# Response-to-Comments Document

**for the**

**August 2008 Peer-Review Draft of the**
**Yuba River Redd Dewatering/Juvenile Stranding Instream Flow Study Report**

**September 2010**

## AUTHORS RESPONDING TO COMMENTS

**U.S. Fish and Wildlife Service**

Mark Gard

## PREFACE

This document contains the comments provided by scientific peers on the August 2008 draft of the report, “Relationships between flow fluctuations and redd dewatering and juvenile stranding for spring and fall-run Chinook salmon and steelhead/rainbow trout rearing in the Yuba River” (Report), and responses to those comments. This compilation is divided into subject-matter sections whereby various comments and responses to authors were organized. To the extent that individual comments crossed over subject matters, the authors collectively addressed those comments. This response to comments only includes responses that are relevant to this report.

Although this compilation may provide useful insight into how the comments were addressed by the authors, the Report itself represents the complete and final synthesis of studies on salmonid redd dewatering and juvenile stranding in the Yuba River, based on the best available scientific information. The authors have reviewed their responses and compared them to the final Report to ensure that all comments have been adequately addressed.

Lastly, the authors of the Report wish to thank everyone who provided comments on the August 2008 draft. The comments greatly assisted the authors and agency in identifying missing or unclear information, focusing the textual and graphic presentations, and thereby producing a better overall Report. Four anonymous reviewers were provided by the CALFED Ecosystem Restoration Program. The fifth peer reviewer (Hal Beecher**,** Washington Department of Fish and Wildlife, Olympia, WA) was provided under a contract with Sustainable Ecosystems Institute.

**TABLE OF CONTENTS**

## LIST OF ACRONYMS

| | |
|---|---|
| 1-D | One dimensional |
| 2-D | Two dimensional |
| 3-D | Three dimensional |
| ADCP | Acoustic Doppler Current Profiler |
| AV | Adjacent Velocity |
| BB_ADCP | Broad Band Acoustic Doppler Current Profiler |
| CVPIA | Central Valley Project Improvement Act |
| COE | Corps of Engineers |
| D | Depth |
| GIS | Geographic Information System |
| HSC | Habitat Suitability Criteria |
| HSI | Habitat Suitability Index |
| IFG4 | Instream Flow Group Program 4 |
| IFIM | Instream Flow Incremental Methodology |
| MANSQ | Mannings Equation Discharge (Q) Simulation Program |
| Max F | Maximum Froude Number |
| Net Q | Net Flow |
| PDF | Portable Document Format |
| PHABSIM | Physical Habitat Simulation Model |
| Q | Flow |
| QI | Quality Index |
| R | ADCP velocity quality control check statistic |
| R | Pearson’s correlation coefficient |
| RIVER2D | Two dimensional depth averaged model of river hydrodynamics and fish habitat |
| RTK GPS | Real Time Kinematic Global Positioning System receiver |
| S | Stage |
| SCUBA | Self-Contained Underwater Breathing Apparatus |
| Sol Δ | Solution change |
| SZF | stage of zero flow |
| USGS | U.S. Geological Survey |
| V | Velocity |
| VAF | Velocity Adjustment Factor |
| VEL | Velocity |
| WDFW | Washington Department of Fish and Wildlife |
| WSEL | Water Surface Elevation |
| WUA | Weighted Useable Area |

## GENERAL COMMENTS

**Hal Beecher**

**Comment 1:** Generally, the report is a straightforward approach to a common concern in regulated rivers. It provides good coverage of the river (not just one or two "representative" sites as often done by others). It provides some clear guidance on flow management options to minimize stranding of juveniles and redds. Some ad hoc approaches were needed in a few places to develop site-specific relationships between flow (Q) and water surface elevation (WSEL or S [stage]), and these seemed reasonable.

**Response:** No response needed.

**Comment 2:** As with the rearing paper, the organization included splitting Habitat Suitability Criteria (HSC) Development into Methods, Results, and Discussion, but they could all be viewed as part of the Methods aimed at the overall objective of assessing stranding flows. Alternatively, HSC Development could have been a separate chapter. Either way, the central focus of the report might be more effectively highlighted. However, this study is much more straightforward than the rearing paper, so I did not find the organization as distracting in the stranding report as in the rearing report. The remainder of this review will address specific comments.

**Response:** We have patterned the format of our reports as closely as possible to that of peer-reviewed journal articles. In this regard, we note that a peer reviewer from the first peer review of the Yuba spawning report stated "All information presented, including data, in the methods section that is actually a result should be extracted and discussed in the Results section." As a result, we have placed all data in the results section for the Yuba flow fluctuations report as well.

**Comment 3:** Hydrographs (or tables) would be helpful to show the natural and present flow regime. It would also be helpful to indicate spawning and incubation timing.

**Response:** A description of the historic and contemporary hydrology of the Yuba River watershed, including displaying both recent hydrographs and pre-project or reconstructed natural hydrographs, will likely be developed as part of the relicensing of the Yuba River hydropower project. We have added a table indicating spawning and incubation timing.

**REVIEWER #1**

**Study Design -** Is the study design sound?

**Comment 1:** The reports do not have a study design per se. They are exercises in model building, calibration and validation. Criteria are presented by which the model suitability will be judged. Whether these are adequate criteria is not well described. The authors simply state what the criteria are to determine validity and then use them to assess the results.

**Response:** Table 1 (taken as a whole) summarizes the study design. Further details on the study design are given in the methods section. Model building, calibration and validation are three components of the study design. The adequacy of the criteria by which the model suitability was judged is documented in the methods section by literature citations. In addition to stating what the criteria are to determine validity, we also provide literature citations for the criteria.

**Methods -** Are the methods technically sound?

**Data** - Is the data adequate?

**Comment 2:** The data are what they are. In some cases they had larger sample sizes than in others.

**Response:** Sample sizes ranged from depth and tailspill depths on 106 steelhead/rainbow trout redds to velocities measured on 851 fall-run Chinook salmon redds.

**Comment 3:** Note that sometimes in the report data are plural and in other places data are singular. I prefer data are plural, but at a minimum authors should be consistent in their choice of singular or plural.

**Response:** We have reviewed and revised the entire report to ensure that we consistently refer to data as plural.

**Presentation -** Is the presentation clear?

**Comment 4:** Parts of the reports, especially those describing in technical terms and lingo the model calibrations and measurement techniques are not clear to the uninitiated reader. It has been standard in professional documents for decades that units should be consistent and normally should be presented as metric and, if not metric, then with metric equivalents in parentheses. At a minimum, reports should not use one system in some places and the other system in other places and even mix them in the same table or figure.

**Response:** We reviewed the portions of the report referred to by the commenter and clarified these sections where possible. The report is unavoidably, due to the content, only understandable by a reader who is familiar with modeling calibration and measurement techniques used in instream flow studies. The data is primarily presented in English units to make the data more understandable to the intended audience, decision makers and stakeholders in the Yuba River basin, who are most familiar with data expressed in English units. We have gone through the report and given metric equivalents in parentheses, except for flows. We have kept flows entirely in English units since flow data is generally presented in English units in the United States. We also note that it is standard to present data in instream flow study reports in English units.

**<u>Figures and tables</u>** <u>- Are the figures and tables clear, complete and adequate?</u>

**Comment 5:** Readability and ability to interpret many of the figures would be improved by the addition of a few vertical gridlines and in some cases horizontal gridlines.

**Response:** We chose not to use vertical and horizontal gridlines because it would be difficult to distinguish between the data presented in the figures and the vertical and horizontal gridlines. Examples include Figures2 to 13.

**Miscellaneous comments:**

**Comment 6:** The reports switch between metric units and English units with no mention of the equivalent in the other unit. Sometimes this occurs even within a single table or figure such as Table 2 in the Yuba spawning report.

**Response:** We changed Table 3 so that all of the data in that table are in metric units. We have gone through the report and given metric equivalents in parentheses, except for flows. We have kept flows entirely in English units since flow data is generally presented in English units in the United States.

**Comment 7:** There is no acknowledgement of what historic flow regimes were like and under what type of conditions the salmon evolved. The reader has no idea whether the flows suggested are similar to or totally different from what was historic flow when presumably the salmon populations were more vibrant. Recent work on salmon habitat often takes into account how flows have changed given dams and land use and how that may affect the ability to recreate suitable habitat for salmon.

