liked the kind of continuous scalar function presented for temperature in Figure 5. Many of the other scalar functions are of the “hockey stick” variety, which can cause some “stiffness” to the model that may not be representative of fish response. I had particular problem with the habitat-type scalar functions presented in Table 4. Further comment on this is given under the “Specific comments” section below.

Specific comments

1) I believe that there may be severe consequences of how the model uses wetted width. For most habitats, except for alcoves, beaver ponds, and backwaters, there is a severe scale down between an active width of 9.9 m and 10.0 m. For example, trench pools within a stream segment that is 9.9 m gets a value of 1.79 coho salmon parr/m², but for the same habitat in a stream 10.0 m wide gets a value of 0.21 coho salmon parr/m², a whopping 88% decrease! Certainly coho salmon do not sense a difference of 0.1 m change in width. See the figure I have attached below, using trench pool habitat as an example. This artifact of the model would not be severe if a basin tended to have mostly small streams feeding a short length of large river (as perhaps in a coastal basin from where the model originates), but my examination of Appendix E leads me to a very different conclusion. In fact, over 770 km of stream in the Klamath Basin have a wetted width (ACW) between 8-12 m (using 7.5-12.4 m). The parr capacity associated with this important transition zone was estimated to be over 1.76 million parr, which is at least 34% of the total estimated capacity (5.2 million). See the table I have attached below. One potential fix to this glaring potential problem is to modify the scalar function so it is not so abrupt between 9.9 m and 10.0 m for mainstem units. This should probably take effect somewhere over the range of 5.0 and 15.0 m ACW.

2) The model gives high weight to alcoves, beaver ponds, and backwaters (Table 4). While the literature supports that these habitats are very important to coho salmon rearing, it is not clear how the authors estimated the quantity of these habitats when survey data were lacking. How were lengths and/or areas of these non-mainstem habitats extrapolated from survey data? Were the effects of geomorphic characteristics (e.g., degree of constraint from valley hillslope) and anthropomorphic disturbance (e.g., roads, ditching) considered on a reach by reach basis? Certainly these factors need to be considered before high value habitat can be assigned to an unsurveyed reach.

As an example why this needs to be transparent in order to evaluate the model, I provide the following from Appendix E:

Subbasin name	Stream	Length (km)	ACW (m)	Parr capacity	Smolt capacity
NF Salmon River	S. Russian Cr.	3.7	9.9	15,044	3,331
NF Salmon River	N. Fk Salmon	3.7	11.8	16,935	3,796

These two reaches have equivalent length and surprisingly similar capacity (parr capacity: 0.39/km and 0.41/km, respectively). However, the ACW straddles the magic line of 9.9 vs 10.0 (see Specific Comment 1). The reach with an ACW=9.9 should far exceed the one with ACW=11.8 if composed of only mainstem pool and non-pool habitats units based on scalar functions in Table 4. What has to make the difference (unless the model calculations are outright wrong) is the amount of habitat associated with alcoves, beaver ponds, and backwaters. To make this more transparent, the authors should show what mix of habitat exists in each reach. This could be done in a new Appendix F, or at the very least an additional column in Appendix E showing the mix of primary categories such as: mainstem pools (n = 5 types combined), mainstem non-pools (n = 4 types combined), and backwater habitats (= alcoves+beaver ponds+backwaters).

3) Coho salmon appear to have been modeled in a vacuum. I found no scalar and no discussion about the limitations represented by interspecific interactions with other fish in the system (native and introduced). Certainly some scalar function could be introduced for the competition and predation represented by other fish species. Perhaps this is handled by another module of the model (?).

4) On p. 7, it states that any habitat falling within the 21.5 C temperature mask was eliminated. It would be potentially highly illuminating to see how much an effect this action had on coho salmon rearing capacity. In this case, and other cases like it, I would think it mandatory that the authors show (e.g., in a table or appendix) what effect these “on-off” switches have on the estimate.

