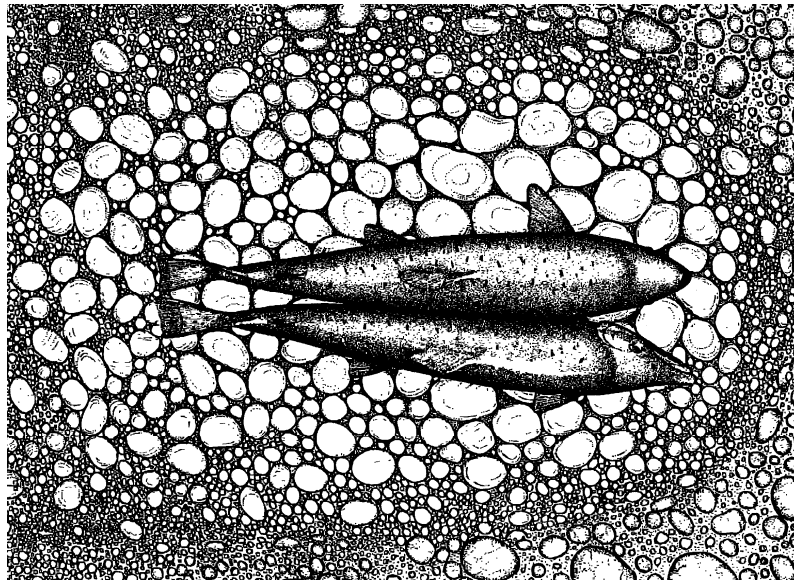
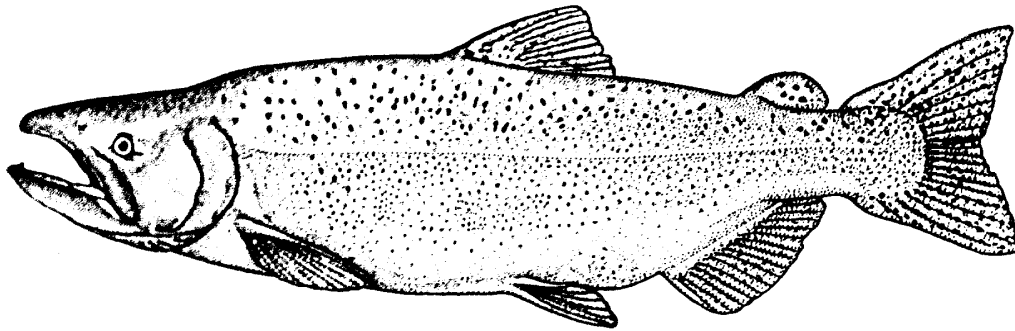


RESPONSE TO COMMENTS
FLOW-HABITAT RELATIONSHIPS FOR SPRING AND FALL-RUN CHINOOK
SALMON AND STEELHEAD/RAINBOW TROUT SPAWNING IN THE YUBA RIVER



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Prepared by staff of
The Energy Planning and Instream Flow Branch

INTRODUCTION

The following is the response to comments document for the final report for the U.S. Fish and Wildlife Service's investigations on anadromous salmonid spawning habitat in the Yuba River between Englebright Dam and the Feather River, part of the Central Valley Project Improvement Act (CVPIA) Instream Flow Investigations, a 6-year effort which began in October, 2001. Title 34, Section 3406(b)(1)(B) of the CVPIA, P.L. 102-575, requires the Secretary of the Interior to determine instream flow needs for anadromous fish for all Central Valley Project controlled streams and rivers, based on recommendations of the U.S. Fish and Wildlife Service after consultation with the California Department of Fish and Game. Consequently, in June 2001 the Service initiated a study to more accurately identify the instream flow requirements for anadromous fish in the Yuba River. Concomitantly, the Yuba County Water Agency (YCWA), California Department of Fish and Game, and four Non-Governmental Organizations (i.e., the South Yuba River Citizens League, Friends of the River, Trout Unlimited, and the Bay Institute), collaboratively with the National Marine Fisheries Service, Pacific Gas and Electric Company, and the Service, diligently worked to develop a comprehensive set of improved flow regimes, which now are now being implemented as the flow schedules of the Lower Yuba River Accord (HDR/SWRI 2007). These Yuba Accord flows are expected to be implemented until at least 2016, when the Federal Energy Regulatory Commission (FERC) license for the YCWA's Yuba River Development Project (FERC #2246) will be proposed for renewal. All parties agree that flows in the Yuba River at present are better for fish populations compared to pre-Yuba Accord flows. However, whether these flows are adequate enough to support the anadromous fish population doubling goal under CVPIA, or other fish species and population protections (e.g., as mandated by the California Fish and Game Code, Endangered Species Act, etc.) is unclear. Several studies to address this uncertainty are underway specifically as part of the Yuba Accord, or being conducted independently by the resource agencies. The investigations will provide scientific information to the U.S. Fish and Wildlife Service CVPIA Program to assist in developing such recommendations for Central Valley rivers. The objective of this study was to produce models predicting habitat-discharge relationships in the Yuba River for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning.

METHODS

Flow-habitat relationships were derived for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning in the Yuba River between Englebright Dam and the Feather River. Habitat availability was evaluated using a two-dimensional hydraulic and habitat model, while habitat suitability criteria were derived using logistic regression and a technique to adjust for availability of deeper waters with suitable velocities and substrates (Gard 1998).

RESULTS

The flow-habitat relationships (Figures 1 to 2) had flows with the maximum amount of habitat ranging from 900 to 3700 cfs. Appendix A provides the results of a peer review of the spawning report, conducted by William Miller of Miller Environmental Services and Dennis Rondorf of the U.S. Geological Survey. Appendix B provides the results of a stakeholder review of the spawning report, responding to comments from Pacific Gas and Electric, Greg Pasternak of the University of California, Davis, and the Yuba County Water Agency. Appendix C provides the results of a second peer review of the spawning report, conducted by four anonymous reviewers provided by the CALFED Ecosystem Restoration Program. Appendix D provides the results of a peer review of the executive summary for the spawning report, conducted by four anonymous reviewers provided by the CALFED Ecosystem Restoration Program. Appendix E provides the results of a peer review of a steelhead spawning sensitivity analysis report, conducted by four anonymous reviewers provided by the CALFED Ecosystem Restoration Program.

DISCUSSION

A previous instream flow study on the Yuba River was conducted in the mid-1980's (Beak 1989). We recognize that Beak's (1989) study reflected the standard practices for instream flow studies in the 1980's. However, the techniques for performing instream flow studies have been significantly refined since the 1980's to increase the accuracy of habitat predictions and reflect the hydraulic complexities of river channels. In particular, the U.S. Fish and Wildlife Service decided to conduct instream flow studies for anadromous salmonids on the lower Yuba River which utilize the improved practices for conducting instream flow studies to develop habitat suitability criteria and hydraulic modeling of available habitat. The specific procedures used in this study that were not used in the Beak (1989) study include: 1) the use of Type II criteria with application of a technique to correct for availability (Guay et al. 2000); 2) collection of habitat use data with equal sampling of different mesohabitat types to address habitat availability and (possibly) at a high enough flow so that fish can select their preferred habitat characteristics; 3) the application of a procedure to adjust spawning depth habitat utilization curves for availability (Gard 1998); 4) the use of a two-dimensional hydraulic and habitat model, instead of the Physical Habitat Simulation system (PHABSIM); and 5) placement of sites for modeling spawning only in heavy spawning-use areas¹.

1 This is one way to address factors, such as permeability and upwelling, other than depth, velocity and substrate, which control the distribution of spawning (Gallagher and Gard 1999).

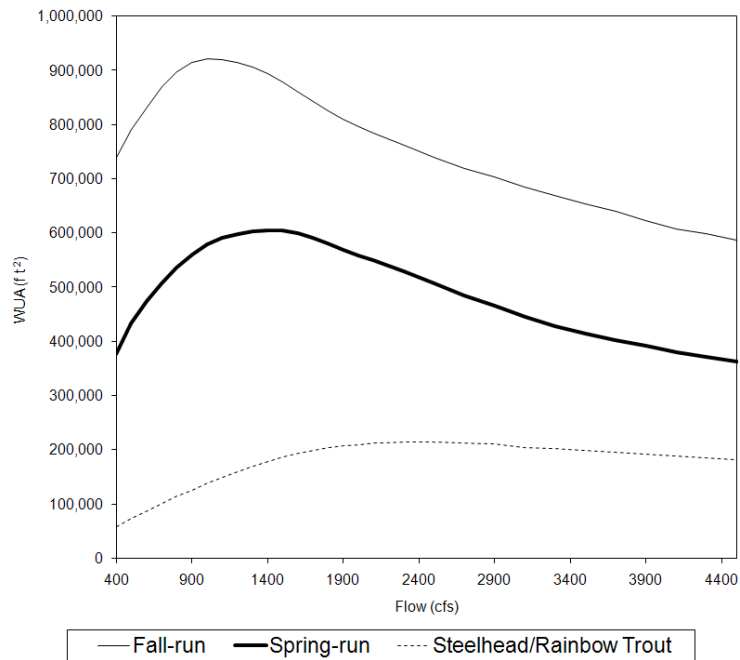


Figure 1. Spring-run and fall-run Chinook salmon and steelhead/rainbow trout spawning flow-habitat relationships above Daguerre Point Dam. The flows with the maximum and fall-run Chinook salmon and steelhead/rainbow trout spawning habitat were, respectively, 1400 cfs, 1000 cfs and 2900 cfs.

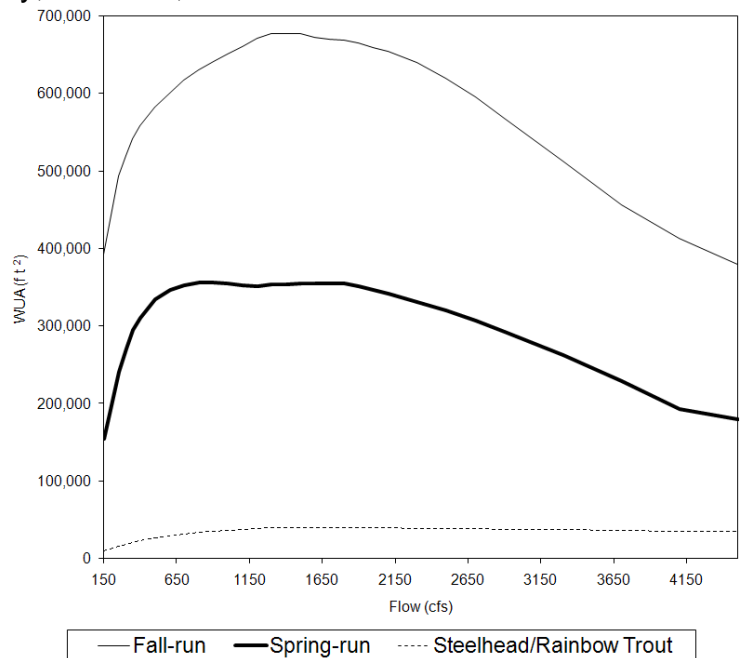


Figure 2. Spring-run and fall-run Chinook salmon and steelhead/rainbow trout spawning flow-habitat relationship below Daguerre Point Dam. The flows with the maximum spring-run and fall-run Chinook salmon and steelhead/rainbow trout spawning habitat were, respectively, 900 cfs, 1400 cfs and 3700 cfs.

Our September 13, 2001 letter inviting stakeholder participation in this study stated:

We are offering interested stakeholders the opportunity to participate in planning these studies through: 1) review and comment on our draft study plan, 2) attending a series of information/technical meetings (at key milestones) to be held during the duration of the study, and 3) providing comments on our draft report prior to its finalization.

Table 1 summarizes the stakeholder involvement for this study.

References

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- Gallagher, S. P. and M. F. Gard. 1999. Relation between Chinook salmon (*Oncorhynchus tshawytscha*) redd densities and PHABSIM predicted habitat in the Merced and Lower American Rivers, CA. Canadian Journal of Fisheries and Aquatic Sciences 56(4):570-577.
- Gard, M. 1998. Technique for adjusting spawning depth habitat utilization curves for availability. Rivers: 6(2):94-102.
- Guay, J.C., D. Boisclair, D. Rioux, M. Leclerc, M. Lapointe and P. Legendre. 2000. Development and validation of numerical habitat models for juveniles of Atlantic salmon (*Salmo salar*). Canadian Journal of Fisheries and Aquatic Sciences 57:2065-2075.
- HDR/SWRI. 2007. Draft Environmental Impact Report/Environmental Impact Statement for the Proposed Lower Yuba River Accord. Prepared for the California Department of Water Resources, Yuba County Water Agency and the U.S. Bureau of Reclamation. June 2007. HDR/SWRI: Sacramento, CA.

Table 1. Stakeholder Involvement.

Date	Type of Stakeholder Involvement
9/13/01	Letter to stakeholders – invitation for stakeholder participation
10/18/01	Meeting with stakeholders – review of study plan
11/7/01	Meeting with stakeholders – review of study plan
11/13/01	Bill Mitchell (Jones & Stokes) – fall-run Chinook spawning HSI data collection
3/3/02	Comments from stakeholders on study plan
12/5/02	FWS response to stakeholders on study plan comments
9/3/03	Yuba River Technical Working Group (YRTWG) – update on IFIM studies (S. Schoenberg)
11/13/03	YRTWG – update on IFIM studies (B. Pelle)
12/29/03	2003 annual report distributed to stakeholders
1/14/04	YRTWG – update on IFIM studies (S. Schoenberg)
4/14/04	YRTWG – update on IFIM studies (E. Ballard)
7/21/04	YRTWG – update on IFIM studies (M. Gard)
10/20/04	YRTWG – update on IFIM studies (B. Pelle)
1/18/05	YRTWG – update on IFIM studies (M. Gard)
1/19/05	Yuba Basin Modeling Forum – presentation on IFIM studies
4/12/05	YRTWG – update on IFIM studies (E. Ballard)
7/13/05	YRTWG – update on IFIM studies (S. Schoenberg)
10/28/05	YRTWG – update on IFIM studies (S. Schoenberg)
1/19/06	YRTWG – update on IFIM studies (M. Gard)
4/13/06	YRTWG – update on IFIM studies, including fall-run Chinook spawning depth and velocity HSC (M. Gard)
7/24/06	YRTWG – update on IFIM studies (S. Schoenberg)
9/5/06	YRTWG – update on IFIM studies (M. Gard)
12/12/06	YRTWG – update on IFIM studies (S. Schoenberg), 2006 annual report distributed to stakeholders
3/13/07	YRTWG – update on IFIM studies (S. Schoenberg)
5/14/07	Draft spawning report provided to stakeholders for review and comment
6/13/07	YRTWG – update on IFIM studies (S. Schoenberg)
6/15/07	Comments from PG&E on draft spawning report
7/16/07	Comments from Greg Pasternak (UC Davis) on draft spawning report
7/25/07	Comments from Yuba County Water Agency on draft spawning report
10/24/07	Presentation to stakeholders on draft spawning report
2/12/08	YRTWG – update on IFIM studies (M. Gard)
4/9/08	Technical workshop on draft spawning report and responses to stakeholder comments
6/8/08	YRTWG – update on IFIM studies (M. Gard)
6/27/08	Meeting with stakeholders on draft spawning report
9/18/08	YRTWG – update on IFIM studies (S. Schoenberg)
11/17/08	Meeting with stakeholders on spawning sensitivity analysis

Table 1 (continued). Stakeholder Involvement.

Date	Type of Stakeholder Involvement
1/27/09	YRTWG – update on IFIM studies (M. Gard)
1/28/09	Meeting with stakeholders on draft spawning report
1/21/10	YRTWG – update on IFIM studies (M. Gard)
2/4/10	Meeting with stakeholders on draft spawning report

Appendix A
Response-to-Comments Document

for the

**May 2006 Peer-Review Draft of the
Yuba River Spawning Instream Flow Study Report**

August 2010

AUTHORS RESPONDING TO COMMENTS

U.S. Fish and Wildlife Service

Ed Ballard

Mark Gard

PREFACE

This document contains the comments provided by scientific peers on the May 2006 draft of the report, “Flow-habitat relationships for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning 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.

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 spawning 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 May 2006 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.

LIST OF PEER REVIEWERS

Dr. William J. Miller

Miller Ecological Consultants, Inc.
Fort Collins, Colorado

Dennis W. Rondorf

U.S. Geological Survey
Western Fisheries Research Center
Columbia River Research Laboratory
Cook, Washington

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LIST OF ACRONYMS

2D	Two dimensional
ADCP	Acoustic Doppler Current Profiler
cm	Centimeter
DWR	California Department of Water Resources
GEOID96	1996 Geoid model
GPS	Global Positioning System
H.I.	Instrument Height
HPGN	High Precision Geodetic Network
HPGN-D	High Precision Geodetic Network Densification
HSC	Habitat Suitability Criteria
HSI	Habitat Suitability Index
m	Meter
Net Q	Net Flow
NGS	National Geodetic Survey
PASS	Parameter Analysis and Storage System
PHABSIM	Physical Habitat Simulation Model
PPM	Parts Per Million
RHABSIM	Riverine Habitat Simulation Model
RIVER2D	Two dimensional depth averaged model of river hydrodynamics and fish habitat
SD	Standard Deviation
SE	Standard Error
STAR*LEV	Start Leveling
USCE	U.S. Corps of Engineers
USFWS	U.S. Fish and Wildlife Service
VAF	Velocity Adjustment Factor
WSEL	Water Surface Elevation
WUA	Weighted Useable Area

GENERAL COMMENTS

William Miller

Comment 1: Thank you for the opportunity to review the draft report “Flow Habitat Relationships for Spring and Fall Chinook Salmon and Steelhead/Rainbow Trout Spawning in the Yuba River.” The report contains a large amount of technical information and as such there are times when it is difficult to fully comprehend the material presented. I recommend major revisions prior to publication. I feel this would provide more clarity on objectives, methodology, results and conclusions.

Response: The methods section of the report has been reorganized so the hydraulic modeling methods are combined and distinctly separate from the HSC data collection and development so the reader does not have to go back and forth between different sections. In addition, we have recently changed the format of our reports to separate out the methods from the results and to add a Discussion section.

Comment 2: The study design itself appears to be sound and follows appropriate techniques for PHABSIM and River2D applications for habitat flow relationships, however, I did not find an explanation of why these methods were selected. No background information is presented that provides details on why or how the decision was made to apply these techniques rather than some other technique. The addition of the rationale for selection of the approach in the introduction would provide your audience with logic behind the study approach. There are a variety of techniques to evaluate spawning habitat. It appears from the introduction that the objective overall in the basin is to increase salmonid population size. It is unclear how the methodology applied will help increase those populations. There is a general lack of biological information provided on the link between habitat and population change, although there are methods available to assess spawning success and recruitment. I think further literature review and discussion would provide the reader with a sound introduction to the problem being evaluated.

Response: We have added a paragraph to the introduction describing available techniques for evaluating spawning habitat, and why the study approach was selected from these techniques. This paragraph includes a review of the literature on this topic. We have also added the assumptions of this study to the introduction, including the assumption that physical habitat is the limiting factor for salmonid populations in the Yuba River. We have also added a conceptual model to the introduction on the link between habitat and population change, addressing how the methodology applied will help to increase salmonid populations. We have recently changed the format of our reports to add a Discussion section. We have included as part of the discussion section a qualitative assessment of how increases in spawning habitat would contribute to increases in salmonid population size.

Comment 3: There are no specific objectives stated for the study. There is one sentence in the introduction that states the purpose of the study, but no objectives were specifically stated. Therefore it is unclear whether the objectives for the study were achieved.

Response: We feel that the objectives for this study are adequately presented in the paragraph containing the purpose sentence to which you refer. The sentences making the objectives clear are as follows: “In December 1994, the U. S. Fish and Wildlife Service prepared a study proposal to identify the instream flow requirements for anadromous fish in certain streams within the Central Valley of California, including the Yuba River. The purpose of this study was to produce models predicting the availability of physical habitat in the Yuba River for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning over a range of stream flows.” The purpose and objective of this study are the same and further elaboration would be redundant. Given that the report exclusively describes our efforts to produce the 2D models and the results provide predicted availability of physical habitat in the Yuba River for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning over a range of stream flows, we believe that it is sufficiently clear that this objective was achieved. We have also added a sentence to the discussion specifically stating that the objective was achieved.

Comment 4: Generally the methods used are appropriate. The data collection techniques, methods used and times of data collection all become intertwined with results in the methods section. One critical concern I have is lack of discussion of the accuracy of the data collected and the implications of measurement error on the final result. The Methods section describes the techniques used. The Results section should but provide a presentation of the accuracy of the measurements, in particular, for blending several techniques to determine bed elevations. There is one brief mention in a sentence in the methods that states that “the bed topography was accurate to one foot.” I’m assuming this is from the photo interpretation work that was completed to generate bed elevations. In other studies of this type where that is used, it generally means that it is \pm one foot relative to the actual topography location. This is an important interpretation of data in generating the bed mesh for the River2D model.

Response: We have added a discussion of the accuracy of the data collected and the implications of measurement error on the final result. All of the measurements were accurate to one foot horizontally and 0.1 foot vertically. We conclude that measurement error would have a minimal effect on the final result.

Comment 5: Other important information is the accuracy of the ADCP depth sounder used to collect underwater topography. Again, this in conjunction with the above water elevations that are \pm one foot can lead to large differences in actual bed elevations and model performance. Some discussion is needed including error values compared with measured values to determine the accuracy of the modeling.

Response: The accuracy of the ADCP (which is not a depth sounder) is 0.1 foot. The water surface elevations are also accurate to 0.1 foot. As a result, there would be minimal differences in actual bed elevations and thus minimal effect on model performance. Since the ADCP was

used only where the depth was greater than 3 feet, the error values of 0.1 foot are less than 3% of the measured values. As a result, we conclude that the modeling was sufficiently accurate with regards to the bed topography data used in the model.

Comment 6: Throughout the methods section, results are interspersed. This makes it difficult to determine whether the discussion is relative to methodology or to what was measured. I recommend a very concise methods section, citing the appropriate reference material for River2D, PHABSIM, and other methodologies with a brief description of how the data was collected and the dates the data was collected in table form. This also applies to the spawning surveys for redd counts. A simple table of dates, flow, species of interest, and the reach of the river would be much more informative than narrative format.

Response: We have recently changed the format of our reports to separate out the methods from the results and to add a Discussion section. As a result, the methods section is now very concise. Due to the unique application of River2D, PHABSIM, and other methodologies in this study, reference material citations would not sufficiently document the methods used, and thus we still needed to include sufficient information in the methods of how the data was collected. The dates the data was collected are already in table form (Tables 8 and 9), which have been moved to the results section. We concluded that there was too much complexity in the spawning survey redd count data to be able to represent it in a table, with the exception of the flow data, and have thus retained this material in narrative format, although the material has been moved to the results section. We have moved the flow data for the spawning surveys to a new table in the results section.

Comment 7: All information presented, including data, in the methods section that is actually a result should be extracted and discussed in the Results section. As written, the current report has a one page introduction, nearly 45 pages of methods and approximately 13 pages of results (not including figures). The large methods section is due to interspersing results within methods. Extracting the results from the methods would provide clarity to the report and enhance the discussion section within the results.

Response: We have recently changed the format of our reports to separate out the methods from the results and to add a Discussion section.

Comment 8: Habitat suitability data was collected and habitat suitability criteria generated for the species of interest. I found most of the criteria curves to be appropriate based on the data presented, however, for steelhead/rainbow spawning the depth criteria generated shows the highest HSC value at the deepest water, and this is not what is reflected by what was measured in the stream. Either I missed how the data was generated or the HSC is inappropriate and should not be applied. In all other criteria, HSC criteria overlap well with the peak of the HSC being at the peak of occurrence of redds. This same shape of the HSC curve should apply to the rainbow and steelhead. Further, there is a large body of literature on spawning requirements for steelhead and rainbow trout and other anadromous salmonids that show that downwelling into the substrate is important for survival and emergence. The HSC criteria for depth would be contrary to this

hydraulic effect that has been documented in the scientific literature. This is one instance where more biological interpretation of the physical results is needed to understand the requirements of the species.

Response: The difference between the frequency distribution of occupied locations (with redds) and the HSC is due to the HSC being developed using a logistic regression. It is well-established in the literature (Knapp and Preisler 1999, Parasiewicz 1999, Geist et al. 2000, Guay et al. 2000, Tiffan et al. 2002, McHugh and Budy 2004) that logistic regressions are appropriate for developing habitat suitability criteria. The logistic regression, by using both occupied and unoccupied points, takes into account availability without using a use to availability ratio. Criteria developed just using use data (which are most of the existing criteria) tend to be biased towards low depths and velocities due to the lower availability of deeper and faster conditions. The steelhead/rainbow trout spawning depth criteria actually have the highest HSC value at depths of 15.1 to 16.9 feet, with suitability dropping for depths greater than 16.9 feet. As shown in Figure 13, the frequency distribution of occupied and unoccupied locations is similar for depths up to around 5 feet, while the relative frequency for depths greater than 5 feet is greater for occupied locations than for unoccupied locations. This pattern of data resulted in the logistic regression having lower suitabilities at shallower depths and suitabilities increasing up to 15.1 feet. The literature on salmonids' preference for downwelling versus upwelling conditions is more equivocal than suggested by the reviewer. For example, Geist et al. (2002) found that Chinook salmon selected downwelling areas but chum salmon selected upwelling areas. Similarly, Vronskii and Leman (1991) found that Chinook salmon selected downwelling areas but chum, sockeye and coho salmon selected upwelling areas. In contrast, Geist (2000) found that Chinook salmon preferred upwelling areas. Other studies have found that brook trout (Webster and Eiriksdoottir 1976, Curry and Noakes 1995) and sockeye salmon (Lorenz and Eiler 1989) select upwelling areas. In addition, the same locations can have upwelling at low flows but downwelling with rapidly rising flows (Malcolm et al 2003). Baxter and Hauer (2000) found that bull trout selected upwelling conditions on the stream reach scale but downwelling conditions on the mesohabitat unit scale. Nor does the literature suggest that a preference for downwelling or upwelling conditions would restrict salmonids to spawn in shallow conditions – fall-run Chinook salmon spawning has been observed in as deep as 35 feet in the Hanford Reach of the Columbia River (Chapman et al. 1983). Gard (1988) found that Chinook salmon redds were primarily found in shallow conditions because of a low availability of deeper water with suitable velocities and substrates, whereas a mechanistic relationship between intragravel velocity and depth explained a minor portion of the depth selection. Thus, even at greater depths, intragravel velocities are sufficient to result in unimpaired survival and emergence.

Comment 9: The results section does not include an interpretation of the biological implications of the flows on habitat or population. While the physical and biological link is generally not well established in instream flow studies, I think there should be some discussion based on professional judgement or the known scientific literature on what expected result to populations should be seen based on the results of the study. Further, the results section, is lacking conclusions and merely presents results without conclusion statements. I suggest adding a separate conclusions section or subsection within the results stating whether the researchers concluded that they achieved their objectives or did not.

Response: We have recently changed the format of our reports to add a discussion section. We conclude that the information requested by the commenter would better fit in the discussion section than in the results section. We have added some discussion based on professional judgement on what expected results to populations could be seen based on the results of the study. We have included conclusions within the discussion section, including the conclusion that the study objectives were achieved.

Comment 10: In conclusion, I feel the report should have some major revisions prior to publication. These include, an executive summary, table of contents, specific statement of objectives with the introduction, an expanded introduction section that provides the background on why the particular methodologies are appropriate to achieve the objectives, and what other methodologies, if any, were reviewed, prior to selection of the PHABSIM River2D approach. Further there should be an addition of conclusions to the results section with additional biological interpretation of the results of the physical habitat study that would be expected on populations.

Response: We have recently changed the format of our reports to separate out the methods from the results and to add a Discussion section. We have added a table of contents to the report. We have also added an Abstract, containing the same information as an executive summary. The introduction contains a specific statement of objectives and has been expanded to provide the background on why the particular methodologies are appropriate to achieve the objectives, and what other methodologies were reviewed, prior to selection of the PHABSIM River2D approach.

Dennis Rondorf

Comment 1: In the cover letter inviting my comments you offered to “save you time and frustration, please note that we are not requesting comments with regards to the format of the report”. The letter further states “inclusion of a Discussion section was not deemed warranted”. The authors do engage in limited discussion in the results section. However, I believe the failure to include a discussion section is a major failing of this report. I tried to follow your direction to the reviewers, but in this case the constraints in format you have imposed determine the content of the report. Therefore, I have some responsibility as a reviewer to address this shortcoming in the content. A compelling reason for a discussion is listed in the letter, the report provides additional scientific information to the U.S. Fish and Wildlife Service (USFWS) to implement revised flow regimes. Revising flow regimes is an important action with consequences. The authors are not the end users of this information because others in the USFWS, other agencies, or the public may use this information. I believe the USFWS has some responsibility to the public (yes, even those who might have an adversarial position), to discuss the strengths and weaknesses of the information so that the user, whether it is the USFWS staff or the public, has a way of evaluating the information. Putting the term “scientific” in front of information in the cover letter does not give it the good housekeeping seal of approval. Avoiding an honest and well-balanced discussion could lead others to mistrust this valuable contribution and it surely puts others at a disadvantage if they want to use the information.

Response: We have recently changed the format of our reports to separate out the methods from the results and to add a Discussion section. The discussion section discusses the strengths and weaknesses of the information and thus provides an honest and well-balanced discussion of the information.

Comment 2: This report would benefit from a conceptual model or schematic showing how the different empirical data and modeling outputs fit together. See comment on P 5, Pr 2. Furthermore, the report would benefit from a concise explanation of why some data was collected and how it fits into the conceptual model. For example, see comments on P 17, Pr 2, L 1, concerning the biological validation data collection section.

Response: We have added a conceptual model showing how the different empirical data and modeling outputs fit together (new Figure 1) in the introduction. Additional material has been added to the introduction to provide a concise explanation of why data was collected and how it fits into the conceptual model.

Comment 3: The authors used several gear types to collect the data and this is a widely accepted and necessary methodology. For example, an ADCP, Marsh-McBirney, and Price AA were used to collect water velocities. Because of the technical nature of this report, I believe it would be useful to document the precision and accuracy of various gear used in an Appendix table. For an example, see P 29, Pr 2, L 3 where the authors give reference to some undefined “amount of variation in the ADCP velocity measurements”. Another example is the use of GPS and total stations and the performance of those instruments can vary widely depending on year of manufacture and model specifications.

Response: We have added a table to the report, rather than an appendix, with the manufacturer’s given precision and accuracy of the instruments used in this study, since the table was relatively small, and thus seemed to fit better within the report. For most of the instruments, information was only available on either their precision or accuracy, but not both. The reference cited by the commenter refers not to the accuracy or precision of the ADCP, but to the range of natural velocity variation at any given location. The table gives accuracy and precision data for the ADCP, Marsh-McBirney, Price AA, GPS and total station.

Comment 4: The study lacks a clear presentation on the prediction of spawning sites and the two possible outcomes 1) the prediction of redds where spawning did not occur, and 2) the prediction that redds would not be at a location, but in fact they were observed. See P 17, Pr 5 L 3. This report would benefit from a clear presentation of the redd location predictions from two perspectives. The first is to describe the error of commission, the case where predicted use is high, but a portion of redd locations predicted to have redds had none (error of commission). Is this error because the model failed or is this because the area is only partially seeded with spawning adults. Does this vary by reach, for example in low flow years might the fish spawn in lower reaches where more flow is present. The second aspect is to describe the error of omission, the case where you found redds, but would not have predicted redds to be likely to be present. I do appreciate that some studies, at least here in the northwest predict large areas of WUA for spawning that is seldom if ever observed to have redds.

Response: The figures in Appendix J clearly show both the prediction of redds where spawning did not occur and the prediction that redds would not be at a location when they were observed there. The hypothesis tested (that the compound suitability predicted by the River2D model is higher at locations where redds were present versus locations where redds were absent) addresses both errors of commission and errors of omission. If there were a large percentage of locations with high suitability but no redds, the hypothesis would tend to fail because it would be unlikely that the locations with redds would have a higher suitability than the unoccupied locations. Conversely if there were a large percentage of locations with low suitability but with redds, the hypothesis would tend to fail because it would be unlikely that the locations without redds would have a lower suitability than the occupied locations. In addition, the report explicitly addresses the error of omission, discussing why some of the redd locations had a combined predicted suitability of zero – in most cases, this was because the model incorrectly predicted the substrate at the redd location. The location of redds relative to combined suitability is best viewed on a probabilistic basis. Areas with high suitability will have a high probability of having redds present, while areas with low suitability will have a low probability of having redds present. Considered on this basis, it would not be an error of commission to have areas with a high suitability but no redds, as long as the percentage of these areas is less than the percentage of areas with high suitability and with redds. As shown in Figures 18, 20 and 22, there were relatively few unoccupied locations which had a high predicted suitability. Since we have concluded that there is not an error of commission, consideration of causes of such an error (model error, partial seeding) or variation by reach is immaterial.

Comment 5: The report would benefit from a discussion of the results relative to the methods the authors used and those used by others doing similar studies. In particular, the use of the logistic regression is of interest to this reviewer and has been used by others recently. For example, McHugh and Budy (2004) created logistic-regression based habitat suitability models. This discussion should take place to tell the users of this research product how it relates to other investigations. In fact, some of the questions they raise on the reach specificity of models seems applicable to this study and should be discussed.

Response: We have moved the discussion on logistic regression to the discussion section and expanded it to include discussion of additional studies, including the one cited by the commenter, that used logistic regression to develop habitat suitability criteria. The information in McHugh and Budy (2004) on the reach specificity of models supports the application of the habitat suitability criteria developed for this study, since they were developed using data from the reach being studied.

Comment 6: The report uses and misuses footnotes. Valuable information that could simply be written in a complete sentence and placed in the text is instead placed in footnotes. Then long lists of site names are placed in the key position of topic sentence when they could be placed in a footnote (e.g., P 29, Pr 3, L 1). Furthermore, footnotes are used to discuss selected anomalies instead of a discussion section.

Response: Our intention was to put material into footnotes that would interrupt the flow of the text. We have reviewed the footnotes and the material that the commenter suggested should be in footnotes and have concluded that the footnotes and text are appropriate to maintain the train of thought. We feel it would be too confusing and lengthy to arrange the text noted by the commenter as a footnote. These footnotes have been used consistently for numerous reports and this is the first time that we have received criticism for their use.

Comment 7: This study would really benefit from a sensitivity analysis. In such an analysis, you might vary an input or measure by 10%, a quartile, SE, or SD whichever is reasonable considering the measure. You would then have some relative measure of what data sources your predictions are most sensitive to using these methods.

Response: A sensitivity analysis is outside of the scope of this report. It is well established that flow-habitat relationships are most sensitive to the habitat suitability criteria.

SPECIFIC COMMENTS

METHODS

STUDY SEGMENT SELECTION

William Miller

Comment 1: What about geomorphic reaches? Slope differences? (Page 2)

Response: The spawning portion of the Yuba River only has one geomorphic reach, consisting of an alluvial channel, and has a relatively uniform slope.

Comment 2: High flows form channels and habitat. Do high flows decrease? (Page 2)

Response: We conclude that average flows are most relevant to determining study segments, since the main purpose of breaking a river into study segments is to be able to calculate habitat for reaches, where each segment has a relatively uniform flow. For the 95th percentile daily average flows, the flow (7390 cfs) at the Marysville gage (USGS gage #11421000) is 18% higher than the flow (6270 cfs) at the Smartville gage (USGS gage # 11418000). Thus, even based on high flows, we would break up the Yuba River into two segments (above and below Daguerre Dam).

Dennis Rondorf

Comment 1: Near the beginning of the Methods the definitions or use of terminology is important. In this report it is somewhat inconsistent near the beginning. Under the heading of Methods is a subtitle “Study Reach Selection” and the first sentence uses study “segments”, are reaches and segments the same thing? In the second sentence the term study area is used. On

page 4 study sites is used repeatedly. Perhaps being more explicit early in the methods with a definition of terminology would be helpful to the reader. (Page 2, Paragraph 2 and Page 4, Paragraph 1, Line 5)

Response: We have corrected the subtitle to read “Study Segment Selection”. Additional changes to the text were made to clarify the terms “reach” and “segment” and the study sites described on page 4 are referenced relative to the two study segments.

TRANSECT PLACEMENT (STUDY SITE SETUP)

William Miller

Comment 1: What is accuracy? (Page 5)

Response: The accuracy of the Corps of Engineers photogrammetry and hydro-acoustic mapping data was 1.0 foot horizontally and 0.1 foot vertically.

Dennis Rondorf

Comment 1: “PHABSIM to provide water surface elevations as an input”. This reviewer understood immediately where you were going with this input, but other readers may not. Prior to this point in the text a conceptual model or schematic should be provided to describe the flow of modeled and empirical information, where it is used, and how it is related to the end product. The authors may argue that this report is written for a small group of informed readers that will not need that roadmap. However, there are perhaps several different routes the authors could have taken to obtain the final product and I believe informing the reader early in the report will lead to greater appreciation and understanding later in the report. The current presentation begs the question, where will they go next?? (Page 5, Paragraph 2)

Response: See our response to Dennis Rondorf’s general comment 2.

Comment 2: “by the U.S. Army Corps of Engineers using photogrammetry and hydro-acoustic mapping. This incorporated data allowed greater refinement of the bed topography for each study site.” While the Corps of Engineers are the preeminent surveyors, sometimes the goal of their surveys is different from flow modeling and those differences are reflected in the data (localized bench marks, wide transect spacing, etc.). The authors should provide meta data on these survey data sets because the methods for collection and scales are not described herein. Furthermore, my concerns seem justified when later in the report, this survey data was associated with a problem. (Page 5, Paragraph 3, Line 6)

Response: The commenter is correct that the Corp of Engineers’ data had wide transect spacing. As a result, we primarily used it to supplement our data, and to develop the bed topography upstream of the study sites to improve the accuracy of the flow distribution at the upstream end of the sites. We concluded that the wide transect spacing was sufficient to use the data for these

purposes, since we were not exclusively using the Corps of Engineers data to develop the bed topography within the area where we simulated habitat. The Corp of Engineers did not use localized benchmarks, as described below. In fact, we used the Corp of Engineers' benchmarks to establish the northings, eastings and vertical elevations of our benchmarks.

FEATHER RIVER CONTROL SURVEY

Purpose

The purpose of the control project is to establish coordinates and elevations for photogrammetric and hydrographic mapping. 107 monument stations and photogrammetric targets were found and/or set in an area from south of Marysville to north of Oroville and parts of Browns Valley, California, covering thirteen 7.5 minute quadrangles. See layout map in monument sketches and description section.

Monumentation

Monumentation consisted of found stations as published in the NGS Data Sheets, found US Corps of Engineers monuments along the Feather River, found Cal Trans, Department of Water Resources and AYRES monuments and newly set monuments and targets. These are described in References and Station description sections.

Survey

The survey was designed incorporating good GPS geodetic network criteria and the use of the GEOID96 geoid model. Areas for vertical constraints and leveling were selected to take advantage of proper geoid modeling. Vector observations were designed so that all adjacent stations were directly observed together using short baselines and ionospheric free solutions and tying into several HPGN stations throughout the network. The survey was conducted over a three-week period in June 1999 and consisted of digital leveling at key locations throughout the project and Fast Static GPS observations on all new monuments and reference monuments. The Wild/Leica NA2002 level was used for the digital leveling and 4 Trimble 4000 SSI dual frequency GPS receivers were used for the GPS observations.

To minimize setup errors 2 meter fixed height forced center antenna tripod poles were used. The leveling was tied into several NGS published benchmarks and several USCE benchmarks as well as some Cal Trans, DWR and AYRES stations. The GPS network consisted of over 380 baseline vectors and was tied to several California High Precision Geodetic Network (HPGN) and Densification (HPGN-D) stations. Copies of field notes are found in the Field Notes Section.