**Response:** An analysis of historic flow regimes will likely be developed as part of the relicensing of the Yuba River hydropower project. The purpose of this report is only to identify the relationships between flow and redd dewatering and juvenile stranding. The report does not suggest flows, but instead notes how changes in flow affect redd dewatering and juvenile stranding. This report is only one part of the information that will be used to develop flow regimes for the Yuba River. The development of flow regimes for the Yuba River will undoubtedly also take into account how flows in the Yuba River have changed given dams and land use and how that may affect the ability of recreate suitable habitat for salmon.

**Comment 8:** Much of the language used to describe the model is vague, e.g. 'we feel there is no significant limitation', 'we conclude' but state no reason why, 'in general, figures are similar'. The reader would have much higher confidence in the conclusions if the authors used terminology that was more precise. For example, x % of the figures are within x % of similarity. There is much reliance on statistical significance that may or may not have biological significance. As I read the discussions in each report, there seems to be no anomaly or discrepancy between the model and measured values that the authors can't explain away or declare as unimportant. And when, for example, the given criteria for acceptance were not met, the authors conclude that is acceptable anyway (e.g., section 3.1 p 67: Rearing report).

**Response:** It is important to distinguish between the numeric criteria that were used to evaluate model performance and the text that was used to elaborate on the model performance. The numeric criteria are specific and precise. For example, for developing stage/discharge relationships the numeric criteria were that: 1) the beta value (a measure of the change in channel roughness with changes in streamflow) is between 2.0 and 4.5; 2) the mean error in calculated versus given discharges is less than 10%; 3) there is no more than a 25% difference for any calculated versus given discharge; and 4) there is no more than a 0.1 foot (0.031 m) difference between measured and simulated WSELs. By necessity, the text that was used to elaborate on the model performance was qualitative and designed to illustrate general trends in the data. The statistical tests that were used have biological significance. For example, the correlation between the depth of the redds and the difference between redd depth and tailspill depth has the biological significance that fish that spawn in deeper water construct redds with higher tailspills. We feel it is important to try to determine what was responsible for anomalies or discrepancies between modeled and measured data and to evaluate the significance of these in terms of the ultimate model output (the flow fluctuation-redd dewatering/juvenile stranding relationships) – our intent is not to explain the anomalies or discrepancies between modeled and measured data away. Our intent is not to conclude whether or not a model is acceptable – rather our intent is to characterize the level of uncertainty in model output as a function of anomalies or discrepancies between modeled and measured data.

**Comment 10:** Overall the report uses mushy terminology and avoids giving the reader any quantifiable definitions of how the model performs, whether it performs better than previous models, and within what accuracy does it represent on the ground physical and biological data.

**Response:** The terminology in the report is as precise as possible to describe the overall trends in the data. The report provides numerous quantifiable definitions of how the model performs and within what accuracy it represents on the ground physical and biological data. Examples of the quantifiable definitions include: 1) the beta value (a measure of the change in channel roughness with changes in streamflow) is between 2.0 and 4.5; 2) the mean error in calculated versus given discharges is less than 10%; 3) there is no more than a 25% difference for any calculated versus given discharge; and 4) there is no more than a 0.1 foot (0.031 m) difference between measured and simulated WSELs. Additional data presented in the report on the accuracy with which the model represents on the ground physical and biological data include the data presented in Appendix B. There are no previous models for redd dewatering and juvenile stranding.

## **REVIEWER #2**

**Study Design** Is the study design sound?

**Comment 1:** The study design is simple and elegant. The report sets out to determine the impact of flow fluctuations on redd dewatering and juvenile stranding. By comparison to the rearing and spawning studies, this is a very concise study that does a nice job of (remainder of sentence missing).

**Response:** No response required.

**<u>Methods</u>** <u>Are the methods technically sound?</u>

**Comment 2:** The majority of the methods are similar to those reported in the other reports and repetitive in that regard (although technically sound). Thus, refer to review comments in other reports for the overlapping methods. This study employs methods which play with the 2D model results from the data to build new habitat suitability model results, which adjust the HSC according to likelihood of redd dewatering and stranding potential. The methods the authors developed here are novel and interesting. I do wonder about the interpretation that anything under 0.2 feet of water depth would necessarily translate to dewatering of a redd. Depending on the timing of the stranding, so long as there is ground water in the gravels above the burial depth (even if dry at surface), aelvin may still be able to emerge as long as the groundwater is hydraulically connected to the main channel. All the same, this is a finer point and the 0.2 feet threshold would essentially be a conservative estimate of dewatering potential. In the context of establishing flows to minimize that potential, it makes sense.

**Response:** We have reviewed the comments from the spawning and rearing reviews. The relevant comments from these reports addressed the topographic densities used for the 2D modeling. See responses below regarding the topographic data quality. The intent of the report is to focus on flow variations with a temporal scale of one month. We would expect that a redd that is dewatered for a month would result in the loss of ground water in the gravels above the burial depth, and thus mortality of the eggs and pre-emergent alevins, and thus used the 0.2 foot threshold as a conservative estimate of dewatering potential.

**<u>Data.</u>** <u>Is the data adequate?</u>

**Comment 3:** The same comments from the spawning and rearing reviews apply to the 2D modeling results here. The new data reported in this study is very interesting and this is the first time I have seen data reported on redd depths versus redd tailspill depths. I liked how they presented the data and used it to make their estimates.

**Response:** We have reviewed the comments from the spawning and rearing reviews. The relevant comments from these reports addressed the availability of raw data in digital format and the topographic densities used for the 2D modeling. We have added information in the preface of the report about how the raw data in digital format can be obtained. The topographic point densities fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 $m^2$, while Jacobson and Galat (2006) had a point density of 6 points/100 $m^2$. This study was one of our earlier River2D studies and we have been using higher point densities in more recent studies to try and improve the hydraulic predictions of our River2D models. We have been able to use higher point densities in our more recent studies because our new equipment (robotic total station and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 $m^2$) within our time constraints for data collection. We also are not aware of any other data comparing redd depths and tailspill depths. We have found the data presentation methods to be an effective means of explaining the results of the analyses.

**Findings, interpretations and conclusions.** Are the findings, interpretations and conclusions valid?

**Comment 4:** The discussion is rather light, as with the other reports. It could be expanded slightly to include better and more explicit references to the peer-reviewed literature to substantiate its conjecture. As I don't know the literature on stranding and dewatering as well, I can't make specific recommendations as to what to cite. My sense is that this is rather novel and there is much smaller literature out there. By contrast there is a rich literature on redd survival, which uses rather different techniques to get at survival than those employed here. Fundamentally, the concern over stranding or dewatering is implicitly linked to survival. Thus, it would be interesting to see a discussion contrasting the information that this provides with traditional egg-tube survival studies (for example).

**Response:** We have expanded the discussion to include better and more explicit references to the peer-reviewed literature (Devries 1997, McMichael et al. 2005, Miller et al. 2008). Several of these (McMichael et al. 2005, Miller et al. 2008) look at redd survival.

**Presentation.** Is the presentation clear?

**Comment 5:** See comments from other reports.

**Response:** We have reviewed the comments from the spawning and rearing reviews. The relevant comments from these reports were as follows:

> "The presentation of the report is consistent and the layout of this report is logical. The introduction could provide a little broader scientific and management context (rather brief as it stands), but I'm not sure that it is necessary so much as a missed opportunity. The lack of conclusion section is a little odd and leads to an abrupt ending after the discussion. I maintain my comment in the executive summary that the links between this report and the other reports are not clear. I do wonder if these reports could not have been more concisely combined into one uber-report (a lot of material is repetitive)."

The introduction provides a broad scientific and management context to support the purpose of the report. We have added a conclusion to the report. The links between this report and the other reports are implied in the preface to all three reports – specifically, that the purpose of all three reports is to provide scientific information to the CVPIA Program to assist in developing recommendations for instream flow needs for anadromous fish in the Yuba River. Each report, plus appendices, is voluminous and intended to stand on its own.