Information needed to make the results more appropriate for use by managers

1) The HLF model provided an answer, i.e., the number of parr and smolts representing capacity of the Klamath Basin under current conditions. This is no minor achievement! However, the next question is how valid is the model. The authors provided one glimpse of this when they compared the outputs of the model to empirically-based observations in the Scott Subbasin (see

p. 37). If there are more subbasins or reaches that are data rich, the authors should duplicate this kind of comparison to the maximum extent possible. This would allow reviewers and users of the output to scale appropriately. Better yet, the modelers would likely be able to modify scalar functions to more appropriately match empirical results.

Another way to insure some reality and usefulness of the model is to run other models (e.g., SHIRAZ, EDT) and compare the results. If not for the entire Klamath Basin, which would undoubtedly be very expensive to do, then subsets (i.e., subbasins or sub watersheds) should be subjected to this treatment. If there is agreement among models or at least 2 out of 3, then more confidence would be gained. If it happens that the HLF model used provides the most unusual result, then managers should be very concerned about the appropriateness and usefulness of the model.

Appendix D

Review of “Klamath Life Cycle Model, Technical Memorandum 6. Disease Effects on Coho Survival in the Klamath River”

Reviewer: Alec Maule

The goal of Tech Memo 6 is to describe how disease mortality will be included in the overall life-cycle model that will be a “predictive tool” to inform management actions. To accomplish this goal the authors were to determine whether or not pathogen prevalence or virulence is greater in the Klamath system than in other similar river basins. If they found that it was not, disease-related mortality would be covered by other parts of the overall model. If disease in the Klamath was greater than in other river basins, then a disease-specific element would be included in the model. The memo began with descriptions of the pathogens that might be responsible for disease in coho salmon in the Klamath, and each pathogen’s prevalence and distribution in the basin. They then discussed pathogen virulence and Klamath River coho salmon resistance, the influence of water temperature, flow and migration timing on disease-related mortality. Finally, the authors presented their plan for incorporating disease in the model and their conclusions. We commend the contractor of selecting the lead author of this technical memorandum, as she is perhaps the most knowledgeable researcher working on disease issues in the Klamath Basin at this time.

In general, the authors conclude that modeling for disease related mortality is only needed for juvenile coho salmon that emigrate through the reach between Iron Gate Dam and the mouth of the Scott River (Reach 1). This conclusion was based on the significantly higher number of *Ceratomyxa shasta* spores found in this reach compared to other reaches. Based on shifting a logistic function of temperature and mortality to a lower critical temperature of 14° C, they predict 50% survival of juvenile coho salmon that migrate through Reach 1 when water temperature is 17° C. Other disease-related mortality is contained within the temperature scalar presented in Technical Memorandum 7.

The authors frequently, and rightfully, point out the limitations of available data and the need for additional work to validate their assumptions. Several examples are (1) in reference to “relying heavily” on data on Chinook salmon and sentinel studies in the Klamath River “However, the data were collected using varied methods and over a range of natural infection doses, making direct comparisons between tests difficult and complicating predictions of disease prognosis...” (Page 4); (2) in reference to differences in mortalities reported in the literature they state “...these differences also demonstrate that disease impacts will be highly variable between years and between locations.” (Page 8); (3) in reference to 3-d exposures of coho salmon near Beaver Creek “These exposures should be repeated under varying exposure conditions to better interpret the relative susceptibilities of Klamath River stocks.” (Page 10); and (4) in reference to laboratory studies of the temperature influence on coho salmon mortalities from columnaris “However, these studies have been done under constant temperatures and may not directly represent disease progress under conditions of fluctuating daily temperatures.” (Page 11).

We believe that the authors have done a good job of gathering existing data and they have accurately evaluated it relative to the Klamath River. The conclusions they have reached are sound—relative to the data available. However, they did not adequately address the question of disease-related, pre-spawning mortality in adult coho salmon. In September 2002 there was a huge die-off of adults—primarily Chinook salmon but some coho salmon as well. Adult coho salmon spawning peaks later in the fall, but with unknown future thermal inputs and flow regulations, adult impacts should not be discounted.

We do have serious concerns about using these conclusions to build a predictive model. The authors themselves acknowledge the pitfalls of the data:

“The objective of this section is to assess the decrease in smolt survival during out-migration as a result of disease effects. There is little empirical data available on which to base this assessment....The data also largely focuses on Chinook salmon, and differences in coho salmon behavior and disease susceptibility may alter survival predictions.” (Page 16-17).

