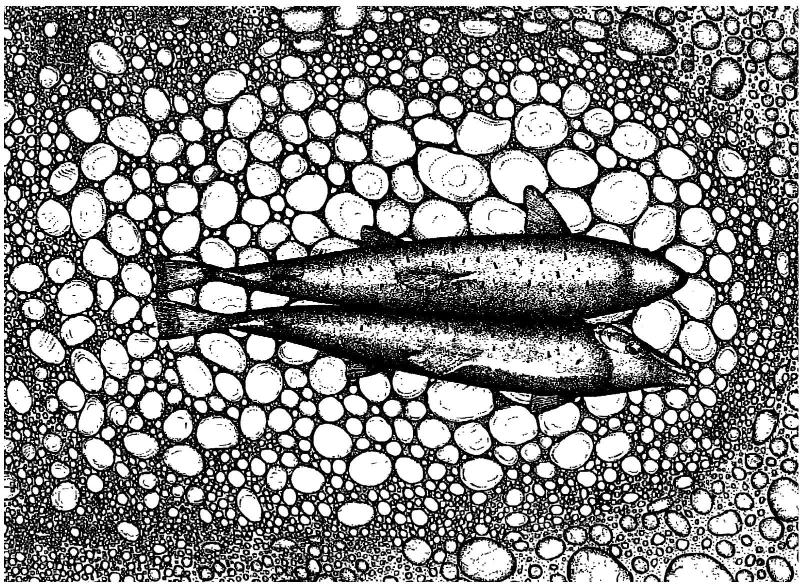
RESPONSE TO COMMENTS
FLOW-HABITAT RELATIONSHIPS FOR JUVENILE FALL/SPRING-RUN CHINOOK SALMON AND STEELHEAD/RAINBOW TROUT REARING IN THE YUBA RIVER



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Prepared by staff of
The Energy Planning and Instream Flow Branch

INTRODUCTION

The following is the response to comments for the final report for the U.S. Fish and Wildlife Service's investigations on anadromous salmonid rearing 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, developed a comprehensive set of improved flow regimes, which now are 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 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 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 fry and juvenile rearing.

METHODS

Flow-habitat relationships were derived for spring and fall-run Chinook salmon and steelhead/rainbow trout rearing in the Yuba River between Englebright Dam and the Feather River. Habitat availability was evaluated using a two-dimensional hydraulic and habitat model, while depth, velocity, adjacent velocity and cover habitat suitability criteria were derived using logistic regression.

RESULTS

The flow-habitat relationships (Figures 1 to 2) had flows with the maximum amount of habitat ranging from 400 to 4500 cfs. Appendix A provides the results of a peer review of the rearing report, conducted by four anonymous reviewers provided by the CALFED Ecosystem Restoration Program and Hal Beecher of the Washington Department of Fish and Wildlife. Appendix B provides the results of a stakeholder review of the rearing report, responding to comments from Pacific Gas and Electric, Greg Pasternak of the University of California, Davis, and the Yuba County Water Agency.

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) the use of cover and adjacent velocity criteria; and 3) the use of a two-dimensional hydraulic and habitat model, instead of the Physical Habitat Simulation system (PHABSIM).

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.

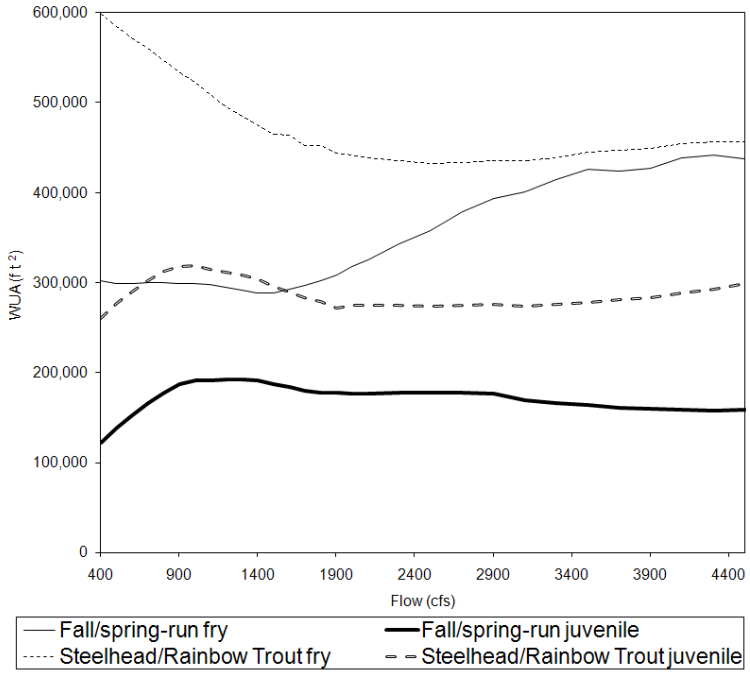

Figure 1. Fall/spring -run Chinook salmon and steelhead/rainbow trout fry and juvenile flow-habitat relationships above Daguerre Point Dam. The flows with the maximum and fall/spring-run Chinook salmon fry and juvenile and steelhead/rainbow trout fry and juvenile habitat were, respectively, 4300 cfs, 1300 cfs, 400 and 1000 cfs.

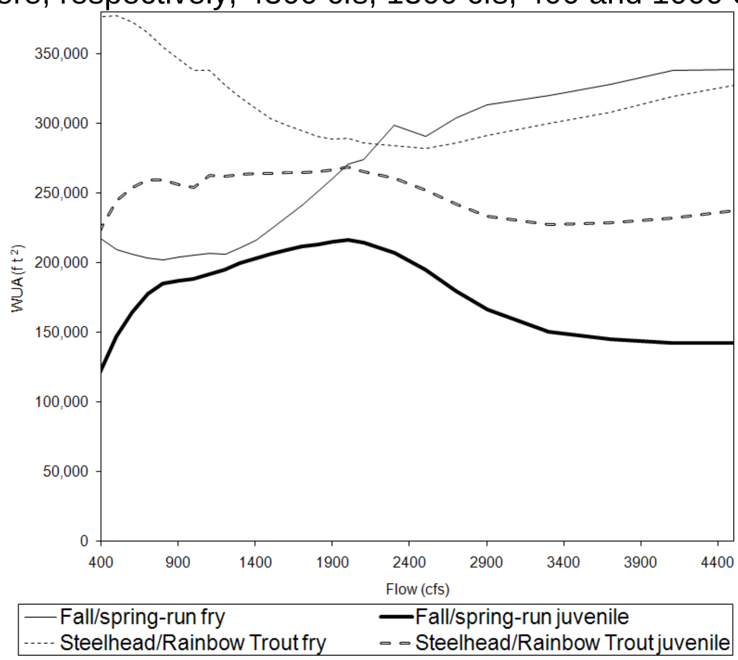

Figure 2. Fall/spring -run Chinook salmon and steelhead/rainbow trout fry and juvenile flow-habitat relationships below Daguerre Point Dam. The flows with the maximum and fall/spring-run Chinook salmon fry and juvenile and steelhead/rainbow trout fry and juvenile habitat were, respectively, 4500 cfs, 2000 cfs, 500 and 2000 cfs.

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
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 (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)
6/13/07	YRTWG – update on IFIM studies (S. Schoenberg)
2/12/08	YRTWG – update on IFIM studies (M. Gard)
6/8/08	YRTWG – update on IFIM studies (M. Gard)
9/12/08	Draft rearing report provided to stakeholders for review and comment
9/18/08	YRTWG – update on IFIM studies (S. Schoenberg)
12/10/08	Comments from PG&E on draft rearing report
12/11/08	Comments from Greg Pasternak (UC Davis) on draft rearing report
1/14/09	Comments from Yuba County Water Agency on draft rearing report
1/27/09	YRTWG – update on IFIM studies (M. Gard)
1/28/09	Meeting with stakeholders on draft rearing report
1/21/10	YRTWG – update on IFIM studies (M. Gard)
2/4/10	Meeting with stakeholders on draft rearing report

References

Beak Consultants Inc. 1989. Yuba River fisheries investigations, 1986–88. Appendix B. The relationship between stream discharge and physical habitat as measured by weighted usable area for fall run Chinook salmon (*Oncorhynchus tshawytscha*) in the Lower Yuba River, California. Prepared for State of California, Resources Agency, Department of Fish and Game.

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.

Appendix A
Response-to-Comments Document

for the

August 2008 Peer-Review Draft of the
Yuba River Rearing Instream Flow Study Report

October 2010

AUTHORS RESPONDING TO COMMENTS

U.S. Fish and Wildlife Service

Mark Gard

PREFACE

This document contains the comments provided by scientific peers on the August 2008 draft of the report, "Flow-habitat relationships for spring and fall-run Chinook salmon and steelhead/rainbow trout rearing in the Yuba River" (Report), and responses to those comments. This compilation is divided into subject-matter sections whereby various comments and responses to authors were organized. To the extent that individual comments crossed over subject matters, the authors collectively addressed those comments.

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 rearing in the Yuba River, based on the best available scientific information. The authors have reviewed their responses and compared them to the final Report to ensure that all comments have been adequately addressed.

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

TABLE OF CONTENTS

LIST OF ACRONYMS

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

GENERAL COMMENTS

<u>Hal Beecher</u>

Comment 1: Interpretation of instream flow study results depends on the ecological context. To what extent is habitat a limiting factor? To what extent is some other part of the hydrologic regime acting to limit a population (e.g., pulse flow to stimulate migration, flow to move migrants past predators, flow to ensure channel form is favorable, flow that scours redds or abrades gills, flow that results in unfavorable water quality)? If a population is depressed, is flow the problem? Is it a contributing problem? If a population is greatly under-seeded because of problems elsewhere (e.g., marine overharvest, pollution), instream flow is still needed to provide habitat for recovery. Instream flows should address the desired recovered population size because prior appropriation of water rights does not easily allow for adjusting flows upward to accommodate recovery once water rights have been allocated. Thus, it may not be reasonable to expect a population to track habitat or flows if the population is being depressed by other factors. If readers assume a tight link between flows and fish population, rather than a ceiling on the population imposed by flows through habitat, then readers may wrongly discredit the significance of the study and resulting management recommendations.

Response: An assumption that physical habitat is the limiting factor is true of all instream flow studies. To our knowledge, the data needed to assess: 1) to what extent habitat is a limiting factor; 2) to what extent some other part of the hydrologic regime is a limiting factor; and 3) if flow is the problem or a contributing problem for a population being depressed, does not exist. For example, information is lacking to be able to determine if doubling the amount of rearing habitat would double the salmonid populations. We agree with the commenter’s statements regarding needs of instream flow to provide habitat for recovery and address the desired recovered population, and the reasonableness of populations tracking habitat or flows. We have added text to make it clear that when other factors limiting the population are alleviated, flows may act by placing a ceiling on the population.

Comment 2: As part of ecological context, it would be very helpful to display hydrographs. Ideally, these would include both recent hydrographs and pre-project or reconstructed natural hydrographs.

Response: A description of the historic and contemporary hydrology of the Yuba River watershed, including displaying both recent hydrographs and pre-project or reconstructed natural hydrographs, will likely be developed as part of the relicensing of the Yuba River hydroelectric project.

Comment 3: This study uses a 2-dimensional hydraulic model (River2D) and associated habitat modeling capabilities to evaluate habitat in a relatively large California river, the Yuba. Any instream flow modeling effort attempts to simulate hydraulic conditions at a scale that is relevant to the fish (or other organism or value or use). It also attempts to model habitat quality and/or quantity at a relevant scale. Ideally (but infrequently) seasonal differences in activity due to temperature are incorporated. Two-dimensional (2-D) instream flow models are newer approaches to hydraulic modeling, incorporating a high density of points where habitat conditions are simulated. Algorithms for 2-D models were reported to be more accurate, leading to a better match between real velocity vectors and those in the models. In an Alberta stream, results of a 2-D model compared favorably with the one-dimensional PHABSIM (Waddle, et al., 2000). Habitat can be modeled using more relevant features of the channel and hydraulics than the relatively simple depth, velocity, and substrate typically used in older one-dimensional instream flow models, such as PHABSIM.

Response: Our intent was to use the most accurate modeling approach available.

Comment 4: My review of this study report centered on several questions:
Did study sites represent the river, including flow-sensitive areas?
How well does the hydraulic model reflect what the Yuba River is doing?
How well do the habitat suitability criteria reflect fish behavior and habitat sensitivity?
Given any unsatisfactory results, were reasonable decisions made?
Were the interpretations reasonable for managing stocks that are not thriving?

Response: These are appropriate questions to consider in reviewing the report.

Comment 5: The Yuba River is a large river and it poses some challenges for instream flow modeling. Those challenges include sampling in deep water and strong currents. They also include the question of whether 2-D is good enough for fish that exist in a 3-dimensional habitat. In small rivers and streams, a single velocity to represent a water column may work reasonably well (Beecher et al. 1995, 2002), but is this still true in water that is 2+ m in depth.

Response: We agree that sampling in deep water and strong currents poses challenges for instream flow modeling. While three-dimensional models could offer a better representation of fish habitat than a two-dimensional model, three-dimensional models are not yet at the stage of development where they can be used for instream flow studies.

Comment 6: I was struck by the organization; it was a finer-scale organization than I prefer. Specifically, many elements (site selection, hydrology, hydraulic modeling, habitat suitability criteria development and testing) that I would prefer to see in Methods were divided among Methods, Results, and Discussion. Another more satisfactory (to me) organization would be to have chapters: hydraulic modeling, habitat suitability criteria development and testing, and finally, instream flow modeling. I believe one of these organization schemes would make the report easier to read.

Response: We have patterned the format of our reports as closely as possible to that of peer-reviewed journal articles. In this regard, we note that a peer reviewer from the first peer review of the Yuba spawning report stated "All information presented, including data, in the methods section that is actually a result should be extracted and discussed in the Results section." As a result, we have placed all data in the results section for the Yuba rearing report as well. Given the length of the material, we feel that a fine-scale organization was required, and that an organization by chapters of hydraulic modeling, habitat suitability criteria development and testing, and instream flow modeling would cause confusion for the reader.

Comment 7: A minor concern was some switching back and forth between metric and imperial units.

Response: We changed Tables 2 and 6 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 8: Representation of the River
The study used a number of study sites that reflected a variety of habitat types in the Yuba River. While I am unfamiliar with the Yuba River, the approaches described in the report left me with the sense that Dr. Gard and colleagues had done a fairly rigorous site selection, an important first step in such a study.

Response: No response required.

Comment 9: Hydraulic Model Performance
Hydraulic model performance should, in my view, reasonably represent the distribution of depth and velocity co-distributions in the river at the range of flows studied. (Not all instream flow biologists agree with my views on hydraulic model calibration. My friend and colleague, Tom Payne [Thomas R. Payne and Associates, Arcata, CA] believes that the hydraulic model represents the river more generally, so it need not match details of a specific site too closely, provided reasonable measures have been taken to ensure that the model performs in a hydraulically realistic manner.) It appears that Dr. Gard has a similar philosophy about hydraulic model calibration to mine because he and his colleagues evaluate how well their models match depths and velocities measured at the sites.

Response: We agree that hydraulic model performance should reasonably represent the distribution of depth and velocity co-distributions in the river at the range of flows studied.

Comment 10: *Depth and Water Surface Elevation Calibration*
One of the supposed advantages of 2-D models over PHABSIM is that 2-D models are supposed to allow uneven water surfaces in response to roughness and surrounding conditions in a way that should mimic real rivers. PHABSIM assumes a horizontal water surface elevation across a transect. Real rivers and streams usually deviate from that assumption on some cross-sections, and modelers are challenged to accommodate those deviations in a way that is realistic for fish habitat over a range of flows. The errors that the authors found in water surface elevation (WSEL) indicate that River2D is no guarantee of perfect WSEL modeling. I don't know if it was better than PHABSIM would have done because there was no comparison (this is not

intended as a criticism because the authors made a decision on a modeling approach and it was not their intent to evaluate the two models against one another; in fact, they integrated limited PHABSIM use into their model).

Response: We used River2D because of its improved modeling characteristics over PHABSIM as noted above by the commenter. We assume that a comparison of River2D and PHABSIM in this case would have shown that River2D was better than PHABSIM, since both measurements and River2D simulations showed considerable across-channel variations in WSELs. Comparisons of River2D and PHABSIM are problematic because they can only be compared for portions of rivers where PHABSIM can be used (Gard 2009).

Comment 11: In deep water, errors in WSEL are inconsequential for fish habitat if there is not clear avoidance of deep water (although the present study suggested there may be some depth avoidance at very deep depths). Errors in WSEL in shallow water, near the lower limit of fish depth preference or tolerance, can lead to modeling errors. The authors identified such cases. For example, River2D predicted a site to be dry that had Chinook or steelhead fry (p. 61 and Appendix M), but it is not clear what is the magnitude of the stage error. Likewise, one juvenile steelhead occupied location was predicted to be dry (p. 66). I believe that some such error is tolerable, particularly in a large river at a moderate flow (as opposed to extremely low flows). Misleading conclusions are more likely if there are substantial WSEL errors at extremely low flows or extremely high flows; at intermediate flows WSEL errors may lead to minor transpositions of suitable modeled habitat laterally, rather than miscalculating the quantity and quality of habitat.

Response: We attribute the errors in predictions of fish at dry locations to errors in bed elevations, rather than stage errors. Based on the River2D calibration, we feel that WSEL errors were minimal and did not affect the overall conclusions of the report as noted above by the commenter.

Comment 12: My conclusion is that the authors made a reasonable effort to model and evaluate their models for depth simulation. Although improvement in depth modeling might be possible, as the authors discussed, they have probably reached a point of diminishing returns and any further improvements would cost much effort.

Response: In addition to having a low benefit to cost ratio, further improvements to the model, which would require collecting additional bed topography data, are not possible given the changes to the Yuba River topography caused by high flows that occurred after the data collection for this study was completed.

Comment 13: *Velocity Calibration*
As mentioned above, 2-D modeling in a 3-D environment becomes more uncertain as the third dimension (vertical or depth) becomes greater in big rivers. There are no obvious advantages of 2-D over 1-D (PHABSIM) in deep rivers for velocity simulation.

Response: While three-dimensional models could offer a better representation of fish habitat than a two-dimensional model, three-dimensional models are not yet at the stage of development where they can be used for instream flow studies. We disagree with the commenter regarding advantages of 2-D over 1-D in deep rivers for velocity simulation. Specifically, River2D has the potential to model velocities over a range of flows more accurately than would PHABSIM because River2D takes into account upstream and downstream bed topography and bed roughness, and explicitly uses mechanistic processes (conservation of mass and momentum), rather than Manning's Equation and a velocity adjustment factor (Leclerc et al. 1995).

Comment 14: Use of Acoustic Doppler Depth Profiler (ADCP) simplifies the field work for obtaining velocities in deep water (>1 m). It may provide data for evaluating the vertical variation in velocity and the biological significance of that vertical variation. Indeed, the authors have used the concept of adjacent velocity in a vertical dimension in their habitat suitability criteria and habitat modeling. Their use of ADCP appears consistent with other ADCP uses I have reviewed (I have been in the boat and watched, but have not operated it nor modeled with it myself).

Response: Although we agree that use of ADCPs simplifies fieldwork for obtaining velocities in deep water, and can provide data for evaluating the vertical variations in velocity, we have not been able to take advantage of this capability because two-dimensional models are based on depth-averaged velocities. The commenter is incorrect that we used the concept of adjacent velocity in a vertical dimension – rather, we used adjacent velocity in a horizontal dimension by selecting the highest depth-averaged velocity within 2 feet of occupied and unoccupied locations in deep water to use in developing the adjacent velocity habitat suitability criteria. We have clarified the text in the report in this regard.

Comment 15: Habitat Suitability Criteria
Habitat suitability criteria (HSC) were one of the most sensitive aspects of this or any instream flow habitat model. The authors made diligent efforts to develop high quality HSC, but were thwarted by low numbers of fish. Their decision to use groups of fish rather than individuals has been a discussion point in many instream flow studies. I have seen one case where use of individuals was clearly wrong, but where densities appeared low and fish were not crowded, habitat was likely better or at least good where a group of fish was found. Considering the small sample size they achieved, it might have been informative to develop HSCs both ways, for individuals and for groups. If results converged or were well supported in the validation, that might have given more support to their end results.

Response: As noted in the discussion, we considered an alternative that would have used fish densities rather than individuals, but did not use it for the following reasons: 1) we had low confidence in the accuracy of our estimates of the number of fish in each observation; and 2) while it is reasonable to assume that a school of fish represents higher quality habitat than one fish, it is probably unreasonable to assume that, for example, 100 fish represents 100 times better habitat than one fish. The same reasons would be applicable to the commenter's suggestion to use individuals to develop HSCs.

Comment 16: Did all variables (depth, velocity, adjacent velocity, cover) contribute to habitat suitability? It did not appear that such tests were conducted. Multivariate criteria seem not to have been considered. Bivariate criteria were rejected, but there may be some simple thresholds (e.g., juvenile steelhead in Washington streams seem to have an almost absolute threshold of 0.5 ft depth before water is usable).

Response: The statistical tests associated with the logistic regressions confirmed that all variables (depth, velocity, adjacent velocity, cover) contribute to habitat suitability. Multivariate (including bivariate) criteria are rarely used in instream flow studies and thus we did not consider them here. The commenter is incorrect that we rejected bivariate criteria – rather we rejected binary criteria. We observed the same threshold as the commenter for juvenile steelhead depth utilization. More generally, we address thresholds by truncating the HSC at the slowest/shallowest and deepest/fastest ends, so that the next shallower depth or slower velocity value below the shallowest observed depth or the slowest observed velocity had a SI value of zero, and so that the next larger depth or faster velocity value above the deepest observed depth or the fastest observed velocity had an SI value of zero.

Comment 17: Were Reasonable Decisions Made in cases where Results were Less than Desired?
Given the poor resolution of HSC and the velocity simulations that appeared underwhelming, how usable is the model? Big rivers are usually challenging. In a study on Washington's Skagit River (about 10,000 cfs mean annual flow), very little difference in habitat showed up over a wide range of flows (for details, contact Phil Hilgert, R2 Resources Consultants, Redmond, WA). More telling was connection of side channels, which was a relatively straightforward metric that was relevant for rearing salmonids. Another analysis concerned the varial zone, the zone where much young-of-the-year rearing occurs that is exposed at lower flows. If mid-channel areas are used by fish, then simpler metrics may be available for evaluating flows. Sometimes it is worth using a number of different metrics, not just WUA, and looking for convergence or looking for most protective.

Response: We feel that the resolution of the HSC and velocity simulations were adequate, and thus that the model is usable for purposes of developing recommendations for instream flow needs for anadromous fish in the Yuba River. As shown in Figures 25 to 32, this study differed from the Skagit River study in showing considerable difference in habitat over a wide range of flows. We address connection of side channels in another report, which examines the effect of flow fluctuations on redd dewatering and juvenile stranding. Accordingly, we do not consider it necessary to address connection of side channels in this report. Analysis of the varial zone is generally only relevant for evaluating ramping rates for hydropeaking facilities, and thus is not called for in this report, since the Yuba River is not operated for hydropeaking. While we did observe some mid-channel areas used by fish, such use was limited primarily due to high velocities and lack of woody cover. We do not feel that simpler metrics, such as connection of side channels, would not address the biological needs of the fish, as reflected by the habitat suitability criteria, such as preference for woody cover.

Comment 18: This study struck me as a lot of very good work that resulted in models that did not inspire high levels of confidence, particularly in the face of potentially competing study results. I believe at this point what is known about the ecology of Yuba River salmonids should be summarized and reviewed for flow limitations that are subject to a simpler analysis.

Response: While we acknowledge that there are uncertainties in the flow-habitat relationships resulting from the models, we feel that the models are adequate for purposes of developing recommendations for instream flow needs for anadromous fish in the Yuba River. The results of this study are based on methods intended to provide a more accurate assessment of the relationship between flow and anadromous salmonid fry and juvenile rearing habitat compared to an earlier study, namely 1) HSC generated only from use data in the earlier study, as opposed to the criteria generated with logistic regression in this study; 2) the lack of use of cover or adjacent velocity criteria in the earlier study; and 3) the use of PHABSIM in the earlier study, versus 2-D modeling in this study. We believe that the report contains sufficient information about the ecology of Yuba River salmonids to develop flow recommendations. We do not feel that simpler metrics, such as connection of side channels, would not address the biological needs of the fish, as reflected by the habitat suitability criteria, such as preference for woody cover.

Comment 19: In big rivers, I think modeling that is 3-dimensional is needed. When snorkeling in the Skagit River, I have observed much fish use in deep water where surface velocities might have been unsuitable for rearing fish. Models that are 1- or 2-dimensional model velocity at a single point in the water column that may not be related to the velocity where fish hold in a big river.

Response: While three-dimensional models could offer a better representation of fish habitat than a two-dimensional model, three-dimensional models are not yet at the stage of development where they can be used for instream flow studies.

Comment 20: Given the challenges discussed in the previous section, it is not clear that the study results were a strong support for flow recommendations. Doing a study to support flow recommendations is not the ideal approach, which is the impression given on page 100: "The results of this study are intended to support or revise the flow recommendations in the introduction." However, I could not find such an incriminating statement in the introduction.

Response: Our study is intended to provide some of the scientific information that is needed to determine instream flow needs for anadromous fish in the Yuba River. The purpose of our study was to develop flow recommendations; as such, there are two options for the results – they could either support or be used to revise previous flow recommendations. The flow recommendations in the introduction were "improved flows for all life history stages of Chinook salmon and steelhead." Thus, the flow recommendation were qualitative rather than quantitative, which is likely why the commenter was unable to find the flow recommendations in the introduction.

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: The disadvantages of biological response correlations and demonstration flow assessment basically resulted in these methods being infeasible to use. Habitat modeling is then left as the only available method. 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 - Is the data adequate?

Comment 3: The data are what they are. In some cases they had larger sample sizes than in others. The sample sizes in the Rearing Report were quite small and likely problematic for model development.

Response: We acknowledge that the sample sizes for the occupied locations used to develop habitat suitability criteria for juvenile Chinook salmon and steelhead/rainbow trout were small. We would characterize the consequences of the small sample sizes as increasing the uncertainty in the resulting flow-habitat relationships, rather than characterizing them as being problematic for model development.

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.

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

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 clarifed 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.

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

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: We added a scale, flow direction indicator and north arrow to the figures in the text and appendices that illustrate habitat suitability. For Figure 34, which already showed flow direction, we only added a scale and north arrow.

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 33, 38 to 54 and 56 to 60.

Comment 8: In <u>many</u> 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, Figure 2 in Clear Creek Spawning and Figures 2 and 3 in Yuba Flow Fluctuation.

Response: The Yuba rearing report did not have any instances where Y axes were not scaled the same.

Miscellaneous comments:

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 6 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. As I read the discussions in each report, there seems to be no anomaly or discrepancy between the model and measured values that the authors can’t explain away or declare as unimportant. And when, for example, the given criteria for acceptance were not met, the authors conclude that is acceptable anyway (e.g., section 3.1 p 67: Rearing report).

Response: It is important to distinguish between the numeric criteria that were used to evaluate model performance and the text that was used to elaborate on the model performance. The numeric criteria are specific and precise. For example, for 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 feel that the commenter is misinterpreting the intent of the discussion. 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 these anomalies or discrepancies away. Likewise, 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 study is 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 results as unique, since we have used the same methods for other studies on habitat use by Chinook and steelhead. The depth and 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: 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 $R = Vel_n/(Vel_{n-1} + Vel_{n+1})/2$ 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 fish were present versus locations where fish 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 D, E, G, H, and J. 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 rearing 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. As with the spawning counterpart study, one could argue that the study design is not ambitious scientifically. Again, the authors make little attempt to identify any really compelling and interesting unanswered scientific questions, nor do they pose any hypotheses that they then set out to test. As with before, 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.

Response: The intent of the study was to apply well established techniques to the Yuba River to quantify flow-habitat relationships for anadromous salmonid rearing. 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 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. Again, 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. Unlike the spawning study, for this rearing study I was not provided with evidence of past review of the methods. The ecohydraulic modeling used should not be expected to perform as well for rearing as it did for spawning. The methodological/results discussion on page 90-92 is very enlightening, and I like how the authors have approached this (particularly the adjacent velocity concept). I think that the model is applied at too coarse of a resolution with too coarse of a topographic input to do the rearing model justice.