**<u>Figures and tables.</u>** Are the figures and tables clear, complete and adequate?

**Comment 6:** Unlike the reviews in the other reports, I have no problems with the figures presented in this report. I particularly like figures 5-12 and appreciate the tables of the raw calculations in the appendix.

**Response:** No response required.

**Miscellaneous comments:**

**Comment 7:** Of all the reports, this is the one that I think could be turned into an interesting journal article. It would require a little clearer focus up front on the scientific question, but strikes me as a more novel contribution. However, in the context of these collective Yuba reports, I still think this could be stripped down to its basics and act as its own chapter (and just reference earlier chapters on the repetitive aspects of model development) in a larger report.

**Response:** We will take the commenter's advice into consideration if we decide to turn the report into a journal article. We do not feel that it would be practical to physically combine all three Yuba River reports, given how voluminous each of the three reports is.

**<u>REVIEWER #3</u>**

**<u>Study Design</u>** <u>Is the study design sound?</u>

**Comment 1:** Not entirely. The authors attempt to model the effects of flow reductions of certain magnitudes on 1) the potential to harm either incubating or intragravel rearing embryos/alevins, and 2) the potential to harm post-emergent rearing juvenile salmonids by stranding and/or entrapment. The authors make some assumptions which are very conservative. For example, they assume that spawning habitat is dewatered if the top of the redd tailspill is exposed as well as some metrics related to the difference of the redd depth and tailspill depth, and finally velocities measured at redds. They do not provide enough information regarding the time periods of data collection that are relevant here. More information is required to assess whether the assumptions made by the authors are justified. As the authors summarily discuss near the end of the report, salmonid embryos and alevins can withstand some periodic dewatering. They also selected the shallowest egg pocket depths from the Chapman et al. (1986) work to try to support their use of the top of the tailspill as the appropriate benchmark for assessing impact. Chapman et al. (1986) found that most egg pockets of fall Chinook salmon were actually 1 foot below the undisturbed river bed, which is much deeper than the 0.3 feet these authors site as the minimum reported in Chapman et al. The authors should rethink the criteria they used for assuming negative impact based on the work in the published literature. A method of tying the developmental stage (using degree days after fertilization) to the flow fluctuations would be more defensible, as it has been clearly shown that pre-hatch salmonid embryos can withstand dewatering for extended periods and that post-hatch embryos are more susceptible. McMichael et al. (2005) capped Chinook salmon redds in areas that were dewatered (their definition of dewatering was that the WSEL was lower than the surface of the substrate at the redd location) after hatching survived to emergence at fairly high levels (16.9-66.6%, mean = 29.2%).

**Response:** We agree that the assumptions are conservative. Information on the time periods of data collection are given in U.S. Fish and Wildlife Service (2010). We have added additional information to the discussion that can be used to assess whether the assumptions are justified. Specifically, Devries (1997) in a literature review found considerable variation in estimates of egg burial depths, ranging from 0 to 0.80 m. Given the uncertainty as to the location of the egg pockets within the redd, we believe that exposure of the tailspill is a reasonable conservative estimate of reduced survival. Given that our analysis is based on flow fluctuations with a temporal time scale of one month, it does not appear that consideration of the developmental stage would change our analysis. McMichael et al. (2005) found a survival rate averaging 29.2% from eggs to fry for redds which were dewatered 3.1% of the time during the posthatch intragravel rearing period. In contrast, since our dewatering analysis is based on flow variations with a temporal scale of one month, and thus dewatering during 100% of the time during the posthatch intragravel period, we would expect no survival of eggs and pre-emergent alevins for our redd dewatering estimates.

**Comment 2:** Further, the authors assumed that areas of the riverbed that were isolated from the main channel were 'stranded'. They did not provide adequate study area description to educate the reader about the flow operations in these river sections. I am unclear on whether there are large daily elevation changes – or whether they are typically more gradual seasonal changes. In situations where there are large daily fluctuations (e.g., the Hanford Reach of the Columbia River) due to load-following operations at hydroelectric dams, these isolated areas (which are really 'fish isolations' – not stranding locations, even according to these authors' definitions on page 1). If there are daily flow fluctuations that may reconnect these isolated areas there may be no detrimental effects to rearing juvenile salmonids (e.g., reduced growth or survival). There may even be positive consequences in larger entrapments where water temperatures and food resources are more favorable for growth in the early season and the juvenile salmonids may have refuge from piscivorous predators. Finally, though they do not clearly state this, it appears as if the authors assumed that juvenile salmonids were uniformly distributed throughout all habitats – so that 2000 $ft^2$ isolated in one area would have the same impact as the same area isolated in another location. In other words, the simple assumption –that fish that are isolated from the main channel should be considered a uniformly distributed negative impact – is not well founded.

**Response:** As mentioned in the introduction, the report focuses on flow variations with a temporal scale of one month. This focus was based on the lack of large daily elevation changes in the Yuba River, but instead the presence of more gradual seasonal changes. Since we are focusing on more gradual seasonal variations, it is reasonable to expect overall negative consequences for juveniles that are trapped in isolated areas because a one month period of isolation would likely be sufficient to result in increased mortality from elevated water temperatures or avian predation. This study made no assumptions about the distribution of juvenile salmonids among different habitats. We do not have any data on how juvenile salmonid distribution varies through all stranding areas, and thus were left with an area-based metric for juvenile stranding, rather than a fish-based metric (i.e., number of fish stranded with a given drop in flow).

**Methods** Are the methods technically sound?

**Comment 3:** Apart from the comments above regarding the assumptions the authors made (which relate directly to methods), I have a few more comments. The discussion of the relative strengths and weaknesses of River2D and PHABSIM seem a little defensive and would probably best be moved to the discussion section. The methods section should be limited to what was done.

**Response:** We have deleted the discussion of the relative strengths and weaknesses of River2D and PHABSIM.

**Comment 4:** A typical hydrograph in the study area description would help the reader who is not familiar with the flow operations in the Yuba River. Case in point, I do not know whether there are daily flow fluctuations that may have influenced the collection of the habitat suitability data that were used to predict habitat loss or what the potential is for reconnecting entrapped areas before water temperatures became lethal, etc…

**Response:** A description of the hydrology of the Yuba River watershed, including displaying recent hydrographs, will likely be developed as part of the relicensing of the Yuba River hydropower project. U.S. Fish and Wildlife Service (2010) presents flow data prior to and during the collection of the habitat suitability data that were used to predict habitat loss – this flow data does not show daily flow fluctuations. Since the report focuses on flow variations with a temporal scale of one month, there is little potential for reconnecting entrapped areas before water temperatures or other habitat conditions, such as dessication or avian predation, result in juvenile mortality.

**Comment 5:** Was the drainage rate of entrapped areas measured or assumed to be uniform throughout? Or, alternatively, the authors may have assumed that any water that was isolated from the main channel for any period of time should be considered detrimental to salmonid productivity? The authors might consider a more thorough approach to estimating production impacts related to flow fluctuations that take into account recent published information, fish distribution and developmental stage data, and things like predation susceptibility or increased/decreased growth.

**Response:** Drainage rates of entrapped areas were not measured and no assumption were made about the variation of drainage rates within or between study sites. Since the report focuses on flow variations with a temporal scale of one month, we assumed that any water that was isolated from the main channel for a month should be considered detrimental to salmonid productivity. Our goal was to establish the presence of flow thresholds; therefore, estimating production impacts would not have affected the conclusions of the report.

**Comment 6:** The ‘stranding’ flow should be at the bed elevation, not 0.1 feet above. That is over an inch deep and these early rearing fish will pass through and even use these habitats, even if HSI data do not include those values. These fish are unlikely to remain in these shallow areas if a person is standing or snorkeling nearby.

**Response:** We have no data that suggest that early rearing fish will pass through areas with depths of less than 0.2 feet. Our observations of undisturbed fish indicate that anadromous salmonid fry do not rear in areas with depths less than 0.2 feet. Our assumption is that when the depth at the stranding point drops below 0.2 feet, any fish present in the stranding area will move farther into the stranding area where depths are greater than at the stranding point. As a result, we assume that fish will not volitionally leave stranding areas when the depth at the stranding point drops below 0.2 feet.