Despite all of the author-identified limitations in the available data, it appears that the development of a predictive model will proceed. We believe that this is pre-mature and that the value of this modeling will be to identify data gaps and specific research to fill those. We are aware that some of that research is ongoing and recommend that modeling disease effects on coho salmon in the Klamath River be put on hold until more data specific to the species and area of concern are completed.

**Additional comments provided by John Beeman:
Disease Memo (#6)**

This memo describes the current state of knowledge about several diseases in juvenile salmon in the Klamath River downstream from Iron Gate Dam. Cramer Fish Sciences is planning on including a disease scalar in their life-cycle model to address people’s concerns over this issue. The general premise is to apply a scalar to decrease survival of juveniles upstream from approximately the Scott River to account for the effects of disease. This premise generally fits with what is known about the presence of the intermediate host for *Ceratomyxa shasta* and *Parvicapsula minibicornis* as well as the 2006 finding of the coho salmon survival study indicating that survival between Iron Gate Dam and the Scott River was lower than in other reaches downstream. As a member of the team working on the survival study, I believe the similarity of the occurrence of the host and the survival results from 2006 are likely a coincidence rather than expression of a causal mechanism. I use four lines of evidence for this conclusion: 1) yearling coho salmon in the coho salmon survival study spent most of their time from release to river kilometer (rkm) 33 in the reach from release (rkm 309) to the Scott River (rkm 234), suggesting the low survival in this reach could be largely due to their time in the reach rather than something about the conditions in the reach itself; 2) the time from exposure to death from *C. shasta* is long enough that much of the mortality that might have occurred during the coho salmon survival study in 2006 would likely be attributed to reaches downstream from the Scott River. At the June 8, 2007 life cycle model workshop, Jerri Bartholomew cited results of an ongoing study indicating that at 18 C they were seeing mortality in Iron Gate Hatchery

coho salmon at 18 days post exposure. Some hatchery fish in the coho salmon survival study do reside in this reach longer than that, but many leave before that time and would not be included in mortalities attributed to that reach; 3) the prevalence of *C. shasta* and subsequent mortality of coho salmon in 2006 trials were quite low (Jerri Bartholomew presentation at the June 8, 2007 workshop); and 4) the timeframe of the coho salmon survival study does not overlap with the timeframe of the summer period the life cycle model seeks to represent. Few tagged fish were upstream of the Scott River after approximately June 01, 2006 when temperatures were approximately 17 C. Thus, the combination of few fish, water temperatures below critical thresholds, differences in time periods, and the time required between exposure and death suggest the low survivals from the coho salmon survival study in 2006 were not likely due to disease. For these reasons, I believe using the estimated survival from that study as a target for the disease scalar is unjustified.

Appendix E

Review of Tech Memo #8—life cycle of Klamath coho

Reviewer: Matt Mesa

Fundamentally, this model will simulate the effects of temperature and flow on adult and juvenile coho salmon as they migrate through the Klamath. Most of the anticipated effects will occur on the juvenile life stages. On pg. 4, the authors note that survival of eggs and of juvenile coho salmon is density dependent, but they give no justification for this. Please clarify. On pg. 7, the authors state that temperatures may vary widely during the adult life stage. Don't they know this? That is, isn't there adequate water temperature data to actually document the variation in temperatures during the adult migration season? If not, an array of thermographs in the basin would be helpful. On pg. 9, the authors state that there is much uncertainty and variability around adult migration timing, yet it's critical to the model's output. This is a good example of a key area for focused research that should be done prior to using a simulation model. Otherwise, it might be hard to put much credibility on the model's output. The authors state that they have no data on pre-spawn mortality for coho salmon as a result of high water temperatures. Thus, they were forced to use data from studies on Chinook salmon. Not much they could do, I guess, but again it begs the question of data borrowing, assumptions, and validity of output from the model. On pg. 13, the authors describe how temperature and pre-spawn mortality will be analyzed in the model. I think their approach is too general. Their metric of interest is the highest mean weekly temperature, but I think fish will rarely get exposed to this. And, if they do, it may not be for very long because they're migrating and moving through areas fairly quickly. It seems to me that the way this analysis will be done is analogous to static temperature exposures like one would do in a lab. Again, this is another area where data are lacking and a focused research effort could be quite informative. For example, imagine a study that monitors the movements and distribution of upstream migrating adults that have been implanted with temperature-sensitive radio tags. See Figure 12. Although it is based on five total points, clearly one point is driving the relation and leads to a high r-squared value. But, one careful look at the figure indicates that this is a pretty weak relation.