Response: See responses below regarding the summary of other techniques and the topographic data quality. The rearing study did not have a past review of the methods. We know of no reason why the ecohydraulic modeling used would not be expected to perform as well for rearing as it did for spawning, and no insight is provided in the comment above. We applied the model at the highest possible resolution, given constraints on computer run speeds and memory. Similarly, we collected as dense a topographic input as possible, given the constraints of the equipment we had available at the time and the amount of time we had available to collect topographic data. While we acknowledge that a finer level of resolution and finer level of topographic input would have improved the rearing model, we feel that the level of resolution and topographic input is adequate for purposes of simulating rearing flow-habitat relationships.

Data. Is the data adequate?

Comment 3: The authors should again 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 again 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 was unclear why the habitat mapping (example shown in Figure 33) was not included in the appendix and instead only summarized in tabular form.

Response: We feel that the tabular summary of the habitat mapping is sufficient for purposes of this report, and that the graphical display of all of the habitat mapping would have added too much material to an otherwise already very long report. The graphical display of all of the habitat mapping is really only useful in electronic format (i.e., GIS shapefiles).

Comment 5: I see the topographic data is generally of much higher resolution in this study then it was in the spawning study. That is critical! However, I'm still unclear about the mesh construction and grid resolution relative to this data resolution.

Response: We added information on mesh construction and grid resolution under *Hydraulic Model Construction and Calibration* **RIVER2D Model Construction**.

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

Comment 6: The habitat mapping discussion is way too brief and needs to be elaborated. As with the spawning report, there is a tendency to throw out unsubstantiated conclusions (e.g. 'We decided that the multiple regression… was acceptable'), without explaining why.

Response: We have added additional text to the habitat mapping discussion. We have reviewed the conclusions in the report to ensure that the conclusions are substantiated by explanations. For example, the conclusion cited by the commenter was substantiated by the following explanation that was given in the subsequent sentence: "Specifically, the maximum difference between measured and simulated WSELs of 0.11 feet (0.033 m) was much less than the maximum difference with *IFG4* and *MANSQ*, and reflected the additional errors implicit in predicting WSELs from two different flows (from the Yuba and Feather Rivers), versus Predicting WSELs from only one flow. "

Presentation. Is the presentation clear?

Comment 7: The presentation of the report is consistent and the layout of this report is logical by itself. The introduction could provide a little broader scientific and management context (rather brief as it stands). The lack of conclusion section is a little odd and leads to an abrupt ending after the discussion.

Response: The introduction is intended to provide sufficient 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). We have added a conclusion section to this report.

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

Comment 8: My same general comments about the figures in the spawning report apply to this report. Namely, the figures are adequate, but overall are of poor cartographic quality, exhibit inconsistent font sizes, and lack some basic information (e.g. north arrows, scale bars, flow directions, flow rates, and informative captions within the appendix). At least the inadequacies are relatively consistent. Again, the downsampling of the figures for the PDF has resulted in some very poor text and image quality. I think a big part of the problem is that most of the figures are just screen captures from River2D instead of really making bespoke figures in a GIS and/or vector graphics drawing package. The legends are subsequently difficult to read for all the model screen shots and the lack of scale bars and north arrows is unacceptable. The tables are generally fine. The distinction between what figures are shown in the report and what are shown in the appendix appears a little arbitrary.

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. The commenter is correct that most of the figures are screen captures from River2D. We have chosen to use screen captures from River2D rather than creating figures in a GIS or vector graphics drawing package to keep the figures consistent with how data is displayed in River2D.

Miscellaneous comments:

Comment 9: I found the text here really tedious and repetitive when compared with the spawning report. Most of it has been copied verbatim and I fail to see why these different scopes of work needed to be separated out into different reports. It would be much better to have one front-end, and then use chapters to describe separately the different aspects (e.g. rearing, spawning, spawning sensitivity analysis, and flow fluctuation analysis).

Response: Much of the text of the rearing report, particularly in the methods, has been copied directly from the spawning report. However, most of the results and appendices present completely different information than the spawning report. We feel that combining all results into one overall report would be too voluminous.

REVIEWER #3

Study Design Is the study design sound?

Comment 1: The study design is essentially sound. However, I have some problems with the authors' distribution of their level of effort (especially in regards to biological verification, more below). The authors assumed that juvenile physical rearing habitat was limiting production of both species/life stages. This assumption was never justified or discussed. They also assumed that habitat quality for these species/life stages could be adequately characterized by depth, velocity, adjacent velocity, and cover. See comments below regarding adjacent velocity.

Response: Our primary goal was to develop flow-habitat relationships. Biological verification necessarily falls out as a lower priority, and thus leads to less level of effort for this task than developing the relationship between discharge and available habitat. In addition, development of the hydraulic models and habitat suitability criteria, which dominated the effort, are necessary to be able to conduct biological verification. An assumption that physical habitat is the limiting factor is true of all instream flow studies. To our knowledge, the data needed to test this assumption does not exist. We believe that it is a reasonable assumption that habitat quality for fry and juveniles Chinook salmon and steelhead can be adequately characterized by depth, velocity, adjacent velocity, and cover because these variables are linked to the biological needs (bioenergetics and predation avoidance) for these species and life stages. See responses below regarding adjacent velocity.

Methods Are the methods technically sound?

Comment 2: For the most part, the methods appear to be sound. I looked harder at the biological verification and habitat suitability criteria sections than I did at the 2D model development. I think the use of the adjacent velocity parameter, while a good biologically-based concept, may have been misused in this work. Using a hydraulic model output to try to incorporate very small scale habitat features such as the relationship between a holding position (assuming this is the type of location where most fish were observed) and areas where fish may make short duration feeding movements is problematic. It appears the authors selected the highest nearby (within 2 feet) velocity as the adjacent velocity – this may bias the WUA results toward higher discharges – and may help explain the huge differences between their results and those presented by Beak et al. (1986). The authors' lengthy defense of their use of River2D over PHABSIM is unnecessary.

Response: The habitat features that we were representing by the adjacent velocity parameter were not the relationship between a holding position and areas where fish may make short duration feeding movements. Rather, the adjacent velocity parameter represented the transport of invertebrate drift from fast-water areas to adjacent slow-water areas where fry and juvenile salmonids reside via turbulent mixing. While we acknowledge that there are likely errors in the hydraulic simulation of this type of habitat feature, we feel that imperfect modeling of this habitat feature results in a better representation of fry and juvenile habitat than ignoring this critical habitat feature. Since the amount of invertebrate drift transported is expected to be proportional to current velocity, it is reasonable to select the highest nearby (within 2 feet) velocity as the adjacent velocity. Rather than the adjacent velocity criteria biasing the WUA results toward higher discharges, the criteria may allow more accurate estimation of the amount of habitat present at lower flows. The Beak et al. (1986) results may have overestimated the amount of fry and juvenile habitat at lower flows because their study did not include the consideration of adjacent velocity. Without adequate food supply via invertebrate drift from fast-water areas, fry and juvenile Chinook salmon and steelhead are unable to grow and survive, a critical aspect of their habitat requirements. We included text comparing River2D to PHABSIM because we are comparing the results of this study to an earlier study that used PHABSIM.

Data - Is the data adequate?

Comment 3: The hydraulic modeling data are adequate in most sections. However, the low correlation values for predicted versus measured velocities calls into question the overall quality of the predicted habitat data.

Response: A correlation of 0.5 to 1.0, which was the case for six of the eight sites, is considered to have a large effect (Cohen 1992).

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

Comment 4: I question the validity of the findings for reasons mentioned above and below.

Response: See responses above and below.

Presentation. Is the presentation clear?

Comment 5: The report is generally well written and edited. The level of detail is weighted heavily toward the hydraulic modeling and away from the collection use/interpretation of biological data. It seems that the authors are more interested in the tool than in the research question.

Response: We spent a considerable amount of time collecting habitat suitability data, which is the fundamental source of data to use in interpreting the biological data. Our primary research question was to develop flow-habitat relationships, which includes both the hydraulic model and the biological data used to develop the habitat suitability criteria. Developing a better understanding of the relationship between discharge and available habitat necessarily falls out as a lower priority than developing the relationship between discharge and available habitat. Based on the methods applied, we feel that we have developed a sufficient understanding of the biological data by focusing on the four key variables that are linked to biological needs (bioenergetics and predation avoidance) for Chinook salmon and steelhead fry and juveniles. The hydraulic modeling has an important role in developing flow-habitat relationships, along with the biological habitat suitability criteria.

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

Comment 6: For the most part, the tables and figures are clear and adequate. Some figures do not stand alone in terms of explanations of the units in the figures (e.g., m/s).

Response: The only location in the report where we use m/s is in Appendix H. In the figures in Appendix H, the y-axes are labeled "Velocity (m/s)." In this context, we feel that the figures stand alone in terms of explanations of the units. We note that m/s is a commonly used abbreviation in peer-reviewed scientific journal articles.

REVIEWER #4

Overall Review Comments:

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

Response: No response needed.

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

Response: No response needed.

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

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

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

Response: The amount of detail in the report reflects peer reviews of previous reports. We have added additional details in response to this peer review. Study area/site descriptions are given where needed to provide information for specific portions of the report. 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 the Yuba spawning report stated "All information presented, including data, in the methods section that is actually a result should be extracted and discussed in the Results section." As a result, we have moved all data to the results section for the Yuba rearing report as well. 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. The format of the report follows as closely as possible to that of peer-reviewed journal articles. We have added numerous details in response to the peer review of this report.

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

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

Response: The 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 needed to establish that habitat capacity for rearing is a limiting factor contributing to fish population declines does not exist. For example, information is lacking to be able to determine if doubling the amount of rearing habitat would double the salmonid populations. 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.

Methods Are the methods technically sound?

Comment 7: 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 m^2, while Jacobson and Galat (2006) had a point density of 6 points/100 m^2. It should be noted that this study was one of our earlier River2D studies and that we have been using higher point densities in more recent studies to try and improve the hydraulic predictions of our River2D models. We have been able to use higher point densities in our more recent studies because our new equipment (robotic total station and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 m^2) within our time constraints for data collection. 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) for six of the eight sites. 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 rearing habitat.

Comment 8: The hydraulic modeling efforts in these studies are primarily focused on predicting local hydraulics at the scale of individual redds (or fish locations). In these cases, hydraulic modeling research has shown that the computational mesh and topography resolution (density of computational nodes and density of topographic data, respectively) should be similar to the spatial scale and resolution at which the hydraulic predictions are being applied (i.e., redds and fish locations in this study). The density of riverbed elevation data, and subsequent mesh resolution, for these studies appear to be too sparse to accurately model local hydraulics at the scales of interest. Similarly, the application of a constant friction coefficient (roughness) across the model domain, as used in these studies, contributes to poor prediction in local scale hydraulics. The comparisons of measured vs. modeled velocities in these studies demonstrate poor model performance (and plots of measured vs. modeled velocity vectors are not provided).

Response: We used as fine a computation mesh as possible given constraints on computer run speeds and memory. The topographic point densities fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 m^2, whereas Jacobson and Galat (2006) had a point density of 6 points/100 m^2. Accordingly, our computational mesh and topography resolution were as close as possible to the spatial scale and resolution at which the hydraulic predictions are being applied. We acknowledge that the density of riverbed elevation data, and subsequent mesh resolution will contribute to errors in modeling local hydraulics at the scales of interest, but would characterize this as increasing the uncertainty in the resulting flow-habitat relationships. The commenter is incorrect that a constant friction coefficient was applied 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) for six of the eight sites. It should also be noted that 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 9: 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: As noted above, we did not use a constant roughness coefficient. The model also produced suitable results at the scale of individual fish, given the limitations on model mesh and resolution discussed above.

Comment 10: 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 performs[1], 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).

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

Comment 11: 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).

[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.

Response: **T**he topographic point densities fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 m^2, while Jacobson and Galat (2006) had a point density of 6 points/100 m^2. This study was one of our earlier River2D studies and we have been using higher point densities in more recent studies to try and improve the hydraulic predictions of our River2D models. We have been able to use higher point densities in our more recent studies because our new equipment (robotic total station and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 m^2) within our time constraints for data collection. 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 our Yuba spawning study (which preceded 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 the Yuba spawning study and 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 12: 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.

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

Comment 13: 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.

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

Comment 14: 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

INTRODUCTION

<u>Hal Beecher</u>

Comment 1: Were Interpretations of Model Results Reasonable for Managing Fish Protection?
The first paragraph of this review discussed the need to explain the ecological and population context for the study. On page 2 of the report, there is a brief discussion of how habitat might affect population. Text is very general but leaves the idea that habitat could determine population trajectory. If the issue is to be raised, it would be good to discuss the concept of seeding and of allowing recovery.

Response: We have added text discussing the concept of seeding and of allowing recovery, and that habitat may not determine population trajectory.

Comment 2: Pages 2-3 contain a good concise discussion of different ways of evaluating flow effects on juveniles.

Response: No response needed.

Comment 3: On page 3, assumption 1) for rearing habitat modeling should say that physical habitat is a limiting factor rather than the limiting factor.

Response: We have made the suggested change.

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 produce a model that predicted rearing 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 accountability for the level of uncertainty in the flow-habitat relationships. The action should not be viewed in terms of success or failure, but rather in terms of the level of uncertainty of the action. A flow-habitat relationship with a high level of uncertainty would not be a failure, in terms of making it unusable, but rather should be viewed within the context of needing to make decisions about flow regimes with imperfect data. The action also needs to be evaluated within the context of alternative sources of information that could be used to make decisions about flow regimes – if the action has less uncertainty than other sources of information, it would be appropriate to use that action to make decisions about flow regimes.

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 rearing habitat is limiting and that increasing the availability of such habitat will indeed increase salmon populations does not exist. For example, information is lacking to be able to determine if doubling the amount of rearing habitat would double the salmonid populations. 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.

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 (tasks), but I like again how the authors have clearly defined the objective associated with each task. There is little context about an overall aim of the study. The first paragraph in the introduction sets up some generic context (verbatim from previous report). However, a more clearly defined aim might help. I can't really see why the rearing and spawning portions of the study were separated into two separate reports.

Response: We added this information in response to a stakeholder comment that we specify task objectives. Although the first nine 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. The rearing and spawning portions of the study were separated into two separate reports because a combined report would not have been practical, given how voluminous each of the two reports is.

REVIEWER #3

Objectives Are the objectives clear?

Comment 1: No. The first 10 (of 11) objectives are methods, not objectives.

Response: We added this information in response to a stakeholder comment that we specify task objectives. While we agree that the first nine objectives are methods, within the scope of the entire report, we feel that 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

Objectives Are the objectives clear?

Comment 1: 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 objectives are intended to support the need of the flow-habitat study, namely 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 2: 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 3).

Comment 3: 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. 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.

Comment 4: 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 5: The focus on rearing habitat is unfounded, because rearing 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 rearing habitat.

Response: To our knowledge, the data needed to evaluate whether rearing habitat is limiting and that rearing habitat is contributing to fish population declines does not exist. For example, information is lacking to be able to determine if doubling the amount of rearing habitat would double the salmonid populations. The preface is intended to explain the focus on rearing habitat, specifically that this report is to provide scientific information to assist in developing instream flow needs for the Yuba River. Flow-habitat relationships, including those involving rearing habitat, are the standard method to determine instream flow needs.

Comment 6: 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 7: The previous narrative identified the focus on rearing habitat, while here there is mention of all life stages, and the next paragraph goes back to rearing habitat. This is distracting to the reader, and more clarity should be provided by limiting the narrative to the rearing life stage.

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

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 rearing habitat and population change -- especially given that rearing 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 introducing the need/explanation/content of a conceptual model. We have added references that support the strength of the described link between rearing habitat and population change, and that the relationships in this conceptual model are meaningful. To our knowledge, the data needed to evaluate whether rearing habitat is limiting and that rearing habitat is contributing to fish population declines does not exist. For example, information is lacking to be able to determine if doubling the amount of rearing habitat would double the salmonid populations.

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 rearing 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. snorkel surveys and screw traps), demonstration flow assessments and habitat modeling.

Comment 11: 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.

METHODS

APPROACH

REVIEWER #1

Comment 1: 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 #4

Comment 1: 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 describe where this study occurred. We feel that the section on *Study Segment Delineation*, which we believe to be what the commenter is referring to regarding the narrative of segments and reaches, most properly belongs in the Methods section, since it describes the methods that were used to delineate the study segments.

Comment 2: 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 3: 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: We feel that 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 location.

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

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

FIELD RECONNAISSANCE AND STUDY SITE SELECTION

REVIEWER #3

Comment 1: On page 7 site selection is said to be 'randomly selected' and 'stratified' in the same sentence. Which was it? It does not appear that site selection was really random – as the authors explain how they moved transect boundaries to accommodate the modeling (p. 8).
Response: The sites were selected based on a stratified random selection method, where we randomly selected a habitat unit, out of all of the habitat units of that habitat type, for each habitat type which was not adequately represented in the spawning sites. We have clarified the text in this regard. With regard to moving transect boundaries to accommodate modeling, it

should be noted that in most cases where the boundaries were moved, the additional river area included in the site was not used to model habitat. We think that it is still reasonable to consider the sites to be randomly selected even if transect boundaries were moved, since in all cases as much as possible of the habitat unit that was randomly selected was modeled.

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) was assigned 1 of the 10 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 #3

Comment 1: Table 3 (p. 9) are these the manufacturer’s specs – or were calibration data collected?

Response: These are the manufacturer’s specifications. Calibration data was not collected.

Comment 2: Did they overlap in collection of velocity data where possible to determine whether one methods was consistently biased in one direction? For example, did they use the ADCP and the Marsh McBirney in overlapping areas where both systems could be operated?

Response: There were some limited areas where the ADCP and Marsh McBirney were used in overlapping areas where both systems could be operated. The limited data from these areas does not seem to indicate any consistent bias in one direction for the ADCP versus Marsh McBirney data.

Comment 3: The video sled with the fixed grid method would likely lead to variable results if the sled were not kept at the constant 1 foot from the river bed. Use of lasers provides a more flexible method that would eliminate this source of error.

Response: We were able to obtain accurate results by repeatedly lowering the sled to the bottom and raising it 1 foot to obtain the substrate size. We were also able to visually identify where there were changes in substrate size because of the relatively large area displayed on the video monitors, particularly for the monitor attached to the 45 degree angle camera. When we detected a change in the substrate size, we lowered the sled to the bottom and raised it 1 foot, while the boat held position, to obtain the changed substrate size. Use of lasers would have required having an electric winch to adjust the height of the video sled. We needed to raise and lower the winch manually to prevent the sled from getting hung up on obstructions. Thus, we would not have been able to use lasers in this application.

Comment 4: No work was done above the narrows – is there rearing habitat in this area? The authors should discuss how this truncation may have affected the results of interpretation of them.

Response: There is rearing habitat in the area above the Narrows. We do not feel that the lack of sites above the Narrows affected the results or interpretation of the results because we were able to have a site at the upstream end of the Narrows which represented the habitat types present upstream of the Narrows.

Comment 5: On page 12, the authors say data was collected at 'enough points'. What does this mean? How many points were required to assess bed topography, substrate composition, and cover…?

Response: The statement that data were collected at enough points is a qualitative assessment that we could accurately linearly interpolate bed topography and determine substrate composition and cover between the points at which data were collected. It is not possible to determine how many points were required to assess bed topography, substrate composition and cover, other than that higher point densities would result in a more accurate assessment of bed topography, substrate composition and cover. We note that the topographic point densities fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 m^2, while Jacobson and Galat (2006) had a point density of 6 points/100 m^2. It should be noted that this study was one of our earlier River2D studies and that we have been using higher point densities in more recent studies to try and improve the hydraulic predictions of our River2D models. We have been able to use higher point densities in our more recent studies because our new equipment (robotic total station and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 m^2) within our time constraints for data collection. 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 our Yuba spawning study (which preceded 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 the Yuba spawning study and this study had a large effect in improving our assessment of bed topography.

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. We note that the topographic point densities fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 m^2, while Jacobson and Galat (2006) had a point density of 6 points/100 m^2. It should be noted that this study was one of our earlier River2D studies and that we have been using higher point densities in more recent studies to try and improve the hydraulic predictions of our River2D models. We have been able to use higher point densities in our more recent studies because our new equipment (robotic total station and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 m^2) within our time constraints for data collection. 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 our Yuba spawning study (which preceded 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 the Yuba spawning study and 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 a table to the Results section under *Hydraulic and Structural Habitat Data Collection* providing a summary report of the vertical benchmark surveys, and added text in this section on the standard we used for vertical benchmark surveys. The data files no longer exist to generate a summary report of the horizontal benchmark survey, nor can such data be generated from the survey data available. 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: See comments in the review of USFWS (2008), as all of those comments pertain to this report as well.

Response: We reviewed the commenter's comments on the Yuba Spawning study [USFWS (2008)] and did not find any comments that the commenter did not make on this report.

Comment 5: 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). We note that ADCPs are now commonly used for measuring depths in instream flow studies, and that the U.S. 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 have successfully used an ADCP for bathymetric data collection in the past, as described in Gard and Ballard (2003).

Comment 6: 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 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."

Comment 7: 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 8: 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: We note that the topographic point densities fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 m^2, while Jacobson and Galat (2006) had a point density of 6 points/100 m^2. It should be noted that this study was one of our earlier River2D studies and that we have been using higher point densities in more recent studies to try and improve the hydraulic predictions of our River2D models. We have been able to use higher point densities in our more recent studies because our new equipment (robotic total station and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 m^2) within our time constraints for data collection. We have added scale information to the bed topography point location maps in Appendix B.

PHABSIM WSEL CALIBRATION

Hal Beecher

Comment 1: (p. 14-15) – I cannot find in the Data Collection how far apart the velocities were recorded. ADCP generally collects velocities (V) and depths (D) continuously, so the separation along the transect where the depth and velocities are actually recorded need to be set. The degree to which the R statistic standard is appropriate will depend on how far apart the readings are recorded.

Response: The spacing of the velocities varied depending on the speed of the boat and the configuration file used for the data collection. For a given configuration file, the ADCP measures a depth and velocity every xx seconds, where xx is typically in the range of 3-4 seconds. Based on a cursory review of the ACPD data, the typical separation between adjacent depth and velocity measures was in the range of 4 to 9 feet. We do not have any data to determine how to vary the appropriate range of R statistic values based on how far apart the readings are recorded. In this regard, we note that the peer-reviewed journal article in which the R statistic was presented (Gard and Ballard 2003) just has one range of R statistic values to use for quality assurance/quality control of ADCP data.

Comment 2: This section would be clearer with an introductory paragraph separating out the WSEL calibration (or stage [S]) from velocity calibration. Where velocity calibration is needed for flow (Q) determination to generate a S-Q relationship (or datum), that should be explained. A brief overview of the whole process would make it clearer: stage of zero flow (SZF) & WSEL; Q (includes V, D, and width); slope (importance for Manning's equation). Paragraph in middle of p. 16 is good – this should be moved up front in this section. Discussion of criteria for different models would be useful in a table.

Response: Since the velocity calibration portion of PHABSIM is not being used in this application, since PHABSIM is only being used to generate a stage-discharge relationship, an introductory paragraph separating out WSEL and velocity calibration is not needed. Velocity calibration was not needed for flow determination because flows for generating a stage-discharge relationship were obtained either directly from gage records, or indirectly from gage records via a flow-flow regression. We have not added a brief overview; instead we chose to rely on the existing text to explain the process. We have let stand the current organization of this section, sequentially by each step through the calibration process, as it is intended to provide a clear description of the calibration process. We have reviewed the discussion of criteria for the different models and believe that this information is best communicated in text, rather than tabular, format.

Comment 3: On the last paragraph on p. 16, I would appreciate a caveat about the acceptable range of Velocity Adjustment Factors (VAFs) – these are good for velocity simulation when it is based on a single set of velocities, but if velocity regression is used the acceptable range is much narrower.

Response: We are not aware of a narrower range of VAFs that are used for velocity regression. Since we are not using a velocity regression in this situation, we feel that it is not necessary to discuss what range of VAFs would be used for velocity regression.

RIVER2D MODEL CONSTRUCTION

Hal Beecher

Comment 1: (p. 17) **–** Discussion of ADCP traverses does not state how closely these were spaced (you have to go to Table 10, p. 32) in order to get a fairly high density of verticals typical of 2D models. I know that uniform spacing is not necessary, but it would be helpful to visualize the grids used.

Response: Information on the spacing of the ADCP traverses is given in the Methods section under *Hydraulic and Structural Habitat Data Collection*, specifically "the ADCP was run across the channel at 50 to 150-foot (15 to 45 m) intervals," so we do not repeat this information in the *River2D Model Construction* section. The information presented in Appendix C should help with visualization of the grids.

Comment 2: p. 17 **–** With Army Corps of Engineers (COE) photogrammetry, ADCP traverses add info that should improve calibration, but ADCP data may not even be necessary for modeling, except for the substrates.

Response: The COE photogrammetry data was only available for the higher elevation portions of the site. The COE bathymetry data, which covered same portion of the channel as the ADCP data, had too large a spacing to be used to develop the topography for the deeper portions of the site, in the absence of the higher-density ADCP data.

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.

RIVER2D MODEL CALIBRATION

Hal Beecher

Comment 1: (p. 18) – inconsistent use of m and ft.

Response: We were unable to determine what the commenter was referring to – page 18 had two references to foot and no references to meter. More generally, we have gone through the report and given metric equivalents to English units 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. As a result, the report now consistently uses m and ft.

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.

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.

Comment 2: An additional model calibration procedure can include comparing empirical and modeled WSEL along a longitudinal centerline of the channel; oftentimes this can help ascertain model performance within the interior of the model domain.

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

RIVER2D MODEL VELOCITY VALIDATION

Hal Beecher

Comment 1: (p. 20) – The authors use a velocity criterion for successful calibration of R $\geq$0.6 for relationship between measured and simulated velocities. At Washington Department of Fish and Wildlife (WDFW), I use a sliding scale that allows greater differences for low velocities (<1.0 fps) based on presumed fish sensitivity, but the target relationship is that the simulated velocity should be within 20% of the measured velocity. This standard is not met at every vertical, but one measure of calibration success is the percent of verticals where the standard is met. It should be noted that most of WDFW's studies are PHABSIM based on velocity regression with at least 3 velocity sets, so standards are not directly comparable.

Response: We use a similar approach to present the differences between measured and predicted velocities in Appendix H, showing the absolute difference between measured and simulated velocities for measured velocities less than 3 feet/sec, and the percentage difference between measured and simulated velocities for measured velocities greater than 3 feet/sec. As shown in Appendix H, only three of the eight sites would meet the WDFW criteria for average percent difference and none of the sites would meet the WDFW criteria for maximum percent difference. We agree that the WDFW standard is not directly comparable to the performance of two-dimensional models. Specifically, at least at flows close to those at which velocity data were collected and at locations close to the transect, PHABSIM will typically do a good job in predicting velocities, since it calculates the Manning's *n* value for each cell from the measured depth and velocity, and then calculates the simulated velocity from the Manning's *n* value (Gard 2009). In contrast, River2D does not use any measured velocity data to predict velocities. We feel that evaluating the performance of the model by the correlation between measured and simulated velocities is appropriate in the 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.

REVIEWER #3

Comment 1: A correlation of 0.6 seems pretty weak to me. The authors cite a paper to justify their acceptance of this level of agreement between predicted and measured velocities – but this seems like poor agreement to me.

Response: A correlation of 0.6 would be considered to have a large effect based on the paper we cite. It should also be noted that 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.

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 betweeen measured and modeled velocity; in many cases with an increasing variance as velocity increases, suggesting a lack of correlation.

Response: We would characterize the velocity validation results shown in Appendix G as indicating a 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 measured and simulated vectors of velocity magnitude and direction. 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 six out of eight sites, the correlation between the measured and modeled velocities would be considered to have a strong effect, 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.

RIVER2D MODEL SIMULATION FLOW RUNS

Hal Beecher

Comment 1: (p. 21) – I have not run River2D, so I accept the files and criteria as stated. The preceding steps are reasonable and should ensure that model hydraulics are a good match to real hydraulics.

Response: No response needed.

HABITAT SUITABILITY CRITERIA (HSC) DATA COLLECTION

Hal Beecher

Comment 1: (pp. 21-23) – Authors use logistic regression for developing HSC. They collected data on physical habitat conditions where fish were absent and where they were present, based on random selection of locations. Logistic regression appears to have considerable merit as an approach where fish are sparse and probably well under carrying capacity (see discussion above about ecological context for instream flow studies). If the population is well below carrying capacity, however, the approach to aggregations of fish deserves more discussion. It seems unlikely that a large aggregation is somehow crowded into less suitable habitat while a dominant fish occupies the preferred habitat. It is likely that aggregations are a social response or a predator avoidance response, perhaps a function of a large river with *Ptychocheilus* or other predators. How do these aggregations relate to habitat suitability? I am left thinking the approach the authors took of reducing, but not eliminating, the aggregations was a reasonable approach, but deserving of a little more discussion.