Data Processing & Adjustments

Data processing and adjustments were conducted in two parts. The digital leveling was edited and checked against field notes recorded along with the digital data, reduced with the STAR*LEV NA3000 program and adjusted with the STAR*LEV least squares adjustment program holding published elevations from NGS and/or other sources. A couple of one way leveling runs were processed as checks on existing elevation data and/or for GPS vertical checks. The vertical adjustment with standard errors is included in the Adjustment Results section. The GPS network consisted of post processing the data using Trimble's GPSurvey and adjusted using Trimnet Plus. The vector results were all ionospheric error free and reviewed and/or reprocessed for high quality solutions. These were incorporated into the least squares adjustment using vector standard errors as initial vector weighting. A Standard error for station occupations was estimated at 0.005 feet for 04/08/02 centering and 0.01 feet for H.I. The least squares adjustment was run both in a minimally constrained adjustment (holding only one point fixed) and fully constrained, adjusting all observations including the GPS observed vectors and the Geoid96 modelled values for each point. The fully constrained adjustment was run iteratively adding horizontal and vertical constraints and analyzing the results between adjustments. The final adjustment was run based on the best fit of horizontal and vertical constraints, i.e. accepted control point values, and the adjusted observations yielding a high precision network based on high confidence observation solutions and good network integrity.

Adjustment Results

The minimally constrained adjustment was of high precision with horizontal standard errors at 0.03 feet and an average precision of 0.34 PPM at 95% confidence, indicating the observations and the network itself was of good quality and strong integrity. The constrained adjustment runs indicated possible problems with some of the control stations, especially in the vertical components. A constrained adjustment was run not holding the elevations determined from USCE stations and not holding the published GPS derived elevation of station PASS. This adjustment produced statistics that indicated a good fit between the observations and the constraints, 0.025feet horizontal and 0.10 feet in orthometric heights (NGVD29 elevations) at 95% confidence. However, the resulting GPS derived elevations on several points leveled from the USCE stations differed from 0.2 to 0.6 feet from the leveling produced elevations. Another adjustment was run still not holding the elevations determined from USCE stations but holding the GPS derived elevation on station PASS. This adjustment produced acceptable standard errors of 0.027 feet in latitude, 0.023 feet in longitude and 0.13 feet in orthometric heights, again all at 95% confidence. Moreover the adjustment holding the elevation on

PASS yielded results more consistent with elevations determined from USCE stations (which were still not held). The final adjustment then is a fully constrained adjustment holding all NGS published GPS derived horizontal values and all NGS published elevations, including the GPS derived elevation for PASS, with an average precision over 5643 possible lines of 0.387 PPM.

HYDRAULIC AND STRUCTURAL DATA COLLECTION

William Miller

Comment 1: How collected? Total station? GPS? (Page 6)

Response: The equipment and methods used for collecting the bed topography data between the transects are already presented on pages 7-8. This data was collected with a total station.

Comment 2: Accuracy? (Page 6)

Response: See response to William Miller's general questions 4 and 5.

Comment 3: Need error reported. (Page 8)

Response: See response to William Miller's general questions 4 and 5.

Comment 4: Does this mean that the control section was downstream? (Page 10, Paragraph 3)

Response: Yes, a highest low point in the thalweg downstream of the bottom transect that is higher than the bottom transect thalweg will serve as the control (stage of zero flow). We have rewritten this paragraph to make it more understandable.

Dennis Rondorf

Comment 1: "substrate and cover" The calculation of bed roughness is one of only a few inputs to the 2D model and warrants a sentence, not a footnote. Furthermore, footnotes in this report are used as a substitute for well-crafted sentences placed in the appropriate order in a paragraph (i.e., the style is footnote happy). (Page 6, Paragraph 2, Line 8)

Response: See our response to Dennis Rondorf's general comment 6.

Comment 2: "All substrate and cover data on the transects were assessed visually" Proposed text: "using the classification schemes described in tables 2 and 3". (Page 7, Paragraph 1, Line 7)

Response: On page 6, we have already included the following explanation of the use of these classification schemes: "The data collected at the inflow and outflow transects included: 1) WSELs, measured to the nearest .01 foot at a minimum of three significantly different stream

discharges using standard surveying techniques (differential leveling); 2) wetted streambed elevations determined by subtracting the measured depth from the surveyed WSEL at a measured flow; 3) dry ground elevations to points above bankfull discharge surveyed to the nearest 0.1 foot; 4) mean water column velocities measured at a mid-to-high-range flow at the points where bed elevations were taken; and 5) substrate and cover classification at these same locations (Tables 2 and 3) and also where dry ground elevations were surveyed. We do not feel it is necessary to repeat this explanation on the following page.

Comment 3: “depths from the ADCP” recommend reporting the estimated or manufacturers specified precision/accuracy for depth from the ADCP. Report specs from ADCP along with specs from total station. (Page 8, Paragraph 1, Line 10)

Response: See response to Dennis Rondorf’s general comment 3.

Comment 4: “The stage zero flow”, “determining, using”, “highest low point”, “highest low point”, “downstream of the bottom transect”: maybe I’m wrong, but read this paragraph twice, and read fast to a colleague down the hall and see what the response is. Perhaps this text could be rewritten to read more clearly. (Page 10, Paragraph 3)

Response: We have rewritten that paragraph to make it more understandable.

HABITAT SUITABILITY CRITERIA (HSC) DATA COLLECTION

William Miller

Comment 1: Accuracy? 1 m or 1 cm? (Page 16)

Response: The rated horizontal accuracy of the GPS unit we used to mark redd locations was 3 to 7 meters.

Dennis Rondorf

Comment 1: “redd construction in all three years”. Is the number of redds in each reach for the study period reported in this report? If so this seems an appropriate location to cite in text. (Page 11, Paragraph 3, Line 16)

Response: The report already provides the number of redds in each reach for the study period for the spring-run Chinook salmon and steelhead. We have added this information for the fall-run Chinook salmon.

Comment 1: “Depth and water velocity were measured over the redds with an ADCP”. The next paragraph on page 17 starts “All data were entered into spreadsheets for analysis”. How were these water velocity data from ADCP used? Was a water column average used? (Page 16, Paragraph 2, Line 10)

Response: We calculated the water column average velocity for each ensemble and then calculated the average of these for all of the ensembles. We have added this language to the text to clarify this procedure. The resulting average water velocity was used to develop the HSC. The referenced paragraph states that the ADCP was used for redds in deep water. Given that this section of the report deals entirely with collecting depth, velocity and substrate size data for developing the HSC, it would seem adequately clear that the velocity data was collected for this purpose.

Comment 1: Insert “Habitat” to describe data. (Page 17, Paragraph 1, Line 1)

Response: We have modified the text as suggested.

BIOLOGICAL VALIDATION DATA COLLECTION

William Miller

Comment 1: Physical validation? (Page 17)

Response: We prefer the term “Biological Validation” since the objective is to validate that the model is accurately predicting the suitability of the habitat for salmonid spawning compared with what was observed in the study sites. See also our response to Dennis Rondorf’s specific comments 1 and 2 immediately below. Physical validation of the model is addressed by the River2D Model Velocity Validation section of the report.

Comment 2: Did this affect results? (Page 17)

Response: We do not think that this affected the results, since almost all of the spring-run spawning was above Daguerre Dam and we collected biological validation data for spring-run for all of the sites above Daguerre Dam.

Dennis Rondorf

Comment 1: Define “biological validation” in topic sentence rather than the “recording by sighting of horizontal locations”. The first sentence of the Habitat Suitability Criteria section describes what the HSC curves are used for. The same approach describing how the Biological Validation data is used should be described for the reader. Note, the purpose is currently located at P 17, Pr 5, L 1. (Page 17, Paragraph 2, Line 1)

Response: That paragraph has been reorganized and language added to the biological validation section to provide the suggested topic sentence and better clarify the biological validation process.

Comment 2: Biovalidation data is undefined at this point in text and should be spelled out. (Page 17, Paragraph 2, Line 7)

Response: Language has been added to that paragraph that further clarifies what collection of biovalidation data collection included.

Comment 2: “compound suitability predicted by the River2D model” at this point the term “compound suitability” is undefined, furthermore, I’m not sure this is the appropriate location in the text if you are doing to define and continue to use. (Page 17, Paragraph 5, Line 1)

Response: The compound suitability is the product of the depth suitability, the velocity suitability, and the substrate suitability. We have provided this definition at the beginning of that paragraph.

PHABSIM WSEL CALIBRATION

WILLIAM MILLER

Comment 1: Was the ADCP calibrated? (Page 18)

Response: There is no calibration done for ADCPs.

Dennis Rondorf

Comment 1: “This equation was developed from four measurements of flow”. Seems like a minimal effort compared to others for this project. (Page 20, Footnote 15)

Response: We believe that this was a sufficient number of flows to develop the relationship. Furthermore, the flows from the Goldfields has a small effect on the flows for the Upper and Lower Daguerre and Pyramids study sites because the Goldfields flows are much smaller magnitude than the main channel Yuba River flows. Typically, when the Marysville gage flow is less than the Smartville gage flow, there is no flow out of the Goldfields.

Comment 2: “We did not regard these slightly low VAF values as problematic since RHABSIM was only used to simulate WSELs and not velocities.” The authors are best able to judge the statement above. On the other hand, what were WSELs used for? (Page 23, Paragraph 3, Line 5)

Response: As stated in the first paragraph of the PHABSIM WSEL calibration section:

“By calibrating the upstream and downstream transects with PHABSIM using the collected calibration WSELs, we could then predict the WSELs for these transects for the various simulation flows that were to be modeled using RIVER2D. We then calibrated the RIVER2D models using the highest simulation flow. The highest simulation WSELs predicted by PHABSIM for the upstream and downstream transects could be used for the upstream boundary condition (in addition to flow) and the downstream boundary condition. The PHABSIM predicted WSEL for the upstream transect at the highest simulation flow could

also be used to ascertain calibration of the RIVER2D model at the highest simulation flow. Once calibration of the RIVER2D model was achieved at the highest simulation flow, the WSELs predicted by PHABSIM for the downstream transect for each simulation flow were used as an input for the downstream boundary condition when running the RIVER2D model production run files for the simulation flows.”

RIVER2D MODEL CONSTRUCTION

William Miller

Comment 1: Why such a wide error for horizontal accuracy? (Page 26)

Response: We conclude that a 1.0 foot horizontal accuracy was sufficiently accurate for purposes of developing the bed topography of a river that is over 100 feet wide (i.e., an error of less than 1%).

RIVER2D MODEL CALIBRATION

Dennis Rondorf

Comment 1: “In the cases of Hammond.....” Does this agree with P 19, Pr 3, L1? On page 19 there is reference to six sets of measured WSELs were used, but I only see 5 named on page 19. (Page 27, Paragraph 2, Line 1)

Response: Yes, this agrees with page 19, paragraph 3. line 1; five sets of measured WSELs were used to develop the stage-discharge relationships for the Hammond site that are an input to RIVER2D, but only one set of measured WSELs was used to calibrate RIVER2D. See our response to Dennis Rondorf comment 2 for the PHABSIM WSEL Calibration section. We have added additional language at the end of the first paragraph in that section to further clarify the relationship between RIVER2D calibration and PHABSIM calibration. The six sets of WSELs referred to on page 19 were for Upper Daguerre, Lower Daguerre, Hallwood and Plantz sites. As stated on page 19, paragraph 3, first line, there were only five sets of WSELs for the Hammond site.

Comment 2: “As a result, we felt that it would be more accurate to calibrate these sites using the measured WSELs for the highest flow within the range of simulated flows.” If this is sound logic half the time, why not use it the other half of the time? This is a good example where a brief discussion section would give the authors the opportunity to explain some of the “xxxx” statements in this report. (Page 27, Paragraph 2, Line 8)

Response: Our general rule is that it is more accurate to calibrate sites using the WSELs simulated by PHABSIM at the highest simulation flow because the RIVER2D model is more sensitive to the bed roughness multiplier at higher flows, versus lower flows. However, when we have concluded, as for these sites, that the simulation of the WSEL at the upstream transect at the

highest simulation flow by PHABSIM is inaccurate, it no longer makes sense to calibrate RIVER2D using the WSELs simulated by PHABSIM at the highest simulation flow. In these cases, we use the fall-back option of calibrating RIVER2D using the WSELs measured at the highest flow within the range of simulation flows. We have added the above to the new discussion section.

Comment 3: “Accordingly, we conclude the calibration for these five sites was acceptable”. This reviewer thanks the authors for the detailed, candid, and honest explanation of the calibration process. However, there are many details I mention elsewhere in this review that I would rather read about and I think would better inform the reader. This section was a torturous read. (Page 28, Paragraph 2, Line 13)

Response: Although the RIVER2D model calibration process may not make the most interesting read, this step in the modeling process is perhaps one of the most important in verifying that the end product is reliable. Given that this particular section is only about a page and a half long, we have retained it in its entirety.

Comment 4: “Velocity validation is the final step...” This is a good introductory topic sentence and is the kind of topic sentence that could be at the start of each section to link this text to a conceptual model for the process. (Page 28, Paragraph 3, Line 1)

Response: We appreciate this suggestion. Similar introductory topic sentences have been added as needed to the various Methods sections.

RIVER2D MODEL VELOCITY VALIDATION

Dennis Rondorf

Comment 1: “magnitude that fall within the amount of variation in the ADCP velocity measurements”. I hate to admit this, but the first ADCP that I purchased had serial number 5 and a lot has changed since then. This is an perfect example of where you should provide some idea of that amount of variation. Please see also P 31, Pr 2, L 2 We attribute this to errors in the ADCP measurements (being too low). (Page 29, Paragraph 2, Line 3)

Response: See response to Dennis Rondorf’s general comment 3.

Comment 2: “For U.C. Sierra” The location in the text would suggest this is a topic sentence, but this can not be English we are dealing with here, right? First, this is a rather poor topic sentence. Second, of all the information that the authors place in footnotes that should simply be in the text in a complete sentence, this information should be removed from the text and placed in a footnote. (Page 29, Paragraph 3, Line 1)

Response: We have rearranged this sentence to make it a more readable topic sentence. We have not placed the list of deep beds where RIVER2D over or under-predicted the velocities on one or both side of the channel in a footnote. We believe this information is necessary to understanding the paragraph and would create an excessively large footnote.

Comment 3: Some of theses simulated velocities are over and under by a large measure. (Page 31, Paragraph 2)

Response: In this paragraph we specifically describe the fact that RIVER2D over-predicted the simulated velocities for these deep beds. Hammond deep bed D, Lower Daguerre deep beds A and M, and Pyramids deep bed C each show a small area where the simulated velocities were under-predicted, but the vast majority of these deep bed runs were over-predicted.

RIVER2D MODEL SIMULATION FLOW RUNS

Dennis Rondorf

Comment 1: Appendix G. the Timbuctoo site: What is the mechanism that causes the Net Q <0.01% at 600 cfs when other flows of that magnitude have Net Q >1%. (Page 31, Paragraph 4, Line 5)

Response: For a number of flows higher than 600 cfs, there appears to have been an eddy that set up on the far left side (looking downstream) of the wetted channel next to the downstream boundary that likely had to do with a somewhat higher bed elevation in that area. By 600 cfs, the location of the eddy was nearly dry and the eddy had almost completed dissipated. The net Q, which had previously been negative due to the upstream flow at the boundary caused by the eddy, appears to have been in transition toward a positive net Q that for the successive lower flows exceeded 1%. By 500 cfs, the eddy was completely gone and the net Q had become decidedly positive, with a net Q exceeding 1%. We don't have a definite explanation for the positive net Q that developed at the lowest flows, but it appears that at 600 cfs, the slight remaining eddy countering the trend toward the excessive positive net Q resulted in a very low net Q for that run.

HABITAT SUITABILITY CRITERIA (HSC) DEVELOPMENT

William Miller

Comment 1: HSI does not match biological use. Literature on spawning would refute HSI. (Page 42)

Response: See response to William Miller's General Comment 8.

Dennis Rondorf

Comment 1: “where Exp is the exponential function....” There is no diagnostic information for the logistic regression and this seems an appropriate location to discuss fit, etc. (Page 33, Paragraph 2, Line 6)

Response: We have added McFadden’s Rho-squared values to Table 9 to give a quantitative assessment of the fit of the logistic regression. The fit of the logistic regression can also be assessed qualitatively by comparing the HSI to the occupied and unoccupied frequency distributions in Figures 8 to 13. The text already includes the following discussion of the fit:

” In general, the criteria track the occupied data, but drop off slower than the occupied data due to the frequency of the unoccupied data also dropping over the same range of depths and velocities.”

BIOLOGICAL VALIDATION

Dennis Rondorf

Comment 1: “We ran the RIVER2D.....”. What was the length of the time period from the start of the spawning season to redd location. Perhaps this could be presented in a small table in text. (Page 37, Paragraph 4, Line 5)

Response: The time periods have been added to the text in this paragraph.

HABITAT SIMULATION

Dennis Rondorf

Comment 1: The acronym WUA is undefined in the document at this point I believe. (Page 45, Paragraph 2, Line 4)

Response: We have defined the acronym WUA where it is first used in the report (in the second paragraph of the introduction).

Comment 2: “we used the fall-run Chinook salmon multipliers because we didn’t do a synoptic survey to count spring-run Chinook salmon redds in the entire river.” This is an explanation as to why, but is this justified? (Page 46, Paragraph 1, Line 1)

Response: We feel that this is justified because it is the best method we have available to extrapolate spring-run spawning habitat to the entire river. Further, the choice of multipliers does not affect the relative amount of habitat at one flow versus another flow, and thus would not affect flow-related decisions that might be made with the flow-habitat relationships.

RESULTS

BIOLOGICAL VALIDATION

Dennis Rondorf

Comment 1: “significantly higher for locations with redds (median = 0.23, n = 146)...”. if locations without redds were sampled, why did you create an unbalanced sample size by using 8 X as many locations or 1,200 sites for locations without redds. I understand the comparison and have no problem with that, just that the authors created 8 times as many unused sites. Is that driving the high significance level. (Page 46, Paragraph 5 Line 2)

Response: We felt that an unbalanced sample size was required because the unoccupied locations had a much greater range of depths, velocities and substrates than the occupied locations. Thus, we needed 200 unoccupied locations per site to adequately capture the variation in unoccupied depths, velocities and substrates. To get a general idea whether an unbalanced sample size affected the results, we repeated the Mann-Whitney U test with only the first 146 unoccupied locations – this still resulted in a p-value of less than 0.000001. Thus, we conclude that the unbalanced sample size did not drive the high significance level.

Comment 2: “The 2-D model predicted that 23 of the 146 (16%) redd locations had a combined suitability of zero.” See general comments about prediction of redd locations and errors of commission and errors of omission. (Page 47, Paragraph 1, Line 2)

Response: See response to Dennis Rondorf General Comment 4. This is a specific example of where we evaluated errors of omission.

HABITAT SIMULATION

Dennis Rondorf

Comment 1: The Appendices in this report are a bit of a jungle because they are not adequately labeled according to which appendix an individual plate is located in. Furthermore, the titles are at best cryptic and the style is inconsistent. The style is inconsistent because sometimes the titles are at the bottom of figures, and sometimes at the top. (Page 51, Paragraph 1, Line 2)

Response: We have added headers at the top of each page in the appendices that provide the appendix letter. This should eliminate any confusion about which appendix the reader is looking at. We are confused by your comment that the style is inconsistent because sometimes the titles are at the bottom of the figures, and sometimes at the top. A review of the figures in all of the appendices found the figure titles to be located at the top of each figure in all cases.

Comment 2: This is a general comment about how the important results are presented in this paragraph. I recommend a summary figure to show the peak WUA flow levels and the spawning period from this study for each species on one figure. This summary figure would depict all of the results described in this paragraph. (Page 51, Paragraph 1)

Response: We disagree with this suggestion since we would prefer that the results for each species be considered on an individual basis, given that spring and fall-run Chinook salmon and steelhead/rainbow trout spawn during different time periods. We also feel that keeping the results for each species separate will make for easier scrutiny of each WUA curve compared with trying to discern between co-mingled curves. In addition, because there are marked differences in the WUA peaks between the spring and fall-run Chinook salmon and steelhead/rainbow trout (ranging from 174,650 ft.² for the steelhead/rainbow trout to 920,840 ft.² for fall-run Chinook salmon) the scale required to accommodate the fall-run Chinook salmon WUA curve would significantly affect the shapes of the spring-run Chinook salmon and, particularly, the steelhead/rainbow trout curves, considerably flattening them.

EVALUATION OF ALTERNATIVE SUBSTRATE DATA COLLECTION METHODS

BIOLOGICAL VALIDATION

Dennis Rondorf

Comment 1: “verses the alternative substrate (polygon) data.” Here the authors define in an exemplary manner the two substrate data collection methods in the topic sentence and I still have a comment. In this case, why not just define the second method as the polygon method. Inasmuch as the standard method is equally vague, could that be defined as the transect method? Just trying to keep things simple and descriptive for the reader. (Page 51, Paragraph 2, Line 2).

Response: We have provided language in the Evaluation of Alternative Substrate Data Collection Methods methods section referencing the standard method as that described in the Hydraulic and Structural Habitat Data Collection Section. We have also changed all references in the report for the “alternative polygon” to “polygon” to simplify the text for the reader. We believe that use of “standard” and “polygon” as the descriptive terms for the two methods is adequately clear for the reader.

APPENDIX F

Dennis Rondorf

Comment 1: Placing a header of “Appendix F continued” on the top of pages would help the reader know where they are in numerous pages of Appendices. (Page 104, Appendix F)

Response: See our response to your Habitat Simulation comment 1.

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Appendix B

Responses to Stakeholder Comments

for the

**May 2007 Stakeholder-Review Draft of the
Yuba River Spawning Instream Flow Study Report**

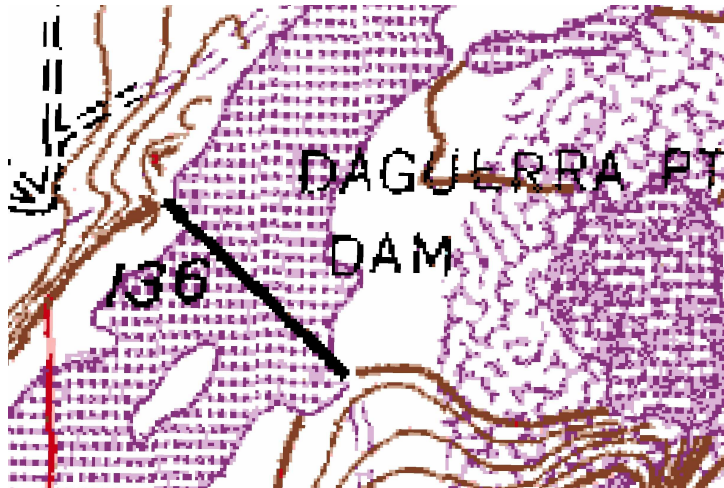
August 2010

PG&E Comments

General Comments

PG&E 1. *References to “Daguerra Dam” need to be changed to “Daguerre Point Dam.”*

Response: As shown below, the USGS quad map shows the spelling as Daguerra Point Dam. However, our research indicates that the correct spelling is Daguerre Point Dam (Gilbert 1917, Sumner and Smith 1939, Hagwood 1981), and that the spelling on the USGS quad map is incorrect. In contrast, the oldest record that we could find (Price and Nurse 1896) uses the spelling Daguerra Point. We have submitted a request to the U.S. Board on Geographic Names to correct the spelling of Daguerre Point and Daguerre Point Dam. Throughout the report we have changed Daguerra Dam to Daguerre Point Dam on the assumption that the U.S. Board on Geographic Names will approve these corrections.



PG&E 2. *As discussed in the comments below, this report needs significant revisions in order to provide a more objective and verifiable assessment of conditions in the Lower Yuba River. The habitat suitability criteria presented in the report, especially those for steelhead, appear to be significantly influenced by a very small number of outlying observations. Perhaps as a result, many of the habitat predictions appear to have a poor correlation with spawning sites. We recommend that the report and the accompanying simulations be revised as suggested below and re-circulated for stakeholder review.*

Response: As discussed in our responses to PG&E's comments 3-20 below, we have made some additions to the report. We believe that the report provides an objective and verifiable assessment of conditions in the Lower Yuba River. We have added an analysis to the report that shows that the steelhead depth habitat suitability criteria are not significantly influenced by a small number of outlying observations. Our analysis shows that the habitat predictions have a good correlation with spawning sites. The low combined suitability index for steelhead/rainbow trout shows that there is little optimal spawning habitat present at existing flows. We have made some additions to the report. Accordingly, we believe that recirculation of the report for stakeholder review is not necessary. Prior to this report being signed, FWS provided an opportunity for technical representatives of YWCA and other interested parties to discuss these responses.

Introduction

PG&E 3. General. *The introduction needs to address the overall context and background of the Lower Yuba River. For example, why did USFWS and CalFed decide to repeat instream flow studies for salmon on the lower Yuba River? Please discuss the Lower Yuba River instream flow study that CDFG conducted in collaboration with other stakeholders (Beak 1989). It is important to present the current study in context, since Beak (1989) has been used in the development of the Yuba River Accord flow schedules. The introduction would also be improved by addressing other questions, e.g., what involvement did other stakeholders have in selecting study sites and collecting data for the USFWS study? What effect did the flood event of January 1, 2006 have on the sites modeled in this report, and how does this affect the conclusions in the report, given the focus on specific sites rather than a representative sampling of habitat?*

Response: The introduction addresses the overall context and background of the Yuba River. We recognize that Beak's (1989) study reflected the standard practices for instream flow studies in the 1980's. However, the techniques for performing instream flow studies have been significantly refined since the 1980's to increase the accuracy of habitat predictions and reflect the hydraulic complexities of river channels. In particular, USFWS¹ decided to conduct instream flow studies for salmon on the lower Yuba River which utilize the improved practices for conducting instream flow studies to develop habitat suitability criteria and hydraulic modeling of available habitat. The specific procedures used in this study that were not used in the Beak (1989) study include: 1) the use of Type II criteria with application of a technique to correct for availability (Guay et al. 1991); 2) collection of habitat use data with equal sampling of different mesohabitat types to address habitat availability and (possibly) at a high enough flow so that fish can select their preferred habitat characteristics; 3) the application of a procedure to adjust spawning depth habitat utilization curves for availability (Gard 1998); 4) the use of a two-dimensional hydraulic and habitat model, instead of PHABSIM; and 5) placement of sites for modeling spawning only in heavy spawning-use areas². The above discussion of the Beak (1989) study has been incorporated into the executive summary for this report. Other stakeholders were not involved in selecting study sites and collecting data for the USFWS study, with the exception of Bill Mitchell of Jones and Stokes, who participated in collecting fall-run Chinook salmon HSI data on November 13, 2001. However, presentations were made to the stakeholders in the Lower Yuba River Biological Technical Team on the selected study sites and data collection for the USFWS study. We have added material to the discussion regarding the effect of the January 1, 2006 flood event on the Yuba River channel and how this affects the conclusions in the report. We conclude that the spawning sites are a representative sampling of high-use spawning habitat in the Yuba River.

PG&E 4. Page 2, last paragraph. *We believe that the authors may be misinterpreting the citations presented in support of logistic regression approach to develop HSC. Please expand this discussion to include the specific quotes from these citations that the authors believe support their position. Also, please expand the paragraph presented on pages 2 and 4 to discuss other more commonly used techniques for HSC development, and comparisons that have been done of different approaches to developing HSC, such as TRPA (2001). Since HSC are fundamental*

¹ CalFed was not involved in the decision to conduct an instream flow study on the Yuba River.

² This is one way to address factors, such as permeability and upwelling, other than depth, velocity and substrate, which control the distribution of spawning (Gallagher and Gard 1999).

drivers of a habitat flow model, a more extensive and balanced discussion of differences between the logistic regression approach to HSC development and other more commonly used techniques is needed.

Response: We believe that the report correctly interprets the citations presented in support of logistic regression approach to develop HSC. However, we deleted one reference (Rubin et al. 1991) because it discusses a similar method to logistic regression using density instead of presence-absence. We also rewrote the sentence in question to make it more general. This paragraph of the introduction already contains a summary of one citation (Guay et al. 2000) regarding the use of logistic regression to develop HSC. We have added a specific quote from the most recent of the references (McHugh and Budy 2004). The text on pages 2 and 4 already includes the most commonly used technique for HSC development (criteria developed only from use data). The other previously common method of developing HSC (preference ratios of use divided by availability) is referenced in the HSC development portion of the methods section, and thus does not need to be repeated on pages 2 and 4. As noted in TRPA (2001), the use of preference ratios is no longer recommended because preference ratio HSC consistently do not transfer to the river from which they came. TRPA (2001) examined the three above HSC methods, plus a density method. The density method is not commonly used, and is not appropriate for spawning HSC because spawning HSC data do not have a density associated with them (every occupied data point consists of one redd). We have not included TRPA (2001) in this section because the references already include comparisons of different approaches to HSC. For example Guay et al. (2000) compares logistic regression to preference ratio HSC. Since there are sufficient comparisons of different approaches to HSC in the scientific literature, we believe that it is not necessary to cite non-peer-reviewed gray literature (i.e. TRPA (2001)). Additionally, we note that TRPA (2001) suggest that their use criteria are biased towards low velocities due to availability, and found little difference between the HSC produced by the other three methods used. Both of these conclusions are consistent with the text on pages 2 and 4. We believe that the discussion of HSC on pages 2 and 4 is balanced and sufficiently extensive. We also note that there is additional discussion of logistic regression versus use HSC in the discussion section of the report.

Methods

PG&E 5. Page 4, last paragraph. *The statement is made that “The 2-D model avoids problems of transect placement, since data is collected uniformly across the entire site.” However, the report provides no data or graphics to show the location or density of field data points collected at each site. Please provide an appendix with a graphic for each site noting the location of field data points (not the computational mesh) used as input for the 2-D model.*

Response: The report currently includes data on the density of field data points collected at each site (Table 7). In addition, we have added an appendix showing the location of field data points used as input for the 2-D model (new Appendix B).

PG&E 6. Page 7, second paragraph. *Please provide more detail on the selection of the study sites, such as what proportion of the population of spawning salmon used the sites, and how representative was the habitat at the selected sites to the rest of the lower Yuba River. Please*

explain why you believe modeling only sites with active spawning under current hydrology provides a valid extrapolation of conditions under altered flows, without having to model other representative habitats as was done by Beak (1989).

Response: The habitat simulation portion of the results section currently gives information on the proportion of the population of spawning salmon using the sites. Specifically, the ratios of total redds in the segment to the number of redds in the modeling sites is the inverse of the proportion of the population of spawning salmon using the sites. Thus, 45 percent (1/2.2) of spawning fall-run above Daguerre and 42 percent (1/2.37) of spawning fall-run below Daguerre used the study sites. We believe that the spawning sites are a representative sampling of high-use spawning habitat in the Yuba River, based on the number of sites and the proportion of the population using the study sites. Only high-use spawning areas are relevant for evaluating spawning habitat, based on the conceptual model given in the introduction. Specifically, redd superimposition would be expected to almost exclusively occur in high-use spawning areas. Spawning would not be expected to occur at high enough densities in other portions of the Yuba River, due to factors independent of flow such as low gravel permeability or upwelling, to result in redd superimposition, and thus habitat does not need to be assessed in such areas. Support for this is given by Gallagher and Gard (1999), who found that high spawning use occurred in the same areas over a range of flows.

PG&E 7. Page 20, 2nd paragraph, page 22 last paragraph. *These paragraphs describe measurements of spring-run Chinook redds primarily in late September 2002, but no detail is provided to explain how these sites were determined to be spring-run and not early-arriving fall-run Chinook, which are abundant in the Yuba River in August and September. The Yuba River is thought by NMFS to be significantly influenced by Feather River Hatchery spring run, which have been excluded from the ESU (NMFS 2005). A recent comment by Alice Low of CDFG regarding spring-run recovery planning pointed out that “A genetically distinct spring-run population has not been identified to date in the lower Yuba River, using microsatellite methodology.” Given the uncertainty surrounding the nature of Chinook salmon spawning in the lower Yuba River in September, information needs to be provided regarding how spring-run Chinook were identified before the HSC presented in this report as representing spring-run can be accepted as such.*

Response: It is the long-standing and current practice in Central Valley spawning HSI data collection to assume that any redds constructed during the month of September (i.e. prior to October 1) are spring-run and that any redds constructed between October 1 and December 31 are fall-run (Vogel and Marine 1991). We have followed this practice for this study.

PG&E 8. Pages 20, 21, and 22, Habitat Suitability Criteria (HSC) Development. *The procedure described for HSC development allows for a very few observations of use and availability to drive the HSC. This is most evident in the distorted steelhead spawning depth HSC illustrated in Figure 16 on page 45. This HSC suggests that the optimum suitability is at depth is 15.1–16.9 feet, despite the fact that only five fish were observed spawning in such very deep water. The vast majority of the steelhead spawning was observed at depths less than four feet, which is given a suitability less than 0.4. This distortion no doubt contributes to the extraordinarily poor relationship between steelhead redd locations and predicted suitability demonstrated in the site graphics presented in Appendix J. Distortion of HSC by outliers in the data distribution is also apparent to a lesser degree in the other HSC presented in this report.*

One possible way to deal with this issue would be to adopt the approach used by the USFWS in Hampton (1988) to eliminate the influence of outliers in the distribution. Hampton (1988) noted that when both the use and availability simultaneously enter the limits of their distribution, there is a danger of misrepresenting actual preference. To avoid this problem he applied nonparametric tolerance limits to each frequency distribution which included 90% of the use observations at a 90% confidence level. Such an adjustment would likely bring the HSC presented in this report into a more believable range.

Response: We have added an analysis to the report (see Discussion HSC development, Figures 36 and 37) that shows that the steelhead depth habitat suitability criteria are not significantly influenced by a very small number of outlying observations. When the upper 5 percent of occupied and unoccupied observations were excluded from the logistic regression, so that we were using the middle 90 percent of the distribution, the logistic regression still reached a peak in deep water. We found similar results when we only used occupied and unoccupied values with depths less than 5.8 feet (the 95th percentile unoccupied value), indicating that the shape of the curve was driven by the relative number of occupied and unoccupied values with depths of 2 to 5 feet versus 5 to 5.7 feet. As noted in the report, 24 percent of the steelhead redds were in depths greater than 5 feet. We feel that the steelhead depth curve correctly represents the preference of steelhead/rainbow trout for deep water in the Yuba River, and is not distorted. The actual percentage of steelhead/rainbow trout redds in depths less than four feet was 64 percent. Our analysis shows that the habitat predictions have a good correlation with spawning sites. The low combined suitability index for steelhead/rainbow trout shows that there is little optimal spawning habitat present at existing flows. Given that we have found that the steelhead/rainbow trout HSC are not distorted by outliers, we believe that the other HSC presented in this report are similarly not distorted by outliers. As noted above, our application of a method comparable to that suggested by Hampton (1988) still resulted in a peak in deep water. The logistic regression for steelhead/rainbow depth clearly shows that steelhead/rainbow are preferentially selecting deeper areas than are commonly available in the Yuba River. What is seen in the use data is what the fish are limited by due to the low availability of deeper waters in high-use steelhead/rainbow spawning areas. The intent of HSC are to show what the fish prefer, not what they are forced to use due to low availability of preferred habitat conditions. We conclude that the HSC presented in this report already have a believable range.

PG&E 9. Comment: *It is worth noting that the problem with the steelhead depth HSC was already identified by one of the peer reviewers whose comments were provided with the report; but his comment was ignored by the authors.*

Response: We responded to the peer reviewer's comment in the response-to-peer-review-comments document (Appendix A).

PG&E 10. Page 23, Habitat Simulation. *According to the report “ The WUA values for the sites in each segment were added together and multiplied by the ratio of total redds in counted in the segment to number of redds in the modeling sites to for that segment to produce the total WUA per reach.” This approach seems questionable for several reasons. First, no evaluation is presented as to how representative the modeled sites are of habitat in the lower Yuba River as a whole, and what effect of the redd-based extrapolation has in comparison to a conventional habitat-based extrapolation. Second, no literature support is provided to justify the use of this technique over a habitat-based extrapolation. Is this method unique to the Sacramento Fish and*

Wildlife Office? Third, even if extrapolation based on redd density is valid, the use of fall-Chinook redd density to extrapolate for spring-run Chinook density is not. Because spring-run Chinook tend to migrate upstream during the course of the summer, they should use habitat closer to Englebright Dam at a higher proportion than would fall-run Chinook. Finally, any redd-based extrapolation should first occur on a site-by-site basis to account for differences in hydraulics and use between sites before being summed to provide a segment total.

Response: Our approach is to represent high-spawning-use habitats in the Yuba River, rather than the entire Yuba River. Based on the conceptual model presented in the introduction of the report, spawning habitat can affect salmonid populations by changing the amount of redd superimposition. Redd superimposition would be expected almost entirely in high-spawning-use areas. Thus, the amount of habitat present in areas without high spawning use would not be expected to affect salmonid populations. In addition, modeling of habitat in high-use-spawning areas captures characteristics of spawning habitat, such as permeability and upwelling, which are key characteristics of spawning habitat and are not captured by depth, velocity and substrate. In contrast, a habitat-based extrapolation does not take into account characteristics of spawning habitat, such as permeability and upwelling, and would greatly overestimate the amount of available spawning habitat. See also response to PG&E comment 20. We have determined that the modeled sites are representative of high-spawning-use habitats in the Yuba River, based on the number of sites and the percentage of the entire river spawning which is in the sites. On this basis, we conclude that the effect of the redd-based extrapolation provides a more accurate assessment of the effects of habitat on salmonid populations than would a conventional habitat-based extrapolation. A literature reference (Gallagher and Gard 1999) has been added to the report to provide more information regarding the use of this technique over a habitat-based extrapolation. We do not know if this method is unique to the Sacramento Fish and Wildlife Office; however, we do note that instream flow studies frequently place transects specifically to model spawning habitat. It should be noted that the redd extrapolation method is merely a scaling mechanism, and thus does not change the shape of the flow-habitat relationship. As such, we believe that the use of fall-Chinook redd density to extrapolate for spring-run Chinook density is acceptable because it would not affect flow management decisions. Extrapolation first on a site-by-site basis would double-count the effects of habitat area, since the number of redds in a site is proportional to the amount of habitat area in a site (Gallagher and Gard 1999). Thus, it would be inappropriate to first perform a redd-based extrapolation on a site-by-site basis. In this regard, differences between sites in hydraulics and use are already accounted for in the amount of spawning habitat at different sites.

Results

PG&E 11. Page 24, Study Segment Selection. *The discussion of study segment selection would be improved by including a discussion of how segments used in this study compare with the segments modeled by Beak (1989). Beak (1989) identified four study segments based on both flow and habitat: The Narrows Reach (from Englebright Dam downstream 3.5 km.), Garcia Gravel Pit Reach (from the end of the Narrow Reach 17.2 km to Daguerre Point Dam), Daguerre Point Dam Reach (From Daguerre Point Dam downstream 12.6 km), and Simpson Lane Reach (from the end of the Daguerre Point Dam Reach 5.6 km downstream to the confluence with the Feather River). Since the USFWS report does not model habitat in either the Narrows or Simpson Lane reaches, it would be helpful to discuss the possible implications of this difference.*

Response: A comparison of the study segment selection in this study with that in Beak (1989) is not warranted. Bovee (1995a) notes that the flow regime is the primary determinant of segments. Thus, Beak (1989) was incorrect in identifying four study segments, since there are only minor differences in flow between the Narrows and Garcia Gravel Pit Reaches and between the Daguerre Point Dam and Simpson Lane Reaches. It should be noted that our habitat typing showed differences between the Narrows and Garcia Gravel Pit Reach and between the Daguerre Point Dam Reach and Simpson Lane Reach, with a greater proportion of flatwater habitat units in the Narrows and Simpson Lane Reaches and a greater proportion of bar complex habitat units in the Garcia Gravel Pit and Daguerre Point Dam Reaches. For rearing habitat, the differences in mesohabitat composition between Beak's (1989) reaches would capture the variation between the reaches. For this study, the lack of sites in Beak's (1989) Narrows and Simpson Lane reaches would not have affected the flow-habitat relationships, since there were no high-spawning-use areas in either reach, and therefore has no implications on the results of this study.

PG&E 12. Page 25, Field Reconnaissance and Study Site Selection. *See comment on page 7.*

Response: See response to comment on page 7 (PG&E comment 6).