**Comment 7:** Regarding the ‘tailspill’ exposed criteria **–** and the ‘insufficient intragravel flow’ **–** these criteria need to be updated to take advantage of published literature regarding the relative vulnerability of the different developmental stages of incubating embryos to alevins. A more comprehensive criteria might use something like less susceptibility to dewatering before 500 degree days and more susceptibility for dewatering occurring after 500 dd. Also, see comments above regarding my disagreement with the author’s use of the top of the tailspill as the benchmark. I believe that undisturbed substrate adjacent to the redd or even 30 cm below undisturbed substrate may be more biologically relevant and supported by the literature if they are assigning binary fates to these redds.

**Response:** Given that our analysis is based on flow fluctuations with a temporal time scale of one month, it does not appear that consideration of the developmental stage would change our analysis. McMichael et al. (2005) found a survival rate averaging 29.2% from eggs to fry for redds which were dewatered 3.1% of the time during the posthatch intragravel rearing period. In contrast, since our dewatering analysis is based on flow variations with a temporal scale of one month, and thus dewatering during 100% of the time during the posthatch intragravel period, we would expect no survival of eggs and pre-emergent alevins for our redd dewatering estimates. Devries (1997) in a literature review found considerable variation in estimates of egg burial depths, ranging from 0 to 0.80 m. Given the uncertainty as to the location of the egg pockets within the redd, we have used the exposure of the tailspill as a conservative estimate of reduced survival.

**<u>Data.</u>** <u>Is the data adequate?</u>

**Comment 9:** See comments above. For the most part the modeling methods seem appropriate, though I do not have expertise in the application of the models. Figures 5 and 6 captions indicate they are presenting stranding of salmonids data when in fact they are presenting estimates of the area they predict (based on the model output) would be isolated from the main channel. The caption should be reworded to reduce the chance of misleading readers. Similarly, Figures 7-12 should say <u>predicted</u> dewatering of redds. Similarly, Table 9 should say ‘predicted’.

**Response:** See responses to comments above. We have made the suggested changes to the figure and table captions.

<u>**Findings, interpretations and conclusions.**</u> <u>Are the findings, interpretations and conclusions valid?</u>

**Comment 10:** The interpretations and conclusions are what I would consider to be pretty far out on the 'worst case' end due to problems with the application of the study design described above. I think the author's discussion of their selecting of habitat suitability criteria was weak. It appears they selectively picked values from the literature that would lead to the greatest estimate of impact. The last sentence of the concluding paragraphs illustrates a very simple understanding of the variables that may influence redd site selection in salmonids. The published literature should be reviewed and considered prior to making statements such as this.

**Response:** As discussed above, the application of the study design considers uncertainties in egg burial depths and focuses on flow fluctuations with a temporal scale of one month. We have expanded the discussion to include additional material from the literature regarding the habitat suitability criteria that were selected. We are assuming that the commenter is referring to the following sentence:

> "We do not consider this to be likely in the Yuba River - if there had been conditions where downwelling currents had provided sufficient intragravel velocities at low mean water column velocities, we would have expected to find salmon and steelhead constructing redds in such situations."

We acknowledge that there is a very complex relationship between environmental variables and salmonid redd site selection. However, we feel that the above sentence is a logical conclusion. Given the complexities of the relationships between mean column and intragravel velocities, we do not know of any other method of selecting a mean column velocity criteria to use for the redd dewatering analysis.

<u>**Presentation.**</u> Is the presentation clear?

**Comment 11:** The presentation would benefit from a more well-developed description of the study area, including current flow fluctuation examples and any existing constraints on flow operations.

**Response:** A description of current flow fluctuation examples and any existing constraints on flow operations will likely be developed as part of the relicensing of the Yuba River hydroelectric project.

<u>**Figures and tables.**</u> Are the figures and tables clear, complete and adequate?

**Comment 12:** See comments above regarding captions in figures and tables.

**Response:** We changed the captions in the figures and tables as described in the above response to comments.

**Miscellaneous comments:**

**Comment 13:** I think the intent of the authors and the funding agencies is very good. I see room for improvement in the study design, criteria used, and interpretation of the results.

**Response:** See responses to comments above regarding study design, criteria used and interpretation of results.

## <u>REVIEWER #4</u>

**Overall Review Comments:**

**Comment 1:** The focus of my reviews was on the hydraulic modeling aspects described in the reports. Because all of the reports describe essentially the same hydraulic modeling methods, my comments below generally apply to all the reports. Where I have a comment specific to one report, the report is identified by the number (1) through (6) given above. More detailed comments are provided in the electronic PDF version of each report.

**Response:** No response needed.

**Comment 2:** The authors are to be commended for their efforts in undertaking some complex flow-habitat studies. It is clear that a tremendous amount of thought and work went into the execution of these studies.

**Response:** No response needed.

**Comment 3:** With the exception of the Executive Summary (2), the reports were very difficult to read; not because of their length or technical content, but because they are poorly organized. The reports provide a very inadequate introduction and background to the studies undertaken, which results in the reader having a very limited understanding of the what/where/why of the study. Because of this, there is no clear link identified between study objectives and some need for the study; and subsequently, no understanding of how the results are to be used, or what their relevance is.

**Response:** We have patterned the organization of our reports after that used in the peer-reviewed literature. We have added additional material to strengthen the introduction. In the preface of the report, the reason for conducting the instream flow study is stated as to provide scientific information to assist in developing instream flow needs for anadromous fish, as required by Section 3406(b)(1)(B) of the Central Valley Project Improvement Act.

**Comment 4:** The poor organization of the report contents continues beyond the introduction section. Throughout the reports, too much detail is given where none is needed and not enough detail is given where more is warranted; study area/site descriptions are dispersed; methods are combined with results; results are combined with discussion; discussion sections contain rationale for methodological flaws, rather than focusing on discussion and interpretation of results; and no clear conclusion sections are provided where the authors would summarize the relevance and application of the major findings. In general, the reports seem to be very disjointed. One of the benefits of writing an agency report for these types of studies is that a lot of detail can be included; this benefit can also become a drawback when the detailed information is presented in a disorganized manner, and/or when some of the details that should be presented are omitted.

**Response:** The amount of detail in the report reflects peer reviews of previous reports. We have added additional details in response to this peer review. Study area/site descriptions are given where needed to provide information for specific portions of the report. We disagree that methods are combined with results and that results are combined with discussion. In this regard, we note that a peer reviewer from the first peer review of this draft stated "All information presented, including data, in the methods section that is actually a result should be extracted and discussed in the Results section." As a result, we have moved all data to the results section. We feel that it is important for the discussion section to address the reasons for model errors as well as discussion and interpretation of results. We have added a conclusion section to this report. We disagree that the report is disjointed; instead, the format of the report follows as closely as possible to that of peer-reviewed journal articles. As noted above, we disagree that the detailed information is presented in a disorganized manner. We have added details in response to the peer review of this report.

**<u>Study Design</u>** <u>Is the study design sound?</u>

**Comment 5:** The study designs seem to be incomplete, as for each study there is not an established link with the need for the study, which should be introduced early in each report. As is, there is no reason established for conducting the studies. In addition, the objectives of the studies need to be more clearly articulated, with a clear connection to the need(s) described in the introductory paragraph(s). At present, it's not clear how or why the study objectives became ones of producing habitat-discharge models. Therefore, it is unknown whether or not the study designs are sound (or complete).

**Response:** The need for an instream flow study is given in the preface of the report. Specifically, as noted in the preface, the reason for conducting the instream flow study is to provide scientific information to assist in developing instream flow needs for anadromous fish, as required by Section 3406(b)(1)(B) of the Central Valley Project Improvement Act. The needs described in the previous rewritten paragraph (improved flows for all life history stages of Chinook salmon and steelhead as a high priority action to restore anadromous fish populations in the Yuba River) are intended to relate to the objective of developing flow fluctuation/redd-dewatering and juvenile stranding models, since flow fluctuation/redd-dewatering and juvenile stranding models provide critical information to use in determining the magnitude of improved flows for all life history stages of Chinook salmon and steelhead. The study objective of producing flow fluctuation/redd-dewatering and juvenile stranding models is based on standard methods used to identify instream flow requirements.