Response: We are not aware of any information in the scientific literature that would suggest that logistic regression is not appropriate for use when fish are abundant. It is unknown whether the populations of fish in the Yuba River are below carrying capacity or not. We agree with the commenter's discussions of aggregations, but are not aware of any data in the literature that could be used to assess how aggregations relate to habitat suitability. Our approach essentially eliminated aggregations as a consideration in developing habitat suitability criteria, since we only took one measurement for each group of fish observed. Given the lack of data in the literature regarding how aggregations related to habitat suitability, we think that additional discussion of this would be speculative and thus not appropriate.

Comment 2: On page 22, the authors wrote, "If the location was beyond the sampling distance, based on the information recorded by the snorkeler, "beyond sampling distance" was recorded on that line and the recorder went to the next line at that same location, repeating until reaching a line with a distance from the bank within the sampling distance." It's unclear what is meant by "beyond sampling distance" – is this outside the 20' range, too deep within 20' range, no flag in sight, no flag within 3'?

Response: Beyond sampling distance refers to the distance out from the bank that the snorkeler was able to sample for fish. For example, for most of the 300 feet of bank sampled, the snorkeler may have been able to look for fish up to 20 feet out from the bank, but there may have been a short portion of the bank where, due to fast and deep conditions, the snorkeler had to hug the bank and thus was only able to see 10 feet out from the bank. In such a location, an unoccupied measurement that was specified as, for example, 20 feet from the bank, would have been denoted as “beyond sampling distance” in the databook. We have added the above text as a footnote at this location in the report to clarify this matter. It should be noted that this was a fairly rare circumstance – there were only eight of out 1624 unoccupied observations where “beyond sampling distance” was recorded in the databook.

Comment 3: On page 23, the authors wrote that during ADCP data collection they considered “adjacent velocity” to be the highest velocity in water column for observation – it would be good to validate a correspondence between this and true adjacent velocity. Alternatively, just call it “overhead velocity.”

Response: We have changed the text at this location to clarify that the adjacent velocity was the highest depth-averaged velocity measured, rather than the highest velocity in the water column. Thus, the adjacent velocity was the true adjacent velocity and not an overhead velocity.

REVIEWER #3

Comment 1: It is not very clear how ‘non-use’ locations were selected. I think this was the ‘no tag within 3 feet…’?

Response: We have added “non-use” to the text at this point in the report to clarify that the non-use locations were the locations where there was no tag within 3 feet.

Comment 2: Did the snorkelers/SCUBA divers alter positions of fish. Were there criteria for the observers to disregard positions of fish that may have been forced to move from their positions by the presence of the observer? Young fish typically move to deep water to avoid predators/disturbances. If this had occurred, the result of the bias would have been toward deeper water, and ultimately higher discharges necessary for maximum WUAs.

Response: Based on our observations, it did not appear that the snorkelers/SCUBA divers altered the positions of the fish. Since we did not observe any fish that appeared to have been forced to move from their positions by the presence of the observer, we did not have any criteria for observers to disregard such positions, which did not occur. Young fish are typically more likely to be preyed on by piscine predators than avian predators (Power 1984). Young fish typically move to shallower and slower conditions in the presence of piscine predators (Gard 2005). If such movement had occurred, the result of the bias would have been toward shallower water, and ultimately lower discharges necessary for maximum WUAs.

REVIEWER #4

Comment 1: 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 throughout the rearing period.

Response: Discharges during all of the data collection periods are given in Table 15. We have not added information to the report about how discharges during data collection compare to those throughout the rearing period because observed fish simply reacted to the flows present during data collection and unoccupied data were collected at the same time as the data on fish were collected.

BIOLOGICAL VERIFICATION DATA COLLECTION

Hal Beecher

Comment 1: (p. 24) **–** This is good, clear description of a good approach.

Response: No response needed.

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 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 18 to 23, 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. 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

Hal Beecher

Comment 1: (p. 24) – See comments on adjacent velocity above. This section implies that logistic regression is the best way to develop HSIs, but they are still univariate. The value in using a logistic regression or any other continuous mathematical function is that you get a continuous mathematical function. This study places considerable emphasis on validating the relationships to what fish really do, and this is appropriate. Simpler stairstep HSIs also work (Beecher et al. 1995, 2002). A multivariate HSC that was not just a product of HSIs would be unusual and a step forward (maybe).

Response: See response to comments on adjacent velocity above. We would characterize logistic regression as an appropriate way to develop HSIs based on the scientific literature (Knapp and Preisler 1999, Parasiewicz 1999, Geist et al. 2000, Guay et al. 2000, Tiffan et al. 2002, McHugh and Budy 2004). We used univariate criteria because multivariate criteria are rarely used in instream flow studies. We view the value of using a logistic regression primarily as correcting for effects of availability. We would view simpler stairstep HSIs as less biologically realistic, since they imply a large change in suitability over a small change in depth or velocity. We agree that a multivariate HSC would be unusual; we would think that a considerable amount of effort would be required to demonstrate that multivariate HSC improved habitat predictions, versus the univariate HSC used in this study. Multivariate HSC could also be problematic due to a loss of statistical power associated with the increased number of dependent variables used in the logistic regression.

Comment 2: On page 27, the statement that "adjacent velocities were highly correlated with velocities" should be supported with a correlation coefficient and sample size. If they are highly correlated, does incorporation of adjacent velocities contribute much? This question should be addressed. Given that adjacent velocities were sometimes overhead velocities, use of adjacent velocities seems to miss at least part of the point. Using the fastest velocity in the water column with nose velocity (assuming fish held near the substrate, rather than near where mean column velocity occurs) might be more useful than with mean column velocity. However, some of these approaches would require a 3-D model, and I am unaware of any 3-D models that are available for practical applications. Keeping the adjacent velocity (AV) HSI at 1.0 at high velocities will eventually become nonsense, but, if the V HSI drops off at higher velocities, then there is no problem with this approach.

Response: The correlation coefficient and sample size are given in the results section under *Habitat Suitability Criteria Development* and thus do not need to be repeated in this location. Specifically, Table 26 gives the values of the correlation coefficients (ranging from 0.93 to 0.94) and Table 23 gives sample sizes. Incorporation of adjacent velocities contributes a great deal to the habitat calculation because the relationships for adjacent velocity reflect the component of adjacent velocity that is independent of the velocity at the fish location. We have attempted to address this question in the text. As noted above, adjacent velocities were never overhead velocities. We agree with the commenter that use of the fastest velocity in the water column with nose velocity would require use of a three-dimensional hydraulic and habitat model, and that there are no 3-D models that are available for practical applications. We disagree that keeping the adjacent velocity (AV) HSI at 1.0 at high velocities will eventually become nonsense because the adjacent velocity is meant to capture the food delivery aspect of fish bioenergetics. As noted by the commenter, the decline in the velocity HSI with increasing velocity captures the energetic cost aspect of fish bioenergetics. We view the combination of velocity and adjacent velocity as crucial to capturing the entirety of fish bioenergetics (both food supply and energetic costs).

REVIEWER #4

Comment 1: 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 a portion of the text in question from "should be used to develop" to "are appropriate for developing." The following citation given in the report, from McHugh and Budy (2004), supports the conclusion given 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)."

BIOLOGICAL VERIFICATION

REVIEWER #4

Comment 1: See previous comment regarding this test and all of the detailed explanation that should go along with it.

Response: See response to previous comment regarding this test.

RESULTS

Hal Beecher

Comment 1: As noted above, some of the Results (1, 2, 3) would have fit better in the corresponding Methods sections, at least in my logic.

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 *Field Reconnaissance and Study Site Selection*). We also note that we previously included all of this information in the methods section and a peer reviewer from the first peer review of the Yuba spawning report recommended moving this material to the Results section. Specifically, the peer reviewer stated "All information presented, including data, in the methods section that is actually a result should be extracted and discussed in the Results section." As a result, we have placed all data in the results section for the Yuba rearing report as well.

STUDY SEGMENT DELINEATION

REVIEWER #4

Comment 1: The material from this point through page 39 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 clearly represents the results of the relevant study tasks (*Study Segment Delineation* through *Habitat Suitability Criteria Development*). We also note that we previously included all of this information in the methods section and a peer reviewer from the first peer review of the Yuba spawning report recommended moving this material to the Results section. Specifically, the peer reviewer stated "All information presented, including data, in the methods section that is actually a result should be extracted and discussed in the Results section." As a result, we have placed all data in the results section for the Yuba rearing report as well.

RIVER2D MODEL VELOCITY VALIDATION

Hal Beecher

Comment 1: (p. 37) – See Appendix H. It would be useful to have a table summarizing model velocity (and WSEL) calibration and validation, and overall acceptability over what range of flows.

Site	WSEL	Vel	Range of acceptable flows for model	
Narrows	Diffs >0.1 ft	Poor; model hi XS-1		
Rose Bar		Some vel probs		
Sucker Glide		Poor		
Railroad		Poor		
Diversion				
Whirlpool		Some vel probs		
Side-Channel		Model low XS-1 & 2, except hi N side of		

		channel		
Lower Hallwood	Diffs >0.1 ft	Some vel probs		

Response: We feel that such a table would have little utility, since we view the range of acceptable flows for the model to be the entire range of simulated flows for all sites (ie 400 to 4500 cfs for sites in the Above Daguerre segment and 150 to 4500 cfs for sites in the Below Daguerre segment. Further, we do not think that the results of the WSEL calibration and velocity validation can be captured adequately in such a simple tabular format. We would view the text in the results and discussion sections and the data shown in Appendices D, E, G and H as the best presentation of the WSEL calibration and velocity validation aspects of this report. We view the differences between sites in WSEL calibration and velocity validation not as reflecting differences in ranges of acceptable flows, but rather as reflecting differences in the level of uncertainty associated with the results of the models for each site.

Comment 2: How does model use the groundwater depth? It is hard to envision what this means and it seems inconsistent with the discussions I frequently have with hydrogeologists. Perhaps in the introduction of River2D discussion in the Methods, some explanation of this input would help. As it stands, for someone who has not run River2D, it seems illogical.

Response: Our understanding is that the minimum groundwater depth is used by the model to determine whether nodes are wet (surface water) or dry (groundwater). We have added this information in the methods section under **River2D Model Calibration**, where the minimum groundwater depth parameter is first introduced.

<u>REVIEWER #1</u>

Comment 1: 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: Our approach is to compare absolute values for low velocities because large percentage values for low velocities would not be biologically meaningful. In contrast, we compare percentage values 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. We disagree that the scatter plots show that the relationship is not that strong, since the scatter plots show the same information as correlation coefficients and because scatterplots tend to emphasize outliers. The correlation coefficient (r) is calculated using the following formula:

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

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

Comment 2: Similar comments apply to the Yuba Rearing report[2]. Yes, the velocity models meet the selected criteria of a correlation greater than 0.6, but I question the appropriateness of that single criteria, especially with a large sample size.

Response: We feel that the correlation coefficient is a reasonable criterion to use in evaluating velocity validation since it provides a summary statistic of the strength of the relationship between two variables. In addition, the report presents considerable additional information in Appendix H to use in evaluating the velocity validation, specifically differences between measured and simulated velocities, scatter plots of measured versus simulated velocities and cross-section profiles of measured and simulated velocities. As noted above, the correlation value is not affected by sample size.

REVIEWER #3

Comment 1: Section 5.4 – Model Validation – this appears like weak correlations to me. My experience is primarily on larger rivers than the Yuba – and we see much higher correlations between predicted and measured. I am not sure whether this lower level of agreement is typical in smaller rivers/streams. The model was 'in question' for 3 sites and was above 0.6 for 5 sites. The citation for the use of 0.6 as a 'high' correlation (Baldwin 1997) is extremely weak – it is a slide from something that was on the web. I tried the link and got a 'page not found' error. If the authors feel strongly that the model worked well – and need support for their decision that 0.6 was good enough – they should try to find support for this level of agreement from the peer-reviewed literature.

Response: The correlations for six of the eight sites would be considered to have a strong effect (Cohen 1992). We do not know if correlation values are higher for larger streams versus smaller streams. Data that we have on correlations and stream size are confounded by differences in topographic point densities. We would speculate that correlation values are probably more related to topographic point densities than to stream size. As a result of adding a downstream extension, the model is now in question for two sites and is above 0.6 for six sites. We were not

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[2] The comments referred to were as follows: "The measured and predicted velocities in the Clear Creek Spawning report are not as disparate as in the Yuba Report, but despite the high correlations, examination of the scatter plots shows considerable variation at Lower and Upper Renshaw and the Shooting Gallery. Profile shapes are much closer than in the Yuba study, but there is still much more variability in measured velocities than that shown in predicted velocities. Biologically this could be important, as the fish are honing in on microhabitat features and can undoubtedly discern between variable velocities."

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.

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 correletion 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 H. The scatter plots directly reflect the value of the correlation coefficient. 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 H, 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. 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 areas are not likely to be rearing habitat. 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 minimal. As shown in Table 11, 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 H) were relatively similar in shape] to appropriately summarize the cross-sectional plot data. As shown in the figures in Appendix H, 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 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 juvenile salmonids are not expected to be found in these locations.

HABITAT SUITABILITY CRITERIA (HSC) DATA COLLECTION

<u>Hal Beecher</u>

Comment 1: Table 16 (p. 40) appears to indicate major holes in sampling, particularly in mid-channel. It looks like tables 16-18 could be combined to be more clear about any holes in sampling, and this should probably distinguish mid-channel from near-bank:

Numbers in cells indicate distance (ft) sampled near bank; mid-channel	No cover	cobble	boulder	Fine woody	branches	log	overhead	Under-cut	Aq. Veg.	Rip-rap	Over-head & instream
Bar complex glide											
Bar complex pool											

Bar complex riffle											
Bar complex run											
Flatwater glide											
Flatwater pool											
Flatwater riffle											
Flatwater run											
Side-Channel glide											
Side-Channel pool											
Side-Channel riffle											
Side-Channel run											

Response: The differences in distances sampled between different habitat and cover types reflects primarily the relative abundance of different habitat and cover types in the Yuba River. As noted in the caption for Table 16, sampling in mid-channel habitat was confined primarily to Bar Complex and Flatwater Pools because these were typically the only habitat types that were deep enough to sample with SCUBA. We feel that the data is best presented by not combining Tables 16 through 18 to illustrate differences between sampling in different habitat types and different cover types, and to illustrate differences in sampling between the areas where unoccupied data were collected and areas where unoccupied data were not collected. We note that sampling equal areas of different habitat types and cover types is not critical for this study, since logistic regression addressed effects of habitat availability.

<u>REVIEWER #3</u>

Comment 1: It appears as if there were no ‘unoccupied data’ for July/Sept and Nov/Dec and none for fish less than < 60 mm. The effects of these omissions should be discussed.

Response: The commenter is incorrect – we still collected unoccupied data for July/Sept and Nov/Dec for those areas where we saw fish. We did collect unoccupied data for fish less than 60 mm. For July/Sept and Nov/Dec, we did not collect any data on fish less than 60 mm because by that point we already had enough observations of fish less than 60 mm. We do not feel that the sampling methodology used had an effect on the resultant habitat suitability criteria, because we still had both occupied and unoccupied observations for all areas where we observed fish. While it would have been preferable to collect unoccupied data for all areas sampled, doing so would have drastically reduced the amount of stream that we could have sampled. We felt that it was more important to be able to collect enough observations of fish greater than 60 mm than to collect unoccupied data for all areas sampled.

Comment 2: I assume all HSC data were collected during daylight. This would tend to bias toward faster/deeper water for Chinook fry, as they tend to move to shallower/slower water at night. The influence of this on habitat area predictions should be discussed by the authors.

Response: The commenter is correct that all HSC data were collected during daylight. Although there is abundant literature on changes in fish behavior from day to night (O'Neal 2007, Thurow et al. 2006, Bradford and Higgins 2001, Roni and Fayram 2000, Cunjak and Power 1996, Thurow and Schill 1996, Riehle and Griffith 1993), we do not have any information, nor are we aware of any information in the literature, that indicates that Chinook fry tend to move to shallower/slower water at night. As such, any discussion we might make on the influence of this on habitat area predictions would be purely speculative, and thus we have not added any discussion on this subject.

BIOLOGICAL VERIFICATION DATA COLLECTION

REVIEWER #3

Comment 1: The Biological Verification was limited in scope. This is not the place to save money/resources in a study such as this. The effects of the relatively low level of effort (in comparison to collection of physical data related to model development and validation) should be more thoroughly discussed.

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.

HABITAT SUITABILITY CRITERIA (HSC) DEVELOPMENT

Hal Beecher

Comment 1: Table 21 (p. 43) showed that suitability criteria distinguish a break at 60 mm length. This does not distinguish between species. It is interesting that the fish cluster more by age/size than by species, in contrast to small Washington rivers and streams. This may be a scale issue because of working in such a large river.

Response: Due to small sample sizes, our approach was first to determine which size to use to break between fry and juveniles and then to determine for fry and juveniles if there was an effect of species. Table 21 shows the first part of this analysis while Table 22 shows the second part of the analysis. We do not think that this is a scale issue, since we found similar results for Clear Creek, a much smaller stream than the Yuba River. The relative clustering by size versus species varied by size class and parameter – as shown in Table 21 and 22, there was a greater difference between sizes than between species for most parameters. However, for velocity for fry, there was a slightly greater difference for species (χ^2 = 20.74) than for size (χ^2 = 18.82). In addition, for cover for fry, there was a much greater difference for species (C = 90) than for size (C = 40). This was reflected in the much higher suitability for cobble for steelhead fry versus Chinook salmon fry.

Comment 2: p. 44 – line 2 of text – "June 2926"

Response: We have put the footnote "26" in superscript to make the text clear at this location. The text should have appeared as: "June 29[26]"

Comment 3: Logistic regression did not work in some cases (pages 45-49). See discussion above about trying to fit equations to data rather than simpler categorical approaches. Although I have concerns, that does not mean I object to use of logistic regression, and I believe the authors provide a reasonably good case for it. Other approaches might produce similar results. The authors have tried to test their HSC, which is an important step.

Response: The commenter is correct that we were unable to use logistic regression to develop velocity criteria for juvenile salmonids. We would view simpler categorical approaches as less biologically realistic, since they imply a large change in suitability over a small change in depth or velocity. We appreciate the commenter's assessment of the case we have made for using logistic regression. We are not aware of other approaches that might produce similar results, since other approaches typically do not adequately address the effects of availability on habitat use. We agree with the commenter than testing HSC is an important step, although we were not able to successfully do so in this case.

Comment 4: Adjacent velocities are highly correlated with mean column velocities (Table 26, p. 50). This may just be an indication of a big river with even flow. If they are too closely correlated there may be no explanatory value added by adding adjacent velocity. It would be informative to compare the depth of fish and velocity at fish depth ("nose velocity") with mean column velocity ("velocity").

Response: As noted above, the adjacent velocities were also mean column velocities. Based on similar findings on Clear Creek, a high correlation between the mean column velocity at the fish location and the adjacent mean column velocity is not just an indication of a big river with even flow. We feel that there is significant explanatory value added by adding adjacent velocity, since the adjacent velocity criteria was based on the component of the adjacent velocity that was independent of the velocity at the fish location.

Comment 5: Juvenile Chinook and steelhead velocity preference or suitability are lumped together (Fig. 15, p. 56). Juvenile Chinook velocity suitability is similar to observations in Washington, but juvenile steelhead velocity suitability is lower than Washington observations. It's possible that the steelhead avoid faster water associated with deeper areas in large rivers.

Response: The commenter is correct that we lumped together juvenile Chinook salmon and steelhead velocity preference or suitability, based on our finding that there was no significant difference between juvenile Chinook salmon and steelhead velocity habitat use data (Table 22). We reached a similar conclusion to the commenter regarding the juvenile steelhead velocity suitability from this study versus other studies in California (Figure 49). Given that one of the other studies was on a river that is larger than the Yuba River, it does not appear that the results of this study were related to steelhead avoiding faster water associated with deeper areas in large rivers. Instead, it is more likely that the low optimum velocity for juvenile steelhead in this study is an artifact of the small sample size of occupied observations, which resulted in us having to use a modified fry steelhead velocity criteria for juvenile steelhead.

<u>REVIEWER #3</u>

Comment 1: Separating the habitat use at about 60 mm is justified and comports well with research on Chinook salmon in the Columbia Basin.

Response: No response needed.

Comment 2: On page 44 there is a date of June2926

Response: We have put the footnote "26" in superscript to make the text clear at this location. The text should have appeared as: "June 29[26]"

Comment 3: The adjacent velocity seems a little arbitrary – The selection of criteria (e.g., adjacent velocity) is justified with the Fausch and White (1981) paper which described optimal foraging locations for salmonids which were larger than 150 mm. The authors of the Yuba report erroneously applied the foraging distance (60 cm) described by Fausch and White for larger resident brook and brown trout to the much smaller fry and juvenile Chinook and steelhead in the Yuba.

Response: Our selection of criteria was not based on the Fausch and White (1981) paper. Rather, we mentioned the Fausch and White (1981) paper to generally introduce the concept of adjacent velocity. We clearly state that our adjacent velocity criteria are based on an entirely different mechanism than that presented in Fausch and White (1981), namely the transport of invertebrate drift from fast-water areas to adjacent slow-water areas where fry and juvenile salmonids reside via turbulent mixing. Specifically, as stated in Footnote 18, "Two feet (0.61 meters) was selected based on a mechanism of turbulent mixing transporting invertebrate drift from fast-water areas to adjacent slow-water areas where fry and juvenile salmon and steelhead/rainbow trout reside, taking into account that the size of turbulent eddies is approximately one-half of the mean river depth (Terry Waddle, USGS, personal communication), and assuming that the mean depth of the Yuba River is around 4 feet (1.22 meters) (i.e., 4 feet x ½ = 2 feet)." It is just a coincidence that the turbulent eddy size criteria used in this study was the same as the foraging distance criteria in Fausch and White (1981).

Comment 4: The authors reported Chinook fry in velocities up to almost 4 feet/sec (p 39); This is fast water. A 50 mm fry in 2.5 feet/sec is swimming at 15 body lengths/second – when traditionally accepted burst speed for salmonids is typically 10 bps. Thus, the velocities where fish were observed must be called into question. The bias resulting from overestimating velocities in HSC would be toward higher discharges.

Response: The commenter misunderstood the text at this location – 3.98 ft/s was the fastest velocity that we observed any juvenile anadromous salmonid. The fastest velocity at which we observed a Chinook salmon fry was 3.62 ft/s. We note that the suitability for Chinook salmon fry for velocities greater than 2.5 ft/s was relatively low (0.09 to 0.13). We had three observations of Chinook salmon fry at mean column velocities greater than 2.5 ft/s (specifically at 2.54, 2.59 and 3.62 ft/s). We would expect that the difference between these mean column velocities and traditionally accepted burst speed for salmonids was caused by the fish being at a focal velocity location in the water column where the focal velocity was significantly lower than the mean column velocity. Since HSC are based on mean column velocity, we did not overestimate velocities in the HSC, and thus there would not be any bias toward higher discharges. Further, since both the HSC and habitat availability are based on mean column velocities, the effects of using HSC based on mean column velocity, versus focal velocity, on flow-habitat relationships would tend to be cancelled out by the effect of using habitat availability based on mean column velocity, versus focal velocity.

BIOLOGICAL VERIFICATION

Hal Beecher

Comment 1: On page 61, the authors showed no difference in suitability between unoccupied habitat and habitat occupied by Chinook fry, but found a significant difference for Chinook juveniles compared to unoccupied habitat. Despite p=0.013, authors apologize for "weak" test based on small sample size, but they seem more apologetic than needed. If anything, the small sample size reduced power, yet they still found a significant difference.

Response: We appreciate the commenter's confidence in our findings for juvenile Chinook salmon biological verification, but have chosen to keep the text as is to avoid overstating our case.

Comment 2: Figures 20-22 (pp. 62-64) are good graphics of cover suitability and use.

Response: No response needed.

HABITAT SIMULATION

Hal Beecher

Comment 1: This is typically the punch line of the Results of an instream flow study report. Instead, the text on page 66 refers the reader to Appendix L, a series of tables. Are there any patterns? Or relationships to hydrological benchmarks? Table 30, cited briefly in the text, is the punch line and should probably be highlighted. Table 30 is a helpful summary of Figures 25-32. This is a place where the repeated order of sections in Methods, Results, and Discussion is particularly frustrating because Table 30 shows Chinook fry habitat is maximized at the highest flow and steelhead fry maximized at the lowest flow, with Chinook and steelhead showing opposite trends with age. The reader then has to jump back into the other Discussion before reaching discussion of this point.

Response: Patterns in the flow-habitat relationships are shown graphically in Figures 25-32 and discussed in the Discussion section under *Habitat Simulation*. The patterns varied by species, life stage and reach, and did not appear to have any relationship to hydrological benchmarks. We agree that Table 30 is the key result; however, we do not know how to further highlight the results. We sympathize with the commenter's frustration, but feel that the current organization of the report is needed to make it as consistent as possible with the format of peer-reviewed journal articles.

DISCUSSION

REVIEWER #3

Comment 1: In the Discussion section it is apparent that the authors place too much emphasis on modeling and stats – and not enough on the biological aspects.

Response: A large part of the discussion is centered on the biological aspects (namely the habitat suitability criteria and biological verification). We have tried to ensure that the discussion has an appropriate balance between modeling, statistics and biological aspects.

HABITAT MAPPING

Hal Beecher

Comment 1: Some of red labeling of channel units is not clear on Figure 33 (p. 72).

Response: We have modified the red labeling of channel units to improve the clarity of the labeling.

HYDRAULIC AND STRUCTURAL DATA COLLECTION

REVIEWER #4

Comment 1: 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 Table 11, all errors were less than 0.04 feet.

RIVER2D MODEL CALIBRATION

Hal Beecher

Comment 1: (p. 73-74) – This is a good discussion of decisions on WSEL and models.

Response: No response needed.

Comment 2: I would have included all Methods, Results, and Discussion of model calibration under Methods, based on my assumption that the heart of the matter is the habitat model output.

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

<u>REVIEWER #1</u>

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 in 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 there 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.

<u>REVIEWER #4</u>

Comment 1: 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 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 such locations are not rearing habitat for juvenile salmonids.

RIVER2D MODEL VELOCITY VALIDATION

Hal Beecher

Comment 1: The decision that potentially erroneous models should be used in the absence of any similar habitat models is a difficult one (1^{st} paragraph, p. 74). Risk is that conclusions from model could err in either direction. What about model led authors to decide that it was better than nothing and would not be misleading?

Response: Our assessment was based on the two alternatives we had available, namely: 1) to throw out these sites and represent flatwater habitat in the Below Daguerre segment by bar complex habitat; or 2) to use the sites. We believe that it would be more accurate to model rearing habitat in the Below Daguerre segment using these sites because if we threw out these sites, the rearing habitat would not include results from flatwater habitat types, which comprise 21 percent of the area of the Yuba River between Daguerre Dam and the confluence with the Feather River. We believe that the errors associated with simulated velocities for these sites are less than the errors that would be associated with representing flatwater habitats by bar complex habitats.

Comment 2: It might be better to use simpler tools at this point: wetted width or mean velocity. I would have included all Methods, Results, and Discussion of model calibration under Methods, based on my assumption that the heart of the matter is the habitat model output.

Response: We feel that wetted width or mean velocity would not be biologically realistic. Specifically, wetted width assumes that all wetted habitat has equal habitat value, while mean velocity does not consider the habitat requirements of fry and juvenile anadromous salmonids associated with depth or cover. We have patterned the format of our reports as closely as possible to that of peer-reviewed journal articles. In this regard, we note that a peer reviewer from the first peer review of the Yuba spawning report stated "All information presented, including data, in the methods section that is actually a result should be extracted and discussed in the Results section." As a result, we have placed all data in the results section for the Yuba rearing report as well.

Comment 3: Text (2^{nd} paragraph, p. 74) is general sweeping dismissal of discrepancies and does not appear consistent with my subjective evaluations of all transects in Appendix H: average FAIR for velocity simulations for Narrows; POOR-FAIR for Rosebar; POOR-FAIR for Lower Hallwood; FAIR for Whirlpool; POOR for Sucker Glide; POOR-FAIR for Railroad. It would be better to set a standard for acceptance or rejection of model performance. And if a model is rejected, what would substitute for it? The argument that River2D smoothes out the velocity profile is somewhat contrary to one of the arguments in favor of 2D modeling – that it does a better job of simulating velocity vectors. If it is such a trend-averaging algorithm, is it worth the effort to measure the bed at such a detailed mesh?