PG&E 13. Page 30, RIVER2D Model Velocity Validation. *Please discuss the effect that over or under-predicted velocities may have had on the results.*

Response: We believe that over or under-predicted velocities would have a minimal effect on the overall flow-habitat relationships, given the high correlation between measured and predicted velocities. Specifically, the effects of over-predicted velocities would be cancelled out by the effect of under-predicted velocities. The overall flow-habitat 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.

PG&E 14. Page 31, 3rd paragraph. *Explain the reasons why "it was sometimes necessary to make measurements at a 45 degree angle upstream or to the side." Also, please explain why any data collected 8 feet (or more?) away from a redd was used to represent conditions at the redd.*

Response: If another redd was located immediately upstream of a redd, it was necessary to make measurements at a 45 degree angle upstream or to the side, to avoid the hydraulic effects of the upstream redd. In collecting HSI data, we are trying to capture the conditions that were present prior to redd construction – in the above situation, the conditions present at the time of HSI collection immediately upstream of the redd would not be the same as those present prior to redd construction. We sometimes needed to take measurements at a location 8 feet (up to a maximum of 14 feet) away from the redd to avoid the hydraulic effects of other redds. Our best professional judgment in the field was that the measurement locations were the closest locations to the redds that had the conditions present prior to redd construction. It should be noted that the distance from the redd was measured from the middle of the pit, so for a very large redd, the upstream edge of the pit (the closest location that might have the conditions present prior to redd construction) might be as much as 5 feet from the redd.

PG&E 15. Page 31, 4th paragraph. *The majority (146) of the "spring-run" Chinook redd measurements were made September 23–26, 2002. Explain what steps were taken to assure that these redds were made by spring-run Chinook and not the early migrating fall-run that are common in the lower Yuba River (see comment on pages 20 and 22). Please include a discussion*

of migration patterns observed by CDFG since the installation of the VAKI counting system at Daguerre Point Dam, and any observations of fall-run Chinook movements in the lower Yuba River in 2002.

Response: As noted above (response to PG&E comment 7), it is the long-standing and standard practice in Central Valley spawning HSI data collection to assume that any redds constructed during the month of September (i.e. prior to October 1) are spring-run and that any redds constructed between October 1 and December 31 are fall-run (Vogel and Marine 1991). We have followed this practice for this study. A discussion of migration patterns observed by CDFG since the installation of the VAKI counting system at Daguerre Point Dam, and any observations of fall-run Chinook movements in the lower Yuba River in 2002 would not have an effect on our conclusions as to the run creating the observed redd, given the above standard practice for identifying what run of salmon has constructed a redd.

PG&E 16. Page 45, Figure 16. *See comments on pages 20, 21, and 22, Habitat Suitability Criteria (HSC) Development.*

Response: See responses to comments on pages 20, 21 and 22, Habitat Suitability Criteria (HSC) Development (PG&E comment 8).

PG&E 17. Page 47, Biological Validation. *The relationship between combined habitat suitability and redd distribution would likely be improved if the problems with the HSCs were corrected. The discussion of steelhead validation is particularly inappropriate. A combined habitat suitability at redd locations of 0.008 may be significantly different than the suitability without redds of 0.04, but it is still terrible, and should not be considered to validate the approach presented in this report.*

Response: We believe that there are not any problems with the HSC (see response to PG&E comment 8). As discussed below, we believe that the discussion of steelhead validation is appropriate. The numbers cited in the comment are incorrect – the median combined habitat suitability at redd locations was 0.08 and the median combined suitability at unoccupied locations was 0.004. Our analysis shows that the habitat predictions have a good correlation with spawning sites. The low combined suitability index for steelhead/rainbow trout shows that there is little optimal spawning habitat present at existing flows. Thus, the median combined habitat suitability at redd locations of 0.08 is reasonable, and validates the approach presented in this report.

PG&E 18. Page 72, Biological Validation. *This discussion fails to address the underlying problems in the simulations, particularly for steelhead. The plots of combined suitability and redd locations presented in Appendix J clearly show that the USFWS 2-D simulations almost never predict the location of steelhead spawning.*

Response: We believe that there are not any underlying problems in the simulations (see response to PG&E comment 17). We believe that the plots of combined suitability and redd locations presented in Appendix L (previously Appendix J) show that the USFWS 2-D simulations overall predict the locations with redds to be in areas with higher suitability than locations without redds. See for example the results for U.C. Sierra site. Overall, these plots show that there is little optimal habitat for steelhead spawning at existing flows. Note that Figures 22 and 23 summarize the data presented in Appendix L.

PG&E 19. Page 73, Habitat Simulation, 2nd paragraph. *There are too many differences between the Beak (1989) simulations and the 2-D simulations presented in this report to allow for the extensive speculations presented in this paragraph. Also, the HSC presented by Beak (1989) should not be dismissed out of hand, especially since they were the result of a collaborative process apparently not used in the current study. A more informative approach would be to define the extent different HSC drive differences between the two studies by running the USFWS 2-D model using the Beak fall-run Chinook HSC. Ideally, the authors should also re-run the Beak simulations with the HSC developed for this report.*

Response: We believe that we have captured the major differences between this study and the Beak (1989) study, and that the inferences drawn in this paragraph are supported by the information presented in this paragraph. We believe that it is appropriate to assume that the Beak (1989) criteria are biased towards low depths and velocities since they are based only on use data. In contrast, we have incorporated both use (occupied) and unoccupied data in developing HSC. With respect to collaboration, the response to comments document discusses the extent and frequency of stakeholder engagement in this study. We believe that it would not be appropriate to run the USFWS 2-D model using the Beak fall-run Chinook HSC because the Beak fall-run Chinook HSC are biased towards low depths and velocities. Similarly, we believe that it would be inappropriate to re-run the Beak simulations with the HSC developed for this report since the Beak (1989) study's use of mesohabitat mapping to place transects fails to take into account the key linkages between habitat and salmonid populations (i.e. redd superimposition) or the selection of high permeability locations for spawning (see response to PG&E comment 6).

PG&E 20. Comment: *This discussion assumes that the current study more accurately portrays high gravel permeability sites than did the Beak study, but presents no data on gravel permeability to support this conclusion. It may be equally valid to assume that high use of particular sites may be related to the hydraulic conditions that existed there at the flow present when observations were recorded, and that representative modeling of all the available habitats in the river, as was done by Beak (1989), will provide a more accurate simulation of the habitat over a range of flows.*

Response: We did not collect data on gravel permeability. In reference to this comment, the assumption of this study was that salmonids preferentially select high gravel permeability sites. This assumption was proven for the American River (Vyverberg et al. 1996); it is reasonable to apply this assumption to the Yuba River as well. Furthermore, on the American River the same areas receive high spawning use at a wide range of flows (Gallagher and Gard 1999). Based on this observation, we believe that it is unlikely that the high use of particular sites is related to the hydraulic conditions that existed there at the flow present when observations were recorded. Further support for this conclusion is given by the observation that the flows present during fall-run Chinook salmon HSC data collection (436 to 911 cfs) are significantly lower than the flows with the maximum amount of habitat (1000 to 1400 cfs). As discussed above, we believe that representative modeling of all the available habitats in the river would provide a less accurate simulation of the habitat over a range of flows because it fails to take into account the key linkages between habitat and salmonid populations (i.e. redd superimposition) or the selection of high permeability locations for spawning.

Greg Pasternak Comments

GP (Greg Pasternak) 1. Overview: *This study presents a thorough analysis of the depth, velocity, and substrates used by three target populations on the lower Yuba River. I have reviewed work by this group before, and I must say that this report presents the best quality work I have seen from them so far. They are clearly learning from their past challenges. Overall, I see a lot of value in the work presented. The main challenge with a detail-oriented project like this is clearly presenting each component of the analysis and the sources of uncertainty, and then determining and reporting the effect of that uncertainty on the outcomes. The part of the project that I understand best is all of the hydraulics presented. In the specific comments presented below, I report on several sources of error I see in their study. The biggest problem is still present from past reports, which is a poor understanding of the important role of eddy viscosity in their modeling. I encourage the team to learn more about this variable and how it is affecting their results. In terms of the other main part of the study- admittedly, I am not a leading expert on HSC development- but it seems to me that their comparison of occupied and unoccupied points suffers from the assumption that the unoccupied points- which are located within the pre-selected study sites (biased to represent spawning areas)- represent areas that fish choose to avoid. In fact, since these study sites are NOT limited in terms of physical habitat (according to CDFG and my own data), there ought to be significant areas of preferred habitat that are unoccupied. The actual unoccupied and non-preferred areas mostly lie outside the study sites in long pools and glides. So the very complicated process of developing the HSC seems flawed from the beginning assumption that the sites are physical habitat limited, when in fact they are not. Unfortunately, the final appendix with WUA presents no uncertainty bands. How can that be? The report shows an average error in velocity of ~30% for each site, so if you do a sensitivity analysis varying velocity by plus/minus 30% and then propagate that through the HSC, you can definitely get a range of WUA that is within the uncertainty band. Sensitivity analyses like that should be performed on each variable to help guide management decisions. Finally, it is notable that the report makes no mention of geomorphic processes, which are ultimately responsible for the conditions on the Yuba River. Already, the river has changed dramatically, and so the specific WUA from the report are hopelessly outdated. Overall, I commend the authors for making progress with their approach. I think if they take these comments to heart for their future studies, they will continue to progress.*

Response: We have added a section to the discussion on factors causing uncertainty in the modeled results and the effect of that uncertainty on the outcomes (Discussion – Factors Causing Uncertainty). The effects of eddy viscosity have been adequately accounted for in the hydraulic modeling (see response to GP comments 16 and 18). We have added additional material to the report on eddy viscosity (Appendix H) and believe that the treatment of eddy viscosity in the model has had a minimal effect on the results.

The comparison of occupied and unoccupied points does not suffer from the assumption that the unoccupied points represent areas that fish choose to avoid because the above is not an assumption of the logistic regression method (see response to GP comment 19). In addition, the study sites represent an unbiased sample of unoccupied spawning habitat (see response to GP comment 12). Further, there is evidence that spawning habitat may be limiting in the Yuba River (see response to GP comment 3). Also, the study sites include a substantial proportion of pools

and glides, and these habitat types are used for spawning (see response to GP comment 12). Accordingly, the HSC development process is sound, since the method does not assume that physical habitat is limiting.

We are not aware of any instream flow study that includes uncertainty bands on the flow-habitat relationships. This is likely because uncertainty bands from sensitivity analyses would have limited utility in making flow management decisions. Uncertainty bands would imply an artificially large range of flows with the maximum amount of habitat, and may even have a detrimental effect on reaching consensus among parties with differing resource priorities on flow management schedules. A sensitivity analysis of the effects of velocity simulation errors is much more complicated than suggested by the commenter. Since 2-D models use physical processes of mass and energy balance to simulate velocities, it is not possible to simply vary velocities by 30 percent. Rather, it would be necessary to randomly vary bed roughness on a point-by-point basis throughout the site, then conduct the hydraulic simulations for all of the simulation flows and then translate the resulting hydraulic modeling into flow-habitat relationships. The comment also implies that the measured velocities are an accurate estimate of the real velocities in the sites. In fact, because of errors in velocity measurements, both the measured and simulated velocities differ in an unknown fashion from the true velocities present in the sites. Furthermore, errors in velocity predictions likely have a minimal effect on the overall flow-habitat relationships, given the correlation between measured and predicted velocities. Specifically, the effects of over-predicted velocities would be cancelled out by the effect of under-predicted velocities. The overall flow-habitat 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. As with the general case, sensitivity analyses on each variable would have limited utility in making flow management decisions. We have added to the report a discussion of dynamic equilibrium, which captures the most important geomorphic processes affecting flow-habitat relationships (see Discussion – Habitat Simulation). We have also added a discussion on the effect of changes in the river (see Discussion – Habitat Simulation and Factors Causing Uncertainty). If the Yuba River is in dynamic equilibrium, the the overall flow-habitat relationships from this report apply to the current channel. When a channel is in dynamic equilibrium, there is an approximate balance between sediment supply and transport, so that the channel pattern and cross-sectional profile of the entire stream is consistent (Bovee 1996). For a stream in dynamic equilibrium, it would be expected that changes in the stream from large flow events would not result in a significant change in flow-habitat relationships.

Specific comments:

GP 2. p. 3: *this is a very nice flow chart of their work flow.*

Response: We appreciate the comment.

GP 3. p. 4: *It does not make sense to me to begin with assumption #1 that physical habitat is the limiting factor for salmonid populations in the Yuba River. That should have been checked first. In my own research of Timbuctoo Bend on the lower Yuba River, I found a lot of evidence that*

physical habitat is NOT limiting. For example, there is ample gravel and cobble of suitable size for spawning by Chinook salmon and adequate amounts of finer gravel for steelhead trout. There are also multiple geomorphic units (primarily riffles, riffle entrances, and lateral bars) available with the desired characteristics for spawning and embryo incubation. These units all have suitable depth, velocity, and substrates for these lifestages. Frequent floods rejuvenate these sites every few years. Finally, the observed ratio of spawners to redds is 4:1. This last fact for Timbuctoo Bend has also been confirmed for the entire lower Yuba River by fisheries biologists at the CDFG who are monitoring the river. I'm not sure why this assumption is required in order for this study to have value anyway. A better understanding of habitat is of use regardless of whether physical habitat is limiting or not.

Response: A basic assumption of all instream flow studies is that physical habitat is limiting fish populations (Bovee 1982, page 120). If some other factor such as food or fishing mortality is controlling the population size, rather than physical habitat, changes in physical habitat would not be expected to result in changes in the fish population. In 2002, we noted whether the fall-run redds which we measured HSI data on were superimposed or not; we found that 73 out of 213 redds (or 34%) were superimposed, suggesting that physical spawning habitat may be limiting in the Yuba River. Not all areas with suitable depth, velocity and substrate are used for spawning; salmonids preferentially select areas with high gravel permeability (see response to PG&E comment 10). A ratio of spawner to redds of 4 to 1 suggests that spawning habitat may be limiting – if there was sufficient spawning habitat to enable all adults to spawn, it would be expected that the spawner to redd ratio would be closer to 2 to 1 (in other words, that each spawning pair created a redd).

GP 4. P. 7: *PHABSIM was relied upon to obtain the water surface elevations at the downstream cross-sections of the project. I believe PHABSIM assumes steady, uniform flow, which is a poor assumption for the Yuba River, since its width and depth changes dramatically over short distances. I think this should be mentioned as a factor. I think there should also be some mention of WSEL validation right here. I suggest you refer to p. 12 and appendix B right here so the reader knows where to find that info.*

Response: Both PHABSIM and two-dimensional models assume steady flow (i.e. that flow does not change with time). PHABSIM does not assume uniform flow, but rather assumes gradually varied flow conditions, where depth and width change gradually over short distances. Furthermore, the locations of the downstream transects were selected so they were at areas where depth and width changed only gradually with distance upstream or downstream. Finally, PHABSIM was only used to develop stage-discharge relationships, and in all cases, the IFG4 option was used to develop the stage-discharge relationships. The IFG4 method is simply a log-log regression of flow versus the function (stage minus stage of zero flow). We could have developed these stage-discharge relationships in Excel, and thus not had used PHABSIM at all, and would have come up with identical stage-discharge relationships. The derivation of stage-discharge relationships empirically using regression, as was done in this case, does not require an assumption of uniform flow. Accordingly, we believe that PHABSIM was not a factor affecting the accuracy of the hydraulic modeling. There is no WSEL validation. It would not make sense

to refer to WSEL calibration, as discussed on page 12 and Appendix B, under transect placement (study site set-up), since WSEL calibration is not conducted until the point when hydraulic construction and calibration occurs.

GP 5. p. 8-9: *The information provided to explain the accuracy of ADCP for measuring depth is very misleading and inaccurate. The response to an earlier peer reviewer about this point was inadequate too. It looks like only the manufacturer's specifications are being reported. However, the truth is that the actual accuracy of ADCP to measure depth depends to a great degree on the topography of the bed and the depth itself. Since ADCP produces a downward cone that grows with distance, the area sampled is variable from point to point. When the signal cone hits a steeply sloped bed feature, then it averages the depth value for the whole area where the beam is hitting. Depending on how fast the boat is moving, the averaged area is variable and unknown. Professional hydrographers are widely aware of this problem and thus choose to use a single beam echosounder to collect bathymetric data and use the ADCP only for velocity measurement. If you are going to use ADCP for depth measurement, you would at least want to make comparable measurements with a better method, like the echosounder or a total station, to characterize the accuracy. Again, where people have done that before, the results have always shown that the ADCP is poor in settings like the Yuba.*

Response: We acknowledge that the depth measurements made by the ADCP represent an average depth over an area, but would not characterize this as a lack of accuracy of the ADCP depth data. We feel that the manufacturer's specifications for all of the field equipment correctly characterizes the accuracy of the field measurements. With regards to the ADCP depths, we would characterize the accuracy as being 4 percent of the average depth over the area measured by the ADCP. We do not feel that this is a shortcoming of the ADCP data, since the area averaged by the ADCP corresponds to the scale of the mesh elements of the hydraulic model and to the scale of individual redds. In most cases, the ADCP data was collected in areas with a very gradual slope – adjacent depth measurements typically only differed by 0.1 foot. We had some areas where we ended up with ADCP measurements collected in close proximity (typically within 1 foot) to total station measurements – for the most part, the bed elevations from these two methods were very close (typically within 0.1 foot). We note that ADCPs are now commonly used for measuring depths in instream flow studies, and that the US Geological Survey, the nation's preeminent hydrographers, use ADCP depth measurements for measuring discharges (Simpson 2001). In this regard, Simpson (2001, p. 119) states:

“Near the bank edges, the BB-ADCP beams orientated toward shore will show shallow depths, whereas the beams orientated toward the channel will show greater depths. An average of all four beams will approximate the vertical depth from the center of the BB-ADCP transducer assembly to the bottom. In pitch and roll conditions, averaged depth measurements from all four acoustic beams will be more accurate than depths measured by a single, vertically placed, depth sounder because of the large beam ‘footprint’ or pattern.”

We also note that the use of an ADCP for bathymetric data collection is given in a peer-reviewed journal article (Gard and Ballard 2003).

GP 6. p. 11. *For velocity validation, the report should state the criteria that determines whether the model is "valid" or not. How much deviation is permitted before you would reject the model?*

Response: We are not aware of any criteria in the literature for determining whether a model is valid or not. In fact, Pasternak et al. (2006) state:

“However, exactly what amount of error constitutes ‘validation’ is unsettled in hydrology.”

As such, we have set our own criterion of having a correlation between measured and simulated velocities of at least 0.6. We have added this to the report under velocity validation (page 18) – it would not make sense to state the criterion on page 11, since simulated velocities are not compared to measured velocities until the velocity validation stage.

GP 7. p. 11. *20 s is not the standard time used for velocity measurement. Normally it is 60 s, but that is a long time when you have a lot of data to collect. I've never head of anyone using less than 30 s before.*

Response: The standard time used for velocity measurements in instream flow studies is 40 seconds. In this regard, Rantz (1982) state that in the USA it is customary to observe velocity at a point by current meter for a period that ranges from 40 to 70 seconds. Bovee (1995a) found that magnitudes of errors associated with 20 and 40 second averaging intervals were about the same for velocities greater than 1.5 ft/s. We have found, with the Marsh-McBirney velocity meter, which shows a continuously updated average velocity over the averaging interval, that velocities stabilize in less than 20 seconds for velocities less than 1.5 ft/s. Accordingly, we believe that a 20 second time interval produces a sufficiently accurate velocity measurement.

GP 8. p. 10-11. *Using ADCP for velocity measurement in flows 3-6' is problematic, because there is a blanking distance for the top ~1', so you are losing anywhere from 18-30% of the profile. How did you account for that problem to enable a fair comparison with model predictions?*

Response: ADCPs are routinely used for velocity measurements in depths of 3 feet or greater. In this regard, Simpson (2001, p. 91) states:

“In general, the ADCP operator should look for a cross section with a roughly parabolic, trapezoidal, or rectangular shape, having an average depth of at least 1.5 m (5 ft). The measurement sometimes can be made at locations having less depth, if water modes 5 or 8 are employed.”

The only depth limitation given by Simpson (2001) is that there should be at least two good bins of velocity data collected. We operate our ADCP with the transducer faces 0.25 m below the water's surface and typically use a blanking distance of 10 cm. Accordingly, data starts being collected 1.15 feet below the water's surface. In addition, data is not collected near the bottom of the water column due to side lobe interference. As a result, in water less than 6 feet, the ADCP velocity measurement approximates a measurement at 0.6 of the depth. In water greater

than 6 feet, the ADCP velocity measurements sample from 0.2 to 0.8 of the depth. As shown in Table 2 below, with a typical vertical-velocity curve, a measurement at 0.6 of the depth is a close approximation of the mean column velocity. We calculated the velocities from the ADCP to compare to the model predictions by averaging the velocities measured in each cell. For example, for an ADCP measurement in 3.3 feet of water, we had velocity measurements in three cells (at 1.87 feet, 2.2 feet and 2.53 feet). These measurements are equivalent to measurements at 0.57, 0.67 and 0.77 of the depth. Interpolating from the ratios in Table 2 below, the average of these three velocity measurements is 97% of the mean water column velocity.

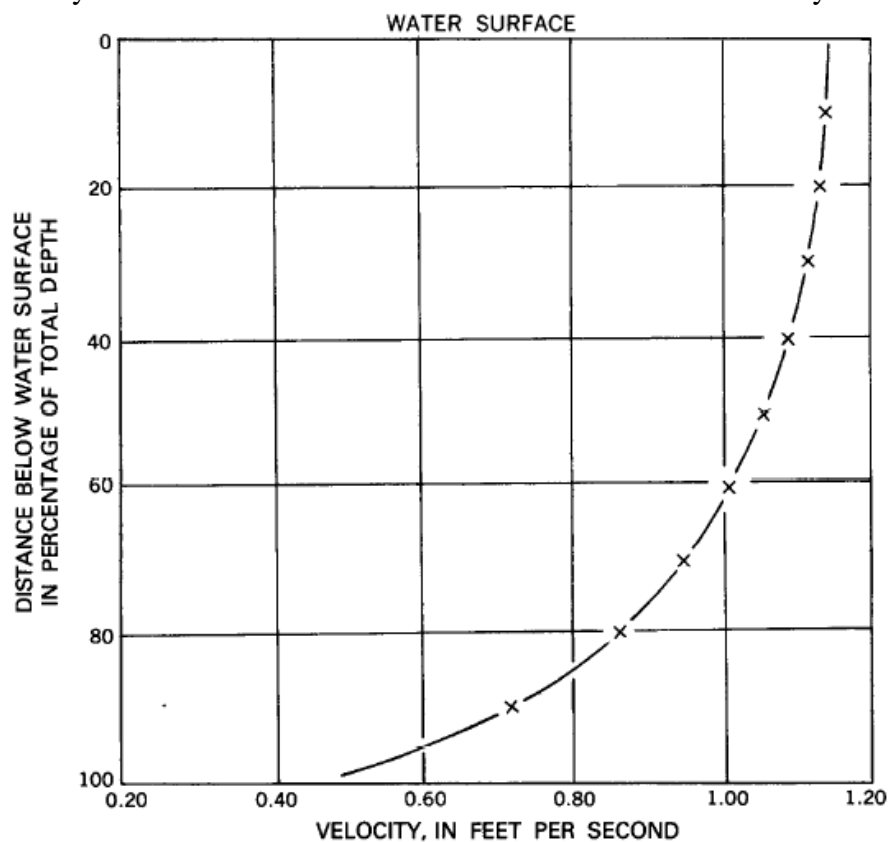


FIGURE 88.—Typical vertical-velocity curve.

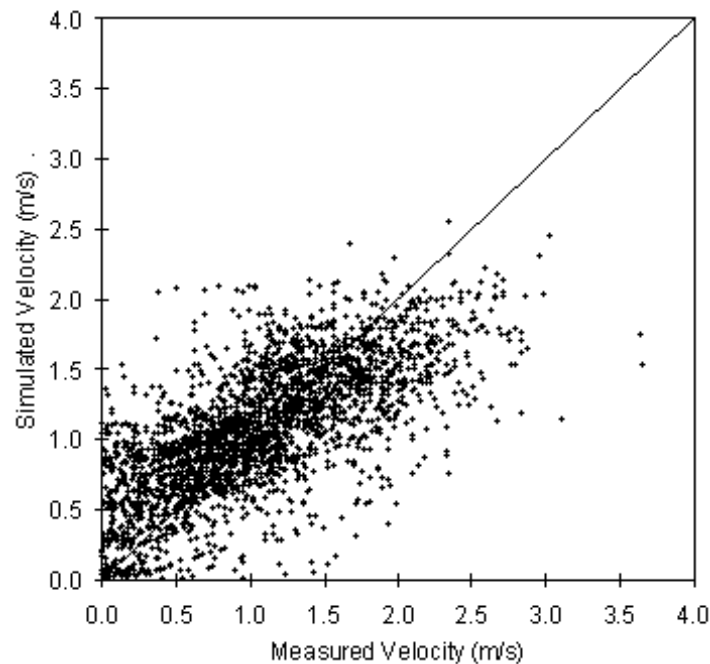
TABLE 2.—Coefficients for standard vertical-velocity curve

Ratio of observation depth to depth of water	Ratio of point velocity to mean velocity in the vertical
0.05	1.160
.1	1.160
.2	1.149
.3	1.130
.4	1.108
.5	1.067
.6	1.020
.7	.953
.8	.871
.9	.746
.95	.648

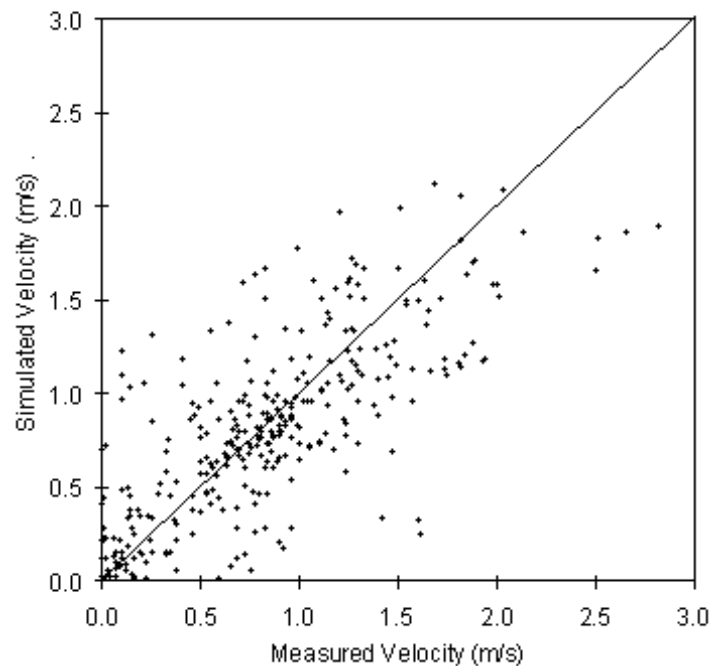
Above figure and table are from Rantz (1982).

We also investigated whether velocity measurements in 3-6 feet of water were less accurate than velocity measurement in greater than 6 feet of water by comparing measured versus predicted velocities for these two depth classes. As shown in the figures below, the correlation between measured and predicted velocities for depths greater than 6 feet (0.74) was only slightly higher than the correlation between measured and predicted velocities for depths of 3-6 feet (0.70).

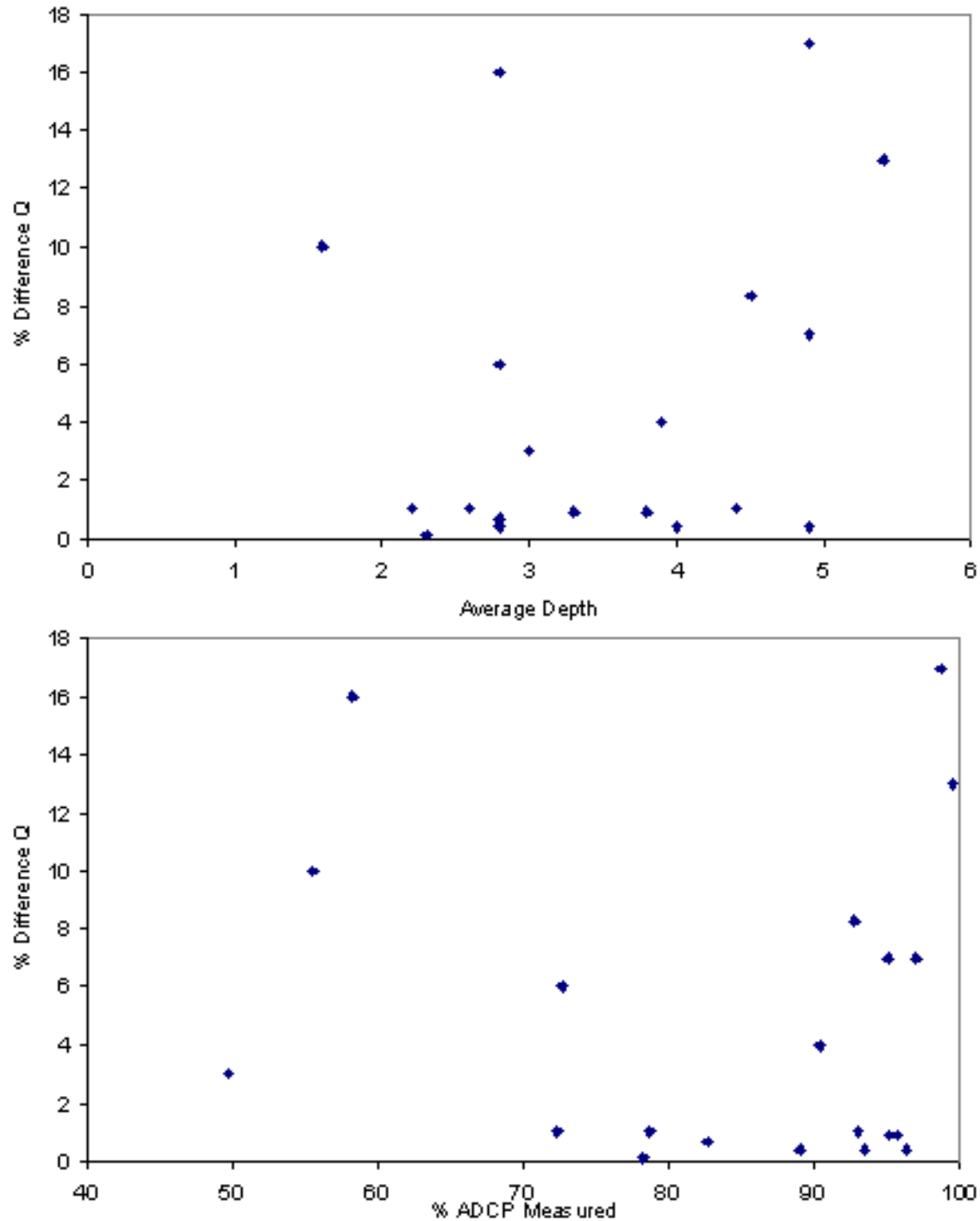
Yuba Spawning Study Sites
ADCP Measured vs. Simulated Velocities
for Depths ≤ 6 feet



Yuba Spawning Study Sites
ADCP Measured vs. Simulated Velocities
for Depths > 6 feet



We also investigated whether the discharge measurements were more accurate for deeper transects and for transects where a smaller percentage of the discharge was measured with the ADCP, versus hand-measurements. As shown in the figures below, we believe that there was no relationship between discharge measurement accuracy (as compared to gage discharge values) and either average depth or percentage of the discharge measured with the ADCP. Accordingly, we believe that the ADCP velocity measurements in 3-6 feet of water are a sufficiently accurate estimate of mean column velocity to enable a fair comparison with model predictions.



GP 9. p. 15. *Some mention should be made of the accuracy of the USACE topo data. The report should state whether the study used their 2' contours or the raw data, and then what contour interval was made.*

Response: We added the following text to the report (Methods – Hydraulic Model Construction and Calibration – River2D Model Construction) to address this comment:

“The accuracy of the hydroacoustic data were 1 foot horizontal and 0.1 foot vertical, while the accuracy of the photogrammetry data were 3 feet horizontal and 1 foot vertical (Scott Stonestreet, U.S. Army Corps of Engineers, personal communication). We used the raw hydroacoustic data and the 2-foot contour photogrammetry data.”

We did not use any contour interval for the raw hydroacoustic data – we just input the raw hydroacoustic data as points into the bed file.

GP 10. p. 18. *Again, you have to state a criteria as to what will count as "validated" and why you choose that.*

Response: See response to GP comment 6.

GP 11. p. 18. *I think that for a spatial distributed model like RIVER2D, it is not enough to just use the quantitative data to validate point velocities, but it is also necessary to provide some qualitative validation measures. There are 2 that I use regularly: 1) does the model exhibit eddies (i.e. recirculating flow) behind obstructions? and 2) does the model show downstream streaking of velocity magnitude, which is caused by using too low of a constant eddy viscosity? Both of these qualitative metrics help constrain the eddy viscosity parameter. Since the report does not actually show any velocity plots or plots with velocity vectors, it is not possible for the reader to make up their own mind about these issues. I understand that you can't show plots for all variables in the report, but I think it would be appropriate to show depth, velocity, velocity vectors, and Froude # for at least one site.*

Response: We have added an additional appendix (Appendix H) to the report providing the above qualitative validation measures. Appendix H includes plots of depth, velocity, velocity vectors, and Froude # for the UC Sierra Site at an intermediate flow (2,500 cfs). The velocity plot does not show any downstream streaking. We also included a plot in this appendix showing velocity vectors for a portion of the Timbuctoo Site at 2,500 cfs, which shows that the model exhibits eddies behind obstructions (in this case an island).

GP 12. p. 20-21. *The study proposes to use a logistic relation to obtain HSC that account for used and non-used conditions. However, non-used areas were only obtained from the selected sites, even though the sites are biased, because they were pre-chosen as sites that are heavily used relative to the non-site channel areas. Since the study sites do not include long stretches of glides and pools, what the report is doing is really just splitting hairs as to whether a particular part of a riffle is used or not. The best HSC would be obtained by not biasing the non-use data by limiting data sampling to only study-site areas. I see that there is some adjustment method*

attempted at the bottom of p. 21 and on 22, but without going through that calculation myself, it is very difficult to understand from the text alone. It might help if HSCs were shown at different stages of the process to see how they change with the different adjustments performed.

Response: The sites are an unbiased sample of the portions of the Yuba River with presumed high gravel permeability, which correspond to high-use spawning areas. Areas without high gravel permeability are not relevant to the effects of depth, velocity and substrate on used versus non-used conditions, since those areas would not be used regardless of the depths, velocities and substrates, since they do not have high gravel permeability. The sites include long stretches of glides and pools – the overall percentage of habitat types (by area) in the spawning sites is 23% glide, 9% pool, 20% riffle and 48% run. We believe that the best HSC, as developed in this report, take into account gravel permeability by limiting sampling of unoccupied locations to only study-site areas, and that the data are an unbiased sample of presumed high gravel permeability areas. We also note that redds were found in all habitat types – for the 829 total redds where we had total station or GPS data to determine which habitat type they were in, 39% were in glides, 10% were in pools, 8% were in riffles and 43% were in runs. The description of the adjustment method at the bottom of p. 21 and on 22 is from Gard (1998). Figures 12 to 17 show the HSC at different stages in the process – the histograms of observed data show the original data before adjustments were performed – and Figures 7 and 8 show the HSC at different points through the technique to adjust depth habitat utilization curves to account for low availability of deep waters with suitable velocity and substrate.

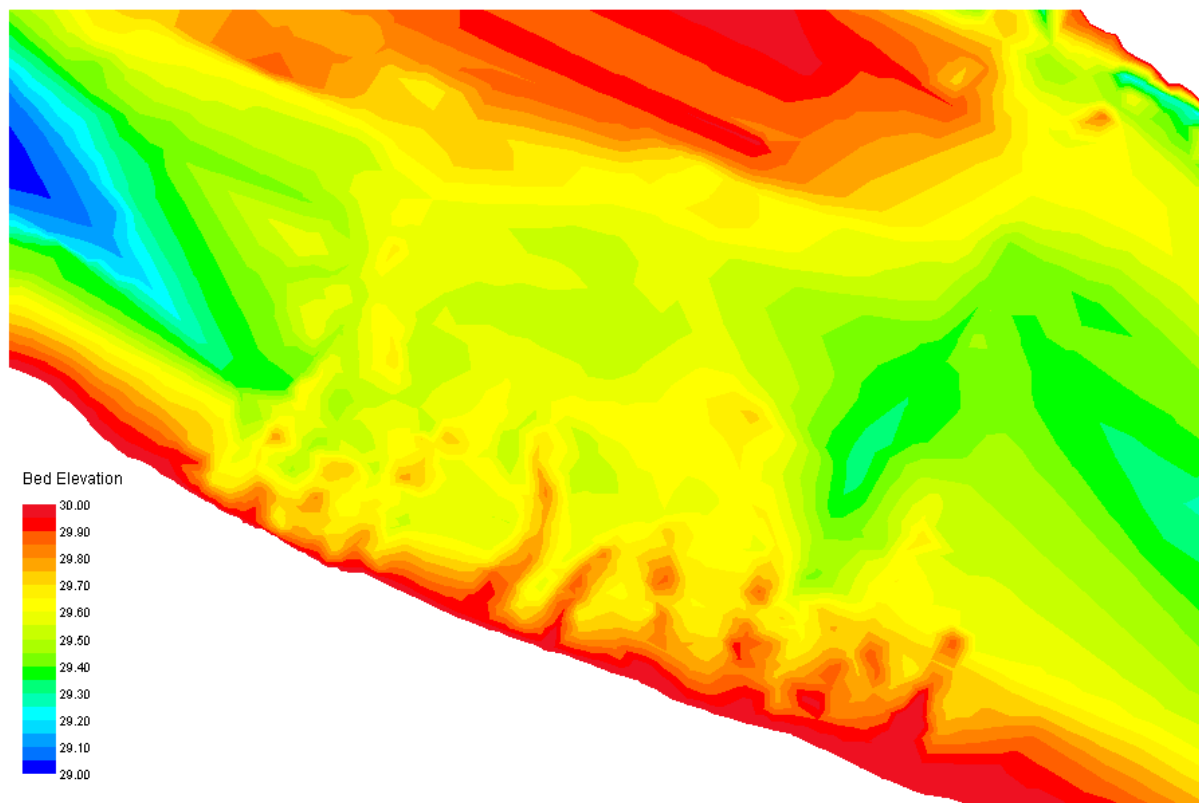
GP 13. p. 22. *For biological validation, you must state a specific criteria that will define whether the model is "validated" or not.*

Response: The specific criterion was the result of the Mann-Whitney U-test – i.e. that the combined suitability of occupied locations was significantly greater than the combined suitability of unoccupied locations at $p = 0.05$. This criterion is given in the methods section.

GP 14. p. 27. *Excellent point density, but it is unclear how this is distributed through the site. Is this calculated with a moving window or just the overall? The problem is that if you use ADCP, you can collect high-frequency data at the same point, and then have no data in between. ADCP averaging causes a lot of overlap. It is also still unclear whether USACE gave you their raw data or contour lines? Please clarify. Looking at appendix D, the resolution demonstrated definitely does not match that reported. For example, on p. 107, the map of the UC Sierra site has a very bizarre gradation of bed elevation, according to the legend, which is non-linear and hard to understand. Why aren't those an even interval? Looking at the map, you just see some variation where the redd dunes are located upstream of the riffle crest, but they barely show up at all. If you really had a point density of 4 pts/m², these dunes would be very clear. Still, looking at the sites as a whole, the mapping produced is much improved over previous studies I have seen from this group. I applaud that.*

Response: There was a typographic error in the heading of the last column of Table 7 – the correct heading is Density of Points (points/100 m²). We note that the densities in Table 7 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 m², while Jacobson and Galat (2006) had a point density

of 6 points/100 m². The densities in Table 7 are the overall point densities, calculated by dividing the total number of points by the area of the site. We have added Appendix B to show the bed topography point locations. The ADCP data points typically were spaced three to four feet apart. USACE gave us their raw hydroacoustic data and contour lines for the photogrammetry data. We have added this information to the report (see response to GP comment 9). River2D displays bed elevation as 10 even intervals between the lowest elevation of the site (26.52 m for UC Sierra) and the highest elevation of the site (37.00 m for UC Sierra). For UC Sierra, the intervals were 1.05 m, with some minor differences due to rounding. Thus, the gradation of bed elevation is linear with even intervals – just not at integer values. The legend shows the one-tenth incremental differences in elevation from the lowest to the highest elevations in the site. We have added this information to the figures in Appendix E. The redd dunes upstream of the riffle crest at the UC Sierra site do not show up well in the bed topography plot in Appendix E (previously Appendix D) because the variation of bed elevation in the dunes is small compared to the overall variation of bed elevation in the study site. As shown below, when the scale of the bed elevations is reduced and the topography is zoomed on the redd dunes, the dunes are clear, despite having an overall point density of 4.17 points/100 m². We collected a higher density of points in areas with rapidly varying topography, such as the redd dunes, and lower densities of points in areas with more uniform topography.