**Comment 6:** The focus on spawning and rearing habitat in these studies is unfounded, because habitat capacity for those life stages has not been established as being a limiting factor contributing to the fish population declines described in the introductory narrative. Some coherent explanation needs to be provided that justifies the focus on habitat limiting factors.

**Response:** To our knowledge, the data that would be needed to establish that habitat capacity for spawning is a limiting factor contributing to fish population declines does not exist. For example, data is lacking to be able to determine whether doubling the amount of spawning habitat would double the populations of anadromous salmonids. The preface is intended to provide a coherent explanation that justifies the focus of the study on flow-habitat relationships. Specifically, as noted in the preface, the reason for conducting the instream flow study is to provide scientific information to assist in developing instream flow needs for anadromous fish, as required by Section 3406(b)(1)(B) of the Central Valley Project Improvement Act.

**Objectives** Are the objectives clear?

**Comment 7:** The objectives are clear, in that they are stated in the introductions and in a table format. However, as described above, it is unclear if these are the correct objectives (or if the objectives are complete), because the need (i.e., the questions to be addressed by the studies) of each of the flow-habitat studies has not been clearly established.

**Response:** As noted in the preface, the reason for conducting the instream flow study is to provide scientific information to assist in developing instream flow needs for anadromous fish, as required by Section 3406(b)(1)(B) of the Central Valley Project Improvement Act.

**Methods** Are the methods technically sound?

**Comment 8:** It is unclear whether or not the hydraulic modeling methods were technically sound. With the information provided in the reports, it seems that the hydraulic modeling results are unreliable, principally because of: poor representation of riverbed elevations given the low sampling density; poor explanation of the accuracy of the elevation data, relative to the benchmarks and the survey data themselves, not to the instruments used; poor correlation between measured and simulated velocity; unusually high Froude numbers predicted along the channel margins. (see the individual reports for more specific comments).

**Response:** We would characterize the hydraulic modeling results not as unreliable, but rather as having a level of uncertainty due to factors such as sampling density. The topographic point densities fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 $m^2$, while Jacobson and Galat (2006) had a point density of 6 points/100 $m^2$. This study was one of our earlier River2D studies and we have been using higher point densities in more recent studies to try and improve the hydraulic predictions of our River2D models. We have been able to use higher point densities in our more recent studies because our new equipment (robotic total station and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 $m^2$) within our time constraints for data collection. For this report, we do not have any information on the accuracy of the elevation data, relative to the benchmarks and the survey data themselves. Issues regarding correlations between measured and simulated velocities and high Froude numbers are not relevant to the flow-fluctuation study.

**Comment 9:** The hydraulic modeling efforts in these studies are primarily focused on predicting local hydraulics at the scale of individual redds (or fish locations). In these cases, hydraulic modeling research has shown that the computational mesh and topography resolution (density of computational nodes and density of topographic data, respectively) should be similar to the spatial scale and resolution at which the hydraulic predictions are being applied (i.e., redds and fish locations in this study). The density of riverbed elevation data, and subsequent mesh resolution, for these studies appear to be too sparse to accurately model local hydraulics at the scales of interest. Similarly, the application of a constant friction coefficient (roughness) across the model domain, as used in these studies, contributes to poor prediction in local scale hydraulics. The comparisons of measured vs. modeled velocities in these studies demonstrate poor model performance (and plots of measured vs. modeled velocity vectors are not provided).

**Response:** We used as fine a computation mesh as possible given constraints on computer run speeds and memory. The topographic point densities fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 $m^2$, while Jacobson and Galat (2006) had a point density of 6 points/100 $m^2$. Accordingly, our computational mesh and topography resolution were as close as possible to the spatial scale and resolution at which the hydraulic predictions are being applied. We acknowledge that the density of riverbed elevation data, and subsequent mesh resolution will contribute to errors in modeling local hydraulics at the scales of interest, but would characterize this as increasing the uncertainty in the resulting flow fluctuation/redd-dewatering and juvenile stranding relationships. A constant friction coefficient was not applied across the model domain – in fact, we applied a roughness that varied spatially based on substrate size and cover. Comments regarding comparisons of measured and predicted velocities are not relevant to the flow-fluctuation study.

**Comment 10:** Where hydraulic models are applied to predict the bulk flows into and out of a river reach, the model meshes, resolution, and constant roughness coefficients across the model domain like those used in these studies are appropriate and will produce suitable results. This can be seen by this study's results of good matches between modeled and predicted WSEL at the upstream and downstream boundaries of the models.

**Response:** We did not use a constant roughness coefficient. The model also produced suitable results at the scale of individual redds, given the limitations on model mesh and resolution discussed above.

**Comment 11:** Because the hydraulic modeling in these studies is so fundamental to the results and application of the findings, much more emphasis should have been focused on assuring that best modeling practices were followed, with support by citations of the peer-reviewed literature in hydraulic modeling – such citations are noticeably absent.

**Response:** Best modeling practices, in terms of quantifiable definitions of how the model performs, are model-specific. We examined the peer-reviewed literature for papers that used River2D and identified five peer-reviewed articles (Waddle et al 2000, Katopodis 2003, Jacobson and Galat 2006, Gard 2006 and Gard 2009). None of these papers specify quantifiable definitions of how the model performs[1], suggesting that such level of detail is beyond that normally given in the peer-reviewed literature. Accordingly, our only choice is to rely on non-peer-reviewed citations for the definitions (U.S. Fish and Wildlife Service 1994).

**<u>Data</u>** <u>- Is the data adequate?</u>

**Comment 12:** Based on review of the hydraulic modeling outputs, it seems like the underlying riverbed elevation data was inadequate (too low of a measurement density for the rivers studied; unknown survey errors) for accurately characterizing the study sites. In addition, data were not presented, or not available, for comparisons of measured vs. modeled WSEL along the channel centerline (longitudinally) and comparisons of measured vs. modeled velocity vectors (magnitude and direction) along a cross-section or elsewhere in the model domains. Any errors from the hydraulic modeling then propagate through the remainder of the study components that rely on the modeling results (e.g., biological verification, HSI, WUA).

**Response:** The topographic point densities fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 $m^2$, while Jacobson and Galat (2006) had a point density of 6 points/100 $m^2$. This study was one of our earlier River2D studies and we have been using higher point densities in more recent studies to try and improve the hydraulic predictions of our River2D models. We have been able to use higher point densities in our more recent studies because our new equipment (robotic total station and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 $m^2$) within our time constraints for data collection. For this report, we do not have any information on the accuracy of the survey data. We did not collect measurements of WSELs along the channel centerline (longitudinally) or measurements of velocity vectors (magnitude and direction) along a cross-section or elsewhere in the model domains. Accordingly, we are unable to present comparisons of these parameters to simulated values. We agree that errors from the hydraulic modeling then propagate through the remainder of the study components that rely on the modeling results (e.g., biological verification, HSI, WUA); however, the effects of these errors on the modeling results are likely minimal. Specifically, the overall flow fluctuation/redd dewatering relationship is driven by the change in the distribution of depths and

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[1] The only exception to this was Jacobson and Galat (2006), who gave one quantifiable definition (that net outflow was less than 5%). Since we used a more restrictive criteria in the Yuba spawning and rearing reports (1% net outflow), we did not feel it was appropriate to use Jacobson and Galat (2006) as a reference.

velocities with flow. The distribution of velocities would not be affected by over or under-predicted velocities because over-predicted velocities would have the opposite effect on the distribution of velocities as under-predicted velocities.

**Findings, interpretations and conclusions.** Are the findings, interpretations and conclusions valid?

**Comment 13:** There is an incomplete discussion of the findings, interpretations, and conclusions. The Discussion sections should be rewritten to provide a coherent narrative that discusses and interprets the results (focusing on the resulting WUA estimates and associated methodological issues) relative to the work of others in the Yuba River, Clear Creek, and elsewhere for similar study issues. Some of this type of discussion exists in the reports, but not enough. As they currently read, the early parts of the discussion sections are not really a discussion section, but a defensive rationale (structured by the methods headings/subheadings) for methodological issues/flaws/errors that were encountered. Some of the hydraulic modeling interpretations and conclusions are inaccurate or incomplete – see comments above and in the individual reports.