Response: We believe that the commenter is referring to the following text: "As shown by the figures in Appendix H, we attribute many 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." It should be noted that transects in this sentence only refers to XS1 and XS2 in Appendix H and not to the Deep Beds, where there was only one ADCP run. This sentence also needs to be viewed within the context of the rest of the text presented in the Results and Discussion sections for **River2D Model Velocity Validation**. We feel that the text, taken as a whole, acknowledges the discrepancies in the model hydraulic performance and is consistent with the data presented in Appendix H. The report already presents a standard for acceptance or rejection of model performance, namely a correlation of at least 0.6 between measured and simulated velocities. If a model is rejected, the only thing that could substitute for it would be a model of a different habitat type in another site. We would view the smoothing of the velocity profile by River2D as a limitation on how much better a job River2D can do than PHABSIM in simulating velocity vectors. It should be noted that the area in which River2D averages can be reduced by using a finer mesh.

Comment 4: The explanation (p. 75) about the Narrows downstream transect and simulated eddy suggests that the model, if it can't be extended, should have the habitat portion run on that part of the reach that is not influenced by the eddy. If this is not possible, it suggests a shortcoming in using 2D rather than 1D, where a transect can be excluded. The discussion of the Side-Channel site is another caution about using River2D: if upstream velocities are based on depth rather than actual velocity distribution, it is important to have the upstream transect in the appropriate location or have a means to modify the incoming velocities.

Response: We agree that only running the habitat portion on the part of the reach that was not influenced by the eddy would have been an option. We ended up being able to add a downstream extension to the Narrows site and thus overcome this potential shortcoming of using 2D rather than 1D. The lesson that we learned from the Side-Channel site is that it is important to collect bed topography data upstream of the site to improve the velocity simulation at the upstream end of the site. Thus, this was not a caution about using River2D, but rather a lesson in what data is needed to produce an accurate hydraulic simulation with River2D.

Comment 5: Is it possible the discrepancies in flow (p. 77) result from flow through gravel at riffles? That would explain underestimates but not overestimates.

Response: It is unlikely that the discrepancies in flow resulted from flow through gravel at riffles, since the sites in question were located in non-riffle habitats (specifically in two glides and a pool). It is more likely that the discrepancies in flow were caused by errors in the ADCP data. While the data in question was from only one ADCP traverse, we have found that at least three ADCP traverses are needed to get a discharge that comes within 5 percent of the known discharge (Gard and Ballard 2003).

REVIEWER #1

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

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.

RIVER2D MODEL SIMULATION FLOW RUNS

Hal Beecher

Comment 1: The decisions and discussions on hydraulic modeling seem acceptable as far as they go. However, the net effect is to say the modeling of hydraulics is not highly accurate. If not, how accurate is the habitat modeling? Should the habitat modeling ignore velocity if it is not modeled well or if depth is so great that mean column velocity is irrelevant. Of course, once the commitment has been made to a particular modeling approach, it is difficult to change, given time and budget constraints. I believe the authors did the best they could under the circumstances, but it is far from an ideal model.

Response: We acknowledge that the modeling of hydraulics was not highly accurate. We feel that the habitat modeling, with respect to the overall flow-habitat relationship, is a better representation of fry and juvenile anadromous salmonid rearing habitat in the Yuba River than would be expected based on the accuracy of the hydraulic modeling. 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. Ignoring velocity in the habitat modeling would be biologically unrealistic since velocity is a key component of fry and juvenile anadromous salmonid habitat. We acknowledge that it is far from an ideal model.

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 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 since juvenile salmonids would not be expected to be found in such locations.

HABITAT SUITABILITY CRITERIA (HSC) DATA COLLECTION

Hal Beecher

Comment 1: The discussion of HSC is good.

Response: No response needed.

HABITAT SUITABILITY CRITERIA (HSC) DEVELOPMENT

Hal Beecher

Comment 1: The discussion of HSC is good.

Response: No response needed.

BIOLOGICAL VERIFICATION

Hal Beecher

Comment 1: The discussion of verification is good.

Response: No response needed.

REVIEWER #3

Comment 1: In areas of the results where there were significant differences between predicted used and unused habitats, the authors dismissed this (p. 95) by saying the model didn't work or they had low sample size in fish observations.

Response: We feel that we have correctly characterized the reasons why there were significant differences between used and unused habitats. Rather than dismissing the differences, we have explained why there were significant differences.

Comment 2: If Guay (2000) on Hardy and Addly (2001) both used the geometric mean – and the authors justified their use of binary data based on Guay – then why did they elect to use the product?

Response: While there are some instances in the literature where combined suitability has been calculated using a geometric mean (Hanrahan et al. 2004, Prewitt 1982, Hardy and Addley 2001), most applications of habitat modeling use a product to obtain combined suitability (Vadas and Orth 2001). Geometric mean calculations imply that good habitat for one variable can compensate for poor conditions for another variable, but yield zero combined suitability when any habitat variable is unsuitable (Vadas and Orth 2001). Vadas and Orth (2001) concluded that the product method was superior to the geometric mean method because it was consistently accurate and was a simpler regression model.

HABITAT SIMULATION

Hal Beecher

Comment 1: The authors compare their habitat-flow relationships to those found in another study. I concur with the authors in believing that HSC based on both occupied and unoccupied habitat, considering habitat availability, is preferable and favors the USFWS study. The use of adjacent velocities gives an intuitive nod to this study, but the description of their evaluation of the adjacent velocities is less than persuasive. Use of 2D modeling in the present study is not clearly preferable to PHABSIM, and the diagnostics in this study, while the right thing to do, did not persuade me that the 2D model was superior to the 1D model (although I can't determine the quality of the PHABSIM model). The purported advantages of River2D, a higher density of modeling points and flow vectors that can vary in 2 dimensions, did not appear to translate into a clearly high quality habitat model.

Response: As discussed above, we have refined our description of our evaluation of the adjacent velocities, and feel that it is now persuasive. We feel that the use of 2D modeling in the present study is clearly preferable to PHABSIM. Specifically, River2D avoids problems of transect placement, since data are collected uniformly across the entire site (Gard 2009). River2D also has the potential to model depths and velocities over a range of flows more accurately than would PHABSIM because River2D takes into account upstream and downstream bed topography and bed roughness, and explicitly uses mechanistic processes (conservation of mass and momentum), rather than Manning's Equation and a velocity adjustment factor (Leclerc et al. 1995). Other advantages of River2D are that it can explicitly handle complex hydraulics, including transverse flows, across-channel variation in water surface elevations, and flow contractions/expansions (Ghanem et al. 1996, Crowder and Diplas 2000, Pasternack et al. 2004). With appropriate bathymetry data, the model scale is small enough to correspond to the scale of microhabitat use data with depths and velocities produced on a continuous basis, rather than in discrete cells. River2D, with compact cells, should be more accurate than PHABSIM, with long rectangular cells, in capturing longitudinal variation in depth, velocity and substrate. River2D should do a better job of representing patchy microhabitat features, such as gravel patches. Based on the above, we feel that the 2D model in this case was superior to the 1D model. While we acknowledge that our application of River2D had flaws, we feel that we ended up with a higher quality habitat model than the previous PHABSIM model.

Comment 2: On page 100, the authors claim that the study models habitat over a range of flows. Although this is true, the accuracy of the model is open to question. Given that another study has addressed the same issue, no basis for comparing the models and their results were presented.

Response: We acknowledge that there are uncertainties in the accuracy of the model results. The discussion section presents the following basis for comparing the models and their results: "We attribute the differences between our study and Beak (1989) to the following: 1) the Beak (1989) study used HSC generated only from use data, as opposed to the criteria generated with logistic regression in this study; 2) the Beak (1989) study did not use cover or adjacent velocity criteria; and 3) the use of PHABSIM in the Beak (1989) study, versus 2-D modeling in this study. We believe that these differences likely biased the flow-habitat results in the Beak (1989) study towards lower flows, since the HSC, generated only from use data and without cover or adjacent velocity criteria, were biased towards slower and shallower conditions. In contrast, our study reduces biases due to availability and includes the important juvenile habitat components of cover and adjacent velocity. We attribute the difference in magnitude of the results from this study versus Beak (1989) primarily to the use of adjacent velocity criteria in this study. A fourth habitat suitability index parameter will tend to result in overall lower amounts of habitat, since the combined suitability index is calculated as the product of the individual suitability indices. The effects of adjacent velocity are most pronounced at low flows, where a large proportion of the channel has low adjacent velocities, and thus low suitability for this parameter. In conclusion, we feel that the results of this study are a more accurate assessment of the relationship between flow and anadromous salmonid fry and juvenile rearing habitat than the results of Beak (1989)."

REVIEWER #3

Comment 1: The authors attribute differences between their results and those of Beak (1986) based on Beak's use of only use data and that he did not use adjacent velocity. See comments earlier on use of adjacent velocity.

Response: See responses to comments earlier on use of adjacent velocity.

Comment 2: The authors did not comment on their assumption that rearing habitat was limiting in the Discussion section.

Response: To our knowledge, the data does not exist to evaluate whether rearing habitat is limiting. For example, information is lacking to be able to determine if doubling the amount of rearing habitat would double the salmonid populations.

Comment 3: Overall, I think the level of disagreement between the WUA vs. discharge relationships these authors came up with compared to those presented by Beak call into question the application of these methods – and assumptions made by the these authors and those of the Beak report. In a simple sense, I fear that the disparate emphasis on the physical modeling and almost secondary nature of the biological information resulted in findings that I do not think are well justified as presented in this report. If the agreement were a little closer between the two studies, I would expect that that may result in a higher degree of confidence in the results, in terms of basing flow management decisions on these results.

Response: Rather, we see the large difference between the results of this study and the Beak study as reflecting the importance of the improvements in methods for conducting instream flow studies since the Beak study was done. We disagree that there was a disparate emphasis on the physical modeling and almost secondary nature of the biological information. Rather, the biological information (principally the habitat suitability criteria) played a critical role in the findings in this report. We feel that the lack of agreement between the two studies simply reflects the improvements in the methods for conducting instream flow studies over the last twenty years, and that use of the state of the art methods for conducting instream flow studies results in an improved degree of confidence in the results, in terms of basing flow management decisions on these results.

REVIEWER #4

Comment 1: 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 #3

Comment 1: On p. 104 the authors say they had 'high correlation between measured and predicted velocities'. This oversimplifies and is not entirely truthful. Regardless of what a stats paper says – 0.6 is not a high correlation coefficient for physical data. It may be great for biological data – but physics should always obey the same laws… Appendix H shows pretty poor correlations, especially for slower velocities (where most fish were observed).

Response: We would not characterize the model performance as poor, since the correlations between measured and simulated velocities would be considered to have a large effect for 6 of the 8 sites. It should also be noted that 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 2: Regarding observer effects on habitat use data, the direction of this bias would, I think, be toward deeper water and higher velocities.

Response: It is unknown if observer effects, if any, would affect habitat use, and what direction any resulting bias, if any, would be. If the fish in this study reacted to the observers the same as they would react to a piscine predator, it is likely that the direction of the bias would be towards shallower waters and lower velocities (Gard 2005).

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Appendix B
Responses to Stakeholder Comments

for the

August 2008 Draft of the Yuba River Rearing and Redd Dewatering/Juvenile Stranding Instream Flow Study Reports

October 2010

PG&E Comments

PG&E 1. *Thank you for the opportunity to review your draft reports on flow-rearing habitat relationships for juvenile salmonids in the Yuba River, and flow fluctuation in the Lower Yuba River. Due to time and staff limitations, PG&E has been able to provide limited comments only on the rearing report (attached). We believe that release of this document is premature. This report relies on much the same theoretical basis as the draft Yuba River spawning flow -habitat report that you previously distributed for review. Since the collaborative group established to resolve issues with that report has not yet reached agreement, we recommend action on the rearing report be delayed. Once agreement is reached on the appropriate analytical methods for that report, it should be easier to revise the rearing report in an appropriate manner.*

Response: All three reports in this series have now undergone two or more rounds of both peer review and stakeholder review, and have been substantially revised in an effort to address the resulting comments. The revisions have included additional data analyses and discussion, and are well documented even if all issues could not be resolved entirely. The work and resulting reports are part of the Central Valley Project Improvement Act (CVPIA) Instream Flow and Fisheries Investigations, an effort which began in October 2001. Funding was provided under Title 34, section 3406(b)(1)(B) of the Central Valley Project Improvement Act, P.L. 102-575, and the reports are the "deliverables" for work identified in CVPIA Annual Work Plans, most recently for Fiscal Year 2010. Lack of agreement among members of the collaborative group notwithstanding, we are required to finalize and release the reports.

General Comments

PG&E 2. *We believe that release of this document is premature, given that the collaborative group established to resolve issues surrounding the draft spawning flow-habitat report has not yet reached agreement on analytical methods used in that report. In particular, since this report relies on the same polynomial logistic regression approach to habitat suitability criteria (HSC) that is under the review in the spawning habitat report, it would be appropriate to wait until agreement is reached in that group before completing the draft analysis contained in this report. Further, since the conclusions presented in this draft report in Figures 56 and 57 suggest that the rearing habitat in the Yuba River is much less sensitive to changes in flow than was previously thought, there should not be any fishery implications associated with delaying this report until all parties can reach consensus on the best scientific methods to use.*

Response: Please see response to comment above. In the rearing report on page 103, we state the following:

> **"**We attribute the differences between our study and Beak (1989) to the following: 1) the Beak (1989) study used HSC generated only from use data, as opposed to the criteria generated with logistic regression in this study; 2) the Beak (1989) study did not use cover or adjacent velocity criteria; and 3) the use of PHABSIM in the Beak (1989) study, versus 2-D modeling in this study.**"**

We stand by this statement.

PG&E 3. *The use of the term "Spring/Fall-run Chinook" throughout this report would be, more correctly presented as "Fall/Spring-run Chinook", or else simply "Chinook", since the vast majority of the fry and juvenile Chinook in the Lower Yuba River are fall-run fish. In addition, there is uncertainty regarding the degree of genetic introgression that exists between late fall-run and spring-run Chinook.*

Response: Throughout the report we have changed spring/fall-run Chinook to fall/spring-run Chinook. We are not aware of any direct data regarding the proportion of fall-run versus spring-run fry and juvenile Chinook in the lower Yuba River. Given the findings of Early and Brown (2004) and McReynolds et al. (2004), it is unlikely that the proportion of fall-run versus spring-run fry and juvenile Chinook in the lower Yuba River could be accurately estimated using the table provided by CDFG (Frank Fisher, Red Bluff, 1994) correlating race with life stage periodicity and total length. If there are similar fecundities and survival rates for fall-run and spring-run, our redd count data (U.S. Fish and Wildlife Service 2008) would suggest that most of the fry and juvenile Chinook in the lower Yuba River are fall-run fish. Specifically, in 2001 we counted over 1,342 fall-run Chinook salmon redds, while in 2002, we collected habitat suitability criteria for 168 spring-run Chinook salmon redds. Based on the above data and assuming similar fecundities and survival rates for fall-run and spring-run, more than 89 percent of the fry and juvenile Chinook in the lower Yuba River are likely fall-run fish. An evaluation of the degree of genetic introgression that exists between late fall-run and spring-run Chinook is outside of the scope of this study.

PG&E 4. *References to "Daguerra Point Dam" appear to be derived from an incorrect designation on the USGS quad map for this area. These should be changed to "Daguerre Point Dam" to reflect the U.S. Army Corps of Engineers name for this structure.*

Response: As noted by the commenter, 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 reports we have changed Daguerra Point Dam to Daguerre Point Dam on the assumption that the U.S. Board on Geographic Names will approve these corrections.

Methods

PG&E 5. Section 2. Study Segment Delineation *The report delineates only two segments in the Yuba River, above and below Daguerre Point Dam. This does not adequately reflect the variation in habitat characteristics between Englebright Dam and the confluence with the Feather River. A collaborative determination of habitat segments done for the California Department of Fish and Game's (CDFG) Yuba River Fishery Investigations in the l 980's identified four study segments based on habitat characteristics, two upstream of Daguerre Point Dam and two downstream.*

Response: Bovee (1995a) notes that the flow regime is the primary determinant of segments, and this is the criterion that we used. Thus, the study referred to by the commenter (Beak 1989) likely used different criteria than we did 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. Accordingly, in this study we captured the differences between Beak's (1989) reaches at the finer scale of mesohabitat types.

PG&E 6. Section 4. Field Reconnaissance and Study Site Selection *This section should be expanded to include a more complete description of the criteria that were considered in selection of additional juvenile study sites, and an explanation of why the standard practice of selecting study sites in collaboration with other stakeholders was not followed.*

Response: The section in question includes all of the criteria that were considered in selection of additional juvenile study sites. This study did not occur as part of the Yuba Accord or some other collaborative process. Selection of study sites in collaboration with other stakeholders is not our standard practice, nor are we aware of how such a selection process would have affected the selection of study sites.

Section 8. Habitat Suitability Criteria (HSC) Data Collection, and Section 10. Habitat Suitability Criteria (HSC) Development

PG&E 7. *As noted in the general comments, the specific application of polynomial logistic regression is currently under discussion for the spawning habitat report. Once a consensus is reached in that forum regarding the best scientific approach for HSC development, revision to this report will be necessary to be consistent.*

Response: While the specific application of polynomial logistic regression was under discussion for the spawning habitat report, the lack of consensus in that forum regarding the best scientific approach for HSC development lead us to obtain additional peer review and finalize the spawning report. We feel that polynomial logistic regression is an appropriate scientific approach for HSC development, based on the peer-reviewed scientific literature (Knapp and Preisler 1999, Parasiewicz 1999, Geist et al. 2000, Guay et al. 2000, Tiffan et al. 2002, McHugh and Budy 2004). Accordingly, this report, without revisions to the method used to develop habitat suitability criteria, is consistent with the spawning habitat report.

PG&E 8. *Some explanation needs to be provided here for the identification procedures leading to the use of the "juvenile salmonid" classification presented in Table 23, and how this classification is used (or not) in this report.*

Response: The following section of the report located on page 47 is intended to provide the explanation for the identification procedures leading to the use of the "juvenile salmonid" classification presented in Table 23:

> "The results of the Mann-Whitney U tests and Pearson's test for association to test for differences between Chinook salmon and steelhead/rainbow trout indicate significant differences (at $p = 0.05$) between species for fry for velocity and adjacent velocity and for juveniles for depth (See χ^2 values in Table 22) and for both fry and juveniles for cover (see C values in Table 22), but there were no significant differences (at $p = 0.05$) between species for fry for depth or for juveniles for velocity and adjacent velocity. Since the p-value for depth for fry was only slightly larger than 0.05, we developed separate criteria for Chinook salmon and steelhead/rainbow fry rearing to reduce Type II error. For juveniles, we lumped together data for both species for velocity and adjacent velocity, but split the data between species for depth and cover."

This classification is used in the report for the development of velocity and adjacent velocity habitat suitability criteria which were used for both juvenile Chinook salmon and steelhead/rainbow trout.

PG&E 9. *The selection of 60 mm as the dividing point between fry and juvenile life stages (rather than the more commonly used 50 mm) needs to be more fully justified.*

Response: The selection of a dividing point between fry and juvenile life stages (either 50 or 60 mm) is arbitrary, since there is essentially a continuous increase in depths and velocities with increasing fish size. For example, we found significant differences in depths for fish less than or greater than 40, 60 and 80 mm (Table 21). We were unable to consider 50 mm post-hoc as a dividing point between fry and juvenile life stages because we lumped all fish between 40 and 60 mm into one size category, and thus were not able to separate out observations of fish less than versus greater than 50 mm, within the 40-60 mm size category.

PG&E 10. *Please describe the procedures that were used to assure that habitat use measurements were collected using proportionate effort among mesohabitat types.*

Response: We are not aware of habitat use measurements being collected using proportionate effort among mesohabitat types. However, we are aware of habitat use measurements being collected with equal effort (i.e. equal distance sampled) in different mesohabitat types, to attempt to address effects of availability. While we did make an effort to sample all mesohabitat types, so as to sample all of the available habitat for fry and juvenile anadromous salmonids, we did not sample equal areas of different mesohabitat types for the following reasons: 1) equal area sampling of different mesohabitat types does not adequately address the effects of availability on habitat use; 2) the use of logistic regression takes into account the effects of availability on habitat use; and 3) at least for larger rivers, mesohabitat type has little to do with fry and juvenile anadromous salmonid habitat use, based on our snorkel surveys on the Sacramento River (U.S. Fish and Wildlife Service 1996). Our snorkel surveys indicated that the scale of mesohabitat units for large rivers is so much larger than the scale of habitat use that habitat use is controlled by microhabitat parameters, rather than mesohabitat types.

PG&E 11. *Data collection methods for adjacent velocities are not clearly explained. Footnote 17 on page 21 indicates that "The adjacent velocity was measured within two feet on either side of the location where the velocity was the highest." This footnote raises at least three questions. 1) How inconsistent were velocities at a location where a school of fish was observed, if the highest velocity was picked to represent use? 2) Why was the highest velocity and not the lowest velocity (or modal velocity or some other measure) selected as the focus point for your observations? 3) The explanation for a two-foot distance appears to assume that the habitat where fry and juveniles were observed was turbulent, but why does the report provide no information to support that assumption? In addition, the technique for integrating adjacent velocities into the development of HSC, and their influence on the results, does not appear to be clearly explained anywhere in the report.*

Response: We have modified the text in question as follows to clarify the procedures we used to collect adjacent velocity measurements:

> "The adjacent velocity was measured where the velocity was the highest within 2 feet (0.61 meters) on either side of the residence location."

We did not measure the fastest velocity at each location where a fish was observed, rather we measured the mean column velocity at the exact location where the school of fish were observed. The adjacent velocity was a second velocity measured within 2 feet of the fish location. The highest velocity was selected for the adjacent velocity measurements to be consistent with the mechanism of transport of invertebrate drift from fast-water areas to adjacent slow-water areas where fry and juvenile salmonids reside via turbulent mixing. Since the amount of invertebrate drift transported is expected to be proportional to current velocity, it is reasonable to select the highest nearby (within 2 feet) velocity as the adjacent velocity. Without adequate food supply via invertebrate drift from fast-water areas, fry and juvenile Chinook salmon and steelhead are unable to grow and survive, a critical aspect of their habitat requirements. Flow in rivers is generally turbulent – laminar flow is rare in natural systems (Chow 2009). This was a general assumption on our part, and we did not assess the turbulence specifically in areas where we located fry and juveniles. The technique for integrating adjacent velocities into the development of HSC is explained in the following two portions of text from the methods section of the report:

> "Because adjacent velocities were highly correlated with velocities, a logistic regression of the following form was used to develop adjacent velocity criteria:
>
> $$\text{Frequency} = \frac{\text{Exp}\,(I + J * V + K * V^2 + L * V^3 + M * V^4 + N * AV)}{1 + \text{Exp}\,(I + J * V + K * V^2 + L * V^3 + M * V^4 + N * AV)}, \quad (4)$$
>
> where Exp is the exponential function; I, J, K, L, M and N are coefficients calculated by the logistic regression; V is velocity and AV is adjacent velocity. The I and N coefficients from the above regression were then used in the following equation:

$$\frac{\mathrm{Exp}\,(I + N * AV)}{1 + \mathrm{Exp}\,(I + N * AV)} \qquad (5)$$

We computed values of equation (4) for the range of occupied adjacent velocities, and rescaled the values so that the largest value was 1.0. We used a linear regression on the rescaled values to determine, using the linear regression equation, HSI_0 (the HSI where the AV is zero) and AV_{LIM} (the AV at which the HSI is 1.0). The final adjacent velocity criteria started at HSI_0 for an adjacent velocity of zero, ascended linearly to an HSI of 1.0 at an adjacent velocity of AV_{LIM} and stayed at an HSI of 1.0 for adjacent velocities greater than AV_{LIM}."

and

"The software calculates the adjacent velocity for each node, then uses the adjacent velocity criteria to calculate the adjacent velocity suitability index for that node. This index is then multiplied by the combined depth, velocity and cover suitability indices. This product is then multiplied by the area represented by each node to calculate the WUA for each node, with the WUA for all nodes summed to determine the total WUA for each mesohabitat type, flow, life stage and species."

The influence of adjacent velocities on the results is explained in the following text from the discussion section of the report:

"We attribute the difference in magnitude of the results from this study versus Beak (1989) primarily to the use of adjacent velocity criteria in this study. A fourth habitat suitability index parameter will tend to result in overall lower amounts of habitat, since the combined suitability index is calculated as the product of the individual suitability indices. The effects of adjacent velocity are most pronounced at low flows, where a large proportion of the channel has low adjacent velocities, and thus low suitability for this parameter."

PG&E 12. Section 9. Biological Verification Data Collection and Section 11. Biological Verification *As has been recently discussed relative to the spawning habitat report, an test hypothesis that compound suitability is higher where fish are present than where they are absent does not provide reasonable scientific evidence that the HSC being tested represent the best available choice for predicting habitat use.*

Response: Our intent with the biological verification data collection and biological verification was not to determine that the HSC being tested represent the best available choice for predicting habitat use. Rather, our intent was to determine whether we could verify a combination of the hydraulic modeling and the habitat suitability criteria to increase the confidence in the application of the flow-habitat relationships that were generated from the habitat suitability criteria. In this context, we define "verify" as showing that fish are actively selecting a particular combination of habitat conditions, based on a higher combined suitability for occupied versus unoccupied locations. We believe that the failure of the biological verification was primarily due to errors in predictive accuracy of the hydraulic modeling (see pages 98 to 102 of the rearing report). We view the question of whether the HSC used in a study represent the best available choice for predicting habitat use as being addressed by the selection of the parameters (depth, velocity, cover and adjacent velocity) and the technique (logistic regression) used to develop the habitat suitability criteria. In this regard, we note that most instream flow studies, including the Beak (1989) study, would be characterized as unverified, since they do not include a bioverification component.

Results

PG&E 13. Section 1. Study Segment Delineation *Please refer to comment on Methods, Section 2.*

Response: See response to PG&E 5.

PG&E 14. Section 3. Field Reconnaissance and Study Site Selection *Please refer to comment on Methods, Section 4.*

Response: See response to PG&E 6.

PG&E 15. Section 5.4. River2D Model Velocity Validation *The report states that the models for three of the sites used in this study are questionable because the velocities would not calibrate, yet data from these sites appears to be used in the simulations presented in this report without any further caveats, or sensitivity analyses to evaluate the implications of these questionable sites to the overall results and conclusions. Please explain.*

Response: The addition of a downstream extension to one of the three sites resulted in the velocity simulations for that site validating. For the two sites for which the models are still in question because the velocity simulation would not validate, we added further caveats to the discussion under *Factors Causing Uncertainty*. We did not have sufficient resources to allow us to conduct a post-hoc sensitivity analysis to evaluate the implications of these sites where the models are in question.

PG&E 16. Section 6. Habitat Suitability Criteria Data Collection *Footnote 25 on page 38 notes that only one observation was made for each group of closely associated individuals. Was an estimate of the number of individuals in each group recorded? If so, the discussion on page should indicate how many total individuals are estimated to be represented by these observations. If these data were not collected, the report should explain why not. See also comments on Discussion, Section 5.*

Response: Yes, an estimate was recorded for the number of individuals in each group. We added the following text to the discussion under **Habitat Suitability Criteria Data Collection** to respond to the above comment:

> "Each observation in our study represented between 1 and 300 fish, with a median of 3 fish per observation."

PG&E 17. Section 8. Habitat Suitability Criteria Development *See comments on Methods, sections 8 and 9.*

Response: See responses to PG&E 7-12.

PG&E 18. Section 9. Biological Verification *See comments on Methods, Section 9.*

Response: See response to PG&E 12.

Discussion

Section 5. Habitat Suitability Criteria (HSC) Development

PG&E 19. *As noted previously, the specific application of polynomial logistic regression is currently under discussion for the spawning habitat report. Once a consensus is reached in that forum regarding the best scientific approach for HSC development, revision to this report will be necessary to be consistent.*

Response: See response to PG&E comment 7.

PG&E 20. *The report needs detailed sensitivity analysis to explain the influence of the adjacent velocity function applied here on the flow-habitat relationship that is presented*

Response: We did not have sufficient resources to allow us to conduct a post-hoc detailed sensitivity analysis to explain the influence of the adjacent velocity function on the flow-habitat relationship. See also response to YWCA comment 57, where we conducted a preliminary sensitivity analysis of the effect of the adjacent velocity parameter on the flow-habitat relationship. The following text from the discussion section is intended to address the influence of the adjacent velocity function on the flow-habitat relationship:

> "We attribute the difference in magnitude of the results from this study versus Beak (1989) primarily to the use of adjacent velocity criteria in this study. A fourth habitat suitability index parameter will tend to result in overall lower amounts of habitat, since the combined suitability index is calculated as the product of the individual suitability indices. The effects of adjacent velocity are most pronounced at low flows, where a large proportion of the channel has low adjacent velocities, and thus low suitability for this parameter."