GP 15. p. 29. *Due to transmission losses in the channel (because gravel is so deep and porous), one would not expect observed Q and gage Q to match below the Narrows. At the UC Sierra site, we have calculated the transmission losses for 800-1100 cfs as averaging 9.6%. As Q increases over 10,000 cfs, this effect is diminished. It looks like you have some kind of velocity data adjustment to account for the ADCP blanking problem, but I could not find an explanation for what you did in the report. In any case, you have a problem, because you don't know the*

extent to which the difference between measured Q and gage Q is due to transmission losses or ADCP blanking. This is why ADCP shouldn't be used in shallow waters of 3-6' depth. If you have cross-sections observed with only wading meters, then you could assess transmission losses independently. You need to clarify in the report what is going on here. Why are the data in Appendix C adjusted and how?

Response: We have added a section to the discussion (Discussion – Factors Causing Uncertainty) on factors causing uncertainty in the modeled results – transmission losses in the segment upstream of Daguerre Point Dam in the fall in dry years is included in this discussion. We made three measurements in August to October of 2002 made with only wading meters. These measurements, two made at the Timbuctoo Site and one at the Highway 20 site, indicate transmission losses of 5.09% to 20.94%, consistent with the commenter's data. These measurements were made at gage flows of 670 to 955 cfs. In contrast, the average of 25 measurements made at the sites upstream of Daguerre Point Dam partially with the ADCP and partially with wading meters on March 27, 2002 was 2389 cfs, versus a gage flow of 2348 cfs. Similarly, the average of 15 measurements made at the sites upstream of Daguerre Point Dam partially with the ADCP and partially with wading meters on June 5-6, 2002 average 2041 cfs, versus a gage flow of 2018 cfs. The above flow estimates from ADCP data are sufficiently accurate, based on the discussion regarding the accuracy of ADCP velocity measurement made in 3-6 feet of water (see response to GP comment 8). While there are random errors in the individual ADCP flow measurements, these errors should be reduced substantially by averaging a large number of flow measurements. We are not aware of any bias errors in the ADCP measurements, given the discussion above comparing the average of ADCP measurements in cells to the mean water column velocity. Accordingly, we can accurately assume that there are no transmission losses in the spring and summer. We believe that it is acceptable to use ADCPs in shallow waters of 3-6' depth (see response to GP comment 8). When the sum of the Smartville and Deer Creek flows minus diversions are compared to the Marysville gage flows, a similar pattern is shown. On average, there is a gain in flow for the Marysville gage, versus the sum of the Smartville and Deer Creek flows minus diversions, in all months, ranging from 6.01% in October to 13.81% in July, for the period January 1971 to September 2000. However, in the fall of dry years, the Marysville gage has a lower flow than the sum of the Smartville and Deer Creek flows minus diversions – for example, in October 1994 (a critically dry year), there was a 9.47% loss. We did not do any velocity data adjustment to account for the ADCP blanking – as discussed above, the average water column velocities for the ADCP data were calculated as the average of the velocities measured in each cell. Neither the measured velocity data nor the simulated velocity data from the 2-D model were adjusted. The velocity adjustment factors given in Appendix D (previously Appendix C) are just the ratio of the flow simulated in RHABSIM to the actual flow for all of the simulation flows – this data is only used as a quality-assurance factor for evaluating the adequacy of the stage-discharge relationships used as an input to River2D. There are two potential consequences of the transmission losses for the segment upstream of Daguerre Point Dam: 1) we may have underestimated the stage at the bottom of the sites for lower flows, which would result in an overestimate of velocities and thus an underestimate of the flow with the peak amount of spawning habitat; and 2) additional releases are needed from Englebright Dam in the fall of dry years to get the amount of habitat predicted in this report for spring and fall-run Chinook salmon in the segment upstream of Daguerre Point Dam (see Discussion – Factors Causing Uncertainty).

GP 16. Methods section: *There is nowhere in the methods section where you mention the value of the eddy viscosity parameter for the model. This parameter is common to all depth-averaged 2D models that I know of, so it must be there. How did you obtain this value? Was it constant across the whole model domain? The fact that your velocity validation plots in Appendix F show poor cross-channel velocity variation is directly a result of an inaccurate eddy viscosity value. Whatever approach you used, needs to be explained, as this is a such a key factor. It also controls whether you get recirculating flow or not, which is something you should report. Do your models show recirculating flows? There exist standard methods for estimate eddy viscosity from depth and velocity measurements. You cannot just pick a textbook value.*

Response: We added the following sentence to the methods section (Methods – Hydraulic Model Construction and Calibration – River2D Model Calibration) to give the values of the hydraulic parameters for River2D, including the eddy viscosity parameters (River2D uses three eddy viscosity parameters):

“The values of all other River2D hydraulic parameters were left at their default values (upwinding coefficient = 0.5, minimum groundwater depth = 0.05 m, groundwater transmissivity = 0.1, groundwater storativity = 1, and eddy viscosity parameters $\varepsilon_1 = 0.01$, $\varepsilon_2 = 0.5$ and $\varepsilon_3 = 0.1$).”

As noted above, we used the default values of the eddy viscosity parameters. Although the eddy viscosity parameters were constant across the whole model domain, the actual eddy viscosity used in the model is not constant across the whole model domain. We disagree that the differences between measured and simulated cross-channel velocity patterns shown in Appendix F were a result of inaccurate eddy viscosity values. In this regard, Professor Peter Steffler of the University of Alberta, the developer of the River2D model, states (personal communication, e-mail dated July 30, 2007):

“Personally, I think that bathymetry error, discretization size and bed roughness variability are much more important effects. Older 2D formulations relied heavily on artificially high eddy diffusivity values to stabilize the numerical schemes. River2D was developed specifically to address this problem and by default uses physically realistic values (based on a large number of dispersion studies).”

The models used in this report show recirculating flows. Specifically, we have added an additional appendix to the report including a plot showing velocity vectors for a portion of the Timbuctoo Site at 2500 cfs, where the model shows recirculating flows. River2D uses a standard method to estimate eddy viscosity from depth and velocity measurements. In this regard, Peter Steffler states (personal communication, e-mail dated July 19, 2007):

“The River2D calculation for eddy diffusivity can be approximated as a (dimensionless) constant with a value in the range 0.03 to 0.05 (increasing with channel roughness) multiplied by depth and velocity.”

GP 17. Appendix F: *I think the velocity validation results are reasonable, but you still need to state your metric of reasonable in the text. For the observed data, I think it is appropriate to fit a smoothing function to the field data in your XS plots, because that data is always inaccurately noisy due to grain-scale and pebble-cluster effects in the vicinity of the measurement location and due to the short sampling time of 20 s. The observed smoothing in the 2D model XS prediction lines demonstrates the common problem of using an eddy viscosity value that does not vary adequately across the mesh.*

Response: See response to GP comment 6. In Appendix F, we have added an additional table showing the correlation between measured and simulated velocities for each site. We believe for transparency it is better to show the raw observed velocity data, rather than fitting a smoothing function to the field data in the XS plots. With regards to the effect of the observed smoothing in the 2D model XS prediction lines on variation in eddy viscosity, see response to GP comment 16. We have included figures in Appendix H of example hydraulic model output showing that there is considerable variation in eddy viscosity across the meshes.

GP 18. p. 61-62. *Your attribution of poor cross-channel velocity variation and poor validation is probably wrong, because you do not seem to understand the important role of the eddy viscosity parameter. In MacWilliams et al. (2006) in WRR we compared 1D, 2D and 3D models and showed that in fact it is the use of poor turbulence closure reliant on eddy viscosity parameterization that explains this problem. You really need to investigate this issue and add it into the report. You really should report your eddy viscosity values. For the low flows you studied, ranging from 400-400 cfs, those values should be in the range of 0.3-1 ft²/s. I would bet yours are higher than that. Of course, varying this parameter throughout the flow field (if RIVER2D can do that), helps, but still is inadequate, as our own investigations have found.*

Response: We have reviewed the potential sources of cross-channel velocity variation and validation. With regards to the importance of the role of eddy viscosity, Peter Steffler states (personal communication, e-mail dated July 19, 2007):

“Personally, I think that bathymetry error, discretization size and bed roughness variability are much more important effects. In general, the turbulence model is usually a very insignificant effect. It is only important when it is grossly exaggerated.”

With regards to MacWilliams et al. (2006), Peter Steffler states (personal communication, e-mail dated July 30, 2007):

“There isn't much that's relevant in the paper. They used FESWHMS with a larger than realistic, constant eddy diffusivity. This is one of the older generation finite element models. They conclude, without much presented justification, that the unrealistic eddy diffusivity explains some of the observed discrepancy in velocity. For River2D, the default formulation has eddy viscosity = 0.5*shear velocity*depth which is a smaller value and varies considerably with spatial location and discharge.”

We have included figures in Appendix H of example hydraulic model output showing the eddy viscosity values. The median eddy viscosity values ranged from 0.002 to 0.011 m²/s at the lowest simulation flow (150 cfs downstream of Daguerre Point Dam and 400 cfs upstream of Daguerre Point Dam) to 0.043 to 0.106 m²/s at the highest simulation flow of 4,500 cfs. Since the eddy viscosity is proportional to the product of the depth and velocity, it is reasonable to expect much higher values at the highest simulation flow, versus the lowest simulation flow. In metric units, the eddy viscosity values advocated by the commenter are 0.028 to 0.093 m²/s. Accordingly, on at least a median basis, the eddy viscosity values used in this study are not considerably higher than those advocated by the commenter. River2D varies the eddy viscosity throughout the flow field. Specifically, the following is a summary from the River2D manual (Steffler and Blackburn 2001, p. 23-24):

Depth-averaged transverse turbulent shear stresses are modeled with a Boussinesq type eddy viscosity formulation. For example:

$$\tau_{xy} = \nu_t \left(\frac{\partial U}{\partial y} + \frac{\partial V}{\partial x} \right) \quad (11)$$

where ν_t is the eddy viscosity coefficient. The eddy viscosity coefficient is assumed to be composed of three components: a constant, a bed shear generated term, and a transverse shear generated term.

$$\nu_t = \varepsilon_1 + \varepsilon_2 \frac{H \sqrt{U^2 + V^2}}{C_s} + \varepsilon_3^2 H^2 \sqrt{2 \frac{\partial U}{\partial x} + \left(\frac{\partial U}{\partial y} + \frac{\partial V}{\partial x} \right)^2 + 2 \frac{\partial V}{\partial y}} \quad (12)$$

where ε_1 , ε_2 , and ε_3 are user definable coefficients.

The default value for ε_1 is 0. This coefficient can be used to stabilize the solution for very shallow flows when the second term in equation 12 may not adequately describe v_t for the flow. Reasonable values for ε_1 can be calculated by evaluating the second term in equation 12 using average flow conditions (average flow depth and average velocities) for the modelled site.

The default value for ε_2 is 0.5. By analogy with transverse dispersion coefficients in rivers, values of 0.2 to 1.0 are reasonable. Since most river turbulence is generated by bed shear, this term is usually the most important.

In deeper lakes flows, or flows with high transverse velocity outlets gradients, transverse shear may be the dominant turbulence generation mechanism. Strong recirculation regions are important examples. In these cases, the third term, ε_3 , becomes important. It is essentially a 2D (horizontal) mixing length model. The mixing length is assumed to be proportional to the depth of flow. A typical value for ε_3 is 0.1, but this may be adjusted by calibration.

GP 19. p. 64. *The very first sentence reminds me that the assumption in the report that physical habitat is limiting is just wrong. If you really had massive spawning throughout the sites, then the unoccupied sites would actually be avoided ones. but since the sites are in fact not habitat limited, your unoccupied data is full of preferred sites that were unused due to lack of numbers of spawners.*

Response: See response to GP Comment 1.

GP 20. Comment: *Given the fact that the river has totally changed due to floods in 2005 and 2006, you really ought to state the value of this report in light of the reality that the river is totally different now.*

Response: We have added material to the discussion regarding the effect of the May 2005 and January and April 2006 flood events on the Yuba River channel and how this affects the conclusions in the report (see Discussion – Habitat Simulation and Factors Causing Uncertainty).

Yuba County Water Agency Comments

YCWA (Yuba County Water Agency) 1. Comment: *For the reasons discussed in these comments, YWCA believes that several very important revisions and additions to the draft report are necessary. During 2002, I and my technical representatives met with Mike Thabault and had a clear understanding that YWCA would be provided the opportunity to collaboratively work with the USFWS by ongoing review of the conduct of this study. Now, I request that we establish a working group of technical representatives of the USFWS, YCWA and other interested parties that can collaboratively discuss these critical issues and assist the USFWS in preparing a revised draft report that addresses these issues.*

Response: Our September 13, 2001 letter inviting stakeholder participation in this study stated:

We are offering interested stakeholders the opportunity to participate in planning these studies through: 1) review and comment on our draft study plan, 2) attending a series of information/technical meetings (at key milestones) to be held during the duration of the study, and 3) providing comments on our draft report prior to its finalization.

As shown in Table 1 of the response to comments document, we provided the above opportunities to stakeholders. Since we have only made minor additions to the report, we believe that a revision of the report by a collaborative process as suggested by the commenter is not warranted. FWS provided an opportunity for technical representatives of YWCA and other interested parties to discuss these responses in a technical workshop on April 9, 2008.

INTRODUCTION

GENERAL COMMENTS

YCWA 2. Comment: *The introduction in the USFWS draft report should provide a clear statement of study objectives, as well as task objectives to be used as benchmarks for evaluating whether or not the study accomplished its goals. The addition of defined objectives could aid in focusing tasks to achieve specific goals. Objectives would guide review of data acquisition, presentation of results, data reduction, statistical treatment of data, and development of models.*

Response: The last sentence of the first paragraph of the introduction currently includes a clear statement of the objective of the study. We have changed the word “purpose” to “goal” to make it clearer that this is the goal of the study. The sentence now reads: “The goal of this study was to produce models predicting the availability of physical habitat in the Yuba River for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning over a range of stream flows that meet, to the extent feasible, the levels of accuracy specified in the methods section.” The discussion includes an evaluation that concludes that the study accomplished its goal. We have added objectives for each task to the introduction. The tasks are focused to achieve these

specific goals. These objectives are consistent with the review of data acquisition, presentation of results, data reduction, statistical treatment of data, and development of models in the remainder of the report.

YCWA 3. Comment: *The introduction also should provide the context of this present study by discussing information from previous IFIM studies on the Yuba River. For example, the PHABSIM analysis conducted previously on the Yuba River for CDFG (Beak 1989) developed habitat suitability criteria for spawning. If these data were reviewed and determined to sufficiently fulfill the specific task objectives, it may alleviate the need for additional manipulation of site-specific habitat suitability data for spawning anadromous salmonids. Additionally, a review of the previous PHABSIM analysis completed on the Yuba River may aid in the development of new hypotheses and specific efforts that would build on existing information.*

Response: We recognize that Beak's (1989) study reflected the standard practices for instream flow studies in the 1980's. However, the techniques for performing instream flow studies have been significantly refined since the 1980's to increase the accuracy of habitat predictions and reflect the hydraulic complexities of river channels. In particular, USFWS decided to conduct instream flow studies for salmon on the lower Yuba River which utilize the improved practices for conducting instream flow studies to develop habitat suitability criteria and hydraulic modeling of available habitat. The specific procedures used in this study that were not used in the Beak (1989) study include: 1) the use of Type II criteria with application of a technique to correct for availability (Guay et al. 1991); 2) collection of habitat use data with equal sampling of different mesohabitat types to address habitat availability and (possibly) at a high enough flow so that fish can select their preferred habitat characteristics; 3) the application of a procedure to adjust spawning depth habitat utilization curves for availability (Gard 1998); 4) the use of a two-dimensional hydraulic and habitat model, instead of PHABSIM; and 5) placement of sites for modeling spawning only in heavy spawning-use areas³. Accordingly, a review of the habitat suitability criteria for spawning from the Beak (1989) study and the previous PHABSIM analysis completed on the Yuba River would not aid in the development of new hypotheses and specific efforts that would build on existing information. The preceding discussion of the Beak (1989) study has been incorporated into the executive summary for this report.

YCWA 4. Comment: *A review of existing data (Beak 1989) could include both a review of the data itself including the geographic scope of the study, the sample site selection and rationale, the experimental design utilized, the applicability of the methodologies and sampling techniques that were used to obtain data, the completeness of the data set, the reliability of data collected based on the type of methodologies and experimental design used, the data reduction, the applicability of the statistical analysis performed on data, a review of the data treatment and conclusions including the value of the statistical analysis (e.g., are the analyses appropriate given the type of data, sample size, variance), and the accuracy of the conclusions drawn from the study. Specific review elements of these biological data could include reviewing the methodology for obtaining site-specific habitat suitability criteria, the*

³ This is one way to address factors, such as permeability and upwelling, other than depth, velocity and substrate, which control the distribution of spawning (Gallagher and Gard 1999).

flows at which site-specific habitat suitability criteria observation data were obtained, the locations at which site-specific habitat suitability criteria observation data were obtained including habitat types, the rationale for choosing these locations, the stratified sampling design within habitat types and river reaches, the distribution of observational data in relation to data bins, and the methods used for generation of site-specific habitat suitability criteria including curve-fitting techniques and a review of the possibility of adjusting the existing utilization criteria for availability. Also, this draft report should include discussions of the geomorphology of the Yuba River, including analysis of changes in channel planform geometry, channel incision, stability of the present-day channel, and gravel recruitment, transport and embeddedness since the conduct of CDFG's previous IFIM study.

Response: Since the elements of Beak's (1989) study do not reflect the current state-of-the-art practices for instream flow studies (see response to YWCA comment 3), a review of the data itself including the geographic scope of the study, the sample site selection and rationale, the experimental design utilized, the applicability of the methodologies and sampling techniques that were used to obtain data, the completeness of the data set, the reliability of data collected based on the type of methodologies and experimental design used, the data reduction, the applicability of the statistical analysis performed on data, a review of the data treatment and conclusions including the value of the statistical analysis (e.g., are the analyses appropriate given the type of data, sample size, variance), and the accuracy of the conclusions drawn from the study would not be appropriate. Similarly, a review of the methodology for obtaining site-specific habitat suitability criteria, the flows at which site-specific habitat suitability criteria observation data were obtained, the locations at which site-specific habitat suitability criteria observation data were obtained including habitat types, the rationale for choosing these locations, the stratified sampling design within habitat types and river reaches, the distribution of observational data in relation to data bins, and the methods used for generation of site-specific habitat suitability criteria including curve-fitting techniques and a review of the possibility of adjusting the existing utilization criteria for availability would not be appropriate because the elements of Beak's (1989) study do not reflect the current state-of-the-art practices for instream flow studies. Since a review of Beak's (1989) study is not appropriate, it is not necessary to discuss changes in channel planform geometry, channel incision, stability of the present-day channel, and gravel recruitment, transport and embeddedness since the Beak (1989) study. We have added a section to the discussion on the geomorphology of the Yuba River, specifically regarding whether the Yuba River is in dynamic equilibrium.

SPECIFIC COMMENTS

YCWA 5. *The USFWS draft report states (pg. 1) ... "For the Yuba River downstream of Englebright Dam, the Central Valley Project Improvement Act Anadromous Fish Doubling Plan calls for improved flows for all life history stages of Chinook salmon and steelhead (U.S. Fish and Wildlife Service 1995)."*

This statement should be followed by recognition that YCWA, collaboratively with NMFS, USFWS, CDFG and NGOs, diligently worked to develop a comprehensive set of improved

flow regimes, which now are the Flow Schedules of the Lower Yuba River Accord (see Draft Environmental Impact Report/Environmental Impact Statement for the Proposed Lower Yuba River Accord, June 2007).

Response: We have added the above text to the introduction.

YCWA 6. *The USFWS draft report states (pg. 1) ... "The purpose of this study was to produce models predicting the availability of physical habitat in the Yuba River for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning over a range of stream flows."*

The USFWS draft report does not appear to fulfill its stated purpose. Habitat availability ultimately is associated with water availability, which is further defined by hydrological factors (e.g., snowpack, runoff, carryover storage, etc.) and operational constraints (e.g., flood control, hydropower production, beneficial use deliveries, etc.). The USFWS draft report does not address hydrology (and therefore ultimately does not predict habitat availability), nor does it address the issues of the feasibility or applicability of the habitat-discharge relationships. The purpose or objective of the USFWS draft report therefore may be more accurately stated as ... "to produce models predicting spawning habitat-discharge relationships in the lower Yuba River ...

Response: We modified the sentence in question as follows:

“The goal of this study was to produce models predicting the availability of physical habitat in the Yuba River for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning over a range of stream flows that meet, to the extent feasible, the levels of accuracy specified in the methods section.”

YCWA 7. *On page 1, the USFWS draft report attempts to link the amount of spawning habitat with reproduction success through changes in the amount of redd superimposition. In addition, although it is stated (pg. 5) that the percentage of fall-run Chinook salmon redd superimposition was recorded, the results of this data collection are not presented in the USFWS draft report.*

Moreover, the USFWS draft report does not evaluate or even discuss whether redd superimposition occurs in the lower Yuba River or, if it does, whether it is related to flow or spawning habitat availability. Therefore, this paragraph (and references on pg. 5 and pg. 75) should be revised accordingly or omitted.

Response: The material referenced by the commenter is simply a conceptual model of the link between spawning habitat and population change that would apply to any instream flow study addressing salmonid spawning. As such, an evaluation or discussion of whether redd superimposition is related to flow or spawning habitat availability is not necessary. We added the following sentence to the results section: “We found that 73 out of 213 fall-run Chinook salmon redds in 2002 (or 34%) were superimposed.” Based on the above, we believe that this paragraph (and references on pg. 5 and pg. 75) does not need to be revised.

YWCA 8. *The USFWS draft report states (pg. 1) that ... "Microhabitat features include the hydraulic and structural conditions (depth, velocity, substrate or cover) which define the actual living space of the organisms".*

As further discussed in following comments, for the spawning lifestage the micro-habitat hydraulic or structural conditions characterizing (defining) spawning habitat also include intragravel permeability and flow conditions (i.e., upwelling and/or downwelling), which are not sufficiently addressed in the USFWS draft report. In fact, the methodology used to develop the Habitat Suitability Indices (HSI) and apply them to estimate spawning habitat availability does not specifically address this potentially important component of spawning habitat selection, utilization and availability, and therefore may result in the prediction of suitable (available) habitats which simply may not be utilized because of behavioral selection for specific intragravel conditions (see Comment 6).

Response: Intragravel permeability and upwelling and/or downwelling were incorporated into spawning habitat by placing sites only in high use spawning areas, based on the assumption that spawning salmonids select spawning areas with sufficient intragravel permeability and upwelling and/or downwelling. As such, we believe that the draft report sufficiently addresses intragravel permeability and upwelling and/or downwelling. The methodology used to develop the Habitat Suitability Indices (HSI) and apply them to estimate spawning habitat availability specifically addresses this potentially important component of spawning habitat selection, utilization and availability because habitat selection, utilization and availability were all assessed in areas with high spawning use, and thus with sufficient intragravel permeability and upwelling and/or downwelling. Because spawning was only assessed in high-spawning use areas, the modeling would not result in the prediction of suitable (available) habitats which simply may not be utilized because of behavioral selection for specific intragravel conditions.

YWCA 9. *The USFWS draft report presents (pgs. 1-2) the following three general categories of techniques to evaluate spawning habitat: (1) habitat modeling; (2) biological response correlations; and (3) demonstration flow assessment. The USFWS draft report then lists the disadvantages of biological response correlations and demonstration flow assessment, but it does not list their advantages. Moreover, the USFWS draft report does not list the disadvantages (or advantages) of habitat modeling and compare them to the other two approaches. The USFWS draft report conclusion that habitat modeling is the "best" technique for the Yuba River, therefore is not based on any supporting rationale presented in the report.*

Response: The commenter is correct that the report does not list the advantages of biological response correlations and demonstration flow assessment or the advantages of habitat modeling. However, we believe that a consideration of the disadvantages of biological response correlations and demonstration flow assessment is a sufficient reason to not use these methods. Habitat modeling is then left as the only available method to use. We added text to the report concerning the disadvantages of habitat modeling. We have modified the conclusion sentence as follows to address the commenter's concerns: "Based on the above discussion, we selected habitat modeling as the technique to be used for evaluating anadromous salmonid spawning habitat in the Yuba River."

YWCA 10. The USFWS draft report states (pg. 2) ... *"It is well-established in the literature (Rubin et al. 1991, Knapp and Preisler 1999, Parasiewicz 1999, Geist et al. 2000, Guay et al. 2000, Tiffan et al. 2002, McHugh and Budy 2004) that using a logistic regression is preferable to developing criteria just using use data."* Because Parasiewicz (1999) was not available for review, these comments do not discuss it. For the reasons discussed here, the remaining referenced literature does not support the USFWS draft report contention:

- ❑ Rubin et al. (1991) did not conclude that using logistic regression is preferable to developing suitability criteria using only use data. They collected density of juvenile salmonids and physical habitat data in cells, then converted cell data to curves using a nonlinear regression procedure suited to data with many zero counts of fish in cells. They concluded that by basing suitability curves on relationships between density of juvenile fish and habitat conditions in cells, rather than on frequency distributions of habitat conditions measured at fish observation locations, no adjustment was needed for habitat availability.
- ❑ Knapp and Priesler (1999) did not conclude that using logistic regression is preferable to developing suitability criteria using only use data. They used nonparametric logistic regression model techniques to describe the relationship between independent variables (channel and microhabitat characteristics) and the probability of finding California golden trout redds in a Sierra Nevada stream. They concluded that their use of a generalized additive model, of which nonparametric logistic regression models are a subclass, is a substantial improvement over previous approaches to modeling fish-habitat relationships that used generalized linear models such as traditional logistic regression.
- ❑ Geist et al. (2000) did not conclude that using logistic regression is preferable to developing suitability criteria using only use data. They did not develop habitat suitability criteria per se. Rather, they used logistic regression to determine which explanatory variables (i.e., water depth, velocity, substrate, and lateral slope) from each habitat cell were important in spawning habitat selection by fall-run Chinook salmon in the Columbia River. Fall-run Chinook salmon spawning habitat suitability was the binary response variable (suitable or unsuitable) for the logistic model. They concluded that redds were patchily distributed ("clustered"), and suggested that some unmeasured factor(s) influence redd site selection, such as upwelling from hyporheic habitats.
- ❑ Guay et a. (2000) evaluated the ability of numeric habitat models to predict the distribution of juvenile Atlantic salmon in a small river, and compared predictive capabilities of two biological models - one based on preference curves (HSI), and one consisting of a multivariate logistic regression designed to distinguish between the physical conditions used and avoided by fish (HPI). They concluded that HPI may be a more powerful biological model than HSI, but cautioned that this may be due to the mathematical structure they used, which may have allowed a better representation of the statistical and biological interaction among physical variables with the HPI.

- ❑ *Tiffan et al. (2002) used a logistic regression model to relate the probability of juvenile fall-run Chinook salmon presence in nearshore areas of the Columbia River to measures of physical habitat, as part of a fish stranding evaluation. However, they did not develop habitat suitability criteria (as indicated in the USFWS draft report), nor did they compare or make conclusions regarding use data in HSI development.*
- ❑ *McHugh and Budy (2004) did not develop and compare logistic regression suitability criteria with frequency of use data. Rather, they concluded that river-specific suitability models for Chinook salmon redd site selection (based on logistic regression) provided greater predictive performance than general, generic PHABSIM-type suitability models developed on other rivers, but applied to that specific river.*

Response: We have modified the sentence in question as follows to address the commenter's concerns:

“It is well-established in the literature (Knapp and Preisler 1999, Parasiewicz 1999, Geist et al. 2000, Guay et al. 2000, Tiffan et al. 2002, McHugh and Budy 2004) that logistic regressions are appropriate for developing habitat suitability criteria.”

The above sentence is consistent with the following quote from the most recent of the references (McHugh and Budy 2004):

“More recently, and based on the early recommendations of Thielke (1985), many researchers have adopted a multivariate logistic regression approach to habitat suitability modeling (Knapp and Preisler 1999; Geist et al. 2000; Guay et al. 2000).”

We have deleted the reference to Rubin et al. (1991) because it discusses a similar method to logistic regression using density instead of presence-absence.

YWCA 11. *On page 4, six assumptions for the study described in the USFWS draft report are listed. However, no discussion is provided regarding why these assumptions are necessary, or to what extent each of these assumptions is valid, is partially met, or is not met. Such a discussion is necessary to evaluate the veracity of the study. A brief discussion of these assumptions should include, but not be limited to, the following:*

Assumption 1: Physical habitat is the limiting factor for salmonid populations in the Yuba River.

Presumably, this assumption actually is that spawning habitat is the limiting factor for salmonid populations in the Yuba River, because this draft report only addresses elements of the spawning lifestage. However, there is no evidence to support this assumption. In fact, recent studies conducted by UC Davis indicate that spawning habitat is not limiting for lower Yuba River salmonid populations (G. Pasternak, U.C. Davis, 2007 pers. comm.).

Response: A basic assumption of all instream flow studies is that physical habitat is limiting fish populations (Bovee 1982, page 120). Accordingly, no discussion is needed regarding why this assumption is necessary, or to what extent this assumption is valid, is partially met, or is not met. If some other factor such as food or fishing mortality is controlling the population size, rather than physical habitat, changes in physical habitat would not be expected to result in changes in the fish population. In 2002, we noted whether the fall-run redds which we measured HSI data on were superimposed or not; we found that 73 out of 213 redds (or 34%) were superimposed, suggesting that physical spawning habitat may be limiting in the Yuba River. Further, it should be noted that not all areas with suitable depth, velocity and substrate are used for spawning; salmonids preferentially select areas with high gravel permeability. G. Pasternak, U.C. Davis found a ratio of spawner to redds of 4 to 1. A ratio of spawner to redds of 4 to 1 suggests that spawning habitat may be limiting – if there was sufficient spawning habitat to enable all adults to spawn, it would be expected that the spawner to redd ratio would be closer to 2 to 1 (in other words, that each spawning pair created a redd).

YCWA 12. Assumption 2: Spawning habitat quality can be characterized by depth, velocity and substrate.

This assumption has the potential to be reasonably valid depending on the methodologies used to develop HSIs. As long as the methodologies do not compromise the ability of these three parameters (or combinations thereof) to reflect the influence of unmeasured parameters (e.g., localized upwelling) in spawning habitat quality and concomitant spawning site selection, it may be a reasonable assumption. However, the data collection procedures, methodologies and resultant HSIs developed in the USFWS draft report raise questions as to the extent to which this assumption is valid (see following comments).

Response: As discussed in the responses to YWCA comments 13-19 and 24-43, the methodologies used do not compromise the ability of these three parameters (or combinations thereof) to reflect the influence of unmeasured parameters (e.g., localized upwelling) in spawning habitat quality and concomitant spawning site selection, since the data collection procedures, methodologies and resultant HSIs in the report take intragravel permeability and upwelling and/or downwelling into account because habitat selection, utilization and availability were all assessed in areas with high spawning use, and thus with sufficient intragravel permeability and upwelling and/or downwelling.

YCWA 13. Assumption 3: The depths and velocities present during HSI data collection were the same as when the redds were constructed.

It is critically important that the water depth and velocity data used in HSI development actually reflect the water depths and velocities that existed at the spawning areas at the time of spawning site selection and redd construction. However, this assumption appears to have been violated for fall-run Chinook salmon and steelhead. The USFWS draft report recognizes this concern by stating (pg. 63) that ... "The unstable nature of the flows in both segments from the beginning of fall-run Chinook salmon and steelhead/rainbow trout spawning resulted in some uncertainty that the measured depths and velocities in both segments were the same as present at the time of redd construction in all three years". This apparent violation introduces (unaccounted for) bias into the water depth and velocity HSI curves for both species, and raises concerns regarding their utilities.

Response: We have added text to the discussion section (Discussion – Factors Causing Uncertainty) regarding the effects of flow fluctuations on the water depth and velocity HSI curves for fall-run Chinook salmon and steelhead/rainbow trout.

YCWA 14. Comment: *The extent of bias is largely dependent on the magnitude of flow differences that occurred during the time period extending from the time of actual redd construction to the time of HSI data collection. These flow changes are presented in Table 10 (pg. 32) of the draft USFWS report.*

Response: In all but one case (fall-run Chinook salmon above Daguerre Point Dam in 2002), the flows during HSC data collection were less than the average flows during the period of redd construction. Since depths and velocities increase with flow, on average the depth and velocity HSC data are slightly less than the depths and velocities present during redd construction, which would result in an underestimate of the flow with the peak amount of spawning habitat. Accordingly, the depth and velocity HSI curves likely have a small bias towards lower depths and velocities. This explanation has been added to the report (Discussion - Factors Causing Uncertainty).

YCWA 15. Comment: *Flows varied significantly during the period from the assumed starting dates of fall-run Chinook salmon and steelhead spawning until the dates of HSI data collection. These changes in flow bring into question whether the water depths (and associated velocities) actually selected by, and available to, fall-run Chinook salmon and steelhead resemble those measured and used to develop the HSIs.*

Response: The degree of uncertainty in the flow-habitat relationships given in Appendix K of the report from differences in depths and velocities at the time of redd construction versus at the time habitat suitability criteria data were collected would be proportional to the percent variation in flow prior to HSI data collection, as shown in Table 10 of the report. Accordingly, there would be the most uncertainty in the fall-run Chinook salmon flow-habitat relationships and the least uncertainty in the spring-run Chinook salmon flow-habitat relationships, with regards to differences in depths and velocities at the time of redd construction versus at the time habitat suitability criteria data. This explanation has been added to the report (Discussion - Factors Causing Uncertainty).

YCWA 16. Comment: *The fall-run Chinook salmon habitat suitability criteria (HSI) were based on data collected in 2001, 2002 and 2003. The flows used as the basis for HSI development were the average flows each year from October 1 (the assumed date of the start of the fall-run Chinook salmon spawning season) until the date of HSI data collection (pg. 21). Review of Table 10 (pg. 32) of the USFWS draft report indicates that average flows varied by 620% to 639% during the fall-run Chinook salmon spawning periods prior to HSI data collection.*

Response: The above statement is incorrect. Within each year and segment, average flows only varied by 20% to 39% during the fall-run Chinook salmon spawning periods prior to HSI data collection.

YCWA 17. Comment: *For example, during the 2002 HSI data collection for fall-run Chinook salmon at the Above Daguerre site, average flows (as presented on pg. 32, Table 10) ranged from about 629 cfs to about 1,143 cfs from the assumed start date of the spawning season until the time of HSI data collection. Although it is recognized that stage-discharge relationships are site-specific due to physical and hydraulic interactions, the stage-discharge relationship for the Yuba River near Smartville gage provided by the California Data Exchange Center (<http://cdec.water.ca.gov/rtables/YRS.html>) provides an indication of the potential change in stage associated with these flow levels. Application of the rating table at the Smartville gage indicates that stage would change up to about 1 foot between these two flow rates, and associated water depth (and velocities) were variable from the time of spawning to HSI data collection.*

Response: The above stage change is an overestimate of the stage change that would be expected in high-use spawning areas on the Yuba River. For the upstream and downstream transects of our 10 study sites, the difference in stage for 629 versus 1,143 cfs averaged 0.61 feet, with a range of 0.46 to 0.76 feet.

YCWA 18. Comment: *Assumption 3 may have been violated to a greater degree for steelhead than for fall-run Chinook salmon. For example, for the 2003 Above Daguerre HSI data collection effort for steelhead, average flows, as indicated in Table 10, ranged from about 1,008 cfs to about 3,790 cfs over the period encompassing assumed dates of redd construction until HSC data collection. This difference in flow would result in a change in stage of about 3.3 feet according to the Yuba River rating table for Smartville available from CDEC. The depth HSI curve (and the water velocity HSI curve) for steelhead may contain a considerable amount of bias depending on the number of redds constructed under flow conditions that were different than the conditions when the HSI data were collected.*

Response: We would characterize the comparison between steelhead and fall-run Chinook salmon that there would be greater uncertainty with regards to the depths and velocities present during steelhead redd construction than for fall-run Chinook salmon, rather than that assumption 3 was violated. The above stage change is an overestimate of the stage change that would be expected in high-use spawning areas on the Yuba River. For the upstream and downstream transects of our 10 study sites, the difference in stage for 1,008 versus 3,790 cfs averaged 1.88 feet, with a range of 1.47 to 2.45 feet. We believe that the steelhead depth and velocity HSI curves likely underestimate the true depth and velocities selected by steelhead, since the flows during HSC data collection were less than the average flows during the period of redd construction.

YCWA 19. Assumption 4: *Any steelhead/rainbow trout redds measured in the surveys were constructed during the 30 days prior to the survey dates based on the assumption that redds would not appear fresh after that time period. Insufficient detail is provided to ascertain the validity of this assumption. Additional information should be provided to support the assumption that steelhead redds "not covered with periphyton growth" (pg. 19) have been constructed within 30 days prior to observation. Additional information regarding what constitutes "those not covered with*

periphyton growth" in more descriptive and, if possible, quantitative terms to aid in consistency when assessing the state of redds should be provided in the USFWS draft report, as well as the supporting rationale for using periphyton coverage to estimate the time between redd construction and data collection.

Response: We do not have any additional information, nor are we aware of additional information in the literature, regarding how long it takes for redds to be covered with periphyton growth after redd construction. However, information in Stone (2006) suggests that 30 days is a reasonable estimate. Specifically, Stone (2006) found that lamprey nests were no longer visible after 30 days for nests constructed prior to late May. We evaluated whether a redd was covered with periphyton growth using professional judgment and did not apply any quantitative measures. We do not have any additional supporting rationale, beyond what is already in the report, for using periphyton coverage to estimate the time between redd construction and data collection, other than it is commonly used in instream flow studies when collecting spawning habitat suitability criteria data. Another indication that the steelhead/rainbow trout redds were constructed during the 30 days prior to the survey is that steelhead/rainbow trout adults were observed on 11 percent of the redds (12 percent of the redds with depths < 5 feet and seven percent of the redds with depths > 5 feet).

YCWA 20. Assumption 5: The 10 study sites are representative of anadromous spawning habitat in the Yuba River.

This assumption may be reasonably valid, although supporting documentation is required and not provided in the USFWS draft report. The USFWS draft report (pgs. 5 and 7) states that study sites selected were those that received heaviest use by spring-run and fall-run Chinook salmon, and by steelhead/rainbow trout, as mapped by Jones and Stokes biologists during 2000. It would be very helpful if the term "heaviest use" was supported in the USFWS draft report by documentation such as the percentages of spring-run Chinook salmon, fall-run Chinook salmon and steelhead redds located within each of the 10 study sites relative to the numbers of these redds in the entire lower Yuba River.

Response: The habitat simulation portion of the results section gives information on the proportion of the population of spawning salmon and steelhead/rainbow trout using the sites. Specifically, the ratios of total redds in the segment to the number of redds in the modeling sites is the inverse of the proportion of the population of spawning salmon and steelhead/rainbow trout using the sites. Thus, 45 percent (1/2.2) of spawning fall-run Chinook salmon and 57 percent (1/1.76) of spawning steelhead/rainbow trout above Daguerre and 42 percent (1/2.37) of spawning fall-run Chinook salmon and 80 percent (1/1.25) of spawning steelhead/rainbow trout below Daguerre used the study sites. We have added the above percentages to the report (Results - Factors Causing Uncertainty). The number of redds in each site, versus all of the sites in a given segment, is given in Table 6. We believe that the spawning sites are a representative sampling of high-use spawning habitat in the Yuba River, based on the number of sites and the proportion of the population using the study sites.