Overall, I think this Tech Memo leaves us hanging. No output is actually presented. Shouldn't there be a final figure or table showing the ultimate relations between number of adults in, number of adults that survive to spawning, number of resulting eggs, and number of resulting fry? This could be done for the expected range of the coho salmon adult run based on historical run sizes. Further, I see no way of validating the results of this model in the wild. Or, at the least, it's not spoken of here. Whatever numbers are output from the model, I guess you'll just have to believe it—you'll just have to assume that it represents the true state of nature. There are many examples of unjustified assumptions and data borrowing, which weakens the validity of the model. The whole memo points to the need for more information—it shows the gaps in our knowledge and where the uncertainties lie. Unfortunately, this questions how meaningful output from the model will be. I wonder if bioenergetics can fit in here some how? When you're talking about temperature and flow (which means swimming and exercise), bioenergetics might be a new and valid way to examine possible effects.

Comments on: Klamath Coho Life Cycle Model draft version 1.1, dated September 18, 2007, prepared by Cramer Fish Sciences.

Reviewer: John Beeman, Research Fishery Biologist
Review data: October 26, 2007

The model represents a large amount of work and is based on standard fisheries concepts. Some of the concepts, e.g., the effects of temperature on smolt survival, are taken from the out-of-basin literature and are often from other salmonid species. This was an inevitable outcome of creating a life-cycle model for fish in an area with relatively little in-basin data available. Other concepts, such stock-recruitment functions, are taken from the general body of fishery literature, but which one to use in the model is controversial. Studies to test, or provide data for, the most influential parts of the model should be conducted to verify the existing scalars or update them with more pertinent data if needed. Sensitivity analyses conducted by Cramer Fish Sciences indicated the model was most sensitive to the assumed stock-recruitment function, marine survival, and the smolt migration flow scalar. However, it may be unlikely that the model will be overly sensitive to changes in any one of the variables, given that there are so many of them controlling the output.

The overall numerical output cannot be tested for accuracy. As such, the model can only be useful if the parts making up the whole are accurate. There is considerable uncertainty in the accuracy of the various parts, and there is therefore considerable uncertainty in their sum total. Each part appears to be based on reasonable theory, though often from other species or drainages. Based on this, it seems a reasonable assumption that the model should be true to gross trends. However, this does not appear to be the expected use of the model, and therefore the model in its present form may not be suitable for its intended use. One productive use of the model could be as a framework to identify and fill data gaps. As mentioned in a previous review, there is no measure of variation in the predicted output, and thus the precision of model outputs cannot be evaluated when comparing scenarios.

At the model workshop on October 11, 2007 one of the most contentious issues was the scalars for smolt migration survival. The issue generally revolved around the separation of the effects of discharge, temperature, and disease. There are currently scalars for each of these factors, but they are often inseparable in empirical studies. This is the case with the ongoing study of survival of radio-tagged coho salmon in the Klamath River. That study is not based on experimental discharges, so discharge cannot be completely separated from other factors, such as water temperature and day of the year. In addition, the mortality measured in that study is the product of mortality from all sources. These include effects of water temperature, predation, disease, and others. It appears these data were only used to develop the scalar for the “survival per distance traveled” component. As such, when this component is used together with the temperature and disease scalars, the total mortality may be overestimated by accounting for mortality from these sources more than once. It may be simpler to have a different scalar for smolt survival in reaches

upstream from the Scott River than those downstream, with each scalar being a simple combination of mortality from all sources. This method avoids making distinctions among the effects of discharge, temperature, and disease, but would also eliminate evaluating the effects of the various factors in the model. I suggest that until the various scalars can be verified or are based on in-basin data showing causation, comparisons of the effects of discharge and temperature in model outputs are not appropriate.