PG&E 21. Section 6. Biological Verification *While the report concludes that the biological verification was unsuccessful, there is no investigation of the performance of other approaches. For example, application of HSC developed by BEAK to the FWS River 2D model and testing the predictive ability of that simulation would present a pure validation scenario, where the predictive ability of HSC developed with one years data is tested against another years data. The does not seem to even be an attempt to compare the predictive results of simulations using variations on* *the HSC presented in this report, for example with and without adjacent velocity criteria. Without this sort of rigorous and open- minded evaluation, this report cannot present a convincing case that the methods used in developing these simulations are scientifically credible.*

Response: Our approach is to use the best current scientific information in our studies. The HSC developed by Beak do not include important parameters for anadromous salmonid fry and juvenile habitat (cover and adjacent velocity) and are biased towards low depths and velocities because they were based entirely on use data. We do not think that it would be appropriate in the report to compare the predictive results of simulations with and without adjacent velocity criteria because simulations without adjacent velocity criteria would neglect a critical portion of the habitat requirements for anadromous juvenile salmonids, namely delivery of invertebrate drift. Without adequate food supply via invertebrate drift from fast-water areas, fry and juvenile Chinook salmon and steelhead are unable to grow and survive, a critical aspect of their habitat requirements.

Section 7. Habitat Simulation

PG&E 22. *This section dismisses the habitat simulations developed in the BEAK study on theoretical grounds, but does not provide any objective evidence of the superiority the new simulations presented here. See comments on discussion Section 6, above*

Response: Our intent in this study simply was to use a modern modeling approach (a 2d model); a modern statistical analysis (logistic regression) that could account for habitat availability; and to include estimates of two additional variables (cover and food availability) that are key to habitat selection by salmonid fry and juveniles. In this regard, we note that most instream flow studies, including the Beak (1989) study, would be characterized as unverified, since they do not include a bioverification component.

PG&E 23. *Figures 58 and 59 suggest that the rearing habitat in the Yuba River is much less sensitive to changes in flow than was previously thought. If this conclusion is correct, then there is no scientific urgency in issuing this report before all stakeholders can complete the ongoing evaluation of the draft spawning habitat report, and reach consensus on methods which may also be applicable to this report.*

Response: We agree that the rearing habitat in the Yuba River is much less sensitive to changes in flow than was previously thought. Funding was provided under Title 34, section 3406(b)(1)(B) of the Central Valley Project Improvement Act, P.L. 102-575, and the report is a "deliverable" for work identified in CVPIA Annual Work Plans, most recently for Fiscal Year 2010. We are required to finalize and release the report.

Greg Pasternack Comments

Overview

GP (Greg Pasternak) 1. *I am a strong proponent of the use of 2D modeling for instream flow assessment in rivers with highly non-uniform channels, such as the lower Yuba River. That only makes it that much more painful to see the outcome of this study in which no bioverified 2D model could be produced, despite spending $X00,000. However, I do not think the fault lies in general with 2D models, but rather with the experimental design of this one study. At every step of the procedure reported in this study, there were many highly questionable assumptions or actions that led to the poor outcome. In my judgment, the authors do not know how to produce an accurate topographic map. Further, even though they use RIVER2D regularly, I do not believe they understand fluid mechanics or how to properly tune the model parameters of a 2D model. The approach they use to obtain downstream water surface elevations is very poor and inaccurate. Most of all, it is very concerning that having obtained 2D model predictions that were invalid at several sites, the authors pushed forward with habitat modeling anyway without taking the time to make correct models! Then, even after 3 of 4 habitat models were found to NOT be bioverified by the authors' easiest measure, they went ahead anyway again and generated WUA results for all sites, regardless of whether hydraulic and/or habitat models were verified or not. I cannot express the level of shock I feel reading this report, with my jaw on the floor in disbelief. This report presents among the worst science I have ever reviewed. I feel awful submitting this review, but honesty requires valor in this instance.*

Response: We agree that 2D modeling is appropriate for conducting instream flow studies in rivers with highly non-uniform channels, such as the lower Yuba River. We acknowledge that we were unable to biologically verify the hydraulic and habitat models that were produced. However, the failure to biologically verify these models does not mean that the models are not useful for the purpose of this study, namely providing scientific information to use in determining instream flow needs for anadromous fish in the Yuba River. Rather, it means that we did not have an increased confidence in the use of the flow-habitat relationships from this study for fisheries management in the Yuba River. As discussed below, we feel that the experimental design, assumptions and actions of the instream flow study were appropriate. We would characterize the outcome as having a level of uncertainty, rather than as being a poor outcome. The outcome also needs to be viewed within the context of alternative sources of information that are available to use in determining instream flow needs for anadromous fish in the Yuba River. Specifically, despite the level of uncertainty in the outcome of this study, we feel that the results of this study are a more accurate assessment of the relationship between flow and anadromous salmonid fry and juvenile rearing habitat than the competing study results due to the improved methods used in this study, namely 1) HSC generated only from use data in the competing study, as opposed to the criteria generated with logistic regression in this study; 2) the lack of use of cover or adjacent velocity criteria in the competing study; and 3) the use of PHABSIM in the competing study, versus 2-D modeling in this study.

The accuracy of a topographic map is properly viewed on a continuum, from less accurate to more accurate, based on the density of topographic data. The topographic point densities, which influence map accuracy, fall within the range of reported values in published studies. For example, LeClerc et al. (1995) had a point density of 0.25 to 2 points/100 m^2, while Jacobson and Galat (2006) had a point density of 6 points/100 m^2. This study was one of our earlier River2D studies and we have been using higher point densities in more recent studies to try and improve the hydraulic predictions of our River2D models. We have been able to use higher point densities in our more recent studies because our new equipment (robotic total station and survey-grade RTK GPS) have enabled us to collect higher point densities (on the order of 40 points/100 m^2) within our time constraints for data collection. 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 our Yuba spawning study (which preceded 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 the Yuba spawning study and 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 depths and velocities would not be affected by over or under-predicted depths and velocities because over-predicted depths and velocities would have the opposite effect on the distribution of depths and velocities as under-predicted depths and velocities.

We believe that we have an adequate understanding of fluid mechanics to properly tune the model parameters of a 2D model, since we have followed best modeling practices, in terms of quantifiable definitions of how the model performs, as given in U.S. Fish and Wildlife Service (1994), Steffler (2002), Waddle and Steffler (2002), and Steffler and Blackburn (2002). With regards to the approach used to obtain downstream water surface elevations, see response to GP comment 3. We would not characterize the 2D model predictions as being invalid at several sites, rather we would characterize the predictions of the hydraulic models for those sites as having a higher degree of uncertainty. With the data we had available, making better models was not possible, and further time would not have allowed us to improve the velocity predictions for these sites. Further, because of channel changes since the data was collected, we were unable to collect additional data to improve the velocity predictions for these sites. Given that we had two possible options following the failure to validate the velocity predictions for these sites [1) to throw out these sites and represent flatwater habitat in the Below Daguerre segment by bar complex habitat; or 2) to use the sites], we feel like our decision to use the sites was the best (albeit not ideal) option. We believe that it would be more accurate to model rearing habitat in the Below Daguerre segment using these sites because if we threw out these sites, the rearing habitat would not include results from flatwater habitat types, which comprise 21 percent of the area of the Yuba River between Daguerre Dam and the confluence with the Feather River. We believe that the errors associated with simulated velocities for these sites are less than the errors that would be associated with representing flatwater habitats by bar complex habitats.

We acknowledge that we were unable to biologically verify 3 of the 4 the habitat models that were produced. However, the failure to biologically verify these models does not mean that the models are not useful for the purpose of this study, namely providing scientific information to use in determining instream flow needs for anadromous fish in the Yuba River. Rather, it means that we did not have an increased confidence in the use of the flow-habitat relationships from this study for fisheries management in the Yuba River. In this regard, we note that most instream flow studies, including the Beak (1989) study, would be characterized as unverified, since they do not include a bioverification component. We feel that we have used the best currently available science in conducting this study, given constraints of field equipment and time. While we do not agree with the commenter's assessment of this report, we appreciate his candor.

GP 2. *I recommend rejection of this report. The USFWS should not publish this.*

Response: Funding was provided under Title 34, section 3406(b)(1)(B) of the Central Valley Project Improvement Act, P.L. 102-575, and the report is a "deliverable" for work identified in CVPIA Annual Work Plans, most recently for Fiscal Year 2010. We are required to finalize and release the report.

Detailed comments follow by section.

Methods

GP 3. *On page 4 it says that PHABSIM was used to obtain upstream and downstream water surface elevations for a range of discharges. The problem with this approach is that PHABSIM does not account for momentum conservation, whereas the lower Yuba River is highly non-uniform and requires an accounting of convective acceleration terms. In the absence of directly measuring water surface elevations, what should have been done is that a model such as HEC-RAS that actually uses the 1D form of the momentum equation should have been used together with some known water surface elevations form the many past studies that have done HEC-RAS models on the lower Yuba River- most recently for the Upper Yuba River Studies Program effort. In a recent comparison paper, Brown and Pasternack (2008) showed that a model like HEC-RAS can do reasonably well at getting water surface elevations right, whereas Manning's equation and other empirical tools in PHABSIM will yield results that are far off the mark. Of course this issue has been known for decades, and is not a new finding per se. This is highly significance, because the 2D model takes the water surface elevations as inputs that drive all the depths and velocities in the modeling domain. I always thought in the past that this group used a model like HEC-RAS to get its water surface elevations, but if it has always used Manning's equation, then that would explain why the models perform so poorly. At a minimum, one could run HEC-RAS at a couple locations and see if the water surface elevations come out differently or not, and if so by how much. If it is more than 20% different, then you'd want to re-do the 2D models for sure.*

Response: PHABSIM was only used to develop stage-discharge relationships at the upstream and downstream boundaries. PHABSIM has three options than can be used to develop stage-discharge relationships: 1) *IFG4*, which is a strictly empirical regression method; 2) *MANSQ*, which operates under the assumption that the geometry of the channel and the nature of the streambed controls WSELs; and 2) *WSP*, the water surface profile model, which calculates the energy loss between transects to determine WSELs. As a strictly empirical method, with the exception of the stage of zero flow, *IFG4* does not account for momentum conservation. However, *IFG4* is comparable to the standard method used to develop stage-discharge relationships for gaging stations:

> Discharge ratings for gaging stations are usually determined empirically by means of periodic measurements of discharge and stage (Rantz et al. 1982, page 285).

MANSQ accounts for momentum conservation by assuming that the measured hydraulic slope is equal to the energy slope, based on an assumption of uniform flow (U. S. Fish and Wildlife Service 1994). In *MANSQ*, Manning's equation is solved for Manning's n at one discharge, based on measurements of water surface elevation, discharge, hydraulic slope and dimensions of the channel cross section. *WSP* accounts for momentum conservation by determining the energy slope between adjacent cross-sections, but requires input of a water surface elevation at the downstream-most cross section (U. S. Fish and Wildlife Service 1994). *WSP*, as a step-backward model, can explicitly account for convective acceleration terms by using the 1-D form of the momentum equation (the open-channel form of Bernoulli equation), and thus is functionally equivalent to HEC-RAS. It should be noted that the stage-discharge relationships for all of the upstream and downstream boundaries were developed using measured water surface elevations at the upstream and downstream boundaries; specifically for all sites except Side-Channel Site using water surface elevations measured at four different flows (Appendix D). We feel that it is more accurate to develop stage-discharge relationships using water surface elevations measured at the precise geographic location of the upstream and downstream boundaries, rather than using a HEC-RAS model that was developed for past studies, which likely would not have been developed using measured water surface elevations at the precise geographic location of the upstream and downstream boundaries. We were able to determine from the calibration process how well the stage-discharge relationships developed with *IFG4* or *MANSQ* did at getting water surface elevations right, through comparing simulated to measured water surface elevations at the flows at which water surface elevations were measured (Appendix D). The calibration process indicated: 1) for the locations where *IFG4* was used, there was a log-log stage-discharge relationship; and 2) for the locations where *MANSQ* was used (Lower Hallwood transect 1 at flows less than 1,900 cfs and Sucker Glide transect 2), the assumption of uniform flow was valid at those locations. Brown and Pasternack (2009) evaluated a fundamentally different question than considered in this study. Specifically, Brown and Pasternack (2009) evaluated water surface profiles (spatial variation) at two flows, while this study developed stage-discharge relationships at one location over a range of flows (variation in flow). In all of our past studies, we have used the models in PHABSIM to develop stage-discharge relationships at the upstream and downstream boundaries of our 2-D models and ensured they conformed well to measurements made at the study site boundaries. Given the successful calibration for the stage-discharge relationships, as shown by comparison with measured water surface elevations, we feel that it is unlikely that errors in velocity simulation are due to the stage-discharge relationships used at the downstream boundary or the water surface elevation at the upstream boundary used to calibrate the 2-D model. We feel that running a HEC-RAS model that was developed for past studies would not be useful for validating the stage-discharge relationships developed in this study, since differences between the output of HEC-RAS and the stage-discharge relationships used in this study could as likely be due to errors in the HEC-RAS model resulting from sampling over the coarse spatial scale of the HEC-RAS model, versus errors in the stage-discharge relationships used in this study.

GP 4. *Hydraulic roughness is well known to be a function of the grain size to depth ratio, which varies with discharge. Consequently, the roughness parameter in the model should be different for each discharge. As discharge increases, spatial variation in depth and velocity decreases, so doing calibration at the highest discharge constitutes doing the least rigorous test of the 2D model. I always check the lowest discharge the most thoroughly in calibration and validation, because it has the most complex flow pattern and thus provides the most rigorous test.*

Response: River2D uses a Chezy coefficient for its roughness parameter; the Chezy coefficient is a function of the bed roughness height (a multiple of grain size plus cover size) to depth ratio, and thus the roughness parameter in River2D is different for each discharge (Steffler and Blackburn 2002). Our assumption has been that the water surface elevation at the upstream end of the site, which is what we use for calibration, is most sensitive at the highest simulation flow, versus the lowest simulation flow, and thus that doing calibration at the highest discharge would be the most rigorous test of the 2D model. As shown in the table below, there is no clear trend in spatial variation at high versus low flows. However, overall there is a larger difference in water surface elevation between the upstream and downstream end of the sites at the highest flow where we measured water surface elevations, as compared to the lowest flow where we measured water surface elevations, supporting our assumption that spatial variation in water surface elevation is greater at high, versus low, flows. If our calibration was based on spatial variation in depth and velocity, rather than water surface elevation, we would agree with the commenter that calibration at the lowest discharge would be the most rigorous test of the 2D model.

	Difference Between WSEL (ft) at Upper versus Lower Transect		Highest Divided
Site	Lowest Measured WSEL Flow	Highest Measured WSEL Flow	By Lowest
Diversion	0.79	0.89	113%
Lower Hallwood	0.42	1.19	280%
Narrows	0.22	1.39	630%
Railroad	0.04	0.03	88%
Rose Bar	5.90	5.36	91%
Side Channel	0.00	0.70	
Sucker Glide	0.34	0.19	57%
Whirlpool	2.15	1.56	72%
		Average	190%

We also checked the performance of the 2D model at the lowest simulation flow to further test our assumption[1]. As shown on the following page, the Narrows, Lower Hallwood, Whirlpool and Railroad sites successfully calibrated at the lowest simulation flow, with all WSELs predicted by River2D at the upstream end of the site (transect 2) falling within 0.10 feet of the WSELs predicted by PHABSIM at that location. For Diversion Site, the performance of River2D was equivalent at the highest and lowest simulation flows – River2D overpredicted the WSEL at

[1] We were not able to check the performance of the Side Channel site at the lowest simulation flow since we were unable to develop a stage-discharge relationship for this site.

Narrows Site

XSEC	Br Multiplier	Difference (measured vs. pred. WSELs, feet) Average	Standard Deviation	Maximum
2	1.0	0.08	0.001	0.08

Rosebar Site

XSEC	Br Multiplier	Difference (measured vs. pred. WSELs, feet) Average	Standard Deviation	Maximum
2	0.75	0.24	0.02	0.27
2	0.8	0.19	0.02	0.22
2	1.0	0.25	0.02	0.28

Diversion Site

XSEC	Br Multiplier	Difference (measured vs. pred. WSELs, feet) Average	Standard Deviation	Maximum
2	0.3	0.12	0.01	0.14

Lower Hallwood Site

XSEC	Br Multiplier	Difference (measured vs. pred. WSELs, feet) Average	Standard Deviation	Maximum
2	0.55	0.04	0.001	0.04

Whirlpool Site

XSEC	Br Multiplier	Difference (measured vs. pred. WSELs, feet) Average	Standard Deviation	Maximum
2	0.7	0.01	0.005	0.02

Sucker Glide Site

XSEC	Br Multiplier	Difference (measured vs. pred. WSELs, feet) Average	Standard Deviation	Maximum
2	0.3	0.67	0.001	0.68

Railroad Site

XSEC	Br Multiplier	Difference (measured vs. pred. WSELs, feet) Average	Standard Deviation	Maximum
2	1.0	0.03	0.003	0.03

the highest simulation flow by 0.12 feet[2] and underpredicted the WSEL at the lowest simulation flow by 0.12 feet. The overprediction of the upstream WSEL for Sucker Glide site was due to an eddy at the downstream boundary which would require a downstream extension to resolve, rather than a change in the bed roughness multiplier for this site. For Rosebar Site, the performance of

[2] The Diversion site was calibrated at the highest flow where we measured a WSEL.

River2D at the lowest simulation flow was not sensitive to the BR value, indicating that we could not have significantly improved the calibration of this site at low flows by using a different BR value.

GP 5. *The study site selection explanation is poorly written and very unclear. Ok, I see now that the problem is that the author is considering the delineation, habitat mapping, and site selection as results. Huh? Site selection is not a result, but part of the study methodology. Please state the number and area of the mesohabitat units present in each of the two segments in the methods section. Then state the number of study sites within each unit within each segment. Do any units have zero study sites? How do you define "less common" types? Please provide actual numbers to backup the claim that the composition of the study sites was reflective of the mesohabitat composition of the entire reach. That would be more concise and more informative.*

Response: The current format of our reports, where we have separate methods, results and discussion for each study task in Table 1, was based on peer review comments we received on previous reports. The relative clarity of different report formats is a subjective judgment that varies from reader to reader. We feel that the current format of our reports most closely conforms to that of peer-reviewed journal articles, and thus we have retained the format, with separate methods and results for delineation, habitat mapping and site selection. The number and area of mesohabitat units present in each of the two segments is given in Table 8, while Table 9 gives the number of mesohabitat units in the study sites of each habitat type within each segment. We are assuming that when the commenter mentions units, the commenter is actually referring to mesohabitat types. Since most study sites included multiple mesohabitat units and multiple mesohabitat types, it is not possible to give the number of study sites within each mesohabitat type within each segment. As shown in Table 8, no flatwater glide and flatwater riffle mesohabitat units were present in any of the study sites for the Above Daguerre Segment, and no flatwater riffle, side channel riffle or side channel glide mesohabitat units were present in any of the study sites for the Below Daguerre Segment. In addition, cascades were not present in any study sites. For all of the above, these mesohabitat types were either absent (flatwater riffle in the Below Daguerre Segment), or comprised less than 2 percent of the total area in that segment and there were only 1 or 2 mesohabitat units of that mesohabitat type present in the segment (see Table 8). We defined less common types as those which comprised less than 15 percent of the total area in a given segment. We added Table 10 to the report to provide actual numbers to demonstrate that the composition of the study sites was reflective of the mesohabitat composition of the entire reach.

GP 6. *The claim that you cannot navigate the Narrows to get to the Englebright Dam Reach is misleading, because there is easy drive-in access at the junction with Deer Creek that I use all the time. We were able to make a detailed topographic map of that entire section with no difficulty at all and we have run 2D models of it as well.*

Response: We were unaware of the privately-owned, drive-access at the junction with Deer Creek. Accordingly, based on our knowledge during the study we were unable to sample above the Narrows. We applaud the commenter for making a detailed topographic map and running 2D models of that entire section.

GP 7. *It is very notable to me that for the segment upstream of Daguerre, the study dramatically overrepresents riffles and underrepresents the long, calm glides. For example, between the Narrows Site and Rose Bar Site there is a long glide that has a lot of complex bank topography and riparian vegetation along the south bank. That is where fry and juveniles are likely to be feeding. Same too upstream of the UC Sierra Site. Overall, I consider the site selection to be a failure. The lesson I have learned from this failed effort is to abandon site selection and move on with mapping and modeling entire river corridors with 2D models, retaining the same high resolution that was used at the site scale, thanks the much better 2D models than RIVER2D that now exist. I feel bad about it, given how much work was done by the USFWS, but the effort was in vain in my judgment.*

Response: Glides were well represented in the study sites upstream of Daguerre. As shown in Table 9, the study sites upstream of Daguerre contained a total of four bar complex glides and one side channel glide. In fact, bar complex glides were represented more than bar complex riffles in the study sites upstream of Daguerre, relative to their abundance in the segment. As shown in Table 29, the ratio of total area of bar complex glides to the area of bar complex glides in the study sites upstream of Daguerre was 2.34, while the ratio of total area of bar complex riffles to the area of bar complex riffles in the study sites upstream of Daguerre was 2.86. In other words, the study sites upstream of Daguerre contained, respectively, 43 percent (1/2.34) and 35 percent (1/2.86) of the total area of bar complex glides and bar complex riffles upstream of Daguerre. It should also be noted that the application of the ratios in Table 29 resulted in the total segment habitat exactly representing the habitat composition of the segments upstream and downstream of Daguerre. Accordingly, we feel that site selection was successful. Mapping and modeling entire river corridors serves useful functions, such as evaluating whether study sites are representative of the depth and velocity distributions of entire segments. However, mapping and modeling entire river corridors is not yet able to be of use in simulating juvenile salmonid habitat because cover, a critical element of juvenile salmonid habitat, cannot yet be cost-effectively mapped in for an entire river corridor.

GP 8. *In operating a total station many sources of error accumulate. Temperature, wind, and direct sunlight changes throughout the day are a significant source of error if left unchecked. Reporting the "accuracy" of the instrument itself is not useful. Normal protocols call for measuring known points periodically throughout each day in the field and recording horizontal and vertical deviations. Then in a report one reports the mean and standard deviation of the checks performed throughout a study.*

Response: We agree that many sources of error accumulate in operating a total station. However, based on the limited data we have available, as discussed below, it does not appear that errors from total station operation in this study were significant. To test what effect temperature, wind and direct sunlight changes throughout the day may have had on our data, we did a test with the total station that we used for this study, where we made 50 measurements on the same location. The measurements were made with the measurement location positioned 266 feet from the total station, which was the average slope distance for all of the total station measurements we made for the Yuba River study. Based on the results of this test, as shown in the table on the following page, temperature, wind and direct sunlight changes are an insignificant source of error (i.e., typically within 0.1 ft).

	Northing (feet)	Easting (feet)	Bed Elevation (feet)
Mean Points 1-10	137.9	226.5	102.83
SD Points 1-10	0.09	0.15	0.002
Mean Points 11-20	137.8	226.4	102.83
SD Points 11-20	0.001	0.001	0.000
Mean Points 21-30	137.8	226.4	102.83
SD Points 21-30	0.002	0.002	0.001
Mean Points 31-40	137.8	226.4	102.83
SD Points 31-40	0.001	0.002	0.001
Mean Points 41-50	137.8	226.4	102.84
SD Points 41-50	0.002	0.003	0.001

We acknowledge that the manufacturer's specifications for the total station represent a best-case scenario for the accuracy of the total station data. However, since we do not have any other information available to assess the accuracy of the data, the reported accuracy of the instrument is the best information that we have available, and thus we have retained that information in this report. We did not measure known points periodically throughout each day and so do not have information on horizontal and vertical deviations. However, we do have multiple measurements on the horizontal location of known points made on different days. For the 15 known points where we had known measurements, the median standard deviation of the northing and easting was 0.2 feet. We do not have any multiple measurements on known locations that can be used to determine vertical deviations.

GP 9. *The authors appear to have taken their topo points and built a DEM using those points without going through a careful QA/QC process. That process should include point-by-point inspection to eliminate quantitatively wrong points as well as a qualitative process in which you re-iteratively build surfaces and check the resulting features against visual observations to make sure you have the landforms right. This is particularly important where there are bedrock outcrops that may yield flow recirculations around them if they are pointy or not if they are streamlined.*

Response: The QA/QC process that we went through was not identified in the draft report. The process we went through included a point-by-point inspection to eliminate quantitatively wrong points and a qualitative process where we checked the features constructed in the DEM against aerial photographs to make sure we had represented landforms correctly. We have added a description of this QA/QC process to the final report.

GP 10. *I have heard the report's lead author criticize many others for not making topographic maps and 2D models that capture conditions at the 1' scale at which fry and juveniles reside. Holding the author to the same standard, the methods described in the paper do not come close to resolving conditions at the spatial scale of the bed and banks where fry reside. Since this whole study is about fry and juvenile habitat, I have to wonder whether the model being created actually has any relevance to the question at hand? If you haven't mapped it, then you cannot model it. It is clear from the report that you have not mapped it.*

Response: We feel that, given the constraints of the equipment and time we had to conduct the study, we did the best job we could do to resolve conditions at the spatial scale of the bed and banks where fry reside. While we agree that the model could have done a better job of modeling fry and juvenile habitat if we had been able to collect mapping data at a higher point density, we feel that the model that we have developed of fry and juvenile habitat is better than any other information that is available for the Yuba River. Specifically, despite the level of uncertainty in the outcome of this study, we feel that the results of this study are a more accurate assessment of the relationship between flow and anadromous salmonid fry and juvenile rearing habitat thancompeting study results due to the improved methods used in this study, namely 1) HSC generated only from use data in the competing study, as opposed to the criteria generated with logistic regression in this study; 2) the lack of use of cover or adjacent velocity criteria in the competing study; and 3) the use of PHABSIM in the competing study, versus 2-D modeling in this study.

GP 11. *The statement on p.12 that "these parameters were collected at enough points to characterize the bed topography, substrate, and cover of the sites" is highly questionable. All three of these variables exhibit variation as a function of spatial scale. In other words, the close you look, the more details you see. So what should be stated instead is a) the spatial scale of measurement, b) an explanation of why this is correct for the study at hand, and c) the number and density of points used for each variable at each site. You just can't say you "did enough" and not provide numbers and details. That is not science.*

Response: We agree that bed topography, substrate and cover exhibit variation as a function of spatial scale. We collected bed topography, substrate and cover data for every point. We are not sure exactly what information the commenter suggests should be provided with regards to the spatial scale of measurement. If the commenter is referring to the average spacing of points, the density of points already gives that information. The number and density of points used for all three variables at each site is given in Table 11. It would not be appropriate to provide these numbers and details on page 12 since the data in Table 11 are results and the text on page 12 is part of the methods.

GP 12. *The last paragraph before section 7 on p 14 is incorrect. It claims that the hydraulic control on the downstream transect of a site is governed by gradient and bed elevation. That is not true. The relation between discharge, velocity, and water depth is governed by mass and momentum conservation, including large non-uniform terms. This means that the hydraulic control can be related to bed elevation, but it can also be related to channel width. So if the channel becomes narrower, then that can back water up even if the bed continues to go down. Conversely if the channel is suddenly wider. Also, we know that on the Yuba, like many other rivers, the location of the hydraulic control changes as discharge changes, because the river corridor includes diverse bar forms, bedrock outcrops, terraces, etc. These are all reasons why the PHABSIM method used to get the downstream water surface elevations is highly inaccurate. The method proposed in the paragraph in question on p. 14 does not solve the problem.*

Response: We believe that the commenter has misinterpreted the text in question. It does not claim that the hydraulic control on the downstream transect of a site is governed by gradient and bed elevation. Instead, it describes how the value of the stage of zero flow parameter is determined. We agree that the relation between discharge, velocity, and water depth is governed by mass and momentum conservation, including large non-uniform terms, and that the hydraulic control can be related to both bed elevation and channel width. Effects of channel width as described by the commenter are described by the empirical measured stage data used to develop the stage-discharge relationship. We agree that the location of the hydraulic control changes as discharge changes, because the river corridor includes diverse bar forms, bedrock outcrops, terraces, etc. In fact, we observed this phenomena, otherwise known as compound controls, for the Lower Hallwood site and addressed it by breaking the calibration into two flow ranges. See response to GP comment 3 regarding why the PHABSIM method used to get the downstream water surface elevations was highly accurate. The methods proposed in the paragraph in question are the standard method used to determine the correct value of the stage of zero flow parameter (Bovee 1995a).