METHODS

GENERAL COMMENTS

YCWA 21. Comment: *The USFWS draft report repeatedly represents early spawning Chinook salmon as "spring-run", and defines their spawning period as September. Text needs to be added explaining that for management distinction purposes, Chinook salmon spawning during September are assumed to be spring-run, although no data exists definitively supporting that distinction between spring-run and fall-run Chinook salmon. The report also should mention the lack of genetic information indicating that there are distinct spring-run and fall-run Chinook salmon in the lower Yuba River.*

Response: It is the long-standing and standard practice in Central Valley spawning HSI data collection to assume that any redds constructed during the month of September (i.e. prior to October 1) are spring-run and that any redds constructed between October 1 and December 31 are fall-run (Vogel and Marine 1991). We have followed this practice for this study. Accordingly, we have not evaluated genetic information in this report.

YCWA 22. Comment: *It appears that HSC and HSI are used interchangeably throughout the USFWS draft report. A standard nomenclature should be consistently used, unless specifically intended otherwise.*

Response: HSC refer to the overall functional relationships that are used to convert depth, velocity and substrate values into habitat quality (HSI). HSI refers to the independent variable in the HSC relationships. We have added these definitions to the report (Methods - Habitat Suitability Criteria (HSC) Data Collection), and reviewed the report to verify that we have used the terms HSC and HSI consistently.

YCWA 23. Comment: *Given the suite of issues regarding HSI development (including the depth adjustment procedure and biovalidation) as discussed in the following comments, consideration should be given to recalculating the HSIs and/or using alternative HSIs and re-running the models to estimate WUA-discharge relationships for spring-run and fall-run Chinook salmon and steelhead/rainbow trout spawning in the lower Yuba River with different HSIs.*

Response: We have evaluated the specific issues raised by YWCA regarding HSI development (see responses to YWCA comments 24-43 and PG&E comment 19). Based on our evaluation, we have determined that our HSI development methods are valid and represent the current state of the art (Gard 1998, Guay et al. 2000). Accordingly, we have not re-run the models with recalculated or alternative HSIs to estimate WUA-discharge relationships for spring-run and fall-run Chinook salmon and steelhead/rainbow trout spawning in the lower Yuba River with different HSIs.

SPECIFIC COMMENTS

YCWA 24. *The USFWS draft report (pg. 20) states that a method presented in Rubin et al. (1991) was applied to explicitly take into account habitat availability in developing HSC criteria, without using preference ratios (use divided by availability). However, the USFWS draft report does not explain what specific components of Rubin et al.'s (1991) methodology were applied. Rubin et al. (1991) collected density of juvenile salmonids and physical habitat data in cells, then converted cell data to curves using a nonlinear regression procedure suited to data with many zero counts of fish in cells to be provided. A clear explanation of the specific methodology employed needs to be provided in the USFWS draft report.*

Response: To clarify the methodology employed, we have changed the reference in question to Guay et al. (2000), since Rubin et al. (1991) does not use logistic regression, but instead uses a similar technique using numbers of fish, rather than presence/absence.

YCWA 25. *The HSI development methodology needs to be more clearly described to facilitate understanding the specific steps undertaken, and to resolve what appears to be potentially conflicting rationales. For example, on pg. 20, the USFWS draft report states that the HSC development methodology takes into account habitat availability without using preference ratios (use divided by availability). In the description of the depth adjustment procedure (pg. 22), linear regressions of relative (i.e., "normalized") availability and use were regressed against the midpoint of the depth increments, to achieve "linear" values. Then "linearized" use values are divided by "linearized" availability values. Isn't this essentially a preference ratio (use divided by availability) simply using "linearized" values?*

Response: The statement that

“HSC development methodology takes into account habitat availability without using preference ratios (use divided by availability)”

only refers to the logistic regression procedure. We have modified this sentence in the report for clarification. The separate technique to adjust depth habitat utilization curves for spawning to account for low availability is discussed in another paragraph. As noted in Gard (1998), this technique is an intermediate method between preference and use, and avoids problems associated with a preference ratio by only modifying the upper end of the depth utilization curve. The use of both techniques is necessary to correct for the effects of habitat availability on habitat use.

YCWA 26. Comment: *Then, those linearized "preference" ratios were standardized (i.e., normalized) to achieve "scaled" ratios ranging from 0 to 1. These "scaled ratios" were then treated as dependent variables, and regressed against the midpoints of the depth increments (independent variables).*

Response: As noted in Gard (1998), the above technique is an intermediate method between preference and use, and avoids problems associated with a preference ratio by only modifying the upper end of the depth utilization curve.

YCWA 27. Comment: *The report needs a description of the considerations associated with what appears to be the use of "discrete" dependent variables in a continuous (linear regression) function to produce discrete dependent variables that then are divided by each other to produce ratios, which are then used as discrete dependent variables in a continuous function where that function, in turn, is used to predict an intercept with the X-axis (independent variable, the midpoint of the depth increments).*

Response: We disagree that the method uses discrete dependent variables – rather, the method uses finite-sized increments (0.5 feet) of a continuous variable (depth). The methodological considerations of this technique were subject to peer review and scrutiny prior to publication in a refereed journal (Gard 1998).

YCWA 28. *The selection of the HSC data to be used to establish flow-habitat relationships is often a contentious component of a flow-habitat study (PHABSIM), especially when the decision is made autonomously (Stalnaker et al. 1995; Bovee 1995b). "The habitat suitability criteria are typically the most significant factor in determining the outcome of a habitat study, more so than the type of hydraulic/hydrodynamic model used" (Waddle in USFWS 2003 peer review of the lower American River). Overestimates of available habitat can lead to nonachievable goals for protecting salmonid habitat and can be directly related to use of inaccurate suitability relationship criteria (Geist et al. 2000; McHugh and Budy 2004). The HSC development approach used by USFWS for the lower Yuba River spawning habitat evaluation appears to be based, at least conceptually, on a developing methodology. It is unique and has been characterized by peer reviewers of recent USFWS application as confusing and producing questionable results (USFWS 2003, USFWS http://www.delta.dfg.ca.gov/AFRP/documents/Sacramento_River_Spawning_Response-to-Comments_Document.pdf).*

Response: We agree that the habitat suitability criteria are typically the most significant factor in determining the outcome of a habitat study. If the method utilized in this study had overestimated the amount of available habitat, there should have been a large percentage of unoccupied locations for fall-run Chinook salmon with high combined suitability. Such was not observed in this study, since only 18 percent of the unoccupied locations had suitabilities greater than 0.5 for fall-run Chinook salmon spawning. We would characterize the HSC development approach used in this study as based on a fully-developed methodology which represents the state-of-the-art for developing unbiased habitat suitability criteria. The methodology has been published in a peer-reviewed journal (Gard 1998) and we have applied it on six streams (Merced River, American River, Sacramento River, Butte Creek, Yuba River and Clear Creek). Based on a search of the Science Citation Index database, we are unaware of other published studies using this methodology. We responded to the peer reviewers of the Sacramento River instream flow study regarding whether the method is confusing or produces questionable results. We revised the description of the methods in that report to clarify the methods and demonstrate the validity of the results. Such concerns regularly arise during the peer review process of manuscripts for publication in scientific journals, but do not necessarily negate the validity of a given methodology. Rather, peer review is an opportunity and forum to respond, clarify, and revise where appropriate.

YCWA 29. Comment: *The approach used by the USFWS to develop spawning HSC appears to be a unique combination of "traditional" normalized frequency of use evaluation for substrate suitability and a two-step approach involving: (1) polynomial logistic analysis of depth and velocity suitability; and (2) adjustment of depth suitability (Gard 1998sic) (pg. 21). The only references provided by the USFWS draft report in its discussion of suitability development are Rubin et al. (1991), which does not apply (see Comment 6), and SYSTAT 2002. Because neither of these references nor the discussion provided by the USFWS draft report describes or supports using the method for HSC development, the identified flow-habitat relationships on the Yuba River may not be valid.*

Response: We agree that the approach used is a unique combination of "traditional" normalized frequency of use evaluation for substrate suitability and a two-step approach involving: (1) polynomial logistic analysis of depth and velocity suitability (Guay et al. 2000); and (2) adjustment of depth suitability (Gard 1998). We believe that the technique is unique because it is a state-of-the-art method. As noted in our response to YWCA Comment 10, we have changed the reference to Guay et al. (2000), which applies, and provides a description of the polynomial logistic analysis of depth and velocity suitability. The methods gives a third reference– Gard (1998) – which provides a complete description of the technique for adjustment of depth suitability. The combination of Guay et al. (2000) and Gard (1998) describe and support the method used for HSC development. Further, the discussion in the report both describes and supports using the method for HSC development. Accordingly, we believe that the identified flow-habitat relationships on the Yuba River are valid.

YCWA 30. *Multivariate logistic regression is a developing approach to evaluate habitat suitability. Under this approach, scientists typically take the presence of a fish (or its redd) at a site to imply site suitability, and subsequently model presence/absence across a wide range of sites as a function of a suite of continuous or categorical habitat variables using standard statistical techniques.*

Response: We disagree that multivariate logistic regression is a developing approach – rather we would characterize it as a fully-developed approach, since it was first introduced in 1985 and is well-established in the peer-reviewed literature for developing HSC (Knapp and Preisler 1999, Parasiewicz 1999, Geist et al. 2000, Guay et al. 2000, Tiffan et al. 2002, McHugh and Budy 2004).

YCWA 31. Comment: *The USFWS draft report uses logistic regression to identify significant influences on habitat use by determining the significant independent variable(s), and then developing a probability of habitat use based on those variables.*

Response: We agree.

YCWA 32. Comment: *The USFWS draft report apparently deviates from the approaches in the referenced documents by using univariate logistic regression approach, rather than a multivariate logistic regression approach. As such, the approach used by the USFWS draft report is not directly comparable to the methods reported in the literature, and requires a more detailed description and discussion supporting the applied method. The USFWS draft*

report needs to be edited to discuss the appropriateness of its approach, including a more detailed description of what the approach involved and how it was developed, and the biological rationale for using this approach.

Response: A multivariate approach assumes that there are interactions between depth, velocity and substrate. We consider the use of a univariate approach to be appropriate in this application (see response to YWCA comment 33), and thus a more detailed description and discussion supporting the applied method in the report is not required. We have reviewed the draft report to ensure that it provides a sufficiently detailed description of HSC development protocols and the biological rationale for using this approach (Methods – Habitat Suitability Criteria (HSC) Development).

YCWA 33. Comment: *The USFWS draft report assumed that the three independent variables, depth, velocity and substrate, are equally significant because compound suitability is the product of the HSI of the three variables. The USFWS draft report had the opportunity to test this assumption but did not. Given the uncertainty of the applied assumption of equal significance in the Yuba River, the USFWS draft report needs to be edited to reevaluate these findings, including a determination of the significance of depth, velocity and substrate in determining the quality of spawning habitat in the Yuba River, and then reevaluate the habitat suitability relationships, as appropriate.*

Response: It is the standard practice in instream flow studies to assume that all independent variables are equally significant and to have compound suitability calculated as the product of the HSI of the independent variables (Bovee 1996, page 120). This assumption has previously been tested and validated in the peer-reviewed literature (Vadas and Orth 2001). Accordingly, we did not test this assumption on the Yuba River.

YCWA 34. Comment: *McHugh and Budy (2004) found spawning habitat suitability in Elk Creek was best modeled as a quadratic function of gravel size; in Sulphur Creek, it was best modeled as a positive function of depth alone, and the best model fit to a pooled data set was a hybrid of the two single-stream models. It is not clear if USFWS draft report determined the significances of the three suitability variables. Given that the significances of the three variables on habitat suitability vary with streams, and because the objective of the use of the logistic regression is to improve characterization of the conditions defining habitat suitability, the use of a multivariate approach should be discussed in the USFWS draft report and used as appropriate.*

Response: As noted in the report, the logistic regressions for both depth and velocity for all three runs/species were significant at $p = 0.05$. Although it is not typically given for use criteria, we also performed a chi-squared test to determine if substrate was significant, and found that it was highly significant ($p = 1.2 \times 10^{-29}$ to 2.2×10^{-109}) for all three runs/species. Given the result of Vadas and Orth (2001) that all independent variables are equally significant, it is not necessary to discuss or appropriate to use a multivariate approach in the report.

YCWA 35. Comment: *Geist et al. (2000) hypothesized that PHABSIM estimates would be improved if they incorporated habitat suitability criteria from redd clusters rather than from individual redds. They determined that a logistic model is appropriate because it can*

be used to examine the functional relationship between a binary response (suitable versus unsuitable) and explanatory variables that describe the quality of the habitat. The model estimates the probability of a positive response occurring given a set of explanatory variables. Fall-run Chinook salmon spawning habitat suitability was the binary response variable (suitable or unsuitable) for the logistic model.

Response: This is an accurate summary of Geist et al. (2000).

YCWA 36. Comment: *Geist et al. (2000) used the results of the spatial pattern analysis to determine whether habitat cells fell inside or outside redd clusters. Habitat cells that fell inside redd clusters were assumed to be representative of "suitable" spawning habitat and were coded as 0. Habitat cells that fell outside the boundaries of the clusters were assumed to be representative of "unsuitable" spawning habitat and were coded as 1. Depth, velocity, substrate (dominant and subdominant), and lateral slope values from each habitat cell were treated as explanatory variables. All explanatory variables were treated as continuous variables with the exception of the variables dominant substrate and subdominant substrate, which were both treated as discrete factors with six levels based on substrate diameter.*

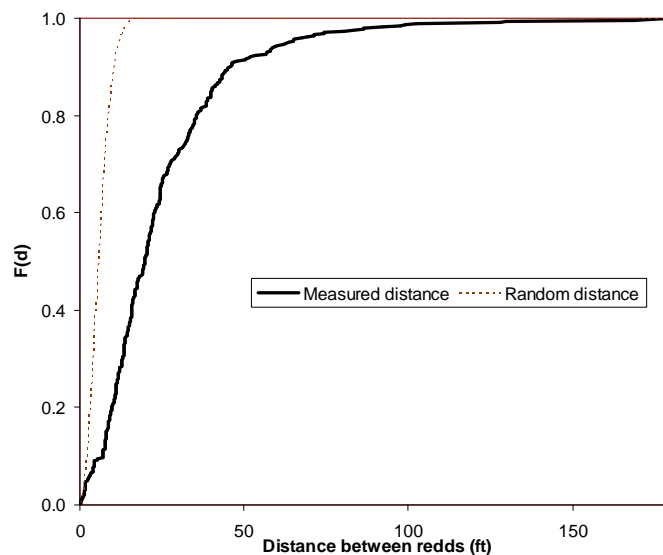
Response: This is an accurate summary of Geist et al. (2000).

YCWA 37. Comment: *Critical components of the approach reported by Geist et al. (2000) include:*

- *Habitat attributes associated with redd clusters were found to be a preferred descriptor of habitat conditions versus individual redds*
- *Variables were systematically evaluated to determine their significance in determining suitability*
- *Not all independent variables were equally significant*

The USFWS draft report appears to have used a similar step-wise approach to determine the significance of expressions in the polynomial equation developed for each variable. The utility of using a regressed relationship between velocities at occupied and unoccupied sites, however, is unclear. The USFWS draft report should discuss in more detail its approach in terms of its need, appropriateness and rationale, and explain contrast to the approach reported by Geist et al. (2000).

Response: The first component assumes that redds are clustered. We performed a preliminary analysis for clustering using our fall-run redd data from our study sites. As shown in the graph on the following page, the redds on the Yuba River are not clustered, since the measured distances fall to the right of the random distances. Accordingly, the first component from Geist et al. (2000) does not apply to the Yuba River.



We systematically evaluated variables to determine their significance in determining suitability – we found that all three variables (depth, velocity and substrate) were significant in determining suitability (see response to YWCA comment 34). It is the standard practice in instream flow studies to assume that all independent variables are equally significant and to have compound suitability calculated as the product of the HSI of the independent variables (Bovee 1996, page 120). This assumption has previously been tested and validated in the peer-reviewed literature (Vadas and Orth 2001). Accordingly, we believe that it is appropriate to use it in this study. We view the differences between Geist et al. (2000)’s approach and our approach as minor, and thus believe that it is not necessary to include a discussion in the report contrasting the methods used by Geist et al. (2000) and in this report. We believe that the utility of using a regressed relationship between velocities at occupied and unoccupied sites is clear, based on the references on logistic regression and the discussion in the report. We have reviewed the draft report to ensure that it includes sufficient detail of our approach in terms of its need, appropriateness and rationale (Methods – Habitat Suitability Criteria (HSC) Development).

YCWA 38. Comment: *Guay (2000) developed two types of biological models to describe habitat use: a habitat suitability model and a habitat probabilistic model. The habitat probabilistic model was used to estimate the probability of observing a fish under given combinations of physical conditions. This was achieved by fitting a multivariate Gaussian logistic regression model to the presence-absence data. The logistic model was intended to predict the probability (0-1) of finding fish in any tile using local substrate composition, current speed, and water depth as independent variables. The USFWS draft report would benefit from a clear discussion of the methods used in its study, and a comparison of these methods to the methods used in the referenced documents.*

Response: The only real difference between the approach used by Guay et al. (2000) and in this report is that Guay et al. (2000) used a multivariate logistic regression and this report used univariate logistic regressions. We consider this difference to be minor, and thus believe that a comparison of the methods in this report to those in Guay et al. (2000) is not necessary. We have reviewed the draft report to confirm that it includes a sufficiently clear description of the methods used in our study.

YCWA 39. *Review of the remainder of the USFWS draft report indicates that coefficients of determination (R^2) and significance levels are not presented for the availability regression, the use regression, or the "linearized" preference (scaled ratio) regression. Provision of these values in the USFWS draft report would facilitate evaluation of these relationships and, together with plots (such as Figures 7, 8), ascertain the degree to which the "linearized" values used as observed dependent variables deviate from the predicted values using the regression equation. A residual analysis would accomplish this important step in the analysis. Results of the residual analysis would be used to evaluate whether a linear regression function is appropriate and to examine whether the variance of the (as yet, unaccounted for) error terms is constant. In fact, cursory examination at Figure 7 (pf. 38) suggests departures from the linear regression model. The USFWS draft report should contain a specific discussion of the ultimate utility of the depth adjustment procedure considering the potential for compound (unaccounted for) error associated with the multi-step procedure.*

Response: We have added R^2 and significance levels to the report for the availability regression, the use regression, and the use to availability regression. We believe that a residual analysis is not necessary, since, as noted by the commenter, the R^2 and significance levels and plots together show the degree to which the linearized values deviate from the predicted values using the regression equation. With regards to the significance levels, we note the following from Gard (1998, p. 101):

“Although the statistical significance of the regression equations has been given in this paper, a lack of statistical significance (for example, relative to $P = 0.05$) should not be viewed as a reason to reject the results of an analysis using this technique. The regressions are used to linearize the data, rather than test the hypothesis of the statistical significance of the relation between two variables. Thus, even though the final regression for correcting the Merced River depth criteria was not significant at $P = 0.05$, the resulting depth at which the scaled ratio equaled zero is still valid. The lack of statistical significance in this case was primarily due to the lower number of degrees of freedom, versus a lack of linearity of the scaled ratio-depth pairs used in the regression.”

In developing this method in the late 1990's, we considered the option of using a non-linear regression in cases where there was a departure from the linear regression model. We believe that any increased strength in the relationship gained by using a non-linear regression would be overshadowed by the extent to which the relationship is affected by noise in the data. As such, the results of a non-linear regression might reflect more the variation in the data rather than biological reality. We believe that it is not necessary for the report to contain a specific discussion of the ultimate utility of the depth adjustment procedure considering the potential for compound (unaccounted for) error associated with the multi-step procedure, since the methodological considerations were evaluated prior to publication of the technique in Gard (1998).

YCWA 40. *The USFWS draft report should contain a specific discussion of the appropriateness of extrapolating the "scaled ratio" (preference) continuous function beyond the largest independent "data" value to predict the intercept with the X-axis (i.e., where "scaled ratio" preference reaches zero). The scope of a regression model ordinarily is restricted to the range of the independent variable, which is particularly important when using estimates of the slope (Neter et al. 1985).*

Response: With regard to this comment, we note the following from Gard (1998, p.100):

“The depth at which the modified HSC curve reaches zero has no biological significance; rather, it is a convenient way to describe in the criteria the rate of decline of suitability with depth.”

Gard (1998) includes the above discussion of the appropriateness of extrapolating the "scaled ratio" (preference) continuous function beyond the largest independent "data" value to predict the intercept with the X-axis (i.e., where "scaled ratio" preference reaches zero). Therefore, we believe that it is not necessary for the report to also include such a discussion, since the report includes a reference to Gard (1998).

YCWA 41. *In addition to extrapolation of the slope of the regression equation beyond the range of the independent variable, the concept of predictability is further vitiated in the USFWS draft report by extending the fall-run Chinook salmon depth HSI past the intercept (4.86 ft.) out to 7.8 feet (albeit at a low 0.02 suitability value) to encompass three redds where the reported depth of HSC data collection ranged from 5.0 to 7.8 feet (pg. 37). The arbitrary extension to accommodate these outliers (3 out of 870 redds, or 0.3%) should be explained within the context of using "linearized" values and resultant predicted zero depth (via linear regression) as a formalized process.*

Response: We view the above as a minor change to the Gard (1998) method, because the sole purpose of the change is to account for outliers (i.e., the three redds in depths of greater than 4.9 feet), and thus account for a habitat suitability slightly greater than zero for depths of 5.0 to 7.8 feet. It should also be noted that this change would have a minimal effect on the resulting flow-habitat relationship due to the low suitability for depths of 5.0 to 7.8 feet. Therefore, we disagree that this change reduces the predictability of the depth suitability curve, nor do we agree that this was an arbitrary extension. We have reviewed the draft report to ensure that it contains a sufficient explanation of this modification of the Gard (1998) method within the context of using linearized values and resultant predicted zero depth (via linear regression) as a formalized process.

YCWA 42. *The R^2 values for the logistic regressions (Table 11, pg. 38) are very low. In the Discussion (pg. 64), the USFWS draft report suggests that these R^2 values are low because the USFWS draft report used a univariate logistic regression approach, rather than a multivariate logistic regression approach used in the other studies, which included additional independent variables. If that is the case, then the Discussion section of the USFWS draft report should be edited to provide additional explanation why the univariate approach was used and the predictive capability of the logistic regression model(s) based on observed values.*

Response: The text of the report states that the overall proportion of variance explained by the three variables is apportioned among depth, velocity and substrate. Thus, the two univariate logistic regressions (depth and velocity), together with the substrate use criteria, overall explain as much of the overall variance as a multivariate logistic regression would. Accordingly, no additional explanation has been included in the discussion section on the selection of the univariate approach and the predictive capability of the logistic regression model(s) based on observed values.

YCWA 43. Comment: *The USFWS draft report correctly states that low R^2 values are the norm in logistic regressions, particularly in comparison with linear regression models (pg. 64). However, the predictive capability of the fitted logistic regression model can also be ascertained by goodness-of-fit and discrimination evaluations. The USFWS draft report should be edited to include such evaluations in the section with the presentation of R^2 values (on pg. 38).*

Response: R^2 values are a measure of goodness-of-fit, and thus no additional information has been added to the report on goodness-of-fit. Discrimination evaluations are not appropriate for a continuous variable such as HSI, because they require categorical variables – i.e., that a location either is or is not habitat.

YCWA 44. *Perhaps a true measure of the fit of the logistic regression model would be one based on a comparison of observed values to predicted values from the fitted model, as suggested by Hosmer and Lemeshow (2000). The results, as presented in the USFWS draft report, are somewhat difficult to follow. Are the comparisons of the predicted values versus observed values presented for the univariate logistic regressions, or just for the combined habitat suitabilities for the 2-D model, as presented under the Biological Validation section?*

Response: The observed values are all either 0 (unoccupied) or 1 (occupied), but the predicted values are continuous between 0 and 1. Therefore, it would be difficult to measure fit of the logistic regression model by comparing observed to predicted values. The comparisons of the predicted values versus observed values in Figures 12 through 17 are for the univariate logistic regressions, while the results in the Biological Validation section are for the combined habitat suitabilities for the 2-D model.

YCWA 45. *The biovalidation approach would benefit from a more clear description of the specific methodology employed, and from exacting use of terminology. For example, from the description provided in the USFWS draft report (Abstract and pg. 20), it appears that a subset of the data collected for each species/run's HSC development also was used for validation. If that is the case, then the same data were used for both "calibration" of the HSCs and the "validation" of the HSCs, and a more thorough discussion needs to be provided explaining the appropriateness of an approach that uses the same data for both calibration and validation. If this was not the case, then the text needs to be revised to clearly demonstrate that it was not.*

Response: We have reviewed the clarity of the description of the specific methodology employed for biological validation. The commenter is correct that a subset of the data collected for each species/run's HSC development also was used for validation. In the strict sense of the term, validation uses a different dataset than does calibration. Accordingly, we have changed the term validation to verification. Only a subset (the redds that were in our study sites) of the calibration data (all redds) was used for verification. Although verification of the model is a less rigorous test than validation, a failure to verify the model would clearly show a problem with the HSC or hydraulic model. No such failures were observed.

YCWA 46. *Regarding biovalidation, in the referenced reports (see Comment 6) the logistic regression approach was used to predict the probability of presence/absence in the response variable (in the case of the USFWS draft report, the presence/absence of redds). However, the USFWS draft report (pg. 20) states that the biological validation approach was to test the hypothesis that the compound suitability predicted by the River 2-D model is higher at locations where redds were present than at locations where redds were absent. Presentation of the results (pg. 47), however, focuses on the number of occurrences where redds were actually located where the 2-D model predicted a combined suitability of zero. This presentation implies a binary response evaluation which, apparently, was not the case. Additional text is necessary to clarify this issue.*

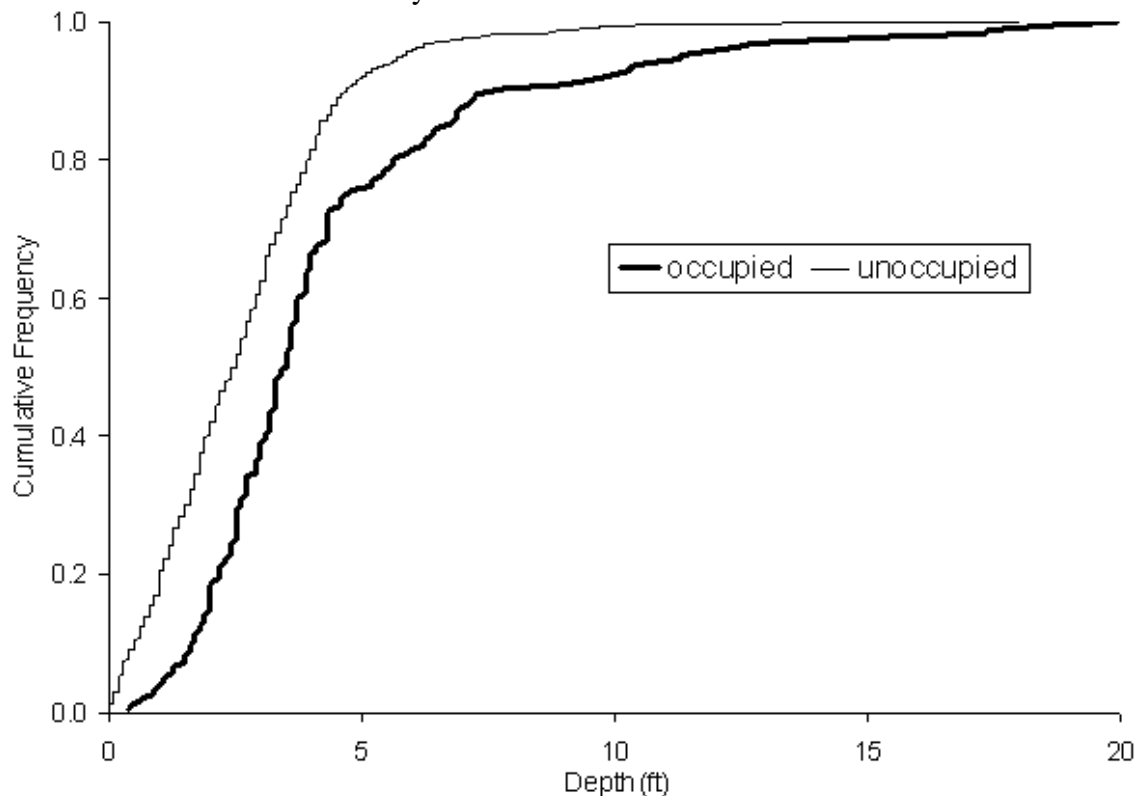
Response: The presentation of the results focuses equally on: 1) the testing of the hypothesis that the compound suitability predicted by the River 2-D model is higher at locations where redds were present than at locations where redds were absent; and 2) the number of occurrences where redds were actually located where the 2-D model predicted a combined suitability of zero. This presentation does not imply a binary response evaluation; rather the discussion of the number of occurrences where redds were actually located where the 2-D model predicted a combined suitability of zero focuses on a subset of the observations where the 2-D model prediction was clearly wrong (i.e., the left-most bar in Figures 18, 20 and 22). To address this issue, we examined the data to determine the cause of the incorrect prediction. In most cases, the incorrect prediction was due to the predicted substrate being too small or too large. Since there was not a binary response evaluation, no additional text has been added on this issue.

RESULTS

GENERAL COMMENTS

YCWA 47. Comment: *For the Yuba River, the USFWS draft report determined that the rate of use of deep water was disproportionately higher than the rate of availability, compared to the rate of use relative to the rate of availability for shallow water. Over 75% of observed steelhead redds were in shallow water; however, the relative availability of suitable shallow water conditions was greater than 75% and, therefore, the USFWS draft report developed a depth suitability curve that identifies the suitability for over 75% of the observed redds to be less than 0.4, while the suitability for less than 5% of the observed redds was 1.0 (the highest possible suitability). This result appears counterintuitive and incongruous.*

Response: The logistic regression demonstrates that steelhead/rainbow trout strongly select deeper conditions in the Yuba River. As such, the large number of steelhead/rainbow trout redds in shallow water can be attributed to steelhead/rainbow trout being forced to use shallow conditions because of the relative scarcity of deeper water conditions in high use spawning areas in the Yuba River. As shown on the following page, for all depths, steelhead/rainbow trout disproportionately selected deeper conditions. Consideration of both use and nonuse data is necessary to evaluate whether the results follow from the data.



YCWA 48. Comment: *The deeper conditions likely occurred in pools (depths > 10 ft), while the shallower conditions likely occurred in riffles. Per the resultant USFWS draft report HSC for steelhead, pools increase in spawning suitability as depth increases from 10 to 15 feet. Further, if the suitability is a function of depth, then deeper riffles should provide more suitable habitat than shallower riffles. However, for riffles to deepen, velocities must increase and overall suitability would likely decrease. The issue is whether increasing depth beyond the majority of depths observed in areas other than pools would actually improve spawning habitat availability and, similarly, if availability would be increased by increasing depths of already deep habitat (i.e., pools).*

Response: We added material to the report investigating whether the data at the upper tails of the distribution (depths greater than 5.8 feet) were driving the shape of the logistic regression curve. The conclusion of this analysis was that the data in deep water (depths greater than 5.8 feet) were not driving the shape of the logistic regression; rather the shape of the curve was driven by the relative number of occupied and unoccupied values with depths of 2 to 5 feet versus 5 to 5.7 feet. As shown in the table on the following page, very few steelhead in either depth range spawned in riffles, and the main shift in habitat type use going from the 2 to 5 feet

depth range to the 5 to 5.7 feet depth range was a near elimination of redds in glide habitats. Further, in the 5 to 5.7 feet depth range, there were equal numbers of redds in pools and runs. The commenter's analysis does not take into account that the flow-habitat relationship is a result of the combination of depth, velocity and substrate suitabilities and available habitat. Thus, the amount of weighted useable area in riffles would tend to decrease with increasing flow because of the increasing velocities. An important factor to consider is that depth suitability increases continuously from 0.4 to 15 feet, so that increases in depth in any habitat type with increasing flow would increase the amount of spawning habitat.

Habitat type	Redds with depths of 2-5 feet	Redds with depths of 5.1-5.7 feet
Glide	62	1
Pool	21	4
Run	27	4
Riffle	3	0

YCWA 49. Comment: *Results in the USFWS draft report are remarkable given the preponderance of shallower depths identified in surveys and state (i.e., California, Oregon, and Washington) criteria for steelhead. The ranges of steelhead spawning depths reported in the literature and obtained from unpublished study results would result in low suitabilities (ranging from 0.0 to 0.29) if the USFWS draft report HSI were applied:*

<i>Source</i>	<i>Min Depth</i>	<i>Max Depth</i>	<i>Suitability per USFWS</i>	<i>Comment</i>
<i>Hannon and Deason</i>	<i>0.66</i>	<i>3.67</i>	<i>0.10 - 0.27</i>	<i>Lower American</i>
<i>Bovee 1978</i>	<i>0.50</i>	<i>3.00</i>	<i>0.09 - 0.22</i>	<i>adopted by CDFG</i>
<i>Briggs 1953</i>	<i>0.75</i>	<i>1.17</i>	<i>0.10 - 0.12</i>	
<i>Smith 1973</i>	<i>0.71</i>	<i>1.37</i>	<i>0.10 - 0.13</i>	<i>Oregon</i>
<i>Moyle 2002</i>	<i>0.33</i>	<i>4.90</i>	<i>0.00 - 0.29</i>	
<i>Reiser and Bjornn 1979</i>	<i>0.33</i>	<i>4.90</i>	<i>0.00 - 0.29</i>	
<i>Hunter 1973</i>	<i>0.39</i>	<i>2.30</i>	<i>0.09 - 0.18</i>	<i>WA in Barnhart</i>
<i>Carrol 1984</i>	<i>0.39</i>	<i>0.95</i>	<i>0.09 - 0.11</i>	
<i>Hampton 1988</i>	<i>-</i>	<i>3.40</i>	<i>0.25</i>	<i>Trinity River</i>

Response: We believe that the above criteria are all likely biased towards shallow depths because of limited availability of deeper water with suitable substrate and velocities, and because the above criteria did not apply a logistic regression to correct for availability. We believe that the Yuba River is unique among the rivers studied in that it has some deeper areas with suitable velocities and substrates, allowing 24 percent of the steelhead to spawn in water 5 feet or deeper. Clearly the criteria cited by the commenter are not applicable to the Yuba River, since they all have zero suitability for depths of 5 feet or greater. Further, the substantial natural flow fluctuations during the steelhead spawning season on the Yuba River would be a strong selective force to shift steelhead spawning behavior towards selecting deeper conditions, since eggs in shallow redds would not survive dewatering or scouring associated with flow fluctuations.

YCWA 50. Comment: *A major concern regarding the steelhead HSI is its low suitability of depths that are associated with over 90% of the observed redds. Theses [sic] shallower redd depths are more consistent with ranges of spawning depths reported in the literature.*

Theoretically, if all useable habitat were used, the frequency of use would identify the suitability, which is why the assumption that all useable habitat is occupied underlies HSI development methodologies (Bovee 1986; McHugh and Budy 2004).

Response: As noted above, the depths of 24 percent of the redds (those in 5 feet or greater) is not consistent with the ranges of spawning depths reported in the literature. Theoretically, if there were equal availability of all depths, the frequency of use would identify the suitability. To the extent that availability is constraining use, the frequency of use deviates from the suitability. For steelhead/rainbow trout on the Yuba River, the results of the logistic regression indicate that availability was strongly constraining use, resulting in most of the redds being in shallow water, and thus suitability differs substantially from the frequency of use.

YCWA 51. *Given the reported discovery of some steelhead redds in unusually deep water in the lower Yuba River (pg. 32), additional description of how they were identified and classified as steelhead redds should be added to the USFWS draft report. Topic areas that should be discussed in the USFWS draft report include, but are not necessarily limited to: (1) video interpretation techniques; and (2) criteria used to classify redds as those of steelhead (e.g., size), including how they were differentiated from lamprey and Chinook salmon. Regarding Chinook salmon, the report should provide discussion of the differentiation of redds of late-spawning Chinook salmon, the potential for the appearance of "recently constructed" redds due to minimal periphyton growth, and how the rate of periphyton growth may differ in deep water relative to shallow water.*

Response: We have added additional description to the report on how steelhead/rainbow trout redds were identified, including video interpretation techniques and the size criteria used to classify redds as steelhead/rainbow trout, rather than late-fall-run Chinook salmon redds. We believe that it is unlikely that any of the redds we considered as steelhead/rainbow trout were actually late-fall-run Chinook salmon redds for the following reasons: 1) there appears to be an extremely low population of late-fall-run Chinook salmon in the Yuba River, based on our only finding one late-fall-run Chinook salmon redd in February 2002; 2) most of the steelhead/rainbow trout redds were measured in April, which is after the end of the spawning season for late-fall-run Chinook salmon; and 3) we observed steelhead/rainbow trout on 11 percent of the redds, but did not observe late-fall-run Chinook salmon on any redds. We did not add any additional information to the report on criteria used to classify redds as steelhead/rainbow trout, rather than lamprey nests, since we did not try to distinguish steelhead/rainbow trout redds from lamprey nests. However, based on the information in Stone (2006) it appears unlikely that any of the redds we considered as steelhead/rainbow trout were actually lamprey nests for the following reasons: 1) lamprey nests do not have a distinct tailspill; 2) lamprey tend to place tailings upstream of the nest and steelhead do not; 3) only two of the redds we measured were smaller than the area (7.7 ft²) of the largest lamprey nest reported by Stone (2006); and 4) we did not observe any lamprey on redds. As noted above, we added information to the report on the differentiation of redds of late-spawning Chinook salmon. We did not add any information to the report on the potential for the appearance of "recently constructed" redds due to minimal periphyton growth or how the rate of periphyton growth may differ in deep water relative to shallow water because we do not have nor are we aware of any information in the literature on these subjects.

YCWA 52. *Also in consideration of the reported unusually deep water selected by steelhead for spawning, additional description is needed of when, relative to what depths, specific steelhead redd HSC data were collected. As indicated in Table 10 (pg. 32), steelhead/rainbow trout HSI data were collected seven times during this study. On two occasions, HSI data were collected during February, and on the other five occasions HSI data were not collected until April. In consideration of Comment 19, was there a difference between steelhead HSC water depth data collected during the February sampling efforts, relative to the data collecting during the April sampling efforts?*

Response: All of the redds measured in February were in shallow water (depths of 0.4 to 1.2 feet). All of the deep redds were measured in April.

YCWA 53. *The water velocities reported as HSC data (pg. 32), intended to represent water velocities selected by fall-run Chinook salmon and steelhead/rainbow trout at the time of redd construction, also are high relative to spawning water velocities reported for other rivers in California:*

CHINOOK SALMON SPAWNING WATER VELOCITY (FT/SEC)

Draft USFWS Report (2007)	Yuba River (Fall-run)	0.23 — 5.31
CDFG (Wheeler et al. 1991)	Yuba River (Fall-run)	0.00 — 4.55
Healey (1991)	California Rivers	0.32 — 4.92
CDFG (1991)	Mokelumne River	0.10 — 4.90
Sommer et al. (2001)	Feather River	0.40 — 4.80
USFWS (Gard 2003)	Sacramento River (Fall-run)	0.32 — 5.79
Flosi et al. (1998)	California Rivers	1.00 — 3.00

STEELHEAD SPAWNING WATER VELOCITY (FT/SEC)

Draft USFWS Report (2007)	Yuba River	0.07 — 6.92
USBR (Harmon and Deason)	American River	0.46 — 5.74
DWR (Payne 2002)	Feather River	0.60 — 2.80
CDFG (1991)	Mokelumne River	1.15 — 2.50
MPWMD (Dettman & Kelly)	Carmel River	0.60 — 3.80

The unusually high spawning site selection water velocities in the USFWS draft report for fall-run Chinook salmon, as well as for steelhead/rainbow trout, indicate a consistent departure from reports on other rivers, and may reflect bias in the measurements as described in Comment 7 [YWCA Comment 14]. The USFWS draft report should be edited to provide additional discussion to address these concerns.