One of the factors in the smolt migration scalars is the survival per distance migrated, but the radio-tag study has shown that this is different in different river reaches. The effects of discharge at IGD on the survival of radio-tagged fish appear to be influenced by the time they spend in a reach in addition to the length of the reach. However, it is not clear if this is due to something inherent in the reach itself, or because it is the area the fish first enters the main stem Klamath River and the fish need time in the main stem before downstream migration. These data are based on both wild fish migrating from the Shasta River and hatchery fish taken directly from a hatchery tank, but the phenomenon is much greater in hatchery fish. There may be other reach-specific factors as well. Fish making the transition from other tributaries to the main stem may also have a lag between entering the main stem and rapid downstream migration. Thus, the survival of Scott River fish in the Scott River to Beaver Creek reach may be similar to Iron Gate Hatchery fish in the hatchery to Scott River reach, based on this lag. It appears the disease scalar is an attempt to incorporate the lower survival in the upper reaches than in lower ones, but the effect may not be due to disease.

One of the benefits of such a model is the integration of the effects of many factors affecting survival. For example, the model suggests increases in Iron Gate Dam discharge late in the summer have a benefit for survival of downstream migrants, but have detrimental effects on adult salmon downstream from the Trinity River. This is an outcome of faster downstream migration, but warmer water for adults to migrate through. This is one example of the gross trends such a model may be useful for: it allows one to look at potential effects on a more holistic basis than is often considered.

The remaining comments are specific to the report:

Page 65 – findings (from sensitivity analyses) – should be phrased to indicate the findings are from model predictions, not that they were determined empirically.

Page 75 – probably cannot separate influence of IGD flow from those of tributary flow, as it seems logical they would be linked in nature. One could surmise, using the same method, that boosting IGD flows during periods of low tributary flows could have a large effect on main stem water temperatures, etc. However, it may be a good idea to have the two factors separated in the model in the event that such data does become available. There is currently great uncertainty about the how effects from these factors should be divided.

pages 80/81 – the effects of temperature, discharge, and smolt survival are generally inseparable in empirical data, and therefore it does not seem appropriate to separate them

in the model. Doing so is mathematically possible, but the effects of the separate variables cannot be supported by empirical information, and doing so in the model serves to increase the uncertainty in its output.

Page 81 – the reduced effects of flow in downstream reaches relative to upstream reaches is, and the authors point out, supported by empirical data. The causes are not clear, but appear to be related in some fashion to the different amounts of time the fish spent in each of the reaches – more time, lower survival.

Page 85 – note smolt survival to ocean....

Page 93/94 – The statements in the text regarding the resilience of coho populations to poor ocean conditions and dry water years are very risky. They assert that, based on the model and the multitude of disparate data sources it is founded on, coho populations “could withstand 10 straight years of worst case freshwater conditions combined with 10 straight years of poor ocean conditions”. The cost of being wrong in this assertion could be catastrophic for the SONCC coho ESU. The data do, however, serve to show the relative effects of ocean conditions on smolt production. Data in Figures 63 and 64 suggest coho smolt production would decline over time in either 2001 or 2004 water years with ocean survivals of 2.0%.

Page 101, paragraph 4: how can increasing ocean survival rate decrease smolt production?

Page 104 – summary of population performance, last bullet: the distance may not be the proximate factor affecting smolt survival in various reaches. The time fish spend within the reach appears to be important. This is primarily true for the upper reaches, where radio-tagged fish spend much of their time prior to downstream migration at a “fast” rate.

Other notes:

Defaults – hatchery smolt post-release survival – 0.5?

Shasta Parr capacity (~11,000) much lower than many others (even Bogus Creek)? Is Shasta smolt production set at the carrying capacity?

IGH smolt releases – “we assume they do not rear in the main stem and do not affect survival of naturally-produced smolts”. Available RT data suggests spring-release coho from IGH can remain upstream from the Scott River for over a month.