**Response:** The discussion addresses the resulting redd dewatering and juvenile stranding estimates and associated methodological issues. There has not been any work of others on the Yuba River related to modeling redd dewatering and juvenile stranding rates associated with flow fluctuations. We feel that it is important for the discussion section to address the reasons for model errors as well as discussion and interpretation of results. See responses above and below for additional discussion of the interpretation of model results.

**Presentation.** Is the presentation clear?

**Comment 14:** As stated in the overall comments, the reports are difficult to read because they are so poorly organized. The sections of the reports seem to be very disjointed, resulting in very unclear presentations of the information.

**Response:** We intended the format of the report to follow as closely as possible to that of peer-reviewed journal articles.

**Figures and tables.** Are the figures and tables clear, complete and adequate?

**Comment 15:** The figures and tables are clear and adequate. The maps in the appendices would benefit from including scale bars.

**Response:** The appendices of the flow fluctuations report do not include any maps.

## SPECIFIC COMMENTS

## INTRODUCTION

**Hal Beecher**

**Comment 1:** The premise of the study is clearly presented in the Introduction. Mark Hunter's (1992) review of salmonid stranding in relation to natural and regulated flow fluctuation in Washington is worth reviewing and citing. Three assumptions are presented (page 2) for stranding of juvenile salmonids, fry, or eggs. The first assumption is generally reasonable, but it might be helpful to clarify that the stranding point is a connection point for a bay or pool, not just anywhere in the pool, although in the Methods this is clear. The second assumption (and perhaps the first) would benefit from a diagram to show where measurements are taken.

**Response:** We have reviewed and added a citation to Hunter (1992) in the introduction of the report. We have added a clarification of the location of the stranding point. We were unable to develop a diagram that improved the understanding of the measurement locations.

**Comment 2:** The third assumption became confusing: "there would be insufficient intragravel flow through the redd if the mean water column velocity at the redd dewatering flow was less than the lowest velocity at which we found a fall-run and spring-run Chinook salmon and steelhead/rainbow trout redd in the Yuba River." At the dewatering flow, velocity at the redd should be 0, otherwise it would not be dewatered. There is diffusion potential even at a velocity of 0. In a recent paper, Miller et al. (2008) addressed some of the limiting factors for salmonid incubation related to dissolved oxygen and flow, indicating buffering from hypoxia. If fry have water into which they can emerge (disregarding some reported ability for them to move laterally through gravel to reach water where they can emerge), the depth and velocity can be quite low. If flow drops to the level of the redd surface, then rises to a level that is sufficient for emergence at the appropriate time, is there a reduction of emergence? This question is addressed in the Discussion, and the divergence of the literature is a legitimate basis for the protective criteria, but it might have been at least summarized in the Introduction so that the reader would realize the literature was being considered.

**Response:** We are using a more expansive definition of dewatering than just drying of the tailspill. Instead, dewatering refers to increased mortality of eggs or pre-emergent fry due to decreases in flow. Thus, dewatering could occur if the tailspill is still inundated but mean column velocities drop to the point where there would be increased mortality due to insufficient intragravel velocities. We have added a citation to Miller et al. (2008) to the discussion. With regards to this report, we found that the most relevant aspect of Miller et al. (2008) was the finding of reduced growth rates of rainbow trout that were exposed to hypoxic conditions associated with low intragravel flow rates. If there is pre-emergent mortality due to dewatering, we would expect that there would be a reduction of emergence after flows increase, given that we are looking at the effects of flow fluctuations with a temporal scale of one month. We considered adding a summary of the literature in the introduction, but felt that such a summary was most useful in the discussion after the results had been reported.

**REVIEWER #1**

**Objectives -** Are the objectives clear?

**Comment 1:** Some confusion exists in reports between goals, which are the outcomes or the purpose of the activity, and the objectives, which are the tasks done to achieve outcomes. It seems that the goal for each report was to produce a model that predicted some habitat component for some species. A clearer and more easily measurable goal would be something like, produce a model that predicts salmon habitat usage within some stated level of accuracy. When no measurable component of a goal is mentioned, there is no accountability for determining success or failure of the action.

**Response:** We have changed the text of the report to state that the goal of the study was to model the effects of flow fluctuations on Chinook salmon and steelhead/rainbow trout redd dewatering and juvenile entrapment stranding in the Yuba River between Englebright Dam and the Feather River within, to the extent feasible, the levels of accuracy specified in the methods section. The above measureable component of the goal is intended to provide accountability for the level of uncertainty in the flow-habitat relationships. The action should not be viewed in terms of success or failure, but rather in terms of the level of uncertainty of the action. A flow-fluctuation/redd dewatering or juvenile stranding relationship with a high level of uncertainty would not be a failure, in terms of making it unusable, but rather should be viewed within the context of needing to make decisions about flow regimes with imperfect data. The action also needs to be evaluated within the context of alternative sources of information that could be used to make decisions about flow regimes – if the action has less uncertainty than other sources of information, it would be appropriate to use that action to make decisions about flow regimes.

**REVIEWER #2**

**Objectives -** Are the objectives clear?

**Comment 1:** As with the other reports, these are clearly laid out in Table 1.

**Response:** No response needed.

**REVIEWER #3**

**Objectives -** Are the objectives clear?

**Comment 1:** The objectives are clearly stated in Table 1. However, in the concluding paragraphs the authors paraphrase the study objectives into one: “This study supported and achieved the objective of modeling the effects of flow fluctuations on Chinook salmon and steelhead/rainbow trout redd dewatering and juvenile entrapment stranding in the Yuba River between Englebright Dam and the Feather River.” As paraphrased here, I think this is a bit of an overstatement. I think it may be more appropriately reworded to read, “This study supported and achieved the objective of modeling the effects of flow decreases on Chinook salmon and steelhead/rainbow trout spawning habitat and maximum potential juvenile entrapment in the Yuba River between Englebright Dam and the Feather River."

**Response:** We have made the suggested change.

**REVIEWER #4**

**Comment 1:** This introductory paragraph does not provide a clear intro. and background for the study described in the report. For example, where in the world did this study take place?...what is the relationship between the population declines and the need for this study?...why the Yuba River, and where is the Yuba River?...etc. This paragraph should be rewritten.

**Response:** We have added information to the introductory paragraph identifying the location of the study and the Yuba River. We also have added information about anadromous fish population declines and the need for this study. Information on why the Yuba River was selected for a study was added to the subsequent new paragraph.

**Comment 2:** Why model the effects of flow fluctuations? The need for this objective has not been established in this introduction.

**Response:** The link with the need for modeling the effects of flow fluctuations is given in the preface of the report. Specifically, as noted in the preface, the reason for conducting the instream flow study is to provide scientific information to assist in developing instream flow needs for anadromous fish, as required by Section 3406(b)(1)(B) of the Central Valley Project Improvement Act.

**Comment 3:** This paragraph belongs in the Methods section. The assumptions should be placed at the end of the Methods section, after all the methods, study sites, etc. have been described.

**Response:** We feel that it is important to present this material prior to the details on the methods to establish the context of the methods relative to the assumptions underlying the study. Also, since these are the assumptions of the study, rather than the assumptions of the methods, it makes sense to present this material in the introduction.

**Comment 4:** A "Study Area" section should precede the Methods section -- as is, the report provides the reader with a very poor understanding of where this study occurred -- the later narrative of segments and reaches could be placed into the new "Study Area" section.

**Response:** Information on the study area is presented in the first paragraph of the introduction. The material added to the first paragraph of the introduction provides the reader with an adequate understanding of where this study occurred. The section on *Study Site Selection*, which we believe to be what the commenter is referring to regarding the narrative of segments and reaches, most properly belongs in the Methods section, since it describes the methods that were used to delineate the study segments.

## METHODS

**Hal Beecher**

**Comment 1:** The rationale for using River2D over PHABSIM is clearly presented.

**Response:** The material in question was deleted in response to a comment by one of the other peer reviewers.

**Comment 2:** Although there are a number of references in the text to the Narrows, this location is not shown in Figure 1.