GP 13. *Equation 1 on p. 20 requires calibration at every cross-section according to standard usage, because the hydraulic controls at each site are often unique. You can't merely take some value like 1.666 out of a hat and assume it works. To use 4 significant figures for that exponent when it can vary over a wide range as proven in the literature shows a complete lack of understanding of what the equation means and how it should be used.*

Response: This equation was only used to calibrate the downstream boundary condition for the Side Channel site. By definition, this equation only applies at the downstream boundary. The upstream transect at the Side Channel site was calibrated by adjusting the bed roughness multiplier until the WSEL simulated by River2D matched the measured WSEL at that transect. Since we only had one WSEL to use in calibrating the Side Channel site, we were only able to calibrate one of the two parameters (K and m) in this equation. Since this equation is more sensitive to the value of K than of the value of m (Peter Steffler, personal communication), we chose to calibrate the equation by varying the value of K. Based on the above, we had no choice but to use the default value of m and assume it would work. Given the constraints of the data we had at hand, we feel we made the best use possible of this equation.

GP 14. *I give the authors credit for finally stating a criterion for concluding whether the 2D model's velocity predictions are "validated" or not. However, the authors still fail to recognize that a 2D model must also be validated for its flow pattern. This is all the more important, because after many years of criticism, they still insist on using a default arbitrary value for the important eddy viscosity parameter in RIVER2D. This parameter controls the ability of the model to predict truly 2D flows, such as recirculations. I do not expect a quantitative measurement of lateral velocities, but at a minimum there should be a check to see if any recirculations exist in the 2D model of a study site, and if so, do they exist in those same locations at the real site too? Conversely, are there real recirculations that the model is not showing? I have never seen the authors show a 2D model showing recirculations or test qualitatively whether they are supposed to be there or not. Neglecting that is tantamount to neglecting the 2D aspect of the model. Considering that fry and juvenile habitat relates to flow* *recirculations, this is a very important thing to check and report on. I note that on p. 21 the authors go out of their way to define and describe "adjacent velocity", but they do not provide any evidence that their 2D model can capture this phenomenon.*

Response: With a few exceptions, such as that noted by the commenter, we do not have the empirical data to use to validate the 2D model for its flow pattern. Based on the advice we have gotten from the developer of River2D, the default value of the eddy viscosity parameter in River2D is appropriate and does not control the ability of the model to predict truly 2D flows, such as recirculations. 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)."

Appendix I includes a figure showing the existence of recirculations in the 2D model of a study site. We lack the empirical data to determine if the recirculations exist in the same locations at the real site or if there are real recirculations that the model is not showing. The figure in Appendix I shows a 2D model showing recirculations. We do not have the empirical data to test qualitatively whether they are supposed to be there or not. We agree that fry and juvenile habitat relates to flow recirculations. We acknowledge that our lack of data in this regard does not allow us to check and report on flow recirculations predicted by the 2D model. While we acknowledge that the 2D models may have errors in predicting adjacent velocities, we feel that it still is important to include adjacent velocity as a component of fry and juvenile habitat because simulations without adjacent velocity criteria would neglect a critical portion of the habitat requirements for anadromous juvenile salmonids, namely delivery of invertebrate drift. Without adequate food supply via invertebrate drift from fast-water areas, fry and juvenile Chinook salmon and steelhead are unable to grow and survive, a critical aspect of their habitat requirements.

GP 15. *In the biological verification description (sections 9 and 11), the test proposed merely looks for differentiation between occupied and unoccupied sites, regardless of the absolute values of the compound suitability index at a site. However, HSC are designed to give values from 0-1, with values close to 1 representing high quality habitat where fish are actually observed. No curves are constructed with values topping out at 0.5. So given that fish actually exist in areas with high index values (0.7-1.0), then for a model to be bioverified, it would also have to produce predictions with high index values where the fish are observed. The authors can develop their own thresholds, but in my peer reviewed publications in which I have reported bioverification of 2D model predictions, I have had no problem confirming the accuracy of predictions of index values of 0.7-1.0 as well as those of 0.4-0.7. So if the mean compound suitability value predicted by the 2D model for occupied sites is less than 0.4, I would have to declare the model to be a failure and NOT verified at all. The authors can pick a different value at their discretion, but to not pick anything at all is to essentially not test the 2D model, and that is not acceptable. The point of science is to provide objective tests.*

Response: We agree that the biological verification test looks for differentiation between occupied and unoccupied sites and does not use the absolute value of the compound suitability index at a site. However, we note that the biological verification test used in this report has been used in a number of peer-reviewed journal articles (Gard 2006, Gard 2009, McHugh and Budy 2004). While the commenter is correct that HSC are designed to give values from 0-1, with values close to 1 representing high quality habitat, the effects of availability can restrict the number of fish that are found in high quality habitat. Specifically for this study, the Yuba River has very limited availability of areas with low velocity and high adjacent velocity. Although the logistic regressions demonstrate that fish have a strong preference for low velocities and high adjacent velocities, the very limited availability of areas with low velocity and high adjacent velocity results in few fish being found under such habitat conditions. The commenter is incorrect that the curves are constructed with values topping out at 0.5 – as shown in Appendix K, all of the criteria have HSI values that reach 1.0. For example, for fall/spring-run Chinook salmon fry, HSI values are 1.0 for a velocity of 0 feet/sec, water depths of 1 to 1.4 feet, adjacent velocities of greater than 3.6 feet/sec, and cover codes 3.7, 4, 4.7, 5, 5.7 and 8. If all four of these conditions are present at a location, the combined suitability, calculated as the product of the four individual suitabilities, equals 1.0. Because of the limited availability of preferred habitat conditions, very few fish will be found in areas with optimal suitability for all four parameters. Accordingly, for a model to be bioverified, we would not expect for the model to predict high index values at many of the locations where fish are observed. The commenter also does not consider the effect of how the compound suitability is calculated on the resulting value of compound suitability. Even if the suitability for each of the four parameters is high (0.7 as suggested by the commenter), the compound suitability would only be 0.24 (0.7 x 0.7 x 0.7 x 0.7). We note that the commenter in his publications only uses two parameters (depth and velocity) and computes the combined suitability as the geometric mean of the individual suitabilities. Thus, it is not surprising that the commenter has many observations where the model predicts a combined suitability of 0.7-1.0, since with two parameters and a geometric mean calculation, the compound suitability would be 0.7 if both the depth and velocity suitabilities are 0.7. Based on the above discussion, the absolute value of the compound suitability at occupied locations is dependent on the availability of preferred habitat conditions, the number of habitat parameters, and the method used to calculate compound suitability. Thus a mean compound suitability of 0.4 for occupied locations can indicate a successful model, and an evaluation based on the mean compound suitability for occupied locations is meaningless. We feel that the test we have chosen, showing that the compound suitability is greater for occupied versus unoccupied locations, correctly captures the biological mechanisms behind habitat choice (that fish are preferentially selecting locations with higher suitability) and that this test is objective.

Results Section

GP 16. *I give the authors credit for providing a reasonably good velocity validation description in section 5.4. Now if you add an assessment of flow pattern you'd have it right.*

Response: We do not have the empirical data that would be required to conduct an assessment of flow pattern.

GP 17. *I am not reviewing the construction of the HSC. I'll leave that to others.*

Response: See responses to PG&E comments 7-11, 16-17 and 19-20, and YWCA comments 1, 4, 9-11, 14, 20-25, 28, 38-49, and 51-55.

GP 18. *Spring/fall-run Chinook fry HSC were found to NOT be bioverified by any measure. Ok.*

Response: The commenter is correct. We define bioverify" as showing that fish are actively selecting a particular combination of habitat conditions, based on a higher combined suitability for occupied versus unoccupied locations. We believe that the failure of the biological verification was primarily due to errors in predictive accuracy of the hydraulic modeling (see pages 98 to 102 of the rearing report). In this regard, we note that most instream flow studies, including the Beak (1989) study, would be characterized as unverified, since they do not include a bioverification component.

GP 19. *Steelhead/rainbow trout fry HSC were found to NOT be bioverified by any measure. Ok.*

Response: See response to comment GP 18.

GP 20. *Steelhead/rainbow trout juvenile HSC were found to NOT be bioverified by any measure. Ok.*

Response: See response to comment GP 18.

GP 21. *Spring/fall-run Chinook juvenile HSC were found to meet the Mann-Whitney U test, but the median index value for occupied sites was just 0.358. Given that the value is below 0.4, I consider that a failure of the 2D model, and thus I would judge that the model is NOT bioverified. Having index values greater than zero does not constitute a meaningful standard for evaluating habitat quality or abundance.*

Response: As mentioned in the response to GP comment 15, the absolute value of the median index value for occupied locations is meaningless and cannot be used to evaluate the success or failure of the 2D model and whether the model has been bioverified. We agree that having index values greater than zero does not constitute a meaningful standard for evaluating habitat quality or abundance. Instead, we feel that a test showing that fish are preferentially selecting locations with higher suitability is a meaningful standard for evaluating habitat quality or abundance.

GP 22. *In summary, ~$X00,000 was spent to produce a model capable of predicting fry/juvenile rearing habitat quality and abundance, and the outcome was that 3 of the 4 predictive tools turned out to be useless by any measure. One of them meets a very low standard of utility, but is so poor that in my judgment it is also useless.*

Response: We acknowledge that we were unable to biologically verify 3 of the 4 the habitat models that were produced. However, the failure to biologically verify these models does not mean that the models are not useful for the purpose of this study, namely providing scientific information to use in determining instream flow needs for anadromous fish in the Yuba River. Rather, it means that we did not have an increased confidence in the use of the flow-habitat relationships from this study for fisheries management in the Yuba River. In this regard, we note that most instream flow studies, including the Beak (1989) study, would be characterized as unverified, since they do not include a bioverification component. We feel that the standard of utility is appropriate since it shows that fish are preferentially selecting locations with higher suitability.

Discussion

GP 23. *Figure 34 shows a recirculation in the model that the authors claim is not actually present at the site. Ok, well that is why your model validation effort needs to include a qualitative flow pattern evaluation every time. If the flow pattern did not match, then why did you move forward from that point with HSC calculations? You should stop right there and get the model right, probably by re-scouting the site and editing the topography to be more correct. Why go forward with the habitat part if the hydraulic part is wrong.*

Response: Subsequent to the draft report that was reviewed by the stakeholders, we added a downstream extension to the site in question. The downstream extension resolved the problem in question.

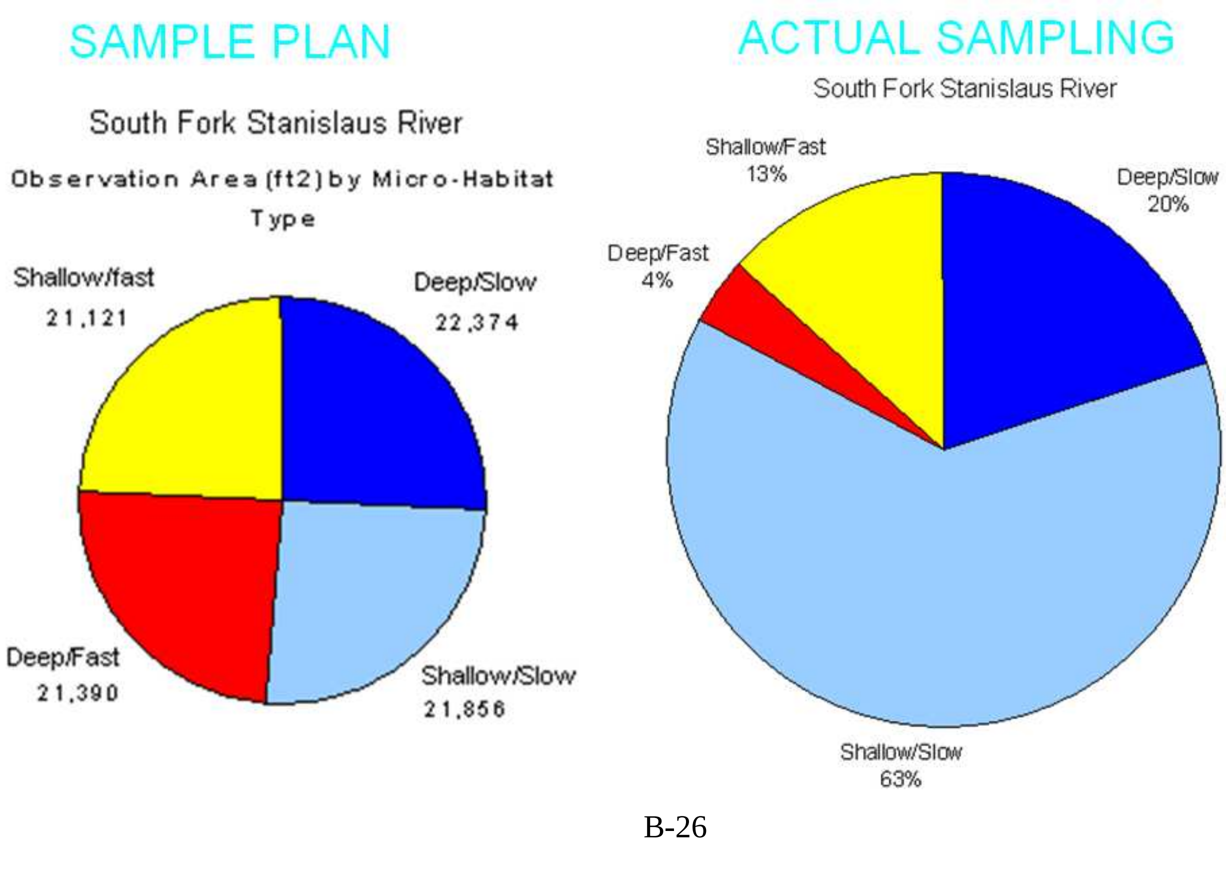
Yuba County Water Agency Comments

YCWA (Yuba County Water Agency) 1. Comment: *As discussed in the enclosed comments, this draft report is not reliable or usable in its current form, because of its reliance on the logistic regression-based approach to develop habitat suitability criteria (HSC) from observational data. The problems of using this approach to develop HSC are demonstrated by Figures 10-16 on pages 51-57 of the draft report. Each of these figures shows a clear and incorrect shift of higher habitat suitabilities towards greater depths and higher velocities. Had the study described in the draft report instead used a sampling strategy for its habitat use observations where the observations were stratified equally over the range of depths and velocities in the lower Yuba River, there would have been less potential for bias in its observational frequencies and probably not even a need for the mathematical adjustment that was made in the study. Such equal-area sampling strategies are the preferred method of the Instream Flow Group.*

Response: We feel that the report is reliable and useable in its current form. We note that 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. For example, McHugh and Budy (2004) state (page 90):

"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)."

While we agree that Figures 10-16 show a clear shift of higher habitat suitabilities towards greater depths and higher velocities, we feel that this shift is correct because it takes into account the effect of the limited availability of deeper and faster conditions in the Yuba River. By taking availability into account, logistic regression results in habitat suitability criteria that are not biased by availability. It has been demonstrated that a sampling strategy which attempts to spend equal effort sampling different ranges of depths and velocities does not work. Specifically, Allen (2001) attempted to sample equal areas of two different ranges of depths and velocities in collecting habitat suitability criteria. However, as shown in the figure below, he was unable to sample equal areas of the two different ranges of depths and velocities due to the limited availability of faster deeper conditions. We attempted a similar approach on the Yuba River to sample equal areas with and without cover and were similarly unsuccessful, sampling 6.5 miles without cover and only one mile with cover. With regards to equal stratification of sampling, we believe the commenter may be referring to habitat use measurements being collected with equal effort in different mesohabitat types, to attempt to address effects of availability. While we did make an effort to sample all mesohabitat types, so as to sample all of the available habitat for fry and juvenile anadromous salmonids, we did not sample equal areas of different mesohabitat types for the following reasons: 1) equal area sampling of different mesohabitat types does not adequately address the effects of availability on habitat use; 2) the use of logistic regression takes into account the effects of availability on habitat use; and 3) at least for larger rivers, mesohabitat



type has little to do with fry and juvenile anadromous salmonid habitat use, based on our snorkel surveys on the Sacramento River (U.S. Fish and Wildlife Service 1996). Our snorkel surveys indicated that the scale of mesohabitat units for large rivers is so much larger than the scale of habitat use that habitat use is controlled by microhabitat parameters, rather than mesohabitat types. Since it is not possible to stratify observations equally over the range of depths and velocities in the Yuba River, the mathematical adjustment used in this study, namely logistic regression, is required to generate habitat suitability criteria that are not biased by availability. Based on the studies that have been conducted (i.e., Allen 2001) since the Instream Flow Group, currently part of the U.S. Geological Survey office in Fort Collins, Colorado, made their recommendation in 1996 for equal-area sampling strategies, this approach is not feasible, and thus other approaches, specifically logistic regression, are now applied to address the effects of availability.

YCWA 2. Comment: *The deficiencies of the draft report also are demonstrated by some of its results. For example, the total usable areas in the draft report are only 1.0 to 1.5 percent of the total area of the lower Yuba River. This indicates a serious problem, because results for standard PHABSIM studies become questionable when the calculated usable percentage of total area is below about 5 percent. This is because the very small numbers of sample points used to develop the index are subject to the influence of minor hydraulic modeling errors and even slight shifts resulting from the manner in which the HSC were created.*

Response: We feel that the information in the report is useful for its intended purpose, developing scientific information to use in determining instream flow needs for anadromous salmonids in the Yuba River. The low proportion of the total area of the Yuba River that is useable habitat for fry and juvenile anadromous salmonids reflects the limited availability of preferred habitat conditions in the Yuba River, for example a combination of a velocity of 0 feet/sec, water depths of 1 to 1.4 feet, adjacent velocities of greater than 3.6 feet/sec, and cover codes 3.7, 4, 4.7, 5, 5.7 and 8 for fall-run Chinook salmon fry. We also note that the total useable areas in the Beak (1989) report based on PHABSIM were even lower than in our draft report for Chinook salmon fry at all flows and for Chinook salmon juveniles at higher flows[3]. We do not view the low proportion of the total area that is useable habitat for fry and juvenile anadromous salmonids in our study as problematic. While we are not familiar with the 5 percent rule of thumb mentioned by the commenter, it is unlikely that such a rule of thumb would apply to River2D models. We would imagine that the 5 percent rule of thumb comes from PHABSIM studies with 10-20 transects, where only a few cells per transect have suitable habitat conditions. In contrast, River2D avoids problems of transect placement, since data are collected uniformly across the entire site (Gard 2009). As such, River2D would not have problems with very small numbers of sample points used to develop weighted useable area values. With the entire site being modeled, minor hydraulic modeling errors have a minimal effect on the overall flow-habitat relationship. Similarly, we would expect that slight shifts resulting from the manner in which the HSC were created would not be a problem with the entire site being modeled. Accordingly, such a threshold value for River2D would likely be much less than 5 percent.

[3] There was an error in Figures 56 to 59 of our draft rearing report, in that the WUA values for our study in those figures were plotted in square meters, rather than in square feet. This error has been corrected in the final report.

YCWA 3. Comment: *Another indication of the potential problems of the draft report is the different directions of the changes in weighted usable area (WUA) indices in relation to changes in discharge that are shown for Chinook salmon and steel head fry rearing in Figures 25, 27, 29 and 3 I on pages 68-71. The draft report's WUA habitat indices for these two very similar life stages show opposite responses to changes in discharge, with the Chinook salmon fry index maximizing at very high flows and the steelhead fry index maximizing at much lower flows. This type of result is illogical and raises serious questions about whether or not the draft report's results accurately represent the relationships between flow and anadromous fish juvenile rearing habitat.*

Response: We feel that the different directions of the changes in WUA indices with changes in discharge for Chinook salmon and steelhead fry reflect a key difference in habitat selection by the two species, and thus do not indicate a problem with the draft report. While we agree that these two life stages are similar, there is an important difference in habitat use between the species – namely the much greater use of cobble cover by steelhead fry, versus Chinook salmon fry. As a result, the suitability for this cover category is more than twice as high for steelhead fry (0.57) than Chinook salmon fry (0.25). This difference in habitat use carries over into the flow-habitat relationships. At low flows, most of the Yuba River wetted channel has cobble cover, which together with low velocities at low flows, results in the highest amount of habitat for steelhead fry at these flows. In contrast, the amount of habitat at low flows is less for Chinook salmon because of the lower suitability for cobble cover. At higher flows, where woody cover is inundated, the amount of habitat for Chinook salmon fry is higher than at low flows, due to the large difference in suitability for woody (1.0) versus cobble (0.25) cover overwhelming decreases in habitat associated with increasing velocities. In contrast, steelhead fry, with a much smaller difference in suitability for woody (1.0) versus cobble (0.57) cover, end up with less habitat at higher flows since the negative effect of higher velocities swamps out the positive effect of inundation of high-suitability cover. Accordingly, this result is logical and is consistent with the report results accurately representing the relationships between flow and anadromous fish juvenile rearing habitat.

YCWA 4. Comment: *Because of the problems regarding habitat suitability criteria (HSC) development and related matters that are discussed in the enclosed comments, we recommend that the draft report not be finalized, that the HSCs be re-calculated or alternative HSCs be used, and that the existing or new hydraulic models be run with the new or alternative HCSs to estimate WUA-discharge relationships for juvenile spring/fall-run Chinook salmon and steelhead/rainbow trout rearing in the lower Yuba River. Without these revisions, we believe that this report will not represent the best possible management and decision support tool that can be produced for the lower Yuba River.*

Response: As discussed in our responses to YCWA comments 8-59, we do not feel that there are problems regarding habitat suitability criteria development and related manners that would warrant not finalizing the report. The report uses the current state of the art for conducting instream flow studies and will be a valuable management and decision support tool for the lower Yuba River. Funding for this study was provided under Title 34, section 3406(b)(1)(B) of the

Central Valley Project Improvement Act, P.L. 102-575, and the report is a “deliverable” for work identified in CVPIA Annual Work Plans, most recently for Fiscal Year 2010. We are required to finalize and release the report.

YCWA 5. Comment: *As you may be aware, the Lower Yuba River Management Team (RMT), which is comprised of representatives of California Department of Fish & Game, National Marine Fisheries Service, the U.S. Fish & Wildlife Service, various non-governmental organizations and YCWA, has been undertaking a suite of flow-related studies on the lower Yuba River supported by funds from the Lower Yuba Accord. Several of the studies currently underway (including detailed mapping, 2D modeling, and habitat utilization work) are providing new and potentially better data on habitat utilization in the lower Yuba River. Ultimately, these data will have many applications in understanding actual (rather than modeled) habitat usage in the lower Yuba River. Given our reservations regarding both the juvenile and spawning habitat models developed by USFWS, we believe that the data being collected by the RMT will appropriately be utilized to re-compute and then bio-validate HSC's for the lower Yuba River. We anticipate that this additional mapping and habitat work will likely be undertaken by the RMT within the next couple of years.*

Response: The USFWS is involved in the RMT and supports its efforts to improve understanding of salmon and steelhead habitat utilization in the lower Yuba River through mapping, modeling and habitat utilization studies. We intend to continue working with the RMT to ensure that information from all sources is applied appropriately in decisions regarding management of anadromous fish habitat in the lower Yuba River.

YCWA 6. Comment: *We believe that significant revisions to the draft FWS report are warranted; however we also recognize that there has been considerable effort invested in this draft report, and that there may be some reluctance to make significant changes. We also recognize that YCWA, FWS, and the other resource agencies involved in the management of the lower Yuba River are all keenly interested in seeing that the best possible management tools are available for use on the lower Yuba River. At this juncture, in light of the extensive effort that is being invested in the RMT study work under the direction of the multi-stakeholder RMT and the likelihood that additional and better data will become available for updating flow habitat relationships for the lower Yuba River, we suggest that the current FWS draft report be issued as a provisional report. Once the additional data become available, an updated bio-validated flow-habitat relationship report can be prepared and issued by the RMT.*

Response: The report has now undergone two or more rounds of both peer review and stakeholder review, and has been substantially revised in an effort to address the resulting comments. The revisions have included additional data analyses and discussion, and are well documented. We appreciate the recognition of the considerable effort invested in the draft report. However, our decisions regarding changes to the report are based on the report adhering to the current state of the art for conducting instream flow studies. We join with the stakeholders in our interest in seeing that the best possible management tools are available for use on the lower Yuba River. We feel that this report is an example of such a tool with respect to identifying instream flow requirements for fry and juvenile anadromous salmonid rearing in the Yuba River. Funding for this work was provided under Title 34, section 3406(b)(1)(B) of the

Central Valley Project Improvement Act, P.L. 102-575, and the report is a "deliverable" for work identified in CVPIA Annual Work Plans, most recently for Fiscal Year 2010. We are required to finalize and release the report.

YCWA 7. Comment: *YCWA greatly appreciates the opportunity to provide comments on this draft report, and looks forward to additional discussions regarding study work on the lower Yuba River.*

Response: Although we do not agree with some of the comments provided, we appreciate the effort that the commenter has made in reviewing the draft report and look forward to working with YWCA in the future.

YCWA 8. Comment: *The following comments are provided by the Yuba County Water Agency (YCWA) on the August 12, 2008 USFWS draft report titled "**Flow-Habitat Relationships for Juvenile Spring/Fall-run Chinook Salmon and Steelhead/Rainbow Trout Rearing in the Yuba River**". YCWA's comments focus on the biologic aspects of the study, and generally rely upon other reviewers for comments regarding hydraulic model development, performance and application.*

Response: See responses to GP comments 1-16 and 23 regarding hydraulic model development, performance and application.

YCWA 9. Comment: *Overall, this draft report is not reliable or usable in its current form, particularly because of its critical reliance on the logistic regression-based approach to develop habitat suitability criteria (HSC) from observational data. The logistic regression is only one of many mathematical ways to adjust observational data for potential bias due to sampling methods and habitat availability. The USFWS draft report promotes the use of logistic regression, despite a lack of consensus among the community of PHABSIM users regarding the utility of this particular method. In addition, all known methods of **ex post facto** mathematical adjustments (including logistic regression) are suspected of overcorrecting for this potential bias and have been rejected for use in PHABSIM studies by the Instream Flow Group (IFG – the developers of the method, now with the US Geological Survey), as stated in Bovee (1996):*

> ***"The fundamental problem with all mathematical preference indexes, not just the forage ratio, is that they tend to overcorrect for habitat availability."***
>
> ***"…we recommend that preference criteria developed using a forage ratio or other electivity index no longer be used in PHABSIM applications."***

Response: We feel that the report is reliable and useable in its current form, particularly because of its reliance on the logistic regression-based approach to develop habitat suitability data from observation data. Based on the current scientific literature (Knapp and Preisler 1999, Parasiewicz 1999, Geist et al. 2000, Guay et al. 2000, Tiffan et al. 2002, McHugh and Budy 2004), the logistic regression is the best way to adjust observational data for bias due to habitat availability. Our report uses logistic regression because it is the method currently recommended by the scientific peer-reviewed literature. We acknowledge that the community of PHABSIM users in this regard lags behind the scientific peer-reviewed literature. Our approach is to use the best state-of-the-art science, and the use of logistic regression is appropriate on that basis. We note that logistic regression does not use a mathematic preference index, forage ratio, or other electivity index, but rather is computed from the raw occupied and unoccupied data. Therefore, the recommendation of the Instream Flow Group to not use a forage ratio or other electivity index to develop criteria does not apply to logistic regression. We note that the use of logistic regression has come about largely since the Instream Flow Group's recommendations 16 years ago. More specifically, Bovee (1996) does not mention logistic regression.