Response: With the possible exception of Flosi et al. (1998), the other fall-run velocity criteria are quite similar to those we developed on the Yuba River. We note in particular that the Sacramento River fall-run velocity criteria are actually higher than the Yuba River criteria. As noted in the response to YWCA Comment 14, the velocities measured on the redds are likely biased low, compared to the velocities present during redd construction. We have added discussion to the report on the effects of differences in depths and velocities at the time of redd construction versus at the time habitat suitability criteria data were collected (Discussion – Factors Causing Uncertainty).

YCWA 54. *The USFWS draft report (pg. 47) states that, for all three species/runs, the combined habitat suitability predicted by the 2-D model was significantly higher for locations with redds than for locations without redds, based on the Mann-Whitney U test. Although the USFWS draft report provides the medians, sample sizes and P values, the U test statistic (or z for large samples) should also be provided in the USFWS draft report.*

Response: We have added U test statistics to the report.

YCWA 55. *This comment reiterates the comment provided by PG&E (pg. 72, Biologic Validation) ... "This discussion fails to address the underlying problems in the simulations, particularly for steelhead. The plots of combined suitability and redd locations presented in Appendix I clearly show that the USFWS 2-D simulations almost never predict the location of steelhead spawning."*

Response: We believe that there are not any underlying problems in the simulations (see response to PG&E comment 17). We believe that the plots of combined suitability and redd locations presented in Appendix L (previously Appendix J) show that the USFWS 2-D simulations overall predict the locations with redds to be in areas with higher suitability than locations without redds. See for example the results for U.C. Sierra site. Overall, these plots show that there is little optimal habitat for steelhead spawning at existing flows. Note that Figures 22 and 23 summarize the data presented in Appendix L.

YCWA 56. *Biological validation is intended to determine if the biological criteria are correctly defining habitat usability. If the model identifies a site as suitable and the site is occupied (redd present) then the model correctly identified habitat usability. If the model is developed using redd data collected at a particular location under specific flows and is validated using redd data collected from the same population but not included in the HSC development, does this test biological validation? It would appear that the expectation is that the redds used for validation and the redds used for HSC development would generate the same suitability indices. Therefore, one would expect the "validation" sites to have the same suitability indices as the HSC sites. The question then becomes does the model correctly calculate depth and velocity for the validation sites, rather than does the suitability index correctly portray site selection.*

Response: The commenter is correct that a subset of the data collected for each species/run's HSC development also was used for validation. In the strict sense of the term, validation uses a different dataset than does calibration. Accordingly, we have changed the term validation to verification. Only a subset (the redds that were in our study sites) of the calibration data (all redds) was used for verification. Although verification of the model is a less rigorous test than validation, a failure to verify the model would clearly show a problem with the HSC or hydraulic model. No such failures were observed.

DISCUSSION

YCWA 57. *As stated in the USFWS draft report, the R^2 values in their study were low, reflecting a large degree of overlap in occupied and unoccupied depths and velocities. The USFWS draft report appears to discuss the low R^2 values as the norm in logistic regression, rather than considering the possibility that there was no significance to the influence of depth or velocity on use or nonuse within the range of overlapping conditions. This possibility suggests that depth and velocity do not sustain relative suitability within the range of use. If depth and velocity act more as boundary conditions for use given that all other spawning conditions are suitable (i.e., substrate composition, permeability, and intragravel velocities (Kondolf 2000; Vyverberg et al. 1997), then the HSC for depth and velocity becomes binary. Additional discussion of this point should be provided in the USFWS draft report.*

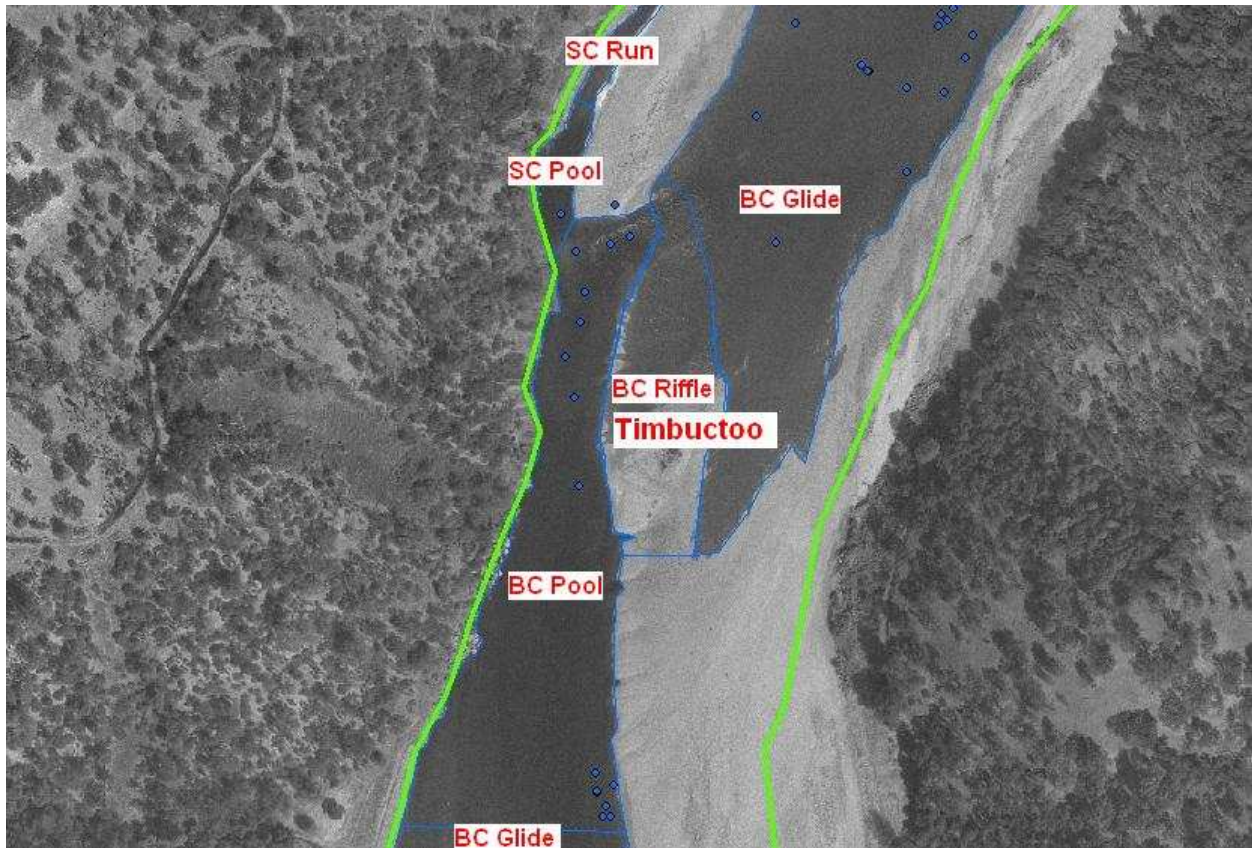
Response: The logistic regressions clearly showed that there was a significant influence of depth and velocity on use or nonuse with the range of overlapping conditions, since the p-values for the logistic regressions and the p-values for the individual terms of the logistic regressions were all less than 0.05. Accordingly, we believe that depth and velocity do not act as boundary conditions for use given that all other spawning conditions are suitable (i.e., substrate composition, permeability, and intragravel velocities). Binary criteria are generally biologically unrealistic – they either overestimate the habitat value of marginal conditions if the binary criteria are broadly defined (for example, setting suitability equal to one for any depths and velocities where the original HSI value was greater than 0.1) or completely discount the habitat value of marginal conditions. The latter case would be biologically unrealistic since many redds would be in areas which would be considered completely unsuitable from the binary criteria. We added this discussion to the USFWS report.

YCWA 58. *Although steelhead spawning in deep pools may be a reality, it is a novelty (based upon the reported ranges of steelhead spawning depths) and its substantial influence in the analysis on depths in non-pool habitats should be further discussed. The USFWS draft report (pg. 73) cites the results of the Beak (1989) Yuba River flow evaluation as being less appropriate than the USFWS draft report investigation due, in part, to use of mesohabitat to describe habitat conditions. However, if the habitat variables (i.e., depth and velocity) described in the USFWS draft report have differential relationships with habitat use relative to mesohabitat, then the conclusions in the USFWS draft report may not be justified. The USFWS draft report needs to be edited to provide a detailed discussion of the relative merits of assuming that depth and velocity affect all spawning habitat equally, regardless of mesohabitat. This discussion should include discussions of the results of numerous investigations relating channel topography to spawning habitat, and that mesohabitat is a function of topography. The USFWS draft report cites the results of Vyverberg et al. (1997) to justify elimination of mesohabitat as an influence on spawning. However, the results reported by Vyverberg et al. (1997) indicated that mesohabitat, as a discrete descriptor, was not related to spawning use, but they did not suggest that mesohabitat conditions could not be used to define spawning habitat. In fact, most references on salmonid spawning conditions describe spawning habitat as occurring at mesohabitat transitions, such as pool-riffle transitions.*

Response: We observed steelhead/rainbow trout spawning in deep conditions in the Yuba River. The shape of the logistic regression for depth for steelhead/rainbow trout was not driven by the deepest observations (those in depths greater than 5.8 feet), but were rather driven by the relative number of occupied and unoccupied values with depths of 2 to 5 feet versus 5 to 5.7 feet (see response to PG&E comment 8). It is not accurate to describe the steelhead/rainbow trout spawning as being in deep pools, since there were equal numbers of redds in pools and runs for redds with depths of 5 to 5.7 feet. As such, the proposed discussion on the influence of the steelhead/rainbow trout depth HSC on depths in non-pool habitats is moot. The table on the following page summarizes the relationships between depth and velocity and habitat use relative to mesohabitat type.

Mesohabitat Type	Mean Occupied Depth (ft)	Mean Unoccupied Depth (ft)	Mean Occupied Velocity (ft/s)	Mean Unoccupied Velocity (ft/s)
Glide	3.3 (n = 70)	2.4 (n = 350)	2.62 (n = 70)	1.94 (n = 350)
Pool	7.3 (n = 59)	6.1 (n = 55)	2.89 (n = 59)	1.88 (n = 55)
Riffle	2.3 (n = 5)	1.8 (n = 208)	1.37 (n = 5)	2.82 (n = 208)
Run	2.6 (n = 50)	2.8 (n = 568)	2.55 (n = 50)	3.35 (n = 568)

As expected, the depths of both occupied and unoccupied locations were deepest in pools and shallowest in riffles. Also, the above table shows that fish preferentially selected deeper conditions in three out of four habitat types, since occupied locations had greater depths than unoccupied locations for glides, pools and riffles. For velocities, steelhead/rainbow trout preferentially selected higher velocities in glides and pools and lower velocities in riffles and runs. We find the existence of differential relationships with habitat use relative to mesohabitat interesting, but are not aware of how these relationships could inform our analyses, or how these relationships would affect the validity of the conclusions in the report. Further discussion, beyond the results reported by Vyverberg et al. (1997), on the relative merits of assuming that depth and velocity affect all spawning habitat equally, regardless of mesohabitat, is not necessary. Relationships between channel topography and both spawning habitat and mesohabitat do not imply a relationship between spawning habitat and mesohabitat. Additionally, we believe that the findings of Vyverberg et al. (1997) lead logically to the conclusion that mesohabitat conditions cannot be used to define spawning habitat. Finally, our data on steelhead/rainbow trout spawning in the Yuba River suggest that steelhead/rainbow trout in the Yuba River do not preferentially select mesohabitat transitions. For example, the GIS overlay on the following page shows that steelhead/rainbow trout did not preferentially select pool-riffle transitions (blue dots are steelhead/rainbow trout redds).



YCWA 59. *In the Discussion section (pg. 66), the USFWS draft report suggests that steelhead spawn in deeper areas to avoid dewatering, relative to Chinook salmon that spawn earlier before the more variable flow conditions occur. This assessment appears more as an explanation for the results of the USFWS methods, and it ignores the facts that over 75% of the observed spawning occurred in shallow water and that only relatively few deep sites were used. The USFWS draft report should be edited to discuss the following issues:*

- *Steelhead routinely spawn in flashy, small streams and tributaries where observed depth of spawning is much shallower than 5 feet*
- *The majority of observed spawning in the lower Yuba River by USFWS was within the range of depths observed by the investigators referenced in earlier comments*
- *The dependence of steelhead spawning depth HSI development on spawning in deep pools, given the observed relative scarcity of pools in the specific spawning reaches sampled*

Response: We have modified the text in question to note that steelhead may select spawning sites in deeper waters to reduce both redd dewatering and redd scour. The HSI data support that steelhead/rainbow trout preferentially select deeper areas (with depths greater than 2 feet) where there would be reduced risk of redd dewatering – specifically, only 18 percent of steelhead/rainbow trout redds were found in depths of less than or equal to 2 feet, while 71 and 82 percent, respectively, of spring-run and fall-run Chinook salmon redds were found in depths of less than or equal to 2 feet. Reducing the probability of redd scour associated with increased

flows would tend to select for redds in deeper water (greater than 5 feet), versus redds in 2 to 5 feet of water, due to the lower near-bottom velocity of deeper conditions with the same mean column velocity, versus shallower conditions. This assessment is consistent with the use data, where over 75% of the observed spawning occurred in shallow water, since spawning was largely restricted to shallow water due to the low availability of deeper water conditions. We have reviewed the discussion text to verify that the discussion adequately addresses these issues. Specifically, the fact that steelhead routinely spawn in flashy, small streams and tributaries where observed depth of spawning is much shallower than 5 feet is not relevant to the Yuba River, since steelhead in such streams are restricted to spawning in shallow water due to the lack of deeper conditions in those streams with suitable velocities and substrates. We have reviewed the adequacy of the report's discussion comparing the Yuba River steelhead/rainbow trout criteria to those developed in other streams. The steelhead spawning depth HSI development was not dependent on spawning in deep pools (see response to PG&E comment 8). Specifically, the shape of the logistic regression for depth for steelhead/rainbow trout was not driven by the deepest observations (those in depths greater than 5.8 feet), but were rather driven by the relative number of occupied and unoccupied values with depths of 2 to 5 feet versus 5 to 5.7 feet. It is not accurate to describe the steelhead/rainbow trout spawning as being in deep pools, since there were equal numbers of redds in pools and runs for redds with depths of 5 to 5.7 feet. Pools were not scarce in the specific spawning reaches sampled – nine percent of the area of the spawning sites was pool habitat, and 37 percent of the redds observed in pools were located in the study sites.

YCWA 60. *The broader range of velocities and substrate size composition used by fall-run Chinook salmon is explained in the USFWS draft report (pg. 66) as being due to the larger number of fall-run Chinook salmon spawners, relative to steelhead/rainbow trout. This statement suggests that with more fish, a broader range of conditions will be used. There is no discussion whether such a response is a problem or not, or how such a concept would influence the determination of spawning habitat suitability. Based on investigation of differences in conditions used by spawning salmonids, size of the spawner may influence the range of conditions used. Larger fish, such as fall-run Chinook salmon versus steelhead, can move larger substrate material and sustain higher velocities.*

Response: We agree that the above statement from the USFWS report suggests that with more fish, a broader range of conditions will be used. We do not know, nor are we aware of anything in the literature, whether such a response is a problem or not, or how such a concept would influence the determination of spawning habitat suitability. Accordingly, we have not added anything to the discussion on this topic. The report states that fall-run Chinook salmon select larger substrates than steelhead/rainbow trout because they can move larger substrate material. The criteria do not support an observation that fall-run Chinook salmon select higher velocities than steelhead/rainbow trout, since the fall-run Chinook salmon have higher suitabilities than steelhead for both high and low velocities. It is likely that the effect of fish size on velocity selection is counteracted by the greater depths at which steelhead/rainbow trout were spawning, where they were able to select lower near-bottom velocities with high mean column velocities.

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Appendix C
Response-to-Comments Document

for the

**March 2008 Peer-Review Draft of the
Yuba River Spawning Instream Flow Study Report**

August 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 March 2008 draft of the report, “Flow-habitat relationships for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning 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.

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 spawning 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 March 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. The four anonymous reviewers were provided by the CALFED Ecosystem Restoration Program.

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LIST OF ACRONYMS

1D	One dimensional
2D	Two dimensional
ADCP	Acoustic Doppler Current Profiler
BB_ADCP	Broad Band Acoustic Doppler Current Profiler
cfs	cubic feet per second
CVPIA	Central Valley Project Improvement Act
FC	Fall Chinook
HECRAS	Hydrologic Engineering Center River Analysis System
HSC	Habitat Suitability Criteria
HSI	Habitat Suitability Index
m	Meter
Max F	Maximum Froude Number
NA	Not Applicable
Net Q	Net Flow
PDF	Portable Document Format
PHABSIM	Physical Habitat Simulation Model
R	ADCP velocity quality control check statistic
R	Pearson's correlation coefficient
r^2	Coefficient of determination
RIVER2D	Two dimensional depth averaged model of river hydrodynamics and fish habitat
RTK GPS	Real Time Kinematic Global Positioning System receiver
Sol Δ	Solution change
VAF	Velocity Adjustment Factor
VEL	Velocity
WSEL	Water Surface Elevation
WUA	Weighted Useable Area

GENERAL COMMENTS

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) and Figure 1 summarize 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?

Comment 2: The methods are probably sound but the justification for choice of method is extraordinarily weak. In the two spawning reports and one rearing report, three methods are mentioned as being available. The disadvantages, but not advantages, of two methods are described and then the third method is selected with no justification or discussion of its advantages or disadvantages as compared to the other methods. This gives the reader no justification for or confidence in the chosen method. All methods have strengths and weaknesses and the choice of method usually depends on how well a method meets the measurable goals.

Response: We believe that a consideration of the disadvantages of biological response correlations and demonstration flow assessment is a sufficient reason to not use these methods. Habitat modeling is then left as the only available method to use. We changed the description of the three methods to focus on the physical infeasibility of implementing two methods and then briefly discussing the advantages and disadvantages of the method we elected to use (i.e., habitat modeling).

Data - Are the data adequate?

Comment 3: The data are what they are. In some cases they had larger sample sizes than in others.

Response: Sample sizes ranged from 32 for the number of steelhead redds used in the bioverification to 5,117 for the number of data points used to develop the topography for the Timbuctoo site.

Comment 4: 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 5: 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, most understandable to 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.

Figures and tables - Are the figures and tables clear, complete and adequate?

Comment 6: Figures in text and appendices in each report that illustrate habitat suitability lack a scale, flow direction indicators and north arrow. These are standards that are well known and I see no reason for this information to have been omitted.

Response: For the Yuba spawning report, there weren't any figures in the text that illustrate habitat suitability. We added a scale, flow direction indicator and north arrow to the figures in the appendices that illustrate habitat suitability.

Comment 7: 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 Figures 4 to 51.

Comment 8: In many places it would be easier for the reader to do comparisons if the Y axes were scaled the same. Examples include Figures 3, 4, and 6 in Yuba Spawning.

Response: We modified Figures 3, 4 and 6 to have the Y axes scaled the same.

Comment 9: 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 Tables 2 and 5 so that all of the data in those tables 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 10: 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 salmonid habitat and flow. The report does not suggest flows, but instead notes how changes in flow could increase the amount of salmonid spawning habitat. It should be noted that 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 11: 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. To say that the predicted use of steelhead/rainbow habitat is significantly greater than non-use when the suitable factors for use and non-use are 0.05 and 0.004 seems to ignore that the model indicates steelhead are selecting habitat that has a suitability factor of 0.05! 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.

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 velocity validation the numeric criterion was that the correlation between observed and simulated velocities was greater than 0.6. 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, a greater suitability for occupied versus unoccupied locations has the biological significance that fish are preferentially selecting locations with higher suitability. We have changed the habitat suitability criteria used for steelhead/rainbow trout spawning based on the results of our sensitivity analysis, so that now the median suitability of occupied and unoccupied locations are 0.245 and 0.0004. Thus, the model now indicates that steelhead are selecting habitat that has a suitability of 0.245. We attribute this low value, relative to an optimal suitability of one, to the scarcity of high quality habitat for steelhead spawning in the Yuba River. 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-habitat relationship) – 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 12: To use the authors' terminology, in general their results show a much greater range of substrate, depth and velocity as suitable habitat (rearing and spawning) than previous studies which allow for much more latitude in flow operations. However, I would be hesitant to suggest changing operations based on their rather unique values for suitable depths and velocities compared to other studies on habitat use by Chinook and steelhead without further corroboration.

Response: Our studies are intended to provide some of the scientific information that is needed to determine instream flow needs for anadromous fish in the Yuba River. We do not agree with the characterization of our values as unique, since we have used the same methods for other studies on habitat use by Chinook and steelhead. The depth suitability correction methodology, which resulted in a wider range of suitable depths for Chinook spawning, has been published in a peer-reviewed journal article (Gard 1998) and we have applied it on six streams (Merced River, American River, Sacramento River, Butte Creek, Yuba River and Clear Creek). The velocity suitability methodology is based on methods presented in multiple peer-reviewed journal articles (Knapp and Preisler 1999, Parasiewicz 1999, Geist et al. 2000, Guay et al. 2000, Tiffan et al. 2002, McHugh and Budy 2004).

Comment 13: Figure 8 in the Clear Creek Spawning report seems to show that unoccupied habitat suitability is higher than occupied habitat suitability at all suitability levels. A comparable graph is not shown in the Yuba Spawning report.

Response: The same information is presented in Figures 18 to 23 in the Yuba Spawning report. The only difference in the Yuba Spawning report is that occupied and unoccupied data are presented in separate graphs, instead of being combined into one graph, as was done in the Clear

Creek spawning report. Figures 18 to 23 show more unoccupied locations at all suitability levels than occupied locations because the overall number of unoccupied locations was much greater than the number of occupied locations.

Comment 14: 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 statistic (R) to provide a quality control check of the velocity measured by the ADCP at a given station n, where $R = \text{Vel}_n / (\text{Vel}_{n-1} + \text{Vel}_{n+1}) / 2$ at station n; 2) the beta value (a measure of the change in channel roughness with changes in streamflow) is between 2.0 and 4.5; 3) the mean error in calculated versus given discharges is less than 10%; 4) there is no more than a 25% difference for any calculated versus given discharge; 5) there is no more than a 0.1 foot (0.031 m) difference between measured and simulated WSELs; 6) VAF values falling within the range of 0.2 to 5.0; 7) a monotonic increase of VAFs with an increase in flows; 8) a QI value of at least 0.2; 9) the WSELs predicted by RIVER2D at the upstream transect were within 0.1 foot (0.031 m) of the WSEL predicted by PHABSIM; 10) a solution change (Sol Δ) of less than 0.0000; 11) a net flow (Net Q) of less than 1%; 12) a maximum Froude Number (Max F) of less than one; 13) the correlation between measured and simulated velocities was greater than 0.6; and 14) a p-value of less than 0.05 for a Mann-Whitney U test of whether the compound suitability predicted by the RIVER2D model is higher at locations where redds were present versus locations where redds were absent. 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 Appendices C, D, F, G, and I. We do not have any data on previous models that could be used to determine if the modeling in this report performs better than the previous models.

REVIEWER #2

Study Design Is the study design sound?

Comment 1: The study design is sound insofar as it sets out to establish improved spawning habitat suitability for fall-run Chinook salmon and steelhead/rainbow trout on the Yuba, uses robust methods to achieve this, and reports the results thoroughly. One might argue that the study design is not ambitious scientifically. The study design makes little attempt to identify any really compelling and interesting unanswered scientific questions, nor does it pose any hypotheses that it then sets out to test. However, this critical observation should not be construed to mean that the study design is not sound relative to its objectives, nor that the results will not be useful from a management perspective. It is simply to highlight that this study is not very original scientifically, other than the fact that it applies a well established technique in a river system that it has not been applied to before. The study design is therefore appropriate as a technical report, but unlikely to be publishable in the peer-reviewed literature in its current form.

Response: The intent of the study was to apply well established techniques to the Yuba River to quantify flow-habitat relationships for anadromous salmonid spawning. The study was intended to be a technical report, and not to be published in the peer-reviewed literature.

Methods - Are the methods technically sound?

Comment 2: The methods are perfectly reasonable and represent a significant improvement over 1D PHABSIM-style implementations of IFIM. The authors (and others) have been using such techniques for at least the past 12 years and they represent the robust end of the standard of practice, but are not the state-of-the-art. The authors are a little sloppy in places about their summary of other techniques and justification of the techniques they used. However, they do generally describe very clearly what they did. As these methods have been reviewed thoroughly by past reviewers (see executive summary) I have provided only limited comments here. My only main major methodological concern is the relatively poor topographic data quality (see below).

Response: See responses below regarding the summary of other techniques and the topographic data quality.

Data - Is the data adequate?

Comment 3: The authors should be commended for so thoroughly reporting all their data. Other investigators doing work in the Yuba basin or nearby streams will likely find both the raw data and the summary curves of great utility. I would suggest that some of the raw data is made available in a digital format when the report is published online.

Response: We have added information in the preface of the report about how the raw data in digital format can be obtained.

Comment 4: I am a little bit confused as to why such low topographic point densities were used and transect based methods. There is nothing you can do about it at this stage, but they do seem low. I expect that the poor topographic resolution in large part can explain the generally marginal agreement between the measured and modeled velocities and depths in Appendix H.

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 m², whereas Jacobson and Galat (2006) had a point density of 6 points/100 m². This study was one of our earlier River2D studies; 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 m²) within our time constraints for data collection. This higher point density has had a moderate effect on the accuracy of the hydraulic models, increasing the average correlation between measured and simulated velocities from 0.74 in this study to 0.80 in our lower Clear

Creek spawning study. As a result, it does not appear that the lower topographic point densities used in this study had a large part in explaining the differences between measured and modeled velocities.

Presentation - Is the presentation clear?

Comment 5: 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).

Response: 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.

Figures and tables - Are the figures and tables clear, complete and adequate?

Comment 6: The figures are adequate, but overall are of poor cartographic quality, exhibit inconsistent font sizes, and lack some basic information. Moreover, the downsampling of the figures for the PDF has resulted in some very poor text and image quality.

Response: The figures are intended to sufficiently support a technical report. As discussed in responses to the following comments, we have added some basic information to the figures. We have not been able to improve the text and image quality in the PDF conversion process.

Comment 7: Figure 1 – Image quality is poor. I would not call this a conceptual model. It is a flow chart of the data collection and modeling effort... there is nothing ‘conceptual’ about it. It is a useful outline of the methods.

Response: We were not able to improve the image quality of this figure. We have changed the caption to “Flow diagram of data collection and modeling” to address the remainder of this comment.

Comment 8: Figure 2 – Image quality is poor. There is no additional context provided with the segment vicinity map (e.g. roads, hillshade of topography, locations of towns, etc.). If context is added, be careful not to make it too busy. The north arrow and scale bar are ridiculously large. It is not clear (although it is obvious) whether the scale bar applies to the vicinity map or the state location map. The California map is very poor quality and the dot apparently indicating the location of the Yuba River could easily be confused with just a poor image quality.

Response: We were not able to improve the image quality of this figure. We intentionally did not include a lot of detail to focus on the stream segments. We reduced the size of the north arrow and the width and size of the text for the scale bar. The scale bar applies to the vicinity map. We have moved the scale bar to make this more clear. We were unable to improve the quality of the California map. Although the California map has poor image quality, the dot indicating the location of the Yuba River is clearly within the map's outline of the state boundary.

Comment 9: Figure 9 - 14– I like how you've shown occupied frequency distributions versus unoccupied frequency distributions... clever.

Response: No response required.

Comment 10: Appendices – Why don't any of the appendices show captions? In my opinion, they should all have a caption such that if they are the only thing taken out of the report or printed, that it stands alone on its own (more than just a title).

Response: Addition of captions to the appendices would require reducing the size of the images in the appendices. The information provided on each page (the appendix in question, the name of the site and the footer with the report title, date and page number) is intended to provide sufficient information if a page of an appendix was taken out of the report or printed.

Comment 11: Appendices A,B, E, H (pgs 220-224), L – Every one of these map figures is missing some essential basics: a) north arrow, b) scale bar, c) flow direction arrow; d) inset location map within broader reach.

Response: We have added a north arrow, scale and flow direction to each of these map figures. No inset map was added since this information is already given in Figure 2.

Comment 12: Appendix L – This should denote on the figure what year the redd surveys correspond to! I think the legend should read 'Spawning Habitat Suitability' instead of just 'combined suitability' and should indicate somewhere (either in legend or caption), that a higher value is better! Also what discharge do these correspond to?

Response: We added the year and discharge to the figure captions. We retained combined suitability in the legend to be consistent with what is used in River2D. We added a footnote to the first page of Appendix L to indicate that a higher value is better, with the following text: "For all pages, Combined Suitability: 1 = optimal, 0 = unusable."

Comment 13: Overall, this is a useful study reflecting an impressive amount of work. The report is not the most interesting report to read as it does not meaningfully explore any science questions or hypothesis testing, and instead is focused on the nuts and bolts of producing spawning habitat suitability criteria for the Yuba. I appreciate the data-dump aspect of the report (including the appendix), but the report fails to ever address convincingly the basic question of 'so what?'. Why should the reader care? Why should the reader keep reading for 82 pages and

then not even have a conclusion. The report could be strengthened considerably by the authors doing a more thorough literature review and then using this to build an actual narrative (currently lacking) and substantiating the introduction, choice of methods and conjecture in the discussion. The discussion is a necessary component and the authors have taken a good stab at this, but it currently is one of the weakest parts of the report. A conclusion is essential. The figures are not used in a convincing fashion and some basic information is missing from a large number of these (see above). Finally, I think this report needs to minimally be tied to the other reports in an explicit way (i.e. citations and explanations) and ideally is physically combined with the others.

Response: Due to the intended purpose of the report, the report necessarily focuses on the nuts and bolts of producing spawning flow-habitat relationships for the Yuba River. The basic questions of ‘so what’ and ‘why should the reader care’ are addressed in the preface, namely that the purpose of the report is to provide scientific information to the CVPIA Program to use in developing recommendations for instream flow needs for anadromous fish in the Yuba River. We have added a conclusion to the report. To the extent possible, we have added references, improved the narrative in the report and added material to the discussion. We have added basic information to a large number of the figures to improve their utility. 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.

REVIEWER #3

Study Design Is the study design sound?

Comment 1: For the most part, the study design is sound. However, there are problems with execution of that design that limit the reliability of the conclusions.

Response: Errors in model performance likely increase the level of uncertainty in the conclusions.

Methods Are the methods technically sound?

Comment 2: No. The authors assumed that physical habitat (for spawning in this case) was the limiting factor for salmonids populations in the Yuba. Without addressing other factors that may be limiting the population (like ocean conditions, harvest, etc...), this assumption is difficult to test. The authors also use the term ‘reproductive success’ which typically refers to a pair of fish or a female; perhaps they meant population productivity...? The authors also selected areas to sample based on heaviest spawning use and then applied those data throughout the watershed. Similar to the other reports, the authors spent a large amount of text defending their use of River2D. This is unnecessary. I found it frustrating that the level of detail on methods (and especially study area description) varied greatly. Way too much detail in some areas – and not enough (or none) in other areas. This makes it very difficult to read for someone not intimately familiar with the Yuba and past work there. How did discharge during the fall of 2001 compare to the 10-year average? Is it possible that drought flows influenced habitat data collected at FC

redds? The Baldwin citation for accepting 0.6 r^2 is very weak. (See similar comment on Yuba Rear report.) Was there any confirmation/validation of the visual assessment of substrate size? Were all observers consistent in estimation of substrate size? The 'biological verification' is not very informative or 'biological'.

Response: An assumption that physical habitat is the limiting factor is necessarily true of all instream flow studies. To our knowledge, the data needed to test this assumption for the Yuba River do not exist. For example, information is lacking to be able to determine if doubling the amount of spawning habitat would double the salmonid populations. In the context of the conceptual model, reproductive success refers to a pair of fish – namely what proportion of the offspring of the fish will survive to emergence as a result of redd superposition. Our approach is to represent high-spawning-use habitats in the Yuba River, rather than the entire Yuba River. Based on the conceptual model presented in the introduction of the report, spawning habitat can affect salmonid populations by changing the amount of redd superimposition. Redd superimposition would be expected almost entirely in high-spawning-use areas. Thus, the amount of habitat present in areas without high spawning use would not be expected to affect salmonid populations. In addition, modeling of habitat in high-use-spawning areas captures characteristics of spawning habitat, such as permeability and upwelling, which are key characteristics of spawning habitat and are not captured by depth, velocity and substrate. In contrast, a habitat-based extrapolation does not take into account characteristics of spawning habitat, such as permeability and upwelling, and would greatly overestimate the amount of available spawning habitat. We felt that it is necessary to include text comparing River2D to PHABSIM, given that we are comparing the results of this study to an earlier study that used PHABSIM. We have increased the level of detail on methods in our reports over the years in response to peer review comments, and have added further details to the introduction on the study area description. Discharges during the fall of 2001, averaging 787 cfs above Daguerre Point Dam and 436 cfs below Daguerre Point Dam, are less than the average October and November flows for the period of record of 1,080 cfs above Daguerre Point Dam (1941-2008) and 1,125 cfs below Daguerre Point Dam (1943-2008). Water Year 2002, which includes the fall of 2001, was classified as a dry water year. We think it is unlikely that drought flows influenced the habitat suitability criteria, since the method used for developing the habitat suitability criteria corrects for availability; one would expect limited availability of deeper and faster conditions during drought flows. While drought conditions likely did influence the depths and velocities of the occupied data, it also influenced the depths and velocities of the unoccupied data. Since both the occupied and unoccupied data were used to develop the habitat suitability criteria, the resulting criteria corrected for the effects of availability. We were not able to find another reference for definitions of what ranges of Pearson's correlation coefficient are considered moderately strong or very strong. Statistics textbooks that we reviewed do not give numeric definitions of what are considered moderately strong or very strong correlations. We also were not able to find numeric definitions of what are considered moderately strong or very strong correlations in the peer-reviewed literature. We replaced the Baldwin (1997) reference with Cohen (1992), which defines correlations of 0.5 to 1.0 as having a strong effect. Although we did not confirm or validate the visual assessment substrate size, we have been using visual assessment of substrate sizes for the last 14 years and have found high consistency among

trained observers. The biological verification is based on the location of redds (the key biological measure for spawning habitat studies) and is based on the biological significance that fish are preferentially selecting locations with higher suitability.

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

Comment 3: I do not think the findings are valid. I am not convinced that measurement error had a minimal effect on the final results. Something did not work correctly, as evidenced by the low correlation coefficients for predicted vs. measured flows, differences in WSELs between the model and measured, and odd looking HSC and WUA curves for steelhead in particular. The authors dismissed the problem of WSEL discrepancies – and compared models to try to figure out what reality really was. This approach seems like flawed logic and shows that the emphasis was on models and not on the habitat that fish were using.

Response: We acknowledge that there is a level of uncertainty in the results because of measurement error. We disagree that the correlation coefficients for predicted versus measured velocities were low, since a correlation of 0.5 to 1.0 is considered to have a large effect (Cohen 1992). We do not consider the differences between measured WSELs and those simulated by PHABSIM to be significant, since all of the criteria for PHABSIM were met. While the HSC and WUA curves for steelhead may look odd on the surface, we have sufficiently documented that these curves accurately reflect the habitat use of spawning steelhead, as affected by habitat availability. Since we did not have measured WSELs at the highest simulation flow, we had to compare River2D and PHABSIM outputs. Where there were differences between the two models, the next logical step is to try to determine which model is correct. We feel that this approach is logical based on the transitive property – since the PHABSIM output was based on measured WSELs, a comparison of River2D and PHABSIM outputs for calibration of River2D is equivalent to calibration of River2D by measured WSELs by the transitive property. Models are necessary to quantify flow-habitat relationships, based on the habitat the fish are using, because of the limitations of the alternatives to modeling (e.g., biological response correlations and demonstration flow assessment).

Presentation. Is the presentation clear?

Comment 4: In some areas the presentation was clear. Overall, the level of detail was way overboard in some areas and lacking in others.

Response: The level of detail reflects peer reviews of past reports. We have added additional detail in response to the peer review of this report.

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

Comment 5: See comments above.

Response: See responses above and below.

Miscellaneous comments:

Comment 6: See comments above.

Response: See responses above and below.

REVIEWER #4

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 required.

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. With regards to methods being combined with results and results being combined with discussion, 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 the 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.

Study Design. Is the study design sound?

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 link with 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 clearly connected to the objective of developing habitat-discharge models, since habitat-discharge 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 became one of producing habitat-discharge models because habitat-discharge models are the standard method 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: The need 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. While the representation of riverbed elevations could have been better, 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 m², while Jacobson and Galat (2006) had a point density of 6 points/100 m². This study was one of our earlier River2D studies; 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 m²) within our time constraints for data collection. This higher point density has had a moderate effect on the accuracy of the hydraulic models, increasing the average correlation between measured and simulated velocities from 0.74 in this study to 0.80 in our lower Clear Creek spawning study. To the extent possible, we have added information to the report on the accuracy of the elevation data, relative to the benchmarks and the survey data themselves. The correlation between measured and simulated

velocities would be considered to have a strong effect (Cohen 1992). The high Froude Numbers predicted along the channel margins need to be viewed within the context of what effect they would have on the overall flow-habitat relationships. Specifically, the Froude Number only exceeded one at a few nodes, with the vast majority of the site having Froude Numbers less than one. Furthermore, these nodes were located either at the water's edge or where water depth was extremely shallow, typically approaching zero. A high Froude Number at a very limited number of nodes at water's edge or in very shallow depths would be expected to have an insignificant effect on the model results because these conditions do not coincide with suitable spawning habitat.

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 m², whereas Jacobson and Galat (2006) had a point density of 6 points/100 m². 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. 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-habitat relationships. We did not apply a constant friction coefficient across the model domain – in fact, we applied a roughness that varied spatially based on substrate size and cover. Correlations between measured and simulated velocities would be considered to have a large effect (Cohen 1992). Differences between measured and simulated velocities reflect both errors in measurements of velocity and errors in simulations of velocity. Further, the performance of the model should be viewed in context of the effect of the model performance on the overall flow-habitat relationships. Specifically, the overall flow-habitat 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 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, is 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 performs¹, indicating 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 (U.S. Fish and Wildlife Service 1994, Steffler 2002, Waddle and Steffler 2002, Steffler and Blackburn 2002).

Data. Is the data adequate?

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 m², while Jacobson and Galat (2006) had a point density of 6 points/100 m². 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

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 this report (1% net outflow), we did not feel it was appropriate to use Jacobson and Galat (2006) as a reference.

and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 m²) within our time constraints for data collection. This higher point density has had a moderate effect on the accuracy of the hydraulic models, increasing the average correlation between measured and simulated velocities from 0.74 in this study to 0.80 in our lower Clear Creek spawning study. As a result, it does not appear that the lower topographic point densities used in this study had a large part in explaining the differences between measured and modeled velocities. To the extent possible, we have added data to the report on survey errors – this information indicates that survey errors were negligible. 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. The effects of these hydraulic modeling errors on the modeling results are expected to be minimal because the overall flow-habitat 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.

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: To the extent possible, we have added material to the discussion to compare our results to those of others. The discussion addresses the resulting WUA estimates and associated methodological issues relative to the work of others on the Yuba River. 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.

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: The format of the report follows as closely as possible to that of peer-reviewed journal articles and reflects responses to comment made by other peer reviewers to improve organization and clarity.

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: We have added scale information to the maps in the appendices.

SPECIFIC COMMENTS

ABSTRACT

REVIEWER #2²

Comment 1: iii, 2 (abstract), The abstract reads more like a summary of the methods instead of a summary of a study. The results and discussion could be summarized better.

Response: We revised the abstract to reduce the amount of details on methods and to add summaries of the results and discussion.

INTRODUCTION

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 produce a model that predicted spawning habitat for spring and fall-run Chinook salmon and steelhead within, to the extent feasible, the levels of accuracy specified in the methods section. The above measureable component of the goal provides addresses 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-habitat relationship with a high level of uncertainty would not be a failure, in terms of making it unusable, but rather should be

² For Reviewer #2, the format of the comments is Page, Paragraph, Comment.