**Response:** We felt that it would be too confusing to show the Narrows in Figure 1and instead added information to the text on where it is located relative to Englebright Reservoir, which is shown in Figure 1.

**REVIEWER #4**

**Comment 1:** This entire section on the 2D approach should either be entirely rewritten or eliminated (just tell the reader which model you used and provide references).

**Response:** We deleted all but the first three sentences, which we feel are necessary to explain the process of 2D modeling.

**Comment 2:** As it is currently written, the paragraph provides a very weak justification for using a 2D model vs. a 1D model. If the authors insist on retaining this justification paragraph in the report, then it should be greatly expanded to provide a more thorough description of the alternative modeling techniques, pros/cons, and discussion of the hydraulic modeling fundamentals available from the engineering literature -- all of this should be well cited with peer-reviewed literature from the hydraulic engineering field.

**Response:** We deleted the justification portion of the paragraph in question.

## STUDY SITE SELECTION

**REVIEWER #4**

**Comment 1:** This narrative should be included in a new "Study Area" section, and should be placed after a thorough description of the physical environment where this work took place.

**Response:** This narrative most properly belongs in the Methods section, since it describes the methods that were used to delineate the study segments. We added material to the first paragraph of the introduction to provide the reader with a thorough description of the study site and environment.

**Comment 2:** The "Study Area" section should also include a clear description of the historic and contemporary hydrology of the watershed(s) (even just some simple hydrographs and discussion), which would make the flow descriptions here make more sense to the reader.

**Response:** A description of the historic and contemporary hydrology of the Yuba River watershed will likely be developed as part of the relicensing of the Yuba River hydroelectric project.

## HYDRAULIC AND STRUCTURAL DATA COLLECTION

**Hal Beecher**

**Comment 1:** Under *Hydraulic and Structural Data Collection* on page 6, the description of SZF determination in wadable channels should indicate that the deepest point was measured at the shallowest cross-section.

**Response:** We did not make this change because we measured the deepest point at the stranding location, based on the assumption that there was not a downstream hydraulic control.

**Comment 2:** The first paragraph on p. 7 ("Flow-flow regression …") appears to belong in the section on p. 6 on *Hydraulic and Structural Data Collection*.

**Response:** The section on *Hydraulic and Structural Data Collection* only contains information on data collected in the field, for example the measurement of the flows used in the flow-flow regression. The paragraph referred to by the commenter instead refers to the performance of the flow-flow regression, which is part of the hydraulic model construction and calibration. Accordingly, we have retained the location of this text.

**Comment 3:** On page 7, the authors state, "A total of two to four sets of WSELs at widely spaced flows were used" [for S-Q relationships]. SZF adds an additional point to these relationships. It would be useful to compare regression error statistics for 2-, 3-, and 4-flow regressions.

**Response:** The SZF is a parameter used in the log-log regression, rather than a point used to conduct the log-log regression. Regression error statistics for 2-flow regressions are meaningless since the use of 2 flows automatically produces regression errors of zero. Our general approach is to use as many flows as possible in developing stage-discharge relationships, as long as the resulting relationships meet the criteria specified in the methods section. Thus, we do not feel that it is appropriate to use a 3-flow regression when stage measurements are available for four flows.

**Comment 4:** On the top of page 8 the authors describe a reasonable approach to S-Q where log-log is nonlinear. In the second paragraph on page 8, Table 7 should be cited for the Feather River backwater equations. Linking a 2-point regression (2 WSELs) to the Maryville (elsewhere listed as Marysville) gage S-Q seems to be a reasonable approach in a pinch, but it would be helpful to explain it or illustrate it.

**Response:** The approach on the top of page 8 is not an approach that we typically apply for instream flow studies, but was viewed as sufficiently accurate for the purposes of this study. Table 7 is in the results section, and thus should not be cited in the methods section. Explanation of linking the 2-point regression to the Marysville gage is given in the results section. We have corrected the spelling of Marysville at this location in the text.

**REVIEWER #4**

**Comment 1:** More detail describing the elevation surveying and associated errors is required in this and other sections of the Methods. Both the peer-reviewed and gray literature (e.g., model user's guides) in hydraulic modeling have thoroughly documented the fundamental and primary importance that source elevation data have on hydraulic modeling results. Errors in the elevation data (cumulative, from survey error and instrument error) and poor characterization of the riverbed structure will cause inaccuracies in hydraulic model results, that then propagate through the habitat modeling steps and into estimates of WUA.

**Response:** For the flow fluctuations report, we do not have any information on errors associated with the elevation surveying. The topographic point densities fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 $m^2$, while Jacobson and Galat (2006) had a point density of 6 points/100 $m^2$. This study was one of our earlier River2D studies and we have been using higher point densities in more recent studies to try and improve the hydraulic predictions of our River2D models. We have been able to use higher point densities in our more recent studies because our new equipment (robotic total station and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 $m^2$) within our time constraints for data collection. Inaccuracies in hydraulic model results would likely not propagate through the habitat modeling steps and into estimates of WUA (used in the redd dewatering computations). Specifically, the overall flow fluctuation-redd dewatering relationship is driven by the change in the distribution of depths and velocities with flow. The distribution of velocities would not be affected by over or under-predicted velocities because over-predicted velocities would have the opposite effect on the distribution of velocities as under-predicted velocities.

**Comment 2:** See comments in the review of the report referred to here as USFWS (2008) -- all of the review comments also pertain to this report.

**Response:** Responses to the review comments of the Yuba spawning report, which we believe is what the commenter is referring to as USFWS(2008) are given in Appendix B of the document "Response to Comments: Flow-habitat relationships for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning in the Yuba River" and thus are not repeated here.

## HYDRAULIC MODEL CONSTRUCTION AND CALIBRATION

**REVIEWER #4**

**Comment 1:** See comments in the review of USFWS (2008), as all of those comments pertain to this report as well.

**Response:** Responses to the review comments of the Yuba spawning report, which we believe is what the commenter is referring to as USFWS(2008) are given in Appendix B of the document "Response to Comments: Flow-habitat relationships for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning in the Yuba River" and thus are not repeated here.

## HABITAT SUITABILITY CRITERIA (HSC) DEVELOPMENT

**Hal Beecher**

**Comment 1:** It is unclear what suitability criteria are being used for depth and velocity for "regular spawning" (i.e., without the elimination for lack of incubation/dewatering). Later Table 8 shows that indeed binary spawning criteria are used; my confusion at this point would have been alleviated by having all HSC Development together.

**Response:** The suitability criteria used for depth and velocity for spawning habitat are given in U.S. Fish and Wildlife Service (2010). The criteria in Table 8 are not spawning criteria, but instead are the criteria used for the redd dewatering analysis. Having all HSC development together would have required combining all three Yuba River reports. We do not feel that it would be practical to combine all three Yuba River reports into one report, given how voluminous each of the three reports is.

**Comment 2:** The standard for velocity may not be realistic. If steelhead (or others) spawn on declining hydrograph, later incubation will experience reduced velocities. It would be worthwhile to consider some type of emergence sampling and relate survival to emergence to incubation velocities. In reality, this is probably a cumulative effect with duration and temperature as well as time during incubation all being factors, so a simpler index should be used, and conservatively this would be velocity at a time shortly before emergence unless a more sensitive index could be found. See also Miller et al. (2008).

**Response:** We are not aware of whether steelhead spawn on a declining hydrograph. Even if steelhead do spawn on a declining hydrograph, there could be egg and pre-emergent fry mortality associated with reduced velocities. Emergence sampling to relate incubation velocities to pre-emergence survival would be helpful to more accurately define the velocity criterion, but is outside of the scope of this study. In the absence of any other information, we feel that the velocity criterion we used is the simplest possible index. The velocity at a time shortly before emergence could underestimate the effects of flow fluctuations if declining flows during incubation resulted in pre-emergent mortality associated with insufficient intragravel velocities. We have added a citation to Miller et al. (2008) in the discussion.

## HABITAT SIMULATION

**<u>REVIEWER #3</u>**

**Comment 1:** The equation at the top of page 9 should probably be x 100.

**Response:** We have made the suggested change.

**<u>REVIEWER #4</u>**

**Comment 1:** See review comments on the USFWS (2008) report; there is significant uncertainty in the accuracy of the 2d model predictions, especially on a node-by-node basis.