YCWA 10. Comment: *The effect of likely overcorrection for habitat availability can be seen in Figures 10-16 on pages 51-57 of the USFWS draft report. In every case, the HSC (inaccurately referred to as HSI in these figures) used in this study show a clear shift of higher habitat suitabilities towards greater depths and higher velocities. Had the USFWS utilized a sampling strategy for their habitat use observations that was stratified equally over the range of depths and velocities in the lower Yuba River, there would have been less potential for bias in their observational frequencies and probably not even a need for mathematical adjustment. Such equal-area sampling strategies are the preferred method of the IFG, as stated in Bovee (1996).*

Response: While we agree that Figures 10-16 show a clear shift of higher habitat suitabilities towards greater depths and higher velocities, we feel that this shift is correct because it takes into account the effect of the limited availability of deeper and faster conditions in the Yuba River. By taking availability into account, logistic regression results in habitat suitability criteria that are not biased by availability. It has been demonstrated that a sampling strategy which attempts to spend equal effort sampling different ranges of depths and velocities does not work. Specifically, Allen (2001) attempted to sample equal areas of two different ranges of depths and velocities in collecting habitat suitability criteria. However, as shown in the figure on page B-26, he was unable to sample equal areas of the two different ranges of depths and velocities due to the limited availability of faster deeper conditions. We attempted a similar approach on the Yuba River to sample equal areas with and without cover and were similarly unsuccessful, sampling 6.5 miles without cover and only one mile with cover. With regards to equal stratification of sampling, we believe the commenter may be referring to habitat use measurements being collected with equal effort in different mesohabitat types, to attempt to address effects of availability. While we did make an effort to sample all mesohabitat types, so as to sample all of the available habitat for fry and juvenile anadromous salmonids, we did not sample equal areas of different mesohabitat types for the following reasons: 1) equal area sampling of different mesohabitat types does not adequately address the effects of availability on habitat use; 2) the use of logistic regression takes into account the effects of availability on habitat use; and 3) at least for larger rivers, mesohabitat type has little to do with fry and juvenile anadromous salmonid habitat use, based on our snorkel surveys on the Sacramento River (U.S. Fish and Wildlife Service 1996). Our snorkel surveys indicated that the scale of mesohabitat units for large rivers is so much larger than the scale of habitat use that habitat use is controlled by microhabitat parameters, rather than mesohabitat types. Since it is not possible to stratify observations equally over the range of depths and velocities in the Yuba River, the mathematical adjustment used in this study, namely logistic regression, is required to generate habitat suitability criteria that are not biased by availability. Based on the studies that have been conducted (i.e., Allen 2001) since the Instream Flow Group made their recommendation in 1996 for equal-area sampling strategies, this approach is not feasible, and thus other approaches, specifically logistic regression, are now applied to address the effects of availability.

YCWA 11. Comment: *The consequence of using these likely-overcorrected HSC within the PHABSIM 2D analysis is difficult to detect due to the habitat computational algorithms used in the USFWS draft report to generate the PHABSIM Weighted Usable Area (WUA) habitat indices – including the unvalidated use of adjacent velocity criteria (see Figures 25-32 on pgs. 68-71). In each of these figures, the USFWS draft report has scaled the y-axis to the range of the data, which provides no indication of the relative percent of total river area shown to be usable. The WUA for a 1D study is normally expressed in units of usable square feet per 1000 linear feet of stream, while WUA results for a 2D study are in units of usable square feet within each study reach. However, without knowing the percentages of the total area that are being expressed in the figures, there is no way to evaluate the significance of the numbers – which could be so low that they are in the range of random modeling noise.*

Response: We feel that the consequence of using the HSC that accurately corrects for the effects of availability are results that appropriately compute flow-habitat relationships based on habitat suitability criteria that are not biased by availability. The commenter is incorrect that the use of adjacent velocity criteria have not been validated; Gard (2006) validated fry and juvenile habitat suitability criteria from the Sacramento River which included adjacent velocity criteria. We agree that the x-axis in Figures 25-32 are scaled to the range of data and thus do not provide an indication of the relative percent of total river area shown to be useable. We note that such scaling of flow-habitat relationships is the standard practice in instream flow study reports to best illustrate the relative magnitude of weighted useable area at different flows. The total river area can be computed by summing the data shown in Table 8. Specifically, the total area of the Above Daguerre Segment is 1,305,200 m^2 (14,049,000 ft^2) and the total area of the Below Daguerre Segment is 1,225,900 m^2 (13,195,425 ft^2). Using the above figures and the segment results in Appendix L, the percent of total river area that is usable ranges from 0.9 to 3.2 percent for the Above Daguerre Segment and from 0.4 to 2.9 percent for the Below Daguerre Segment. We agree that WUA for a 1D study is normally expressed in units of usable square feet for 100 linear feet of stream, while WUA results for a 2D study are in units of useable square feet within each study segment. It should be noted that there was an error in the draft report for the fall-spring/run Chinook salmon juvenile weighted useable area results for the Below Daguerre Segment in Appendix L – these results were given in square meters. This has been corrected in the final report. Based on the above percentages of the total area and the response to YCWA comment 2, which indicates that for River2D studies a threshold for significance of results would be much less than 5 percent, it is unlikely that the numbers are so low that they are in the range of random modeling noise.

YCWA 12. Comment: *The figures comparing the USFWS draft report results to the previous Beak (1989) study (Figures 56-59 on pgs. 98-99) provide some context for the USFWS draft report WUA numbers. Assuming an average width for the lower Yuba River of 200 feet (taken from the Beak data) and dividing the y-axis by this number, the total usable areas in the USFWS draft report WUAs are 1.0-1.5 percent of the total area. This indicates a serious problem, because results for standard PHABSIM studies become questionable when the usable percentage* *of total area is below about 5 percent, because the very small numbers of usable sample points used to develop the index are subject to the influence of minor hydraulic modeling errors and even slight shifts resulting from the manner in which the HSC were created.*

Response: There was an error in Figures 56 to 59 of our draft rearing report, in that the WUA values for our study in those figures were plotted in square meters, rather than in square feet. This error has been corrected in the final report. With the corrected figures and the assumptions made by the commenter, the total useable areas in our draft report WUAs are 10-15 percent of the total area. Thus, even if the rule of thumb for standard PHABSIM studies also applies to River2D modeling, the values in our report do not fall below this rule of thumb. However, it should be noted that we arrived at very different percentages using the actual data, as opposed to the assumptions made by the commenter, as shown in our response to YCWA comment 11. See also response to YWCA comment 2.

YCWA 13. Comment: *HSC are linked to one or two-dimensional hydraulic models to produce the PHABSIM weighted usable area (WUA) habitat indices shown as results in the USFWS draft report in Figures 25-32 on pages 68-71. Another indication of potential problems in the USFWS draft report is the different directions of the changes in WUA in relation to changes in discharge that are shown for Chinook and steelhead fry rearing WUA (Figures 25, 27, 29, and 31). The PHABSIM WUA habitat indices for these two very similar life stages show opposite responses to changes in discharge, with the Chinook fry index maximizing at very high flows and the steelhead fry index maximizing at much lower flows. This type of result is illogical and cannot possibly justify the USFWS draft report (pg. 100) statement that* ***"In conclusion, we feel that the results of this study are a more accurate assessment of the relationship between flow and anadromous salmonid fry and juvenile rearing habitat than the results of Beak (1989)."*** *Until these very fundamental questions concerning the USFWS draft report are resolved, no legitimate conclusion regarding the accuracy of the results presented in the USFWS draft report can be reached.*

Response: We feel that the different directions of the changes in WUA indices with changes in discharge for Chinook salmon and steelhead fry reflect a key difference in habitat selection by the two species, and thus do not indicate a problem with the draft report. While we agree that these two life stages are similar, there is an important difference in habitat use between the species – namely the much greater use of cobble cover by steelhead fry, versus Chinook salmon fry. As a result, the suitability for this cover category is more than twice as high for steelhead fry (0.57) than Chinook salmon fry (0.25). This difference in habitat use carries over into the flow-habitat relationships. At low flows, most of the Yuba River wetted channel has cobble cover, which together with low velocities at low flows, results in the highest amount of habitat for steelhead fry at these flows. In contrast, the amount of habitat at low flows is less for Chinook salmon because of the lower suitability for cobble cover. At higher flows, where woody cover is inundated, the amount of habitat for Chinook salmon fry is higher than at low flows, due to the large difference in suitability for woody (1.0) versus cobble (0.25) cover overwhelming decreases in habitat associated with increasing velocities. In contrast, steelhead fry, with a much smaller difference in suitability for woody (1.0) versus cobble (0.57) cover, end up with less habitat at higher flows since the negative effect of higher velocities swamps out the positive effect of inundation of high-suitability cover. Accordingly, this result is logical and justifies the above statement from the rearing report quoted by the commenter.

YCWA 14. Comment: *Given the suite of issues regarding HSC development that are discussed in the foregoing and following comments, consideration should be given to recalculating the HSCs and/or using alternative HSCs and re-running the models (or developing new hydraulic models) to estimate WUA-discharge relationships for juvenile spring/fall-run Chinook salmon and steelhead/rainbow trout rearing in the lower Yuba River.*

Response: Based on our responses to YCWA comments 1-13 and 15-59, we do not feel it is necessary to recalculate the HSCs or use alternative HSCs and re-run the models or develop new hydraulic models. We feel that the WUA-discharge relationships for juvenile spring/fall-run Chinook salmon and steelhead/rainbow trout rearing in the lower Yuba River utilize the best current scientific techniques available and will be valuable for the purposes of identifying the instream flow needs for anadromous salmonids in the Yuba River.

YCWA 15. Comment: *The remaining specific comments follow the organizational format of the USFWS draft report (i.e., Introduction, Methods, Results, Discussion).*

Response: See responses to YWCA comments 16-59.

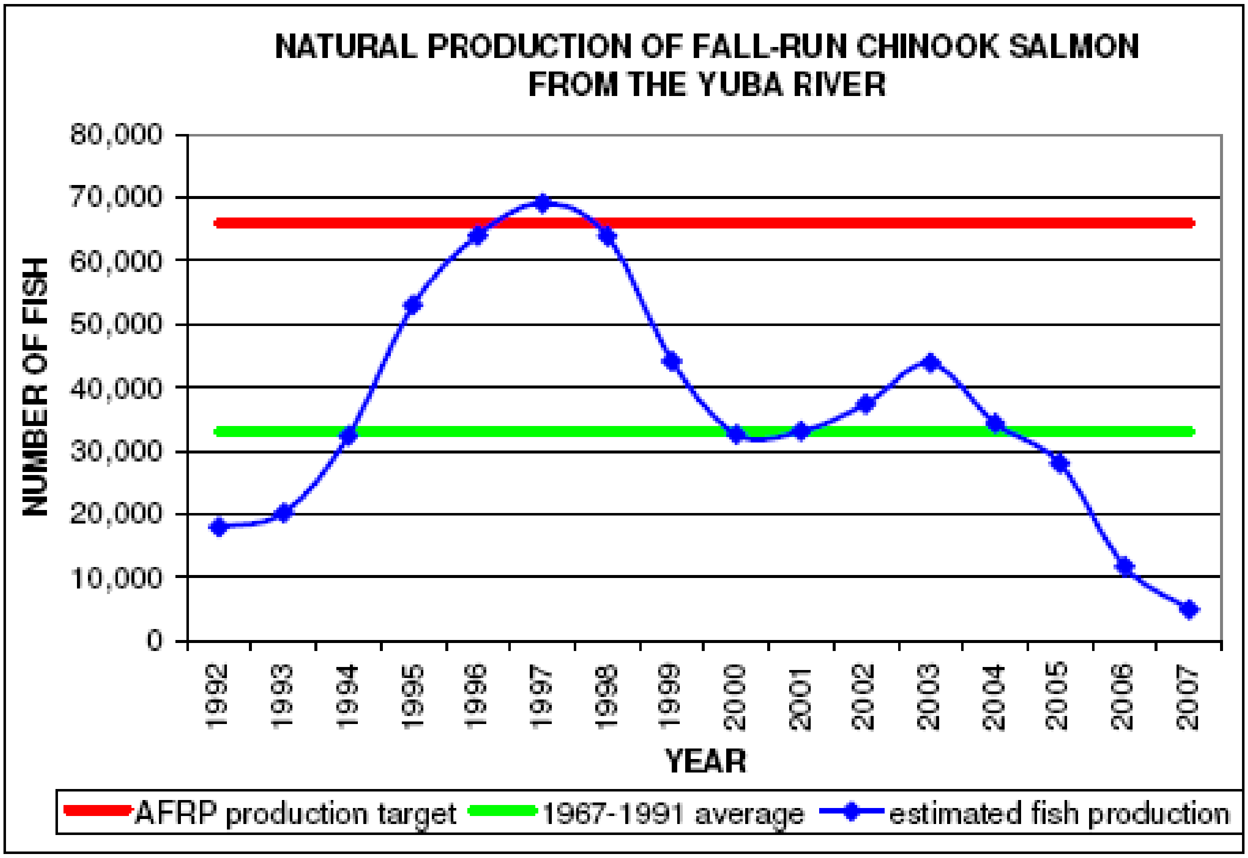
INTRODUCTION

YCWA 16. Comment: *The USFWS draft report states (pg. 1) … **"The lower Yuba River, between Englebright Dam and the Feather River confluence, is a major contributor to anadromous salmonid production in the Central Valley and supports the largest stock of Chinook salmon that is not supplemented by hatcheries."***

YCWA agrees with this statement. Moreover, this statement correctly suggests that current operational practices, including instream flow regimes, have contributed to the status of anadromous salmonid runs in the lower Yuba River.

Response: We agree that current operational practices, including instream flow regimes, have contributed to the status of anadromous salmonid runs in the lower Yuba River. As shown in the figure on the following page, the average production of fall-run Chinook salmon in the Yuba River for the period of 1992-2007 is significantly less than the AFRP's production goal of 66,000 fall-run Chinook salmon. The USFWS will continue to work with YWCA to understand flow-habitat relationships and improve anadromous fish production in the Yuba River.

YCWA 17. Comment: *The USFWS draft report states (pg. 1) … **"The objective of this study was to produce models predicting habitat-discharge relationships in the Yuba River for spring/fall-run Chinook salmon and steelhead/rainbow trout rearing."***



*This objective highlights that the USFWS draft report does not address habitat availability. 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 "**models predicting habitat-discharge relationships in the Yuba River for spring/fall-run Chinook salmon and steelhead/rainbow trout rearing**".*

Response: We agree that the draft report does not address water availability, hydrological factors and operational constraints. We expect that such factors would be considered, in addition to the information in this report, during the upcoming FERC relicensing in developing flow requirements for the Yuba River. Feasibility is not a consideration in models predicting habitat-discharge relationships; rather it is a consideration in developing flow requirements. The models predicting habitat-discharge relationships simply quantify the effects of alternative flow regimes on anadromous salmonids. We view the models predicting habitat-discharge relationships in the Yuba River for spring/fall-run Chinook salmon and steelhead/rainbow trout rearing to be applicable to the Yuba River, since they were developed using the best available science and site-specific data from the Yuba River.

YCWA 18. Comment: *The USFWS draft report states (pg. 1) that … "**Macrohabitat features include longitudinal changes in channel characteristics, base flow, water quality, and water temperature. Microhabitat features include the hydraulic and structural conditions (depth, velocity, substrate or cover) which define the actual living space of the organisms**".*

For the juvenile rearing lifestage, the macrohabitat hydraulic or structural conditions characterizing (defining) juvenile rearing habitat also include the important parameter of water temperature, which is not addressed in the USFWS draft report. In fact, the methodology used to develop the Habitat Suitability Criteria (HSC) and apply them to estimate juvenile rearing habitat availability does not specifically address this potentially important component of juvenile rearing habitat selection, utilization and availability, and therefore may result in the prediction of suitable (available) habitats that may not be utilized because of behavioral selection for specific water temperature conditions.

Response: We agree that water temperature is an important macrohabitat parameter for juvenile rearing habitat, and that the report does not address this parameter. Water temperature is not typically addressed in developing and applying habitat suitability criteria. Rather, water temperature is applied as a macrohabitat parameter by reducing the total amount of habitat by the number of miles of river with suitable water temperatures. We agree that failure to consider water temperature could result in the prediction of suitable habitats that may not be utilized because of behavioral selection for specific water temperature conditions (for example, avoidance of water temperatures exceeding 20 degrees Centigrade). This lead us to include the following sentence in the discussion of the rearing report:

> "Evaluation of such alternative hydrograph management scenarios should also consider the flow-habitat relationships for Chinook salmon and steelhead/rainbow trout spawning, reported separately (U.S. Fish and Wildlife Service 2010), and water temperature modeling information."

YCWA 19. Comment: *The USFWS draft report presents (pgs. 2-3) 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.*

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).

YCWA 20. Comment: *The USFWS draft report states (pg. 24) … "****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 should be used to develop habitat suitability criteria. [underscore added] For example, McHugh and Budy (2004) state (page 90):***

> ***"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)."***

There are several problems with this statement. First, habitat suitability modeling is not synonymous with habitat suitability criteria development. Second, none of the referenced literature states that logistic regressions should be used to develop habitat suitability criteria. Third, the referenced literature (excluding Parasiewicz (1999), which was not available for review) does not support the USFWS draft report contention that logistic regression should be used to develop habitat suitability criteria, as follows:

Response: We have changed a portion of the text of the above statement from "should be used to develop" to "are appropriate for developing." We feel that habitat suitability modeling is synonymous with habitat suitability criteria development because an integral part of any habitat suitability modeling is the development of habitat suitability criteria. As addressed in our responses to YCWA comments 21 to 25, we feel that the referenced literature supports the statement that logistic regressions are appropriate for developing habitat suitability criteria.

YCWA 21. Comment: *Knapp and Priesler (1999) did not conclude that logistic regression should be used to develop habitat suitability criteria. 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.*

Response: Our review of Knapp and Priesler (1999) indicates that it is consistent with the conclusion that logistic regressions are appropriate for developing habitat suitability criteria; the aspect of nonparametric versus parametric logistic regression is a relatively minor point. Specifically, Knapp and Priesler (1999) indicate that the main reason they used nonparametric logistic regression was to avoid an assumption that there is a linear relationship between the independent and response variable. We chose to address this issue by using a polynomial logistic regression, which also avoids the assumption that there is a linear relationship between the independent and response variable.

YCWA 22. Comment: *Geist et al. (2000) did not conclude that logistic regression should be used to develop habitat suitability criteria. 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), such as upwelling from hyporheic habitats, influence redd site selection.*

Response: Our review of Geist et al. (2000) indicates that it is consistent with the conclusion that logistic regressions are appropriate for developing habitat suitability criteria because they used logistic regression to evaluate habitat selection. We view the aspect of a binary versus continuous response variable as a minor point. In this regard, 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 fry and juveniles would be in areas which would be considered completely unsuitable from the binary criteria. Accordingly, the use of continuous criteria developed using logistic regression in this study avoided some of the above problems associated with Geist et al. (2000). The considerations regarding clustered redds and other unmeasured factors, such as upwelling from hyporheic habitats, are not applicable to this report, since they are specific to spawning.

YCWA 23. Comment: *Guay et al. (2000) did not conclude that logistic regression should be used to develop habitat suitability criteria. They 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.*

Response: Our review of Guay et al. (2000) indicates that it is consistent with the conclusion that logistic regressions are appropriate for developing habitat suitability criteria. We view the aspect of multivariate versus univariate logistic regression as a minor point. In this regard, we view both the univariate logistic regressions that we used and the multivariate methods used by Guay et al. (2000) as means to distinguish between the physical conditions used and avoided by fish. We do not see how the caution expressed in Guay et al. (2000) would affect our conclusion that Guay et al. (2000) is consistent with the conclusion that logistic regression are appropriate for developing habitat suitability criteria.

YCWA 24. Comment: *Tiffan et al. (2002) did not conclude that logistic regression should be used to develop habitat suitability criteria. They 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 the appropriateness of using logistic regression in HSC development.*

Response: Our review of Tiffan et al. (2002) indicates that it is consistent with the conclusion that logistic regressions are appropriate for developing habitat suitability criteria. Their use of a logistic regression to relate the probability of juvenile fall-run Chinook salmon presence in nearshore areas to measures of physical habitat is essentially equivalent to the use of logistic regression to develop habitat suitability criteria, which by definition relate the probability of fish presence to measures of physical habitat (in our case depth, velocity, adjacent velocity and cover). Our intent was not to suggest that Tiffan et al. (2002) developed habitat suitability criteria; rather it was to suggest that it is consistent with the conclusion that logistic regression should be used to develop habitat suitability criteria.

YCWA 25. Comment: *McHugh and Budy (2004) did not conclude that logistic regression should be used to develop habitat suitability criteria. 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 their specific river of investigation.*

Response: Our review of McHugh and Budy (2004) indicates that it is consistent with the conclusion that logistic regressions are appropriate for developing habitat suitability criteria. In this regard, we feel that the following quote from McHugh and Budy (2004) supports this conclusion:

> "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 note that our application, in which we used river-specific suitability models, is consistent with the findings of McHugh and Budy (2004).

YCWA 26. Comment: *On Page 3, four 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 information given in YWCA comments 27-30.*

Response: These or similar assumptions are common to all instream flow studies (Bovee 1982). We are unaware of any instream flow study reports that include discussion on why these assumptions are necessary, or to what extent each of these assumptions is valid, is partially met, or is not met. For at least the first assumption, to our knowledge the data needed to test these assumptions is lacking. Accordingly, we did not add a discussion regarding the validity of assumptions. See responses to YCWA comments 27-30 regarding discussion of the assumptions.

YCWA 27. Comment: ***Assumption 1: Physical habitat is the limiting factor for salmonid populations in the Yuba River.***

Presumably, this assumption actually is that rearing habitat is the limiting factor for salmonid populations in the Yuba River, because this draft report only addresses elements of the rearing lifestage. However, there is no evidence to support this assumption. The USFWS draft report should present supporting rationale or justification for this assumption.

Response: A basic assumption of all instream flow studies is that physical habitat is limiting fish populations (Bovee 1982, page 120). We used Bovee (1982) as our justification for this assumption. In addition, to our knowledge, the data needed to determine if physical habitat is the limiting factor for anadromous salmonids in the Yuba River is lacking. 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.

YCWA 28. Comment: ***Assumption 2: Rearing habitat quality can be characterized by depth, velocity, adjacent velocity and cover.***

This assumption has the potential to be reasonably valid depending on the methodologies used to develop HSCs. As long as the methodologies do not compromise the ability of these four parameters (or combinations thereof) to reflect the influence of unutilized parameters (e.g., water temperature) in rearing habitat quality and concomitant rearing site selection, it may be a reasonable assumption. However, we have a concern regarding the unvalidated use of the adjacent velocity criterion as an assumption, or in HSC development. Additionally, the data collection procedures, methodologies and resultant HSCs developed in the USFWS draft report raise questions as to the extent to which this assumption is valid (see following comments).

Response: The methodologies used to develop HSC represent the current state-of-the-art in the scientific literature for developing HSC. Water temperature is not typically addressed in developing and applying habitat suitability criteria. Rather, water temperature is applied as a macrohabitat parameter by reducing the total amount of habitat by the number of miles of river with suitable water temperatures. We justified the use of an adjacent velocity criterion based on the biological mechanism identified in the report (turbulent mixing transporting invertebrate drift from fast-water areas to adjacent slow-water areas where fry and juvenile salmon and steelhead/rainbow trout reside). We note that the use of an adjacent velocity criterion has been validated (Gard 2006). As discussed in the response to YWCA comments 34-39, we feel that the data collection procedures, methodologies and resultant HSCs support the validity of this assumption.

YCWA 29. Comment: ***Assumption 3: The 18 study sites are representative of anadromous salmonid rearing habitat in the Yuba River.***

This assumption requires additional justification as indicated in Comments 7, 8 and 9 under Methods.

Response: See responses to YWCA comments 31-34.

YCWA 30. Comment: ***Assumption 4: Theoretical equations of physical processes along with a description of stream bathemetry provide sufficient input to simulate velocity distributions through a study site.***

For this assumption, we will defer to the comments that discuss hydraulic/hydrodynamic modeling issues.

Response: See response to GP comments 1-16 and 23.

METHODS

YCWA 31. Comment: *The USFWS draft report (pg. 4) states that study segments were delineated within the lower Yuba River between Englebright Dam and the Feather River based on differences in flow, resulting in only two study segments – above Daguerre Point Dam and below Daguerre Point Dam study segments. The USFWS draft report should explain why other features (e.g., fluvial geomorphology, gradient, water temperature, etc.) were not considered in segment delineation.*

Response: See response to PG&E comment 5.

YCWA 32. Comment: *The USFWS draft report (pg. 7) states "****based on the results of habitat mapping, we selected eight juvenile habitat sites that, together with ten previously selected sites (U.S. Fish and Wildlife Service 2008), adequately represent the mesohabitat types present in each segment … As a result, the mesohabitat composition of the study sites, taken together, were roughly proportional to the mesohabitat composition of the entire reach.****"*

First, the USFWS (2008) draft report on spawning habitat states (pgs. 5 and 7) that the ten previously selected study sites were those that received heaviest spawning use by spring-run and fall-run Chinook salmon, and by steelhead/rainbow trout, as mapped by Jones and Stokes biologists during 2000. The USFWS draft report on juvenile rearing habitat needs to explain why it is appropriate to use data from spawning sites to model juvenile rearing habitat.

Response: We added the following text to the discussion section of the report under *Field reconnaissance and study site selection* to respond to this comment:

> "The use of the 10 spawning sites to model juvenile rearing habitat was viewed as appropriate because it increased the area of river modeled for juvenile rearing habitat."

YCWA 33. Comment: *Second, the USFWS draft report would benefit from including the actual proportional representation of mesohabitat units selected for study in each of the study segments.*

Response: We have added a new Table 10 to the report showing the actual proportional representation of mesohabitat units selected for study in each of the study segments.

YCWA 34. Comment: *The methodology (USFWS draft report pg. 7) emphasized obtaining HSC information by distinct mesohabitat units. However, HSC information appears to have been developed by pooling mesohabitat units. This assumes that all HSC information is independent of mesohabitat types. The inconsistency between the methodology and the data reduction steps needs to be explained.*

Response: The text referred to by the commenter (page 7) addresses field reconnaissance and study site selection. Collection of HSC data is addressed starting on page 21 of the draft report. Our methodology emphasized obtaining HSC information in all mesohabitat types. The commenter is correct that HSC were developed by pooling data from all mesohabitat types. Based on snorkel studies we conducted on the Sacramento River (U.S. Fish and Wildlife Service 1996), it appears to be a reasonable assumption that HSC data is independent of mesohabitat types. Specifically, our snorkel surveys indicated that the scale of mesohabitat units for large rivers is so much larger than the scale of habitat use that habitat use is controlled by microhabitat parameters, rather than mesohabitat types. We do not believe that a methodology in which HSC information is obtained in all mesohabitat types is inconsistent with data reduction steps in which HSC data from all mesohabitat types is pooled. It is unclear what the commenter is suggesting as an alternative to the data reduction step that we did. One alternative could be developing different HSC for different mesohabitat types; such an approach would not be possible in this case due to small sample sizes.

YCWA 35. Comment: *The USFWS draft report (pg. 22) states that water temperature data were recorded during data collection activities. However, it does not appear that water temperature data were used in HSC development.*

Response: The commenter is correct that we did not use water temperature in HSC development. We took this approach because water temperature is typically applied in instream flow studies as a macrohabitat parameter by reducing the total amount of habitat by the number of miles of river with suitable water temperatures.

YCWA 36. Comment: *The USFWS draft report (pg. 23) states that data taken by the snorkeler and the measurer were correlated at each tag location. It is unclear what is meant by use of the term "correlated" here.*

Response: We have changed the text in question to read "combined for" instead of "correlated at." The text in question was intended to mean that we recorded the data collected for each tag by the snorkeler (e.g. number, size and species of fish and cover) and later by the measurer (e.g. depth, velocity and adjacent velocity) on the same line in a databook.

YCWA 37. Comment: *The USFWS draft report (pg. 25) states that "**We used Mann-Whitney U tests to test for differences in depth, velocity and adjacent velocity, and Pearson's test for association to test for differences in cover, for the above categories of fry versus juveniles.**"*

The USFWS draft report should explain why these specific tests were selected for these two different applications.

Response: We added the following text to the report to respond to this comment:

> "We used nonparametric tests because the data was not normally distributed. Mann-Whitney U tests are generally used for continuous variables, such as depth, velocity and adjacent velocity, while Pearson's test for association is generally used for categorical variables, such as cover."

YCWA 38. Comment: *The USFWS draft report (pgs. 25-26) states that, when examining the transferability of Sacramento River Chinook salmon rearing criteria, the optimum range for a single variable was defined by a suitability greater than 0.75, and that the usable range for a variable encompassed the interval between suitabilities of 0.1 and 0.75. The USFWS draft report should justify the selection of these values (0.1 and 0.75) for these definitions of suitability.*

Response: We have added a citation to the report (U.S. Fish and Wildlife Service 1997) where these values were derived.

YCWA 39. Comment: *The USFWS draft report (pg. 26) states that HSC were modified by …"**eliminating points not needed to capture the basic shape of the curves**". Additional discussion is necessary justifying the appropriateness of this specific methodologic procedure.*

Response: We have changed the text in question (on pg. 30 of the final report) to read as follows:

> "eliminating points where interpolation from retained points resulted in the same HSI value at the eliminated point."