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.

Comment 2: The verbal paragraph on conceptual model linking habitat to population in the two spawning and one rearing looks like an afterthought and is weak. A better option is to present a figure that shows linkages and feedbacks diagrammatically and to cite literature that supports the assumed linkages. After all, if there is not good documentation that spawning habitat is limiting and that increasing the availability of such habitat will indeed increase salmon populations, what is the point?

Response: We have added a figure that shows linkages and one relevant feedback diagrammatically and have added literature citations (Bartholow 1996, Bartholow et al. 1993, Williamson et al. 1993) that support the assumed linkages. We have retained the verbal paragraph as well to provide multiple techniques of presenting the conceptual model. To our knowledge, the data needed to evaluate whether spawning habitat is limiting and that increasing the availability of such habitat will indeed increase salmon populations does not exist. The point of this report is to provide scientific information to assist in developing instream flow needs for the Yuba River. We agree that an evaluation of limiting factors and determining if increased habitat would increase salmon populations is needed prior to implementing a revised flow regime on the Yuba River.

Comment 3: Figure 1 in Rearing report and in Yuba Spawning report looks more like a flow diagram for the modeling process than a conceptual model.

Response: We have changed the caption for Figure 1 to say flow diagram rather than conceptual model.

REVIEWER #2

Objectives - Are the objectives clear?

Comment 1: The study tasks and objectives are clearly laid out in Table 1 in the introduction. The objectives read rather transparently as a list of methods. There is little context about an overall aim of the study. This is something past reviewers picked up on, and was apparently improved upon. I'd say there is still room for improvement.

Response: We added this information in response to a stakeholder comment that we specify task objectives. Although the first 9 objectives are methods, within the scope of the entire report, for each specific task identified in Table 1, the associated objective is truly an objective of that task. We have not made any changes in Table 1 to be responsive to the stakeholder comment. The

preface is intended to provide context about the overall aim of the study, namely to provide scientific information to the CVPIA Program to use in developing recommendations for instream flow needs for anadromous fish in the Yuba River.

Comment 2: 1, 3, This conceptual model is very poor. It asserts that velocities, depths and substrate are the only things influencing spawning habitat utilization! There are numerous conceptual models of spawning habitat for salmonids in the literature which highlight these local hydraulics as only one of many important factors (e.g. Escobar-Arias and Pasternack, 2009; Wheaton *et al.*, 2004). Habitat suitability modeling based on just these characteristics is still reasonable and commonly done, as these factors alone often explain much of the variability in observations of spawning (Knapp and Preisler, 1999). Don't just gloss over the fact that other things matter. Acknowledge other factors and then cite the literature to justify your approach.

Response: We have added a literature citation (Williamson et al 1993) that presents a conceptual model that support the assumed linkages. We addressed other factors that influence spawning habitat utilization, such as gravel permeability and upwelling/downwelling, by only placing sites in high-spawning use areas, under the assumption that salmon will select areas with suitable characteristics for these other factors. We focus on depth, velocity and substrate since depth and velocity are two key linkages between flow and spawning habitat, and since substrate distribution is critical to spawning habitat utilization. We acknowledge these other factors in the habitat simulation discussion and added citations to the literature (Bartholow 1996, Bartholow et al 1993, Williamson et al 1993) to justify our approach.

Comment 3: 2, 2, Instead of 'Disadvantages of *this* approach' use 'Disadvantages of *biological response correlations*'

Response: We have made the suggested change.

Comment 4: 5, 2, I don't know that I agree that the literature establishes 'logistic regressions should be used to develop habitat suitability criteria.'

Response: We have changed the statement to say "are appropriate to use" rather than "should be used." We rely on the following citation given in the report, from McHugh and Budy (2004), which supports the conclusion given that the literature establishes logistic regressions should be used to develop habitat suitability criteria:

"More recently, and based on the early recommendations of Thielke (1985), many researchers have adopted a multivariate logistic regression approach to habitat suitability modeling (Knapp and Preisler 1999; Geist et al. 2000; Guay et al. 2000)."

Additional references include Knapp and Preisler (1999), Parasiewicz (1999), Tiffan et al. (2002).

REVIEWER #3

Objectives Are the objectives clear?

Comment 1: The first 9 of 10 objectives are really methods. The last one is an objective.

Response: We added this information in response to a stakeholder comment that we specify task objectives. Although the first 9 objectives are methods, within the scope of the entire report, for each specific task identified in Table 1, the associated objective is truly an objective of that task. We have not made any changes in Table 1 to be responsive to the stakeholder comment.

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 background information about the Yuba River, including salmonid population declines, and the need for the study to the introductory paragraph. Information on why the Yuba River was selected for a study was added to the subsequent new paragraph (discussed in the response to Comment 2).

Comment 2: This should be the start of a new paragraph. This paragraph needs to be rewritten to establish a link with the need for an instream flow study, which should be introduced in a rewritten first paragraph. As is, there is no reason established for conducting an instream flow study.

Response: We have changed the introduction to start a new paragraph at this location. 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: The connection between the proposal and study objective is not clear and not very robust. To "identify the instream flow requirements" one would collect empirical data for the species of concern -- it's not clear how or why the study objective became one of producing habitat-discharge models. The objective of the study needs to be more clearly articulated, with a clear connection to the need(s) described in the previous (to be rewritten) paragraph(s).

Response: Developing flow-habitat relationships for use in habitat-discharge models is a standard method for identifying instream flow requirements. In developing the flow-habitat relationships, we collected empirical data for the species of concern in the lower Yuba River. Therefore, the needs described in the previous paragraph (i.e., 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) and the development of habitat-discharge models are connected,

since the habitat-discharge models are intended to provide critical information to use in determining the magnitude of improved flows for all life history stages of Chinook salmon and steelhead.

Comment 4: The focus on spawning habitat is unfounded, because spawning habitat capacity has not been established as being a limited factor contributing to the fish population declines described in the earlier narrative. Some coherent explanation needs to be provided that justifies the focus on spawning habitat.

Response: To our knowledge, the data needed to evaluate whether spawning habitat is limiting and that spawning habitat is 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 population of anadromous fish. The preface explains that this study is intended to provide scientific information to assist in developing instream flow needs for the Yuba River, including those for spawning habitat.

Comment 5: It is not clear why the topic sentence of this paragraph concerns developing flow regimes, when the first paragraph suggests that these flow regimes have already been developed.

Response: This paragraph is meant to provide background information on how flow regimes are developed. The purpose of this study is to determine whether the flow regimes that have been developed accommodate the habitat needs of anadromous species, as stated in the topic sentence of this paragraph.

Comment 6: The previous narrative identified the focus on spawning habitat, while here there is mention of all life stages, and the next paragraph goes back to spawning habitat. This is distracting to the reader, and more clarity should be provided by limiting the narrative to the spawning life stage.

Response: We mention all life stages to put this report into the context of the entire Yuba River study, which addresses all life stages.

Comment 7: Should quantify the spatial scale that defines macro- and micro-habitat.

Response: We have added to the report quantification of the spatial scales of macro and micro-habitat.

Comment 8: This paragraph needs a topic sentence introducing the need/explanation/content of a conceptual model. The conceptual model itself is a rather weak description of the link between spawning habitat and population change -- especially given that spawning habitat has not yet been identified as a contributing factor in population declines. As such, the reader is not convinced that there are meaningful relationships in this conceptual model.

Response: We have added a topic sentence to introduce the conceptual model. Also, we have added references that improve the description of the link between spawning habitat and population change, and that provide support for the relationships in the conceptual model. An evaluation of whether spawning habitat is a contributing factor in population declines is outside of the scope of this study. The intent of this 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 9: The term "evaluate" is very ambiguous and this narrative (here and elsewhere) should be rewritten to be more specific -- e.g., quantify habitat availability, ...or quality, ...functional relationship to discharge, ...???...

Response: We have changed "evaluate" to "quantify the functional relationship between flow and spawning habitat availability."

Comment 10: After the objective of the work is more clearly defined, there will likely be additional alternative techniques that could be used, and should be discussed as to why they were not applied in this study.

Response: We are not aware of any additional alternative techniques that could be used to quantify the functional relationship between flow and spawning habitat availability other than those already discussed, i.e. biological response correlations (e.g. redd surveys), demonstration flow assessments and habitat modeling.

Comment 11: This entire paragraph belongs in the Methods section. The paragraph needs a topic sentence, as the reader has no idea what the "upstream and downstream transects" are because they haven't been introduced yet. Similarly, the reader has no idea of the data sources referred to in Figure 1, as these have not yet been introduced.

Response: We moved this paragraph to the Methods section. It now comes after a paragraph that discusses the upstream and downstream ends of the sites, and we modified the topic sentence to start "Transects at the upstream and downstream ends of the site...". We added Figure 1 before the data source were introduced in response to a comment from the first peer review of this report; the peer reviewer in question wanted an overall description of the data collection process initially to provide context for the individual data sources as they were introduced later in the report. Accordingly, we have kept Figure 1 prior to the introduction of the data sources.

Comment 12: The term calibrate and its derivatives needs to be defined here or elsewhere in the Methods section, as the terms have many different meanings relative to hydraulic and habitat modeling.

Response: Calibrate is defined elsewhere in the Methods section, specifically under *Hydraulic Model Construction and Calibration* **PHABSIM WSEL Calibration**.

Comment 13: This entire paragraph belongs in the Methods section. The paragraph needs a topic sentence.

Response: We have moved this paragraph to the Methods section. The first sentence of the paragraph, starting with “Traditionally, criteria are created from...”, is intended to be the topic sentence, and we have left it unchanged.

Comment 14: This entire paragraph belongs in the Methods section. The paragraph needs a topic sentence.

Response: We have moved this paragraph to the Methods section. The first sentence of the paragraph, starting with “It is well-established in the literature...”, is intended to be the topic sentence, and we have left it unchanged.

Comment 15: The first sentence is too strong, and misleading, for the references cited -- should be rephrased to indicate that logistic regressions are an accepted method for developing habitat suitability criteria, not that they should be used.

Response: We have changed the statement to say “are appropriate to use” rather than “should be used.” We used the following quotation given in the report, from McHugh and Budy (2004), to support the argument that the literature establishes that logistic regressions are appropriate for developing habitat suitability criteria:

“More recently, and based on the early recommendations of Thielke (1985), many researchers have adopted a multivariate logistic regression approach to habitat suitability modeling (Knapp and Preisler 1999; Geist et al. 2000; Guay et al. 2000).”

Because the citation is a direct quote, we have left it to the reader to assess the strength of the argument.

Comment 16: 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 17: 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. We added material to the first paragraph of the introduction to provide the reader with an adequate understanding of where this study occurred. We left the section on *Study Segment Delineation*, which refers to segments and reaches, in the Methods section, since it describes the methods that were used to delineate the study segments.

METHODS

APPROACH

REVIEWER #2

Comment 1: 5, 5, Odd way to start the methods ‘RIVER2D was used... instead of PHABSIM’; Assumes you’re reader knows what River2D is and PHABSIM.

Response: We have added a definition of what River2D is (a two-dimensional hydraulic and habitat model) and have moved the footnote defining PHABSIM to the first sentence of this paragraph.

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 feel that it is important to present this information to set the stage for the comparison in the discussion section of this study to an earlier study using PHABSIM. The same language was used in a recent peer-reviewed journal article (Gard 2009).

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: The description of the alternative modeling techniques, etc. was used in a recent peer-reviewed journal article (Gard 2009). We attempted to include numerous citations to the peer-reviewed literature from the hydraulic engineering field (e.g., Gard 2009, Leclerc et al. 1995, Ghanem et al. 1996, Crowder and Diplas 2000, and Pasternack et al. 2004).

STUDY SEGMENT DELINEATION

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. The material added to the first paragraph of the introduction is intended to provide the reader with a thorough description of the study area.

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 detailed 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. We believe that the flow information given under Results for *Study Segment Delineation* provides sufficient information to make sense of the flow descriptions in the Methods section for *Study Segment Delineation*.

HYDRAULIC AND STRUCTURAL DATA COLLECTION

REVIEWER #1

Comment 1: Each report has a table of substrate codes with 10 categories. This is far more categories that are typically reported and many of the categories have significant overlap. It is not clear from the text how so many categories were visually noted in the field, what the replicability among observers was, and how observers made decisions on which category to record given the large overlaps between categories. As the authors state in the discussion, theirs is a 'unique' system, but no reason is given as to why they would generate a new system with limited comparability to normally used systems.

Response: Each observation (topographic data point or redd) was assigned one of the ten substrate codes. We have been using these substrate categories for the last 14 years and have found high replicability among observers. Observers made decisions on which category to record based on what the dominant size particle range, defined as greater than 50 percent, was at a given location. We have used this system because we have found that it does a better job in capturing the substrate sizes used by adult salmonids for spawning than more traditional substrate classification systems (e.g. modified Wentworth scale [Bain et al. 1995]).

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: We have added as much detail as possible describing the elevation surveying and associated errors. Based on the available information, the errors in the elevation data appear minimal. 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 m², while Jacobson and Galat (2006) had a point density of 6 points/100 m². 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 m²) within our time constraints for data collection. This higher point density has had a moderate effect on the accuracy of the hydraulic models, increasing the average correlation between measured and simulated velocities from 0.74 in this study to 0.80 in our lower Clear Creek spawning study. As a result, it does not appear that the lower topographic point densities used in this study had a large part in causing inaccuracies in hydraulic model results. Furthermore, inaccuracies in hydraulic model results would likely not propagate through the habitat modeling steps and into estimates of WUA. Specifically, the overall flow-habitat 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: In this and related sections, the authors should provide summary reports of the vertical and horizontal benchmark surveys, and the differential leveling surveys that used these benchmarks. These types of error summaries are readily available from the software used to process the data, or can be calculated from the survey data available.

Response: We added tables to the Results section under *Hydraulic and Structural Habitat Data Collection* providing summary reports of the vertical and horizontal benchmark surveys, and added text in this section on the standard we used for vertical benchmark surveys. We do not have any information that can be used to generate summary reports of the differential leveling surveys that tied the elevations of the horizontal benchmarks to the vertical benchmarks because this differential leveling was done with only one backsight and one foresight. Similarly, we do not have any information that can be used to generate summary reports of the differential leveling surveys that used the vertical benchmarks (for determining water surface elevations and dry bed elevations on the transects) because this differential leveling was done with only one backsight on the vertical benchmark.

Comment 3: As this section currently stands, the reader knows about the accuracy of the instruments, but has no information about the accuracy of the topographic surveys themselves.

Response: The topographic surveys relied almost exclusively on a total station, which does not produce data on the accuracy of the topographic surveys, nor can such data be generated from the survey data available.

Comment 4: Surveying errors (in addition to instrument error) from the depth-derived ADCP elevation data should be reported; it is atypical to use ADCP for elevation surveying (more typical to use single-beam or multi-beam echosounders), and some discussion should be provided to justify the use of ADCP for this purpose.

Response: We do not have any information available on surveying errors from the depth-derived ADCP elevation data. With regards to the ADCP depths, we would characterize the accuracy as being 4 percent of the average depth over the area measured by the ADCP. We do not feel that this is a shortcoming of the ADCP data, since the area averaged by the ADCP corresponds to the scale of the mesh elements of the hydraulic model and to the scale of individual redds. In most cases, the ADCP data was collected in areas with a very gradual slope – adjacent depth measurements typically only differed by 0.1 foot. We had some areas where we ended up with ADCP measurements collected in close proximity (typically within 1 foot) to total station measurements – for the most part, the bed elevations from these two methods were very close (typically within 0.1 foot). ADCPs are now commonly used for measuring depths in instream flow studies, and the US Geological Survey, the nation’s preeminent hydrographers, use ADCP depth measurements for measuring discharges (Simpson 2001). In this regard, Simpson (2001, p. 119) states:

“Near the bank edges, the BB-ADCP beams orientated toward shore will show shallow depths, whereas the beams orientated toward the channel will show greater depths. An average of all four beams will approximate the vertical depth from the center of the BB-ADCP transducer assembly to the bottom. In pitch and roll conditions, averaged depth measurements from all four acoustic beams will be more accurate than depths measured by a single, vertically placed, depth sounder because of the large beam ‘footprint’ or pattern.”

We successfully have used an ADCP for bathymetric data collection in the past, as described in Gard and Ballard (2003).

Comment 5: As previously mentioned, provide survey errors for the elevation data described in this paragraph.

Response: We do not have any information that could be used to produce estimates of survey errors for the elevation data referred to by the commenter.

Comment 6: There is a large amount of doubt that the bed elevation points adequately characterized the riverbed topography; the point density (given in a later table) is quite low for a river of this size; in Appendix B, the bed topography point location maps have no scale associated with them, which would allow the reader to ascertain the sampling density.

Response: The topographic point densities used were a function of the equipment we had available at the time and the amount of time we had available to collect topographic data. 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 m², while Jacobson and Galat (2006) had a point density of 6 points/100 m². This study was one of our earlier River2D studies; 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 m²) within our time constraints for data collection.

Comment 7: Visual observations of substrate grain size are very subjective and susceptible to large variability in estimates by different observers – these subjective methods then affect the habitat suitability criteria and WUA; more detail is required, describing how the substrate sizes were determined...one observer?...first grain observed?...mental average of multiple grains?...etc...

Response: Although visual observations of substrate grain size are very subjective, this is the only practical method to collect such data for all topographic survey points. We have been using this technique for the last 14 years and have found minimal variability in estimates by different observers. We changed the sentence in question to read as follows to address the comment: “All substrate and cover data on the transects were assessed by one observer based on the visually-estimated average of multiple grains.”

RIVER2D MODEL CONSTRUCTION

REVIEWER #1

Comment 1: Title information in Table 6 of Yuba Spawning report is very confusing! The roughness values stated in the title do not show up in the table. The text does not help explain it either. This section is not clear to the ‘naïve’ reader. (Same in Table 7 in Yuba Rearing report and Table 3 in Clear Creek Spawning report.) Also, in these tables it is not clear why overhead cover should increase bed roughness.

Response: We added the roughness values in the title to the table and moved the text in question from the title to a footnote to clarify this table. Overhead cover increases bed roughness because overhead cover is defined as any woody cover that starts more than 2 feet above the substrate. Thus, when depths are greater than 2 feet, overhead cover starts to become inundated and thus increases bed roughness.

REVIEWER #2

Comment 1: 16-17, NA, Do you ever report the approximate mesh resolutions you used and total number of nodes per reach?

Response: We added information on mesh resolution and the total number of nodes per reach to the Results section under *Hydraulic Model Construction and Calibration* **RIVER2D Model Construction**.

RIVER2D MODEL CALIBRATION

REVIEWER #4

Comment 1: 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 later sections and Appendix F.

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: 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. 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. Accordingly, it is likely that either method would have generated Froude numbers exceeding one at some locations in the model. 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 2: Given this criteria, the 2D modeling results are problematic and of questionable quality -- as shown in Appendix F, the max Froude numbers were large and exceeded 1.0 at 6 of 10 sites, with two more sites approaching 1.0.

Response: The Froude numbers only exceeded one at a few nodes, with the vast majority of the area within the site having Froude numbers less than one. Furthermore, these nodes were located either at the water's edge or where water depth was extremely shallow, typically approaching zero. A high Froude Number at a very limited number of nodes at water's edge or in very

shallow depths would be expected to have an insignificant effect on the model results because redds typically do not occur in these locations. We have added this explanation to the discussion.

RIVER2D MODEL VELOCITY VALIDATION

REVIEWER #4

Comment 1: The velocity validation results shown in Appendix G are not very robust, and call into question the reliability of the model results. Additional cross-section plots showing vectors of velocity magnitude and direction (for empirical data and model results) should be provided. With the cross-section plots presented, most cross-sections at most sites show a poor relationship between measured and modeled velocity. In addition, the scatterplots for most sites show a poor relationship between measured and modeled velocity; in many cases with an increasing variance as velocity increases, suggesting a lack of correlation.

Response: The velocity validation results shown in Appendix G indicate the level of uncertainty in the model results. We do not have empirical data on velocity vectors and thus are unable to provide additional cross-section plots showing vectors of velocity magnitude and direction (for empirical data and model results). The relationship between measured and modeled velocities in the cross-section plots needs to be evaluated within the context of the accuracy of the velocity measurements. As shown in the figures in Appendix G, we attribute most of the differences between measured and predicted velocities to noise in the measured velocity measurements; specifically, for the transects, the simulated velocities typically fell within the range of the measured velocities of the three or more ADCP traverses made on each transect. An increasing variance between measured and modeled velocities does not suggest a lack of correlation; in contrast, for all 10 sites, the measured and modeled velocities were moderately or strongly correlated, with correlation coefficients of greater than 0.6.

Comment 2: Baldwin (1997) is an inappropriate reference for this material, and needs to be replaced.

Response: We were not able to find another reference for definitions of what ranges of Pearson's correlation coefficient are considered moderately strong or very strong. Statistics textbooks that we reviewed do not give numeric definitions of what are considered moderately strong or very strong correlations. We also were not able to find numeric definitions of what are considered moderately strong or very strong correlations in the peer-reviewed literature. We replaced the Baldwin (1997) reference with Cohen (1992), which defines correlations of 0.5 to 1.0 as having a strong effect.

HABITAT SUITABILITY CRITERIA (HSC) DATA COLLECTION

REVIEWER #4

Comment 1: The use of the terms HSC, HSI, Suitability Indices (HSC), etc., in this paragraph are inconsistent and confusing -- these sentences should be rewritten for consistent use of the terms and abbreviations.

Response: We have deleted the term “Suitability Indices,” and have attempted to consistently use the terms “HSC” and “HSI.” We previously added the following sentences to increase the clarity of the usage of the terms:

“HSC refer to the overall functional relationships that are used to convert depth, velocity and substrate suitability into measures of habitat quality (HSI). HSI refers to the independent variable in the HSC relationships.”

Comment 2: State in this and the next paragraph the discharges during all of the data collection periods, and how those discharges compare (or are related to) to those during redd construction.

Response: This information is provided in Table 11 and Figures 3 to 6 in the results section. In response to comments from earlier reviewers, we have attempted to consistently separate methods from results.

Comment 3: See previous comment regarding subjectivity of visual substrate size estimation method.

Response: Please refer to the response above regarding our visual substrate size estimation method.

BIOLOGICAL VERIFICATION DATA COLLECTION

REVIEWER #4

Comment 1: Here and elsewhere pertaining to this topic, provide more descriptive details regarding the hypothesis testing to convince the reader that this is a robust and appropriate test for the stated purpose; e.g., why not use a parametric test when you have such large sample sizes; what assumptions of parametric stats were violated (and how was that determined) indicating the appropriateness of nonparametric tests; state whether you used a one-tailed test, as suggested by the narrative; state sample sizes; explain why such a large unbalanced sample size was used, and how this unbalanced design is appropriate for this test and not biasing the results; provide references from the statistics literature (preferably biostats lit.) to support your explanations.

Response: We specified that the test was one-tailed and gave citations to the peer-reviewed literature at this location. Sample sizes are provided in the results section under biological verification. We added the following text in the discussion section under biological verification to address the remainder of this comment:

“We did not use a parametric test because the assumption of normality of parametric tests was violated, as shown in Figures 18 to 23, indicating the need to use nonparametric tests. Nonparametric statistical methods were appropriate to use with the large, unbalanced sample size of this study to reduce type II errors, since unoccupied depths, velocities and substrates have a much greater range of values than occupied depths, velocities and substrates. Analogously, Thomas and Bovee (1993) found that a minimum of 55 occupied and 200 unoccupied locations were required to reduce type II errors.”

HABITAT SUITABILITY CRITERIA (HSC) DEVELOPMENT

REVIEWER #3

Comment 1: Regarding the HSC data, the report states it was almost always collected within 8 feet of the pit – while non-use data were collected 3 feet away from ‘any other point’. I assume this means that use data could be collected from a non-use area 8 feet away from a redd and that non-use data could likewise be inadvertently be collected in an area that was used.

Response: The commenter appears to have misinterpreted the text in question – the depth and velocity measurements were taken at a location that was assessed to be similar to that present at the pit of the redd prior to redd construction. Accordingly, the depth and velocity measurement location is not the use location (the pit of the redd). The use location was the location used in the selection of non-use locations. Thus, the use and non-use locations were mutually exclusive. We could have defined non-use data as being over 10 feet from a redd, but that would potentially result in eliminating a large amount of non-use habitat (as defined by the absence of a redd).

REVIEWER #4

Comment 1: See previous comment regarding the subjectivity of this visual substrate assessment method; the method calls into question the robustness of the resulting substrate criteria and WUA calculations; e.g., how does WUA estimate vary as a function of substrate observer differences?

Response: Given the response to the previous comment, the resulting substrate criteria and WUA calculations are robust, in that we would expect the WUA estimate to vary little as a function of substrate observer differences. In addition, the shape of the WUA curve is generally not very sensitive to substrate, since substrate does not vary with flow. We examined a similar question on the American River, where we used breaklines to more accurately characterize the

substrate distribution of a site using the collected data, and found that there was a very small difference in the value of WUA where substrate distribution was represented with and without breaklines (Gard 2009).

Comment 2: This approach for assigning depth and velocity data at unoccupied sites is problematic, as it is based entirely on RIVER2D model output; as such, the approach assumes that the modeled depths and velocities are as accurate as what empirical measurements would/could have been at unoccupied locations (an empirically-based alternative approach); and as previously discussed, there is significant uncertainty regarding the accuracy of the hydraulic modeling output.

Response: We did not collect depths and velocities at unoccupied locations, and thus cannot use the empirical approach suggested by the reviewer. Modeled depths and velocities likely are not as accurate as what empirical measurements would have been at unoccupied locations. However, the hydraulic modeling output should be sufficiently accurate for purposes of determining the relative frequency (over a range of depths and velocities) of unoccupied locations. Specifically, since the effects of over-predicted depths and velocities would be cancelled out by the effects of under-predicted depths and velocities, errors in the hydraulic modeling output would have a minimal effect on the frequency distribution of unoccupied depths and velocities.

BIOLOGICAL VERIFICATION

REVIEWER #4

Comment 1: ...compared what?...this is incomplete...just redd locations to one another (how?) or between redd and non-redd locations...

Response: We change “compared” to “computed.” A comparison of redd and non-redd locations does not occur until the last sentence of this paragraph. The first sentence was meant to identify the redd locations that were used in the biological validation.

Comment 2: See previous comment regarding this test and all of the detailed explanation that should go along with it.

Response: See response to previous comment for additional explanation regarding this test.

RESULTS

STUDY SEGMENT DELINEATION

REVIEWER #4

Comment 1: The material from this point on pg. 26 through the second paragraph on page 41 describes the study area and methods, and should be placed in those sections -- these are not results.

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 Segment Delineation* through *Habitat Suitability Criteria Development*). We previously included all of this information in the methods section and a peer reviewer from the first peer review of this 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.”

FIELD RECONNAISSANCE AND STUDY SITE SELECTION

REVIEWER #1

Comment 1: Table 7 in Yuba Spawning report, what does --- signify?

Response: We added the following text to the caption for Table 7 to explain what --- signifies: “Entries with --- reflect that data was not collected for the race or species in the Below Daguerre segment.”

HYDRAULIC AND STRUCTURAL DATA COLLECTION

REVIEWER #2

Comment 1: 28, Table 8, The total number of points and point densities are rather low for this scale of 2D ecohydraulic modeling. The point densities and topographic sampling is more consistent with the data collection one does for 1D PHABSIM /HECRAS modeling. Point densities are usually reported in points per square meter instead of points per 100 square meters. Typically point densities for 2D models are 0.15 to 0.3 pts/m², with high point densities > 1 pt/m²; for comparison those in this report are 0.01 to 0.04 pts/ m² (rather low). It just means that by not feeding it higher resolution topography the authors are not really taking full-advantage of the 2D model.

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 m², while Jacobson and Galat (2006) had a point density of 6 points/100 m². This study was one of our earlier River2D studies; 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 0.4 points/m²) within our time constraints for data collection. This higher point density has had a moderate effect on the accuracy of the hydraulic models, increasing the average correlation between measured and simulated velocities from 0.74 in this study to 0.80 in our lower Clear Creek spawning study.

RIVER2D MODEL CALIBRATION

REVIEWER #3

Comment 1: Five of 11 sites (p. 31) had WSELs that were more than 0.1 foot different than PHABSIM or measured values. This reduces confidence (mine, at least) that the model works well – or that the empirical data were not collected with a high degree of quality control.

Response: There were only 10 sites and four of those sites had WSELs that were more than 0.1 foot different than PHABSIM or measured values. In all four cases the WSELs simulated near both banks, where all empirical data was collected, were within 0.1 foot of the measured values. Given that WSELs on the Yuba River can vary going across the channel by as much as 0.65 foot, it is not surprising that WSELs simulated by River2D in the middle of channel might differ from WSELs measured near the banks by more than 0.1 foot. These differences likely would occur despite the quality of the River 2D model simulation or the quality of the empirical data.

RIVER2D MODEL VELOCITY VALIDATION

REVIEWER #1

Findings, interpretations and conclusions - Are the findings, interpretations and conclusions valid?

Comment 1: The introductory paragraph on page 32 of the Yuba Spawning report sends a very mixed message. First the report states that the velocity model is validated because the correlation is greater than 0.6. Then it states that individual predicted and measured velocities showed much variation, but no models were in question and in general cross-channel velocity profile shapes were similar. However, if the reader examines the scatter plots and the velocity profiles, one might draw a different conclusion. I certainly did. Even though the scatter plots and profiles did show a lot of variation, the authors conclude that the velocity models were good and not in question.

Response: We have reviewed the introductory paragraph to ensure that it is an accurate representation of the velocity performance of the model. The apparent mixed message is due to two different aspects of the velocity validation that are considered in this paragraph: 1) whether the models were validated and thus not rejected; and 2) the deviations between measured and simulated velocities. With regard to the first aspect, we concluded, as the commenter correctly notes, that the model was validated because the correlations between measured and simulated

velocities were greater than 0.6. We would characterize evaluations of whether the models were in question or good as relating to the second aspect, which concerns the degree of uncertainty in the resulting flow-habitat relationships, rather than whether the model has been validated.

Comment 2: Why are the differences between measured and predicted velocities reported as absolute values for velocity < 3 ft/s and percents for velocity > 3 ft/s? This occurs in Yuba Spawning, Yuba Rearing and Clear Creek Spawning reports. The reports also make strong note of the ‘high’ correlations between measured and predicted velocities, but careful review of the scatter plots show that the relationship is not that strong and that the correlation value is heavily influenced by the large sample size.

Response: We compared absolute values for low velocities because large percentage values for low velocities would not be biologically meaningful. In contrast, we compare percentages for higher velocities to be consistent with the methods used to compare discharges, given that most high velocity areas have little habitat value but rather reflect the degree to which the hydraulic model is accurately routing flow through the site. The scatter plots show the same information as correlation coefficients, but tend to emphasize outliers. The correlation coefficient (r) is calculated using the following formula:

$$R = \frac{\sum xy}{\sqrt{\sum x^2 \sum y^2}},$$

where x and y are the individual measured and simulated velocity values. Accordingly, the correlation coefficient value is independent of sample size.

REVIEWER #2

Comment 1: 32, 1, This seems like an unsubstantiated comment ‘The models for all sites were validated, and thus no models were in question...’. Just because the correlation was greater than 0.6 doesn’t mean they were not in question. It means you’re in the ballpark a lot of the time, but not actually doing that great (especially when you factor in how sensitive the types of habitat suitability models you are applying are to hydraulic model results!).

Response: We defined whether a model was validated, and thus not in question, based on a correlation of greater than 0.6, and that is the basis for this statement. We would view the model not as being in question, but rather as having a higher level of uncertainty, for correlations of 0.6, versus say 0.9.

REVIEWER #4

Comment 1: This statement that all models were validated and were not in question is too strong (too confident) given the limited supporting information presented. Based on the correlation scatterplots (not just the correlation coefficient), the plots of cross-section velocity magnitude (modeled and empirical), and the high Froude numbers predicted from the model, it

seems like the hydraulic modeling results are in question -- and the underlying cause of the model uncertainty is likely the riverbed elevation data on which the models are based (though, this is unknown to the readers, because survey error information is not provided). Vector plots of velocity magnitude and direction (modeled and empirical) would go a long way toward substantiating the authors' claim that the model results are not in question.

Response: Supporting information may be found in appendix G. The scatter plots directly reflect the value of the correlation coefficient. The relationship between measured and modeled velocities in the cross-section plots should be evaluated within the context of the accuracy of the velocity measurements. As shown in the figures in Appendix G, most of the differences between measured and predicted velocities likely may be attributed to noise in the measured velocity measurements; specifically, for the transects, the simulated velocities typically fell within the range of the measured velocities of the three or more ADCP traverses made on each transect. The high Froude Numbers predicted along the channel margins should be viewed within the context of what effect they would have on the overall flow-habitat relationships. Specifically, the Froude Number only exceeded one at a few nodes, with the vast majority of the site having Froude Numbers less than one. Furthermore, these nodes were located either at the water's edge or where water depth was extremely shallow, typically approaching zero. A high Froude Number at a very limited number of nodes at water's edge or in very shallow depths would be expected to have an insignificant effect on the model results because these areas are not likely to contain redds. We would characterize the hydraulic modeling results as indicating a level of uncertainty in the model results, rather than that the results are in question. We agree that the underlying cause of model uncertainty is the riverbed elevation data on which the models are based, specifically, the density of topographic data. We have added as much detail as possible describing the elevation surveying and associated errors. Based on the available information, the errors in the elevation data appear to be minimal. As shown in Tables 8 and 9, all errors were less than 0.07 feet. We do not have empirical data on velocity vectors and thus are unable to provide additional cross-section plots showing vectors of velocity magnitude and direction (for empirical data and model results).

Comment 2: This assertion is weak and open to interpretation -- even if the comparisons are similar in shape, the velocity magnitudes (the variable of interest) are very dissimilar.

Response: We intended this statement [In general, the simulated and measured cross-channel velocity profiles at the upstream and downstream transects (Appendix G) were relatively similar in shape] to appropriately summarize the cross-sectional plot data. As shown in the figures in Appendix G, most of the differences between measured and predicted velocities may be attributed to noise in the measured velocity measurements; specifically, for the transects, the simulated velocities typically fell within the range of the measured velocities of the three or more ADCP traverses made on each transect.

Comment 3: As indicated in earlier comments, these supercritical Froude numbers are likely signs of poor model performance; and it looks like this was the case for the vast majority of the model runs.

Response: The high Froude Numbers need should be viewed within the context of what effect they would have on the overall flow-habitat relationships. Specifically, the Froude Number only exceeded one at a few nodes, with the vast majority of the site having Froude Numbers less than one. Furthermore, these nodes were located either at the water's edge or where water depth was extremely shallow, typically approaching zero. A high Froude Number at a very limited number of nodes at water's edge or in very shallow depths would be expected to have an insignificant effect on the model results because redds are not expected to be found in these locations.

HABITAT SUITABILITY CRITERIA (HSC) DATA COLLECTION

REVIEWER #3

Comment 1: Does Figure 3 mean hourly discharge?

Response: Figure 3 presents mean daily discharge. We have added this information to the legends for Figures 3 to 6.

Comment 2: Figure 4 – the lower panels should be on the same scale as each other – to allow for easier visual comparisons.

Response: We have changed the lower panels to be on the same scale as each other.

BIOLOGICAL VERIFICATION DATA COLLECTION

REVIEWER #3

Comment 1: Once again, the biological verification was limited in scope due to 'time constraints'. To me, this is a sign that the authors were more interested in the physical model, than in using that model to better understand the relationship between discharge and available habitat. Without a better understanding of the biological side of things, the physical side of things has very limited fisheries management implications.

Response: We collected a considerable amount of habitat suitability data, with the intent of making the model biologically relevant. Our primary goal was to develop flow-habitat relationships, which includes both the physical model and the biological data used to develop the habitat suitability criteria. Developing a better understanding of the flow-habitat relationship necessarily falls out as a lower priority than actually developing the relationship between discharge and available habitat. The study addressed both the biological and physical aspect of anadromous salmonid habitat, since both aspects are needed to develop flow-habitat relationships. Flow-habitat relationships are critical to fisheries management with regards to defining an adequate flow regime to support anadromous salmonid populations.

HABITAT SUITABILITY CRITERIA (HSC) DEVELOPMENT

REVIEWER #1

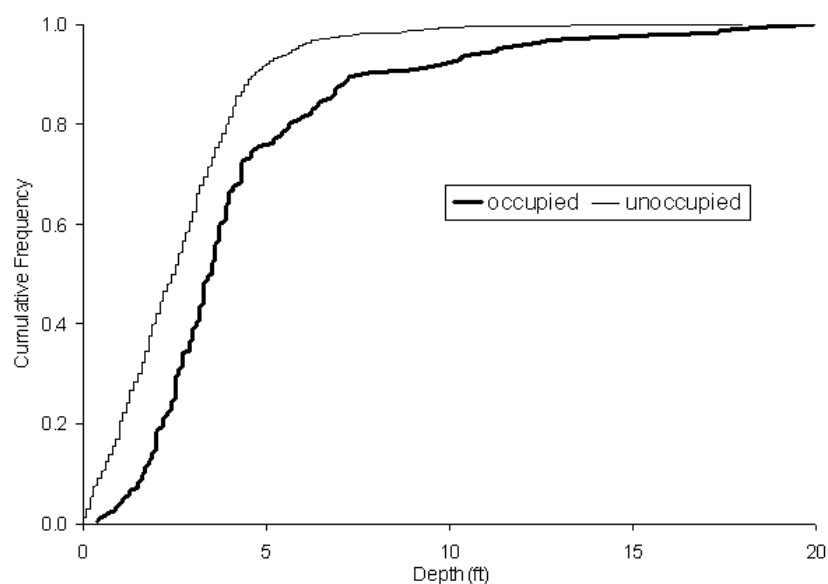
Comment 1: Figures 7 and 8 in Yuba Spawning report do not have a label for the Y axis.

Response: We have added labels for the Y axis for Figures 7 and 8.

REVIEWER #3

Comment 1: The weighting of the few deep steelhead redds on the HSC is mysterious. Obviously the authors understood that this would draw attention and they attempted to explain why their approach was justified, but I was not convinced – or I was unable to understand (or believe) their explanation. They tended to summarily dismiss many other areas of uncertainty as well, throughout this report and the others.

Response: As shown in Figures 36 and 37, the few deep steelhead redds (defined as roughly redds with depths of greater than 10 feet) had a minimal effect on the HSC. The HSC instead was driven by the overall distribution of occupied and unoccupied locations. For example, 24 percent of the steelhead redds had depths of 5 feet or greater (clearly not just a few redds). The logistic regression used to develop the HSC demonstrates that steelhead/rainbow trout strongly select deeper conditions in the Yuba River. As such, the large number of steelhead/rainbow trout redds in shallow water can be attributed to steelhead/rainbow trout being forced to use shallow conditions because of the relative scarcity of deeper water conditions in high use spawning areas in the Yuba River. As shown below, for all depths, steelhead/rainbow trout disproportionately selected deeper conditions. We attempted to identify and assess all areas of uncertainty associated with the methods and the model, and their relative effect on the overall flow-habitat relationship. A section concerning uncertainty is included in the discussion.



Comment 2: With only 2 steelhead redds deeper than 16.9 feet – they did not apply the Gard (1998 (though the copy I have says 1997 at the bottom of each page)) method to adjust the suitability data toward even deeper water. Even so, the suitability curve mysteriously stays very high in the deeper waters relative to the measured use data. The steelhead depth HSC does not appear to match the measured used data very well at all, while the data for the Chinook stocks appears more believable.