**Response:** Responses to the review comments of the Yuba spawning report, which we believe is what the commenter is referring to as USFWS(2008) are given in Appendix B of the document "Response to Comments: Flow-habitat relationships for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning in the Yuba River" and thus are not repeated here.

## RESULTS

**<u>Hal Beecher</u>**

**Comment 1:** On the first full paragraph on page 12 "standing" should be "stranding."

**Response:** We have corrected this error.

**Comment 2:** Consider combining Tables 5 and 6 into Table 5a and b.

**Response:** Since the material in Table 6 is based on data in both Tables 4 and 5, we felt it made the most sense to retain Tables 5 and 6 as separate tables.

## STUDY SITE SELECTION

**<u>REVIEWER #4</u>**

**Comment 1:** These are methods not results; from this point through page 12.

**Response:** We have patterned the format of our reports as closely as possible to that of peer-reviewed journal articles. The material in question represents the results of the relevant study tasks (*Study Site Selection* through *Hydraulic Model Construction and Calibration*). We also note that we previously included all of this information in the methods section and a peer reviewer from the first peer review of the Yuba spawning report recommended moving this material to the Results section. Specifically, the peer reviewer stated "All information presented, including data, in the methods section that is actually a result should be extracted and discussed in the Results section." As a result, we have placed all data in the results section for the Yuba flow fluctuations report as well.

## HABITAT SUITABILITY CRITERIA (HSC) DEVELOPMENT

**Hal Beecher**

**Comment 1:** It is interesting that the difference between redd depth and tail spill is less for steelhead than for salmon. It would be interesting to assess how much of that difference is attributable to difference in fish size and how much to spawn timing in relation to hydrology – do steelhead spawn on a declining hydrograph in the Yuba?

**Response:** We do not know of any data that could be used to determine how much of the difference in redd depth and tail spill for steelhead versus salmon is due to fish size versus spawn timing in relation to hydrology. We do not know if steelhead spawn on a declining hydrograph in the Yuba – presumably the early portion of steelhead spawning prior to snowmelt would not be on a declining hydrograph.

**Comment 2:** As discussed under METHODS, the assumption about velocity may not be appropriate for steelhead if not always measured shortly before emergence. A discussion of spawn timing in relation to the hydrograph would be very informative. What are projections for climate change as far as temperature and timing of spring runoff?

**Response:** The velocity at a time shortly before emergence could underestimate the effects of flow fluctuations if declining flows during incubation resulted in pre-emergent mortality associated with insufficient intragravel velocities. We added a table on spawning timing. A description of the historic and contemporary hydrology of the Yuba River watershed, including displaying both recent hydrographs and pre-project or reconstructed natural hydrographs, will likely be developed as part of the relicensing of the Yuba River hydroelectric project. Although data have been developed by the U.S. Geological Survey on projections for climate change as far as temperature and timing of spring runoff, we are not aware of what these projections are.

**Comment 3:** In Figures 2-4, perhaps a more realistic criterion would be a redd depth-tailspill value that is lower than 50% or 90% of observations. Is there reason to believe that higher standard is needed for being protective? If so, and that seems reasonable, it should be explicitly stated that the standard is conservative.

**Response:** Figures 2-4 are scatterplots showing the entire distribution of redd depth versus redd depth – tailspill depth data. The criterion that we used was the average value, which should be similar to the median value (i.e. the value that is lower than 50% of the observations). Thus it does not appear that the standard we used is higher and there is no need to state that the standard is conservative.

**REVIEWER #4**

**Comment 1:** In this application the p values are meaningless, and the correlations show a poor relationship; moreover, these relationships aren't used in the analysis, so why include them.

**Response:** Correlations are typically evaluated both based on the p values, reflecting statistical significance, and $R^2$ values, reflecting the strength of the correlation. While we agree that the p-values for spring-run Chinook salmon and steelhead show a poor relationship, with $R^2$ values of 0.05 to 0.06, we would consider the relationship for fall-run Chinook salmon, with a $R^2$ value of 0.74, to be moderately strong. We feel that it is important to include these relationships to show the process that we used to develop the depth habitat suitability criteria.

## DISCUSSION

## HABITAT SUITABILITY CRITERIA (HSC) DEVELOPMENT

**Hal Beecher**

**Comment 1:** Discussion covers some relevant literature, but should also consider DeVries (1997). Although DeVries considered scouring, egg burial depth likely also minimizes risk from dewatering. Considering that steelhead eggs often incubate through declining hydrographs, the criteria selected for Table 8 should receive more discussion.

**Response:** We added a citation to DeVries (1997) to the discussion. We added additional discussion related to the criteria in Table 8.

**Comment 2:** Although Figure 13 illustrates high flows, a separate graph on a log scale ($\log_{10}$Q50%exceedence vs date) would be helpful for the context of "typical" flows in the river.

**Response:** We instead rely on the introduction to define typical flows in the Yuba River (i.e. flows up to 4,170 cfs).

## HABITAT SIMULATION

**REVIEWER #4**

**Comment 1:** There is a large amount of uncertainty that the modeling is able to accurately predict WSEL throughout the modeling domain; and thus, accurately predict stranding area.

**Response:** We assume that this comment refers to the 22 stranding sites that were located in our spawning and juvenile rearing habitat modeling sites, since the stranding areas for the remaining sites were either measured in the field, or computed in GIS from water's edge polygons derived from aerial photos. For the 22 stranding sites located in our spawning and juvenile rearing habitat modeling sites, we acknowledge that there are uncertainties in the WSEL predictions throughout the modeling domain, and thus potential errors in the stranding areas. To our knowledge, the stranding areas that we calculated are the best estimates available.

**Comment 2:** Relying on just the model inflow and outflow WSEL for calibration can be problematic, as the model will iterate with these boundary conditions in trying to reach convergence, and in the process will produce erroneous results at model interior nodes; an example of this is physically unrealistic estimates of very high Froude numbers (i.e., >> 1.0) indicating supercritical flow along the channel margins, as was described by the authors in USFWS (2008).

**Response:** The model inflow WSEL is not a boundary condition of the model. We use the model inflow WSEL as a calibration parameter because we can simulate this value with PHABSIM at the highest simulation flow. In contrast, we would only be able to compare empirical and modeled WSEL along a longitudinal centerline of the channel at the highest measured flow. We have decided that it is more accurate to calibrate River2D at the highest simulation flow because the RIVER2D model is more sensitive to the bed roughness multiplier at higher flows, versus lower flows. Also, since we use a uniform bed roughness multiplier for the entire site, calibration at the upstream transect should produce the same result as calibrating to longitudinal WSEL profiles. We believe that either method would have generated Froude numbers exceeding one at some locations in the model.

**Comment 3:** An additional model calibration procedure can include comparing empirical and modeled WSEL along a longitudinal centerline of the channel; oftentimes this can help ascertain model performance within the interior of the model domain.

**Response:** We are unable to compare empirical and modeled WSEL along a longitudinal centerline of the channel because we did not collect empirical WSEL data along the longitudinal centerline of the channel.

**Comment 4:** It is highly unlikely that a river such as the Yuba is in dynamic equilibrium, given the dam-induced changes in hydrology and sediment supply and transport. In the absence of any supporting data, this paragraph should be removed.

**Response:** Our results on the American River, which has much greater dam-induced changes in hydrology and sediment supply and transport than the Yuba River, is provided as evidence that the Yuba River is in dynamic equilibrium. Our findings on the American River were that the January 1997 flood did not result in a substantial change in Chinook salmon or steelhead spawning flow-habitat relationships (US Fish and Wildlife Service 2000).

## LITERATURE CITED

**Hal Beecher**

**Comment 1:** Insert year published (1986) into Chapman et al.

**Response:** We have made the suggested correction.

## APPENDIX B

**Hal Beecher**

**Comment 1:** Footnote should explain that elevations are set relative to a benchmark arbitrarily assigned elevation 100.00 ft (as done in text).

**Response:** We have made the suggested change.

**Comment 2:** In the table that includes calculated vs. given discharge % and difference, it should be noted that direction of deviation is omitted.

**Response:** We have made the suggested change.

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