Essentially we only eliminated points where there was a linear relationship between HSI and the independent variable, which would not have added any information to the relationship. This is a convention carried over from PHABSIM and makes the criteria data presented in Appendix K more concise.

YCWA 40. Comment: The *USFWS draft report (pg. 27) stated that, because adjacent velocity (AV) was highly correlated to velocity (V), a logistic regression (equation 4) that includes not only the explanatory variable AV but also the variables V, V2, V3 and V4 was fitted to the data and then the regression coefficients corresponding to the intercept (I) and the adjacent velocity (N) of the fitted Equation 4 were used in Equation 5.*

$$\textit{Equation 5:} \qquad HSI = \frac{Exp\,(I + N \times AV)}{1 + Exp\,(I + N \times AV)}$$

It is not sufficiently explained why the process of applying Equation 4 to obtain the coefficients for use in Equation 5 was used. Why weren't the coefficients (I and N) estimated directly from fitting Equation 5 to the data? Why wasn't the previously applied logistic regression approach used for depth and velocity HSC development used for adjacent velocity HSC development? How did this specific methodology influence the resultant adjacent velocity HSC?

Response: The coefficients I and N were not estimated directly from fitting Figure 5 to the data because velocities and adjacent velocities were highly correlated. Similarly, the previously applied logistic regression approach was not used for adjacent velocity because velocities and adjacent velocities were highly correlated. The effect of this specific methodology on the resultant adjacent velocity HSC was to produce HSC that reflected the component of adjacent velocity that was independent of velocity.

YCWA 41. Comment: *The USFWS draft report (pg. 27) discussion of the methodology used to develop adjacent velocity HSC states that once the HSC reaches a value of 1.0, all higher adjacent velocities also have an HSC value of 1.0. Appendix K in the USFWS draft report indicates that adjacent velocity HSC was considered to be equal to 1.0 at water velocities of 3.6 fps and higher for Chinook fry, 5.5 fps and higher for Chinook and steelhead juveniles, and 4.7 fps and higher for steelhead fry.*

Response: The commenter is correct.

YCWA 42. Comment: *It does not appear to be reasonable to assume that adjacent velocities of 3.6 fps and higher (without an upper limit, or without reduced suitability at higher velocities) represent the highest suitability of adjacent velocities, due to lack of forage opportunity duration, and excessive bioenergetic demand to capture prey items, at higher velocities. As suggested on Page 21 of the USFWS draft report …"**Both the residence and adjacent velocity variables are important for fish to minimize the energy expenditure/food intake ratio and maintain growth.**"*

Response: An assumption that adjacent velocities of 3.6 fps and higher without an upper limit is reasonable based on the mechanism of turbulent mixing transporting invertebrate drift from fast-water areas to adjacent slow-water areas where fry and juvenile salmon and steelhead/rainbow trout reside. The adjacent velocity parameter addresses food delivery – it is reasonable to expect that faster adjacent velocities with no upper limit would result in more invertebrate drift delivery. Since fry and juveniles stay in the slower velocity areas and food is delivered to them by turbulent mixing, forage opportunity duration is not a consideration – once the food has been delivered to the slower velocity area by turbulent mixing, it would be expected to stay there. Bioenergetic demand to capture prey items is addressed by the velocity HSC component – by staying in slow velocity areas, fry and juveniles minimize their bioenergetic demand to capture prey items, which are delivered to them by turbulent eddies – essentially they can stay in one spot and have their food delivered to them.

YCWA 43. Comment: *It appears unreasonable to weight very high adjacent water velocities (higher than 3.6 fps with no upper limit) as having optimal suitability.*

Response: Weighting very high adjacent water velocities as optimal is reasonable based on the mechanism of turbulent mixing transporting invertebrate drift from fast-water areas to adjacent slow-water areas where fry and juvenile salmon and steelhead/rainbow trout reside – it is reasonable to expect that faster adjacent velocities with no upper limit would result in more invertebrate drift delivery.

YCWA 44. Comment: *The selection of the HSC (draft report pgs. 24-28) to be used to establish flowhabitat relationships is often a contentious component of a flow-habitat study (PHABSIM), especially when the decision is made autonomously (Stalnaker et al. 1995; Bovee 1995). The habitat suitability criteria are typically the most significant factor in determining the outcome of a habitat study (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 with high combined suitability. Such was not observed in this study, since most of the rearing unoccupied locations had suitabilities less than 0.1. 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. We assume that the fourth sentence of this comment was meant to refer to the rearing habitat evaluation (the subject of the draft report), rather than the spawning habitat evaluation. We would characterize multivariate logistic regression 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). 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 45. Comment: *Multivariate logistic regression is a developing approach to evaluate habitat suitability. Under this approach, scientists typically take the presence of a fish 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 46. Comment: *The USFWS draft report apparently deviates from the approaches in the referenced documents (draft report pg. 24) by using a 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 to try to justify 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, cover and adjacent velocity. We consider the use of a univariate approach to be appropriate in this application (see response to YWCA comment 47 below). In this regard, we view both the univariate logistic regressions that we used and the multivariate methods used by Guay et al. (2000) as means to distinguish between the physical conditions used and avoided by fish. 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 47. Comment: *The USFWS draft report assumed that the four "independent" variables (depth, velocity, adjacent velocity and cover) are equally significant because compound suitability is the product of the HSC of the four variables. Given the uncertainty of the assumption of equal significance of these variables in the lower Yuba River, the USFWS draft report needs to be edited to reevaluate these findings, including a determination of the relative significances of depth, velocity, adjacent velocity and cover in determining the quality of rearing 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).

YCWA 48. Comment: *Given that 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: See response to YCWA comment 46.

YCWA 49. Comment: *Guay* ***et al.*** *(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 (e.g., Guay* ***et al.*** *2000).*

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. In this regard, we view both the univariate logistic regressions that we used and the multivariate methods used by Guay et al. (2000) as means to distinguish between the physical conditions used and avoided by fish. We have reviewed the report to confirm that it includes a sufficiently clear description of the methods used in our study.

YCWA 50. Comment: *The USFWS draft report (pg. 28) states that biological verification was performed by using … "Mann-Whitney U tests (Zar 1984) to determine whether the combined suitability predicted by River2D was higher at locations where fry or juveniles were present versus locations where fry or juveniles were absent." The methodology employed, a one-tailed Mann-Whitney U test (rank sum test), actually determined whether two independent samples were drawn from populations having the same mean. It is unclear how such a test addresses "bioverification". In fact, "bioverification" is more appropriately assessed by plotting modeled combined habitat suitability predictions versus actual locational observations as presented Appendix M.*

Response: The test addresses bioverification by testing the hypothesis that fish are selecting preferred habitat conditions (i.e. that occupied locations have a higher combined suitability than unoccupied locations). The test summarizes the data shown in Figures 23 and 24, which in turn summarize the results shown in Appendix M. We would regard the combination of the statistical test, the data shown in Figures 23 and 24 and the results presented in Appendix M as the best way to assess bioverification. Each of these three methods of presenting data has strengths and weaknesses – the stastistical test provides an overall assessment of the biological verification, while the results presented in Appendix M provide the most in-depth presentation of the biological verification data. See also response to GP comment 15.

YCWA 51. Comment: *The USFWS draft report (pg. 80) states that the R^2 values for the logistic regressions are very low. 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 referenced literature (pg. 24), which included additional independent variables. If that is the case, then the USFWS draft report should be modified to provide additional explanations of why the univariate approach was used and of the predictive capability of the logistic regression model(s) based on observed values.*

Response: The intent of the text referred to by the commenter was to indicate that the overall proportion of variance explained by all four habitat variables is greater than the proportion of variance explained by each variable. In this regard, the report states on page 85:

> "It would be expected that the proportion of variance (R^2 value) explained by the habitat suitability variables would be apportioned among depth, velocity, adjacent velocity and cover."

We are not aware of what additional explanations could be added regarding the appropriateness of using the univariate approach. We do not know how to assess the predictive capability of the logistic regression models based on observed values, given that the models address habitat that would be preferred if available rather than habitat that is used because it is available.

RESULTS

YCWA 52. Comment: *The USFWS draft report (pgs. 38-39) describes the substantial amount of snorkeling effort expended in both near-shore and mid-channel habitats. Of the 469 measurements made where YOY Chinook salmon and steelhead/rainbow trout were observed, all but 8 were made near the river banks. These results indicate a volitional selection of near-shore habitats by YOY, and should be considered in the development and application of combined habitat suitability HSC, and in the eventual representation of WUA-discharge relationships.*

Response: The relative abundance of fish in near-shore and mid-channel habitats is confounded by the difference in depths and cover in near-shore and mid-channel habitats. Specifically, it is likely that the low abundance of fish in mid-channel habitats was due to the deeper depths and lack of woody cover, as compared to near-shore habitats, rather than a volitional selection of near-shore habitats by fry and juvenile anadromous salmonids. As such, it does not appear that consideration of near-shore versus mid-channel habitats is warranted in the development and application of criteria and WUA-discharge relationships.

YCWA 53. Comment: *The USFWS draft report (pg. 80) describes the definition of a group of fish as a single observation, and presumably observations of one fish also as a single observation. The appropriateness of using an entire school of fish as a single observation, as well as a single fish as a single observation, and the potential for differential weighting of observations by various numbers of fish should be more thoroughly discussed in the draft report.*

Response: The commenter is correct that both a group of fish and observations of one fish were defined as a single observation. The appropriateness of this practice and the potential for differential weighting of observations by various numbers of fish are addressed in the following text from the discussion of the report:

> "Rubin et al. (1991) present a similar method to logistic regression using fish density instead of presence-absence, and using an exponential polynomial regression, rather than a logistic regression. Rubin et al. (1991) selected an exponential polynomial regression because the distribution of counts of fish resembles a Poisson distribution. We did not use this method for the following reasons: 1) we had low confidence in the accuracy of our estimates of the number of fish in each observation; and 2) while it is reasonable to assume that a school of fish represents higher quality habitat than one fish, it is probably unreasonable to assume that, for example, 100 fish represents 100 times better habitat than one fish. A more appropriate measure of the effects of the number of fish on habitat quality would probably be to select some measure like log (number of fish + 1), so that 1-2 fish would represent a value of one, 3-30 fish would represent a value of two and 31-315 fish would represent a value of three[4]. We are not aware of any such measure in the literature, nor are we aware of how we could determine what an appropriate measure would be."

YCWA 54. Comment: *In Figures 42-49 of the USFWS draft report (pgs. 85-88), comparisons are made of Chinook salmon and steelhead/rainbow trout fry and juvenile depth and velocity HSCs between those in the USFWS draft report, and those in the referenced literature. For 7 of the 8 comparisons of the depth and velocity HSCs, the USFWS draft report HSCs have a clear shift of higher habitat suitabilities toward deeper and faster water than for HSCs in the referenced literature. This proclivity toward deeper and faster water results from the critical reliance on the logistic regression-based approach to develop HSC from observational data.*

Response: We agree that most of the comparisons show a shift of higher habitat suitabilities toward deeper and faster water compared with HSCs from the referenced literature. This pattern suggests that the criteria in the literature are likely largely biased towards shallow depths and slower velocities since the criteria in the literature were not developed using methods that adequately address the effects of availability on habitat use. Based on the following text from the discussion, the shift towards deeper and faster conditions are due to more than just the use of logistic regression:

> "The fall/spring-run Chinook salmon fry and juvenile and steelhead/rainbow trout juvenile depth criteria show non-zero suitability, albeit at low values, for deeper conditions than the criteria from other studies. We attribute this to the use of SCUBA sampling to collect fry and juvenile rearing HSC data in deeper water. Typically, criteria data for fry and juvenile anadromous salmonids are only collected using snorkel surveys, on the assumption that fry and juvenile

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[4] The largest number of fish that we had in one observation was 300 fish.

> anadromous salmonids will not be found in deeper water. In contrast, we found that fry and juvenile anadromous salmonids will use deeper water with suitable velocities."
>
> "we observed steelhead/rainbow trout fry in deeper conditions than for other criteria; we had seven percent of our observations in water ≥ 3 feet (0.91 meters), while both the Feather and Trinity River HSC had zero suitability for depths ≥ 3 feet (0.91 meters)."
>
> "The fall/spring-run Chinook salmon fry velocity criteria show non-zero suitability, albeit at low values, for faster conditions than the other criteria. We attribute this to the fact that we observed fall/spring-run Chinook salmon fry at higher velocities than for other criteria; we had observations at velocities as high as 3.62 feet/sec (1.10 meters/sec), while both the Feather River and Beak (1989) HSC had zero suitability for velocities greater than 2.24 feet/sec (0.68 meters/sec). Similarly, our fall/spring-run Chinook salmon juvenile and steelhead/rainbow trout fry velocity criteria show non-zero suitability for faster conditions than other criteria. We attribute this to the fact that we observed fall/spring-run Chinook salmon juveniles and steelhead/rainbow trout fry at higher velocities than for other criteria. For fall/spring-run Chinook salmon juveniles, we had observations at velocities as high as 3.98 feet/sec (1.21 meters/sec), while both the Feather River and Beak (1989) HSC had zero suitability for velocities greater than 3.24 feet/sec (0.99 meters/sec). For steelhead/rainbow trout fry, we had observations at velocities as high as 3.66 feet/sec (1.12 meters/sec), while both the Feather and Trinity River HSC had zero suitability for velocities greater than 2.69 feet/sec (0.82 meters/sec)."

YCWA 55. Comment: *The use of adjacent velocity as a variable in the combined habitat suitability HSC development is unique, and has not been validated in other studies. The USFWS draft report (pg.91) acknowledges that the only other study that used adjacent velocity HSC for Chinook salmon fry or juvenile rearing was developed by the same lead author for the Sacramento River. Notably, no other studies were located that used adjacent velocity as an HSC variable for Chinook salmon fry or juvenile rearing. Moreover, no studies were able to be found that used adjacent velocity as an HSC variable for steelhead/rainbow trout fry or juvenile rearing. This raises significant questions regarding the appropriateness of using this HSC variable here.*

Response: We agree that the use of adjacent velocity is unique to this study and our previous study on the Sacramento River. The use of the adjacent velocity criteria developed for the Sacramento River study was validated on the Merced River (Gard 2006). We feel that it is appropriate to use adjacent velocity because it is an important aspect of anadromous juvenile salmonid rearing habitat that has been overlooked in previous studies.

YCWA 56. Comment: *The USFWS draft report (pg. 92) acknowledges that … "**In general, our biological verification was unsuccessful**". The USFWS draft report (pg. 93) further states … "**The performance of River2D in predicting the CSI of occupied locations is a combination***

***of errors due to: 1) the predictive accuracy of the HSC; and 2) the predictive accuracy of the hydraulic modeling.**" These statements raise serious questions about the validity of the conclusions in the USFWS draft report.*

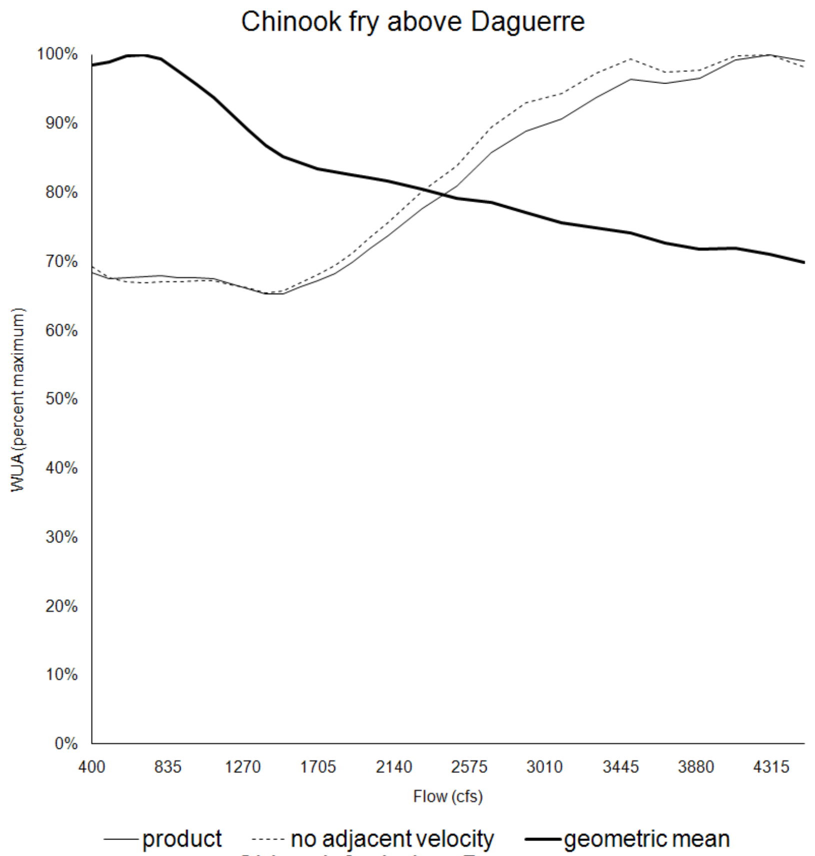
Response: The discussion establishes that the failure of the biological verification was largely due to small sample sizes and the predictive accuracy of the hydraulic modeling. The failure to biologically verify these models does not mean that the models are not useful for the purpose of this study, namely providing scientific information to use in determining instream flow needs for anadromous fish in the Yuba River. Rather, it means that we did not have an increased confidence in the use of the flow-habitat relationships from this study for fisheries management in the Yuba River. In this regard, we note that most instream flow studies, including the Beak (1989) study, would be characterized as unverified, since they do not include a bioverification component.

YCWA 57. Comment: *The USFWS draft report (pg. 97) states that the biological verification in Hardy and Addley (2001) produced better results than those found in the USFWS draft report study due to: 1) presentation of results for an entire study site, rather than just for portions of the sites that were sampled in the USFWS draft report; and 2) calculations of combined suitability as the geometric mean of the individual suitabilities, rather than calculation of combined suitability as the product of individual suitabilities. This discussion suggests that the USFWS draft report may benefit from exploring the use of calculating combined suitability as the geometric mean of the individual suitabilities.*

Response: We have explored the use of calculating combined suitability as the geometric mean of the individual suitabilities. As shown in the figures on the following pages, the selection of product versus geometric mean does not seem to have a consistent effect on the shape of the overall flow-habitat relationships. The use of the adjacent velocity parameter also does not seem to have a consistent effect on the shape of the overall flow-habitat relationship. None of the graphs shown on the following pages show the general shape of the flow-habitat relationships in Beak (1989), namely the highest amount of habitat at the lowest flow, with the amount of habitat rapidly decreasing with increasing flow.

YCWA 58. Comment: *The USFWS draft report (pg. 97) further describes that the bioverification in Hardy and Addley (2001) resulted in … "**large areas with zero suitability (away from the channel margins) and smaller areas of high suitabilities near the channel margins where fish were located.**" [emphasis added]. As noted in Comment 22, over 98 percent of the measurements where YOY Chinook salmon and steelhead/rainbow trout were observed, were made near the river banks. These results indicate a volitional selection of near-shore habitats by YOY, and should be considered in the development and application of combined habitat suitability HSC, and eventual representation of WUA-discharge relationships.*

Response: The relative abundance of fish in near-shore and mid-channel habitats is confounded by the difference in depths and cover in near-shore and mid-channel habitats. Specifically, it is likely that the low abundance of fish in mid-channel habitats was due to the deeper depths and lack of woody cover, as compared to near-shore habitats, rather than a volitional selection of


Chinook fry above Daguerre
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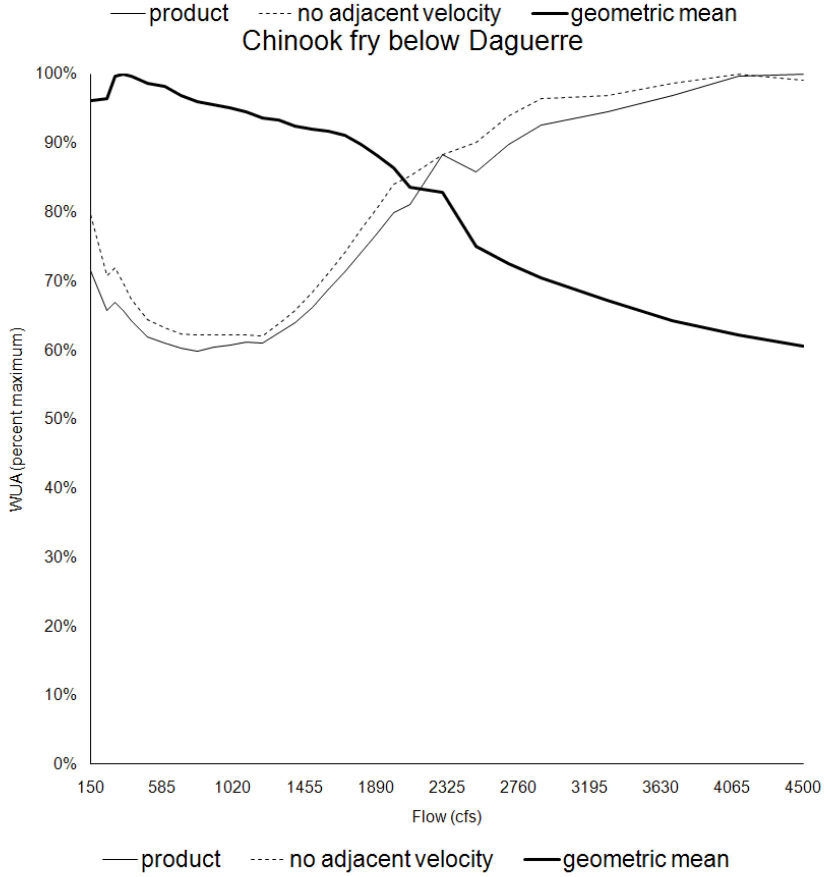
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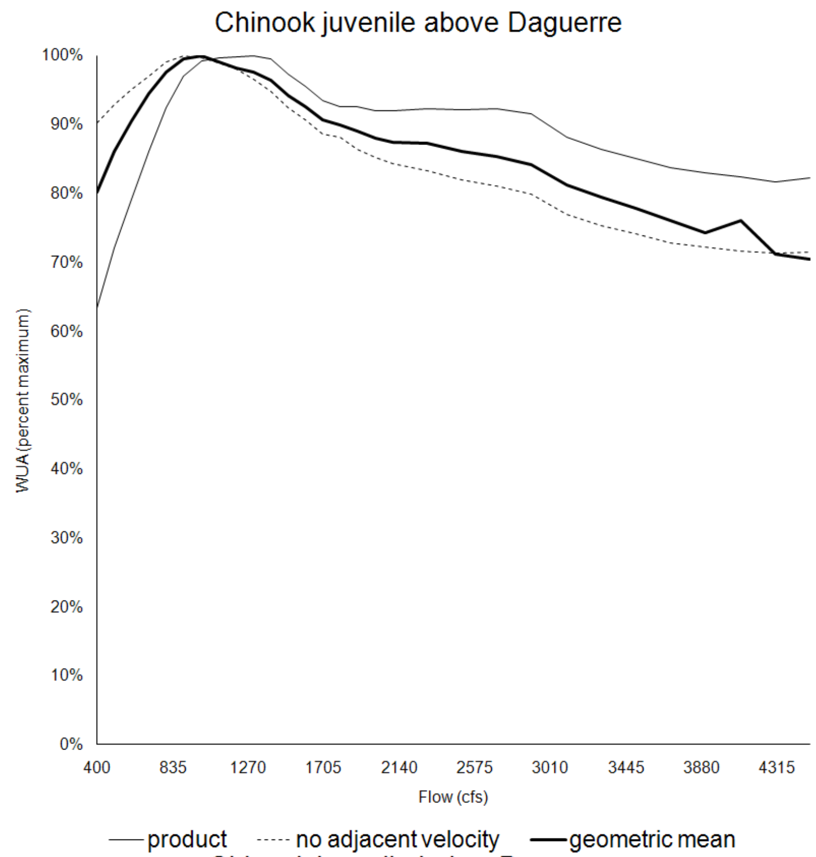
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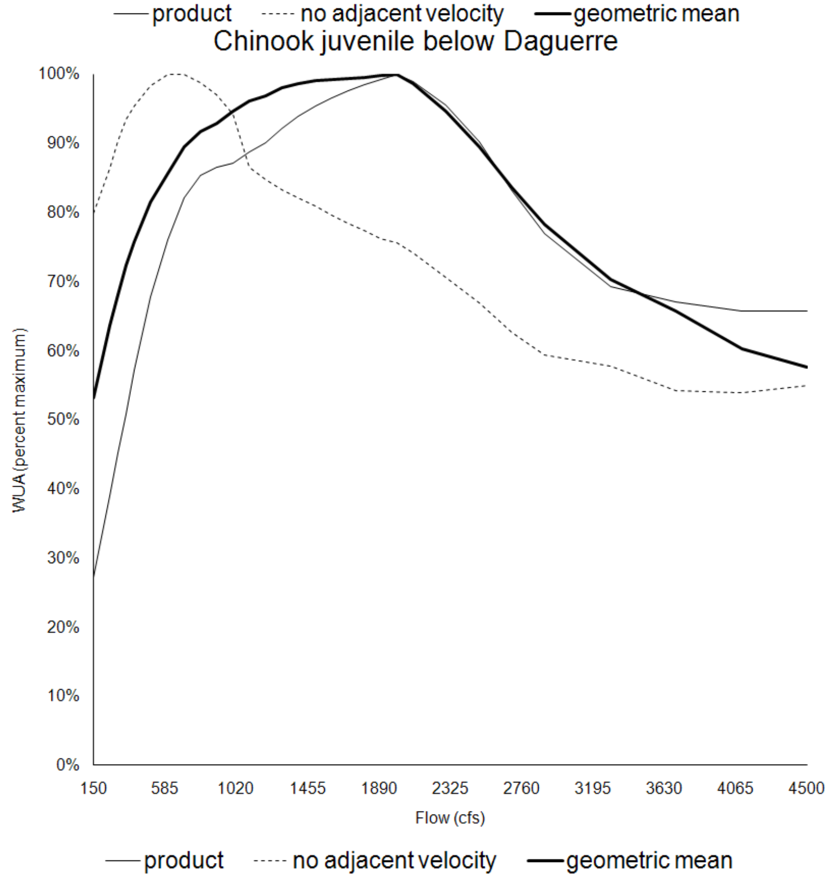
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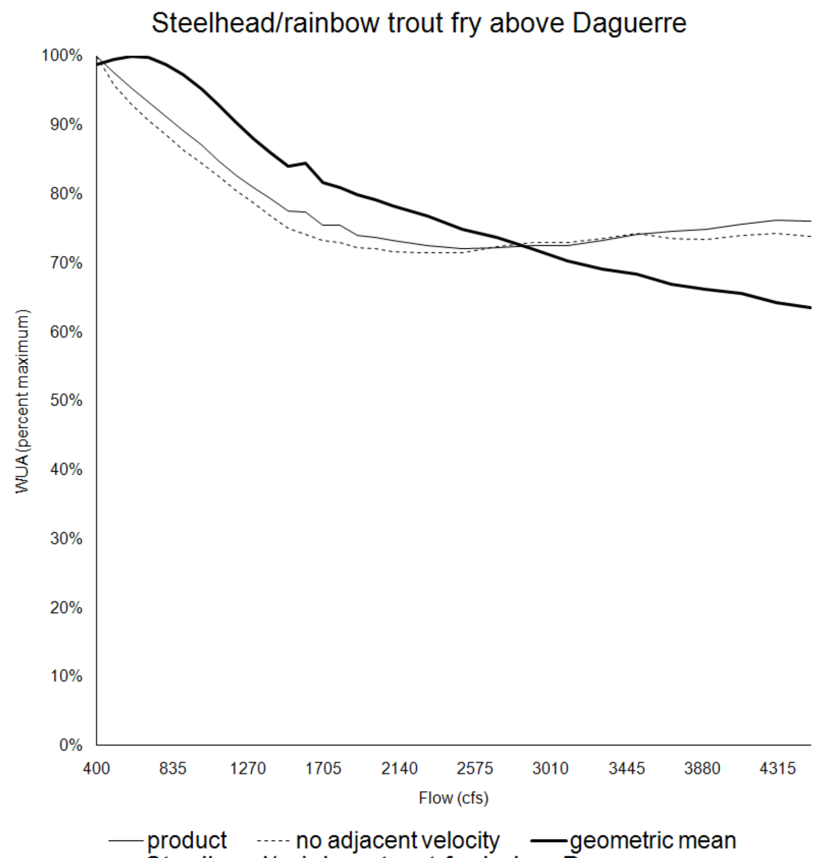
Steelhead/rainbow trout fry above Daguerre
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