Response: Based on the results of the sensitivity analysis, we used an alternative set of HSC for steelhead spawning that only uses occupied and unoccupied data upstream of Highway 20. The alternative criteria have a suitability that reaches 0.9 at a depth of 3.2 feet and an optimum suitability of depths of 7.0 to 19.9 feet. For the alternative criteria, we were also unable to apply the Gard (1998) method because the final criteria stayed at a suitability of 1.00 up to the depth of the deepest steelhead/rainbow trout redd we found. The correct year of the Gard (1998) article is 1998 – there was an error in the year shown on the reprint of the article. The suitability curve stays very high in deep water relative to the measured use data because the use data is driven by steelhead/rainbow trout being forced to use shallow conditions because of the relative scarcity of deeper water conditions in high use spawning areas in the Yuba River. The steelhead depth HSC differs from the measured use data because the HSC applies a correction for habitat availability. Theoretically, if there were equal availability of all depths, the frequency of use would identify the suitability. To the extent that availability is constraining use, the frequency of use deviates from the suitability. For steelhead/rainbow trout on the Yuba River, the results of the logistic regression indicate that availability was strongly constraining use, resulting in most of the redds being in shallow water, and thus suitability differs substantially from the frequency of use.

Comment 3: Table 12 needs more info in the caption (e.g., what are I-M?) and an explanation of which variables are being regressed.

Response: We have added information to the figure caption defining I-M and explaining which variables are being regressed.

Comment 4: Figures 9-12 make sense to me, but Figure 13 does not. This appears to way overestimate the importance of deep spawning areas for steelhead. These data do not comport well with data from other areas (e.g., the Columbia River) where both fall Chinook and steelhead spawn. In the Columbia River and many of its tributaries, the fall Chinook tend to spawn in much deeper (max depth of redds) water than the steelhead.

Response: Based on the results of the sensitivity analysis, we used an alternative set of HSC for steelhead spawning that only uses occupied and unoccupied data upstream of Highway 20. The alternative criteria has a suitability that reaches 0.9 at a depth of 3.2 feet and an optimum suitability of depths of 7.0 to 19.9 feet. The suitability curve stays very high in deep water relative to the measured use data because the use data is driven by steelhead/rainbow trout being forced to use shallow conditions because of the relative scarcity of deeper water conditions in high use spawning areas in the Yuba River. Even the occupied data showed

significant differences between the Chinook salmon and steelhead/rainbow trout redds in the Yuba River – there were only two fall-run redds (0.2 percent) and no spring-run redds (0 percent) with depths of more than 5 feet, while 24% of the steelhead/rainbow trout redds had depths greater than 5 feet. The preference of steelhead/rainbow trout for much greater depths than Chinook salmon may be related to steelhead/rainbow trout spawning during the winter, when flows are much more variable – spawning in deeper water may reduce the probability of redds becoming dewatered with decreases in flow or scoured with increases in flow. The Yuba River may be different than the Columbia River and many of its tributaries in that the Yuba River has some deeper areas with suitable velocities and substrates for steelhead, allowing 24 percent of the steelhead to spawn in water 5 feet or deeper.

Data - Is the data adequate?

Comment 5: Not really.

Page 39; the authors state the slopes are different, though with so few data points, this seems like a stretch. When I look at those plots it is difficult for me to conclude that availability dropped more slowly than use – with increasing depth. Same comment for the FC figure. I did not see a similar plot for steelhead....?

Response: The number of data points is limited by the number of redds with depths greater than 2 feet that were located in our study sites. We could have generated a greater number of data points by using smaller depth bins, but at the cost of increasing the noise in the individual data points, since each bin would be based on fewer redds. The standardized use/availability ratio and use/availability regression clearly shows that availability dropped more slowly than use with increasing depth. There was not a similar plot for steelhead because we were unable to use the Gard (1998) depth correction methodology due to the low number of redds in water deeper than the depth with the optimum suitability.

REVIEWER #4

Comment 1: The HSI for steelhead/rainbow trout spawning depth seems to be in error, as the shape of the curve does not match the shape of the occupied frequency distribution; and the deepest occupied locations with low frequency of redds have a much higher HSI value than shallower locations with much larger frequency of redds; and depths ~16.9 ft have an HSI of ~0.9 when there were only 2 redds deeper than that. More explanation should be given to provide details and justification for the creation of this HSI curve.

Response: Based on the results of the sensitivity analysis, we used an alternative set of HSC for steelhead spawning that only uses occupied and unoccupied data upstream of Highway 20. The alternative criteria have a suitability that reaches 0.9 at a depth of 3.2 feet and an optimum suitability of depths of 7.0 to 19.9 feet. The suitability curve stays very high in deep water relative to the measured use data because the use data is driven by steelhead/rainbow trout

being forced to use shallow conditions because of the relative scarcity of deeper water conditions in high use spawning areas in the Yuba River. We have added additional information, details and justification to the report for the creation of this HSI curve.

BIOLOGICAL VERIFICATION

REVIEWER #2

Comment 1: 50, 1, Woaa... For what flow and what year(s) of spawning are these comparisons made between habitat suitability model results and redds? Presumably a spawning flow? Even if the information is buried back in the methods somewhere, you should repeat it here AND in Appendix L, which makes no mention of it.

Response: The model results were for spawning flows (specifically a subset of the flows in Table 11). We have added information to Appendix L on the flow and year of spawning that these comparisons were made. The years are given in the results section under *Biological Verification Data Collection* and the flows are given in the results section in Table 11.

REVIEWER #3

Comment 1: Page 50; 16, 8, and 11% of redds were in areas where the model predicted zero suitability for Spring Chinook, Fall Chinook, and steelhead, respectively. There were also redds measured in areas that the model predicted would be dry. Again, this decreases confidence that the model is working properly.

Response: We would characterize these results as increasing the level of uncertainty in the results, rather than decreasing confidence that the model is working properly. Most of the redds where the model predicted zero suitability were because the substrate was predicted incorrectly. See response below regarding the effect of substrate on the shape of the WUA curve.

Comment 2: The substrate size was predicted correctly a little of a third of the time. This leads to lack of confidence in either the data collection or model structure/function.

Response: We would characterize these results as increasing the level of uncertainty in the results, rather than lack of confidence in either the data collection or model structure/function. The shape of the WUA curve is generally not very sensitive to substrate, since substrate does not vary with flow. We examined a similar question on the American River, where we used breaklines to more accurately characterize the substrate distribution of a site using the collected data, and found that there was a very small difference in the value of WUA where substrate distribution was represented with and without breaklines (Gard 2009).

REVIEWER #4

Comment 1: See previous comment on the use of this test, especially as related to the large, unbalanced sample sizes -- here and elsewhere in the Results section.

Response: We specified that the test was one-tailed and gave citations to the peer-reviewed literature in the Methods section. Sample sizes are provided in the results section under biological verification. We added the following text in the discussion section under biological verification to address the remainder of this comment:

“We did not use a parametric test because the assumption of normality of parametric tests was violated, as shown in Figures 18 to 23, indicating the need to use nonparametric tests. A large, unbalanced sample size was appropriate for this test to reduce type II errors, since unoccupied depths, velocities and substrates have a much greater range of values than occupied depths, velocities and substrates. Analogously, Thomas and Bovee (1993) found that a minimum of 55 occupied and 200 unoccupied locations were required to reduce type II errors.”

DISCUSSION

REVIEWER #2

Findings, interpretations and conclusions - Are the findings, interpretations and conclusions valid?

Comment 1: The discussion is very weak and some of the conclusions are not robustly substantiated by data (see comments below). There is a lot of potentially interesting conjecture in there that carries little weight because the discussion makes virtually no attempt to tie the findings to other studies in the peer-reviewed literature (e.g. discussion of velocity errors). This is unacceptable. The reader is just supposed to take on faith statements like ‘we conclude the calibration... was acceptable’ and ‘we did not regard the ____ as problematic’ and ‘we consider the solution to be acceptable...’. In general, I don’t think the overall take-home message from the findings, interpretations and conclusions are that far off. It is just that the execution and writing is sloppy insofar as it makes little reference to the literature and little attempt to back up where the ‘feelings, judgments, and impressions’ come from.

Response: We have added material to strengthen the discussion, such as references to topographic point densities used in other studies. It is difficult to relate the hydraulic findings to other studies in the peer-reviewed literature because hydraulic modeling results are site and model-specific, and because of differences in how velocity validation results are reported in the peer-reviewed literature. We were able to compare the habitat suitability criteria and flow-habitat relationships to those of other studies. Statements in the discussion are intended to relate back to the quantitative criteria presented in the results section. 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 velocity validation the numeric criterion was that the correlation between observed and simulated velocities was greater than 0.6. By necessity, the text that was used to elaborate on the model performance was qualitative and designed to illustrate general trends in the data.

Comment 2: I can see from the past peer-reviewer comments (executive summary) that earlier versions of the report did not include a discussion section, so this is probably the first time that section has been thoroughly vetted. I see the past peer-reviewers picked up on similar weaknesses in the introduction with regards to a poor attempt to use the peer-reviewed literature to both justify and contextualize methods used and interpretations made. I can see from the author comments, that the authors made some attempt to rectify this. However, it is still relatively weak. Perhaps a more thorough literature review is in order? The notable exception to this is the habitat suitability curve discussion (pp 65-76).

Response: We feel that the introduction and discussion has an adequate literature review, in terms of justifying and contextualizing the methods used and interpretations made. As noted above, a more thorough literature review for sections of the discussion other than the habitat suitability curve discussion is problematic because results for other sections, such as hydraulic modeling results, are site and model-specific. We have added some references to the report.

HYDRAULIC AND STRUCTURAL DATA COLLECTION

REVIEWER #2

Comment 1: 59, 3, Weak justification for ‘concluding that total station measurement error will have a minimal effect on the final result! Even if the point accuracies are perfect, the sampling design (i.e. low point densities and transect based sampling), will substantially influence your results. I’ve seen and myself produced much better agreement between hydraulic model results and observed hydraulics and it is highly dependent on topographic sampling.

Response: The statement in question is based on the information provided in the previous sentence: “All of the measurements were accurate to 1 foot (0.3 m) horizontally and 0.1 foot (0.031 m) vertically.” Based on our experience, the relatively low point densities used in this report had a small role in the degree of agreement between hydraulic model results and observed hydraulics. 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 m², while Jacobson and Galat (2006) had a point density of 6 points/100 m². 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 0.4 points/m²) within our time constraints for data collection. This higher point density has had a moderate effect on the accuracy of the hydraulic models, increasing the average correlation between measured and simulated velocities from 0.74 in this study to 0.80 in our lower Clear Creek spawning study. The sampling methods used in this study (ADCP) necessitated transect based sampling versus sampling longitudinally along breakline features. Our experience has been that a transect based sampling, if the transects are spaced closely enough, will be as accurate as longitudinal sampling. Further, we have found that, except for some features such as toe of the bank and top of bars, it is difficult to identify breakline features in the field, especially for larger rivers, such as the Yuba River.

REVIEWER #4

Comment 1: This Discussion section 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, and elsewhere for similar study issues. Focus more on the type of discussion starting on pg. 66. As it currently reads, the early 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.

Response: We added material to the discussion section to improve the clarity of the interpretation of the results, as compared to other studies in the Yuba River and elsewhere. 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. Our discussion comparing our WUA estimates with the previous study on the Yuba River was necessarily limited to the habitat suitability criteria and flow-habitat relationships, since the vastly different hydraulic model used in the earlier study (PHABSIM) precludes any more in-depth comparison of the hydraulic modeling from this study, beyond that already given in the introduction. It is difficult to relate the hydraulic findings to other studies in the peer-reviewed literature because hydraulic modeling results are site and model-specific, and because of differences in how velocity validation results are reported in the peer-reviewed literature. We were able to compare the habitat suitability criteria and flow-habitat relationships to those of other studies. 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.

Comment 2: This statement is unsubstantiated because no elevation survey error data are given in the report.

Response: We have added as much detail as possible describing the elevation surveying and associated errors. Based on the available information, the errors in the elevation data appear minimal. As shown in Tables 8 and 9, all errors were less than 0.07 feet.

RIVER2D MODEL CALIBRATION

REVIEWER #1

Comment 1: The discussion states that it makes more sense to use the PHABSIM predicted WSEL to calibrate the 2D model rather than measured WSEL. It's not clear to me why one would ever choose a predicted over a measured value for use in calibration. And it is not clear what information leads the authors to decide that the predicted WSEL is inaccurate and switch to using the measured value.

Response: Our general rule is that it is more accurate to calibrate sites using the WSELs simulated by PHABSIM at the highest simulated flow because the RIVER2D model is more sensitive to the bed roughness multiplier at higher flows, versus lower flows. Typically the highest simulated flow is significantly higher than the highest flow at which we measured

WSELs. The information that led us to decide that the predicted WSEL is inaccurate and to switch to using the measured value was that the highest measured flow had WSELs on the two banks that differed by more than 0.1 foot. Since PHABSIM assumes that the WSEL is the same anywhere on the transect, a situation where WSELs on the two banks that differed by more than 0.1 foot naturally leads to the conclusion that the WSEL predicted by PHABSIM is inaccurate, and thus we should switch to using the measured value.

REVIEWER #4

Comment 1: See previous comments regarding these Froude numbers -- the fact that supercritical flow was predicted along the channel margins (a physically unlikely location for supercritical flow) suggests that the model results are in question.

Response: The high Froude Numbers predicted along the channel margins should be viewed within the context of what effect they would have on the overall flow-habitat relationships. Specifically, the Froude Number only exceeded one at a few nodes, with the vast majority of the site having Froude Numbers less than one. Furthermore, these nodes were located either at the water's edge or where water depth was extremely shallow, typically approaching zero. A high Froude Number at a very limited number of nodes at water's edge or in very shallow depths would be expected to have an insignificant effect on the model results because redds are not likely to be found in these areas.

RIVER2D MODEL VELOCITY VALIDATION

REVIEWER #1

Comment 1: For discrepancies in measured vs. predicted velocities, there is a post-hoc reason given based on channel properties or equipment imprecision. If one needs to look at the channel or blame equipment to argue away model errors, what benefit does the model provide?

Response: The model, relative to empirical methods such as demonstration flow assessments, provides the benefit of being able to simulate depths and velocities over a range of flows, instead of only at the observed flows for demonstration flow assessments.

REVIEWER #2

Comment 1: 63, 2, This is reasonable conjecture, but why not cite studies who have looked at this specifically to justify it (Pasternack *et al.*, 2006).

Response: We have added a citation to Pasternack et al. (2006) at this location.

HABITAT SUITABILITY CRITERIA (HSC) DEVELOPMENT

REVIEWER #3

Comment 1: The finding that steelhead selected small substrate (relative to the Chinook stocks) and deeper and faster water does not make sense – as substrates tend to be larger in deeper and faster water than in shallower/slower water.

Response: The comment assumes that habitat selection (as opposed to the availability of habitat) shows a correlation between velocities, depths and substrates. The Yuba River has sufficient areas with faster velocities, deeper conditions and small substrates that steelhead/rainbow trout can select such locations for their redd construction.

Comment 2: Figure 45 is telling. However, the authors simply state that they compared their HSC data for steelhead with analogous data from other systems. The fact that the data from other systems is tightly clustered and that the result the authors of the Yuba report produced is so far off, should signal that perhaps something went wrong in the Yuba study.

Response: An alternative explanation is that the Yuba River may be very different than the other streams where habitat suitability criteria have been developed. The differences between the steelhead/rainbow trout depth and velocity criteria from this study, versus from other studies, can be attributed to the criteria from other studies likely being biased towards shallow depths because of limited availability of deeper water with suitable substrate and velocities, and because the criteria from other studies did not apply a logistic regression to correct for availability. The Yuba River may be unique among the rivers studied in that it has some deeper areas with suitable velocities and substrates, allowing 24 percent of the steelhead to spawn in water 5 feet or deeper. In contrast, the criteria from other systems all have zero suitability for depths of 5 feet or greater. Further, the substantial natural flow fluctuations during the steelhead spawning season on the Yuba River would be a strong selective force to shift steelhead spawning behavior towards selecting deeper conditions, since eggs in shallow redds would not survive dewatering or scouring associated with flow fluctuations.

Comment 3: On page 75, the last sentence of the second full paragraph is bizarre. It appears the authors are summarily dismissing a major point here.

Response: We are assuming the commenter is referring to the following sentence: “The differences between the steelhead/rainbow trout depth and velocity criteria from this study, versus from other studies, can be attributed to the same reasons as the difference between the steelhead/rainbow trout and Chinook salmon criteria from this study, as discussed above.” We have substituted the text from the response to the previous comment, starting with the second sentence, for the sentence in question to better address this point.

BIOLOGICAL VERIFICATION

REVIEWER #3

Comment 1: The statement in the biological verification discussion, “our results could have been as good as Hardy and Addley’s...” seems odd. Not sure why it is worded this way – or what the authors mean by good.

Response: The meaning of this sentence relates back to the preceding two sentences: “In general, Hardy and Addley (2001) found a better agreement between redd locations and areas with high suitability than we found in this study. We attribute this difference to Hardy and Addley (2001)’s use of polygons to map substrate.” In the context of the sentence in question, good means a better agreement between redd locations and areas with high suitability.

Comment 2: While the authors conclude that the biological verification was a success and that it increased the confidence in flow-habitat relationships for management of the Yuba, I disagree. Just writing it down does not necessarily make it so.

Response: We view the biological verification as successful because for all three races/species, there was a greater suitability for occupied versus unoccupied locations, which has the biological significance that fish are preferentially selecting locations with higher suitability.

HABITAT SIMULATION

REVIEWER #2

Comment 1: 82, 3, This conclusion is total conjecture. You could do a sensitivity analysis to look at influence of topographic uncertainty on hydraulic results and how those propagate into your habitat suitability predictions to substantiate this (e.g. Legleiter *et al.*, 2009). Even if you don’t do it, at least talk about how it could be done (i.e. future work). At least highlight the fact that you did another report sensitivity analysis on the ecohydraulic modeling.

Response: We have added a sentence to talk about how a sensitivity analysis could be done to address the effect of topographic uncertainty on hydraulic results and how this might propagate into the habitat suitability predictions. The report on the sensitivity analysis on the ecohydraulic modeling is now included as an additional appendix to this report and is referenced in the report.

REVIEWER #4

Comment 1: In the absence of any permeability data or references, this narrative concerning preference for high gravel permeability and the justification for extrapolation is unsubstantiated and should be removed.

Response: The justification for extrapolation is supported by two references (Vyverberg et al 1996 and Gallagher and Gard 1999) regarding preference for high gravel permeability, so we retained this discussion.

Comment 2: 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).

FACTORS CAUSING UNCERTAINTY

REVIEWER #4

Comment 1: See previous comment -- this is an invalid assumption without any supporting data or references.

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).

APPENDIX G

REVIEWER #1

Comment 1: Examination of the number of sites with over- or under-predicted segments in Appendix G shows that the percent of transects with over- or under-predicted velocities ranged from 100% at UC Sierra to 27% at Lower Dag. Of the 10 sets of velocity profiles shown, 70% had over- or under-predicted segments of more than 50%. Given this, I don't know that every reader would draw the same conclusions that the models are validated and the profiles are generally similar. Additionally, to arbitrarily decide that 0.6 is a 'good' correlation regardless of sample size and regardless of assessing the scatter plots to see if there is an identifiable relationship neglects careful thinking about the difference between statistical significance and biological significance. Given the shotgun appearance of most of the scatterplots, the 'high' values of correlations are largely artifacts of large sample sizes and not of meaningful relationships.

Response: We were unable to determine what exactly the commenter was referring to in the percentages cited. For example, the Lower Daguerre site had two transects and 15 deep bed ADCP traverses. If the commenter was referring to both the transects and deep bed ADCP traverses as transects, 27% of the transects would be 4.6 transects. The commenter also did not define what was meant by over- or under-predicted velocities – presumably this would refer to differences between measured and simulated velocities that exceeded some threshold, such as 50%. Presumably there would be nearly 100% of the velocities where the simulated velocities over- or under-predicted the measured velocities by at least 0.01 feet/sec. The commenter also did not define what a segment of a transect represented – for example, if this would be one velocity measurement or 10% or more of the width of the transect. Nor is it clear what the 50% refers to – 50% of the transect, or simulated velocities that deviate by over 50% from measured velocities. The scatter plots directly reflect the value of the correlation coefficient, in that lower correlation coefficients will produce a scatterplot which has more of a shotgun appearance. The evaluation of whether profiles are generally similar is necessarily qualitative, and thus it would be expected that different readers might draw different conclusions regarding whether the profiles are generally similar. The selection of 0.6 for the correlation coefficient is based on correlations of 0.5 to 1.0 as having a strong effect Cohen (1992). The selection of a correlation coefficient as a criterion has its basis in biological significance. Specifically, the overall flow-habitat 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. The correlation coefficient (r) is calculated using the following formula:

$$R = \frac{\sum xy}{\sqrt{\sum x^2 \sum y^2}},$$

where x and y are the individual measured and simulated velocity values. Accordingly, the correlation coefficient value is independent of sample size.

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Appendix D
Response-to-Comments Document

for the

**March 2008 Peer-Review Draft of the
Yuba River Spawning Instream Flow Study Executive Summary**

August 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 March 2008 draft of the document, “Executive summary flow-habitat relationships for spring and fall-run Chinook salmon and steelhead/rainbow trout spawning in the Yuba River” (Summary), 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.

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

Lastly, the authors of the Summary wish to thank everyone who provided comments on the March 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 Summary. The four anonymous reviewers were provided by the CALFED Ecosystem Restoration Program. Two of the reviewers did not have any comments on the Executive summary.

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LIST OF ACRONYMS

HSC	Habitat Suitability Criteria
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GENERAL COMMENTS

REVIEWER #2

Study Design Is the study design sound?

Comment 1: The study design is only mentioned briefly in this executive summary (and detailed in other reports). The study design uses well-established and robust methods for the quantification of flow-habitat relationships for fish.

Response: No response needed.

Objectives - Are the objectives clear?

Comment 2: The executive summary does not define objectives explicitly. However, it is implicit by the nature of the executive summary that the objectives are to define flow-habitat relationships for fall-run Chinook salmon and steelhead/rainbow trout spawning life-stages for the Yuba River. The executive summary should make a clearer statement of the rather simple objectives up front. This would help with reader expectations.

Response: We have added the objectives to the introduction section of the response to comments document.

Methods - Are the methods technically sound?

Comment 3: Yes.

Response: No response needed.

Data - Is the data adequate?

Comment 4: Given that this is just an executive summary, only limited data is presented and is adequate for this context.

Response: No response needed.

Findings, interpretations and conclusions - Are the findings, interpretations and conclusions valid?

Comment 5: The only findings really reported are the improved and revised habitat suitability curves. There are no interpretations or conclusions offered, other than the assertion that the newer, fancier techniques must be producing better results. From a scientific perspective, the lack of interpretations and conclusions make the work less interesting, but not necessarily of any less utility or validity.

Response: Interpretations and conclusions are given in the actual spawning report.

Presentation - Is the presentation clear?

Comment 6: Although the graphical presentation is clear, the organization is not at all logical to me. I read this executive summary first (of the six reports provided), assuming it would give some clues about the overall picture and the links between these six documents. Instead, it appears to only be an executive summary of the spawning study on the Yuba. It makes no mention of the other studies nor provides any clues as to how they link together or why they are separated. It does not make any sense to me why the reviewer comments are part of an executive summary. It also is not clear why there would not be an executive summary for all of the reports.

Response: After reviewing the peer review comments, we realized that we had misnamed this document, since it is actually a summary document of responses to comments. As such, we have now renamed the document “Response to Comments.” The commenter is correct that this document only relates to the spawning study on the Yuba, and as such would not mention the other studies. The introduction of the response to comments document provides the context for how the studies link together, namely that the purpose of these investigations is to provide scientific information to the U.S. Fish and Wildlife Service Central Valley Project Improvement Act Program to assist in developing such recommendations for Central Valley rivers. The reports were separated because we felt that it would not be practical to combine all three Yuba River reports into one a combined report, given how voluminous each of the three reports is. The reviewer comments were included as part of the response to comments document to be able to have one accompanying document to the spawning report that addressed both peer review and stakeholder comments. We plan to prepare a response to comments document for the other reports, but had not yet completed that document at the time of this peer review, since the major contents of the other response to comments documents would be the response to peer review comments on those reports from this peer review.

Figures and tables - Are the figures and tables clear, complete and adequate?

Comment 7: There are no tables (of data) provided in the executive summary and the figures are clear enough. However, a better summary diagram would be to have just one figure (instead of six separate figures) with all six panels in it to allow easy inter-comparison of the results (obviously labels would have to be adjusted). The figure would clearly differentiate the three different runs in vertical columns and would differentiate vertically the differences upstream and downstream of the data. Alternatively, all six curves could be placed on one graph with different linetypes.

Response: A summary diagram with six panels would not have been legible since each panel would have been too small. Similarly, having all six curves on one graph would not have been clear due to the difficulty of distinguishing the curves. As an alternative, we developed a combination of the commenter’s two proposals where we have two figures, each with three curves.

Miscellaneous comments:

The reviewer comments appended to this report seem to be poorly placed. They make sense as an appendix, but I don't understand why they should be part of an executive summary (especially when there are over 70 pages of responses to reviewer comments. I only skimmed these to get the sense that the authors very thoroughly considered the reviewer comments. However, it is somewhat worrying when the responses to reviewer comments are nearly as long or longer than the reports themselves. It also confuses me why I am re-reviewing aspects of a report that were already thoroughly reviewed, vetted by stakeholders, and thoroughly addressed by the authors. This is not necessarily the fault of the authors, but some clearer instructions and context would be nice.

Response: After reviewing the peer review comments, we realized that we had misnamed this document, since it is actually a summary document of responses to comments. As such, we have now renamed the document "Response to Comments." We wanted to have one accompanying document to the spawning report that that addressed both peer review and stakeholder comments. We appreciate the commenter's assessment of the thoroughness of our consideration of the reviewer comments. The responses to reviewer comments were of necessity nearly as long or longer than the reports themselves due to the volume of reviewer comments that needed to be responded to. This peer review was undertaken in response to a request by stakeholders. We appreciate the commenter's evaluation that the spawning report was thoroughly reviewed, vetted by stakeholders, and thoroughly addressed by the authors.

SPECIFIC COMMENTS**REVIEWER #4**

Comment 1: An introductory paragraph should be added that describes the background, purpose, objectives and setting for the study.

Response: We have revised the first paragraph to make it an introductory paragraph that describes the background, purpose, objectives and setting for the study.

Comment 2: The first sentence of this paragraph is really the primary results, and should be placed after the methods, which the remainder of this paragraph describes.

Response: We have made the suggested change.

Comment 3: This is a very incomplete executive summary -- which typically are standalone documents that give the reader a complete synopsis of a report. This exec. summary should be rewritten to include introduction/background, study area, methods, results, and conclusions -- these headings/subheadings are not needed, just the narrative.

Response: We rewrote the response to comments document to include, with headings, introduction, methods, results and discussion. The introduction addresses the study area. We have patterned the format of our reports as closely as possible to that of peer-reviewed journal articles. Peer-reviewed journal articles do not typically have a conclusion section and thus we have not added a conclusion section to this report.

Comment 4: The primary results (figures 1-6) should be placed after the methods in the previous paragraph. Additional key results from the report should be included in this exec. summary; e.g., HSC.

Response: We placed the primary results after the methods. We did not feel that it was appropriate to add figures for the HSC to the response to comments document because it would have made the response to comments document too long.

Appendix E

Response-to-Comments Document

for the

**October 2008 Peer-Review Draft of the
Yuba River Steelhead Spawning Sensitivity Analysis Report**

August 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 October 2008 draft of the report, “Sensitivity analysis for flow-habitat relationships for steelhead/rainbow trout spawning 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.

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 the sensitivity of steelhead spawning habitat in the Yuba River to alternative habitat suitability criteria, 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 October 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. The four anonymous reviewers were provided by the CALFED Ecosystem Restoration Program.

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LIST OF ACRONYMS

CVPIA	Central Valley Project Improvement Act
GPS	Global Positioning System
RIVER2D	Two dimensional depth averaged model of river hydrodynamics and fish habitat
WUA	Weighted Useable Area

GENERAL COMMENTS

REVIEWER #1

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 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.

REVIEWER #2

Study Design Is the study design sound?

Comment 1: This is not a stand-alone study! The study design is reasonable as far as a sensitivity analysis of a specific factor. It should be emphasized both that a) this sensitivity analysis looks at the model sensitivity to alternative habitat suitability criteria on flow-habitat relationships and biological validation; and b) there other types of sensitivity analyses that could be explored. This in my opinion should not be a separate report, but rather a concise discussion section in the spawning report (i.e. 1-2 pages) and backed up by an appendix showing the methods and detailing the rest of the results.

Response: We agree that this is not a stand-alone study. We have added the following text to the sensitivity analysis report to address the commenter's concerns: "This sensitivity analysis looks at the model sensitivity to alternative habitat suitability criteria on flow-habitat relationships and biological validation. There are other types of sensitivity analyses that could be explored, but were outside the scope of this report." We revised the spawning report to use the alternative criteria developed in this sensitivity analysis and thus now refer to the sensitivity analysis in the methods section of the spawning report. We have added the sensitivity analysis as an appendix to the spawning report.

Objectives - Are the objectives clear?

Comment 2: Not really explicitly stated, but clear enough.

Response: We have added the following sentence to explicitly state the objectives: "The objective of this report is to examine the sensitivity of steelhead/rainbow trout spawning flow-habitat relationships and biological verification to a number of alternative habitat suitability criteria."

Methods - Are the methods technically sound?

Comment 3: Yes, but a very limited and specific form of sensitivity analysis.

Response: We agree that this a very limited and specific form of sensitivity analysis. The scope of the sensitivity analysis was based on input from stakeholders.

Data - Is the data adequate?

Comment 4: The data is adequate to illustrate the sensitivity the authors are focusing on.

Response: No response needed.

Findings, interpretations and conclusions - Are the findings, interpretations and conclusions valid?

Comment 5: I like that the authors lay out two untested hypotheses and relate this to which criteria should be used. They point out that they are unaware of any data that exist to test the hypothesis. That is fine, but just flag up as potential future work what data would need to be collected to test this robustly.

Response: We do not know what data would need to be collected to test this robustly and thus have not added text to the report regarding potential future work on this subject.

Presentation - Is the presentation clear?

Comment 6: Same comments as other reports.

Response: We feel that the introduction of the main spawning report provides a sufficiently broad scientific and management context for the purposes of the report (providing scientific information to the CPVIA Program to assist in developing recommendations for instream flow needs for anadromous fish in the Yuba River). The links between the spawning 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. 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. As noted above, we have combined the sensitivity analysis with the spawning report as an appendix to the spawning report.

Figures and tables - Are the figures and tables clear, complete and adequate?

Comment 7: I think some of the flow-habitat relationships (Fig 15 – 22) could be more effectively presented in a combined (tiled) figure and be annotated to highlight the important differences. Similarly the combined suitability criteria could be more effectively presented in a combined figure that highlights the differences (instead of flipping between pages). The appendix B figures all need to be fixed per the comments in the other report reviews (i.e. north arrow, scale bar, clearer legend, flow directions, etc.)

Response: A combined figure with multiple tiles would not have been legible since each tile would have been too small. Similarly, a combined figure of the combined suitability criteria would not have been legible since each portion of the combined figure would have been too small. We added a scale, flow direction indicator and north arrow to the Appendix B figures. Addition of captions to the appendices would require reducing the size of the images in the appendices. Accordingly, we have elected to not add captions. We concluded that an inset location map within the broader reach was not necessary since this information is already given in Figure 2 of the spawning report. We added the year and discharge to the figure captions. We have chosen to retain combined suitability in the legend to be consistent with what is used in River2D. We added a footnote to the first page of Appendix B to indicate that a higher value is better, with the following text: “For all pages, Combined Suitability: 1 = optimal, 0 = unusable.”

Comment 8: This is not a stand-alone report. It belongs as an appendix in the spawning report with a concise summary in a subsection of the discussion.

Response: We agree that this is not a stand-alone study. We revised the spawning report to use the alternative criteria developed in this sensitivity analysis and thus now refer to the sensitivity analysis in the methods section of the spawning report. We have added the sensitivity analysis as an appendix to the spawning report.

REVIEWER #3

Study Design Is the study design sound?

Comment 1: I am not sure. The authors compared multiple approaches for performing the habitat modeling for steelhead spawning and came to the same conclusion they did in their 2008 report, which was that they were justified in showing high (plateau) suitability levels for much deeper water for steelhead spawning than has previously been reported on. I am not entirely convinced that having the authors perform the sensitivity analyses on their own work is the best approach to satisfy stakeholder concerns. A review and sensitivity analyses by a third party might be more unbiased.

Response: We feel that the study design was sound. We would characterize our conclusions differently than the commenter. Specifically, we concluded that the data showed high suitability levels for much deeper water for steelhead spawning than has previously been reported on. Our intention for having the peer review of the sensitivity analysis by a third party was to have an unbiased review of the sensitivity analysis.

Objectives - Are the objectives clear?

Comment 2: Yes.

Response: No response needed.

Methods - Are the methods technically sound?

Comment 3: I am unclear on why the GPS data were unreliable for this application – when it appears that GPS data were used widely throughout the rearing, flow fluctuation, and spawning reports. I think that the focus on comparing one model run to another does not address the problem here. I think that the emphasis on modeling and not on the biological verification is the primary source of weakness (and perhaps stakeholder concern?).

Response: GPS data was only used in the rearing, flow fluctuation, and spawning reports in applications where the accuracy of the GPS data (3 to 7 meters) was appropriate for the application. For biological validation, which requires a horizontal accuracy of less than 1 meter, the GPS data was not sufficiently accurate. The focus of this report was comparing multiple habitat suitability criteria, rather than comparing model runs. We feel that biological verification was a major emphasis of this report, and thus disagree that this was a weakness of the report. Stakeholder concern was primarily with the final flow-habitat relationships, which drove having the flow-habitat relationships as the other major emphasis of the sensitivity analysis.

Data - Is the data adequate?

Comment 4: I am not sure. The data are essentially the same data used in the other report(s).

Response: We feel that the data was adequate for purposes of the objective of this report (to examine the sensitivity of steelhead/rainbow trout spawning flow-habitat relationships and biological verification to a number of alternative habitat suitability criteria). We agree that the data are essentially the same data used in the spawning report.

Findings, interpretations and conclusions - Are the findings, interpretations and conclusions valid?

Comment 5: I am disappointed that the authors did not do a more thorough literature review (of peer-reviewed work). They focused heavily on modeling – but not on what types of habitats these animals have been shown to use throughout their range. If they found several areas with similar conditions (e.g., deep clear water with low daily flow fluctuations – I am guessing at all this since none of the reports describe the study area in any detail), then it might be easier for the reader to believe that the Yuba River steelhead really prefer to spawn in very deep water.

Response: The spawning report includes an adequate literature review of habitat suitability criteria from other systems. We do not know if the literature we reviewed on steelhead/rainbow trout spawning criteria from other systems were peer-reviewed, but have not been able to find steelhead/rainbow trout spawning criteria in peer-reviewed journal articles. Since the sensitivity analysis is now an appendix of the spawning report, we do not feel that it is necessary to repeat the literature review in the sensitivity analysis.

We have not been able to find any other studies that have investigated steelhead spawning in streams with deep water with suitable velocities and substrates. We feel that the spawning report gives an adequate description of the study area in sufficient detail. We feel that the data presented in the spawning report adequately documents that Yuba River steelhead prefer to spawn in very deep water.

Presentation - Is the presentation clear?

Comment 6: Not perfectly. There are many confusing sentences and the tone appears to be on the defensive.

Response: We have reviewed the clarity of the presentation and were unable to identify any confusing sentences or defensive tone, and thus have not made any changes to the report in this regard.

Figures and tables - Are the figures and tables clear, complete and adequate?

Comment 7: In most areas the figures and tables are clear.

Response: Since the commenter did not identify in what areas the figures and tables were not clear, we are unable to respond to this comment.

Miscellaneous comments:

None.

Response: No response needed.

SPECIFIC COMMENTS

REVIEWER #4

INTRODUCTION

Comment 1: This type of sensitivity analysis evaluates the effects of using different suitability criteria, and it is equally important to complete a sensitivity analysis of the hydraulic model predictions (principally by varying mesh resolutions, input bed topography resolution and/or vertical adjustment, and roughness coefficients).

Response: A sensitivity analysis of the hydraulic model predictions was outside of the scope of this report. We focused on the habitat suitability criteria because it is generally recognized that flow-habitat relationships are most sensitive to the habitat suitability criteria, and because the sensitivity analysis was conducted in response to stakeholder requests for a sensitivity analysis that addressed the habitat suitability criteria.

METHODS

HABITAT SUITABILITY CRITERIA (HSC) DEVELOPMENT

Comment 1: Visual observations of substrate grain size are very subjective and susceptible to large variability in estimates by different observers – these subjective methods then affect the habitat suitability criteria and WUA; more detail is required, describing how the substrate sizes were determined...one observer?...first grain observed?...mental average of multiple grains?...etc...

Response: We agree that visual observations of substrate grain size are very subjective, but this is the only practical method to collect such data for all topographic survey points. We have been using this technique for the last 16 years and have found minimal variability in estimates by different observers. We changed the text in the spawning report related to observation of substrate sizes to read as follows to address the comment: “All substrate and cover data on the transects were assessed by one observer based on the visually-estimated average of multiple grains ...”.

BIOLOGICAL VERIFICATION

Comment 1: Compared what to what?...add clarity to this paragraph by detailing what the comparisons were.

Response: We change compared to computed. A comparison of redd and non-redd locations does not occur until the last sentence of this paragraph. The first sentence was meant to identify the redd locations that were used in the biological validation.

Comment 2: This approach assumes that the modeled depths and velocities are as accurate as empirical measurements at unoccupied locations; and as discussed in the Yuba River spawning report review, there is significant uncertainty regarding the accuracy of the hydraulic modeling output. A similar sensitivity analysis of the hydraulic modeling is warranted, as it is the basis for this entire study.

Response: We did not collect depths and velocities at unoccupied locations, and thus cannot use the empirical approach suggested by the reviewer. We agree that the modeled depths and velocities are not as accurate as what empirical measurements would/could have been at unoccupied locations. However, we do not agree that the approach is problematic, since the hydraulic modeling output is sufficiently accurate for purposes of determining the relative frequency (over a range of depths and velocities) of unoccupied locations. Specifically, since the effects of over-predicted depths and velocities would be cancelled out by the effects of under-predicted depths and velocities, errors in the hydraulic modeling output would have a minimal effect on the frequency distribution of unoccupied depths and velocities. A sensitivity analysis of the hydraulic modeling was outside of the scope of this report.

Comment 3: Here and elsewhere pertaining to this topic, provide more descriptive details regarding the hypothesis testing to convince the reader that this is a robust and appropriate test for the stated purpose; e.g., why not use a parametric test when you have such large sample sizes; what assumptions of parametric stats were violated (and how was that determined) indicating the appropriateness of nonparametric tests; state whether you used a one-tailed test, as suggested by the narrative; state sample sizes; explain why such a large unbalanced sample size was used, and how this unbalanced design is appropriate for this test and not biasing the results; provide references from the statistics literature (preferably biostats lit.) to support your explanations.

Response: We specified that the test was one-tailed and gave citations to the peer-reviewed literature at this location. Sample sizes are already given in the results section under biological verification. We added the following text in the discussion section under biological verification to address the remainder of this comment:

“We did not use a parametric test because the assumption of normality of parametric tests was violated, as shown in Figures 6 to 14, indicating the appropriateness of nonparametric tests. A large unbalanced sample size was appropriate for this test to reduce type II errors, since unoccupied depths, velocities and substrates have a much greater range of values than occupied depths, velocities and substrates, and thus did not bias results. Analogously, Thomas and Bovee (1993) found that a minimum of 55 occupied and 200 unoccupied locations were required to reduce type II errors.”

CONCLUSION

Comment 1: In the absence of a sensitivity analysis for the hydraulic modeling, this is an unsubstantiated conclusion. An alternative conclusion is that habitat requirements are best characterized by the empirical data collected at redds.

Response: We feel that the conclusion is substantiated within the scope of this report. A sensitivity analysis for the hydraulic modeling is outside of the scope of this report. The data clearly show that the habitat requirements are not well characterized by the empirical data collected at redds, since the data clearly shows that the empirical data collected at the redds are strongly affected by the limited availability of deeper water areas in high spawning use areas in the Yuba River.

REFERENCES

Thomas, J.A. and K.D. Bovee. 1993. Application and testing of a procedure to evaluate transferability of habitat suitability criteria. *Regulated Rivers: Research & Management* 8: 285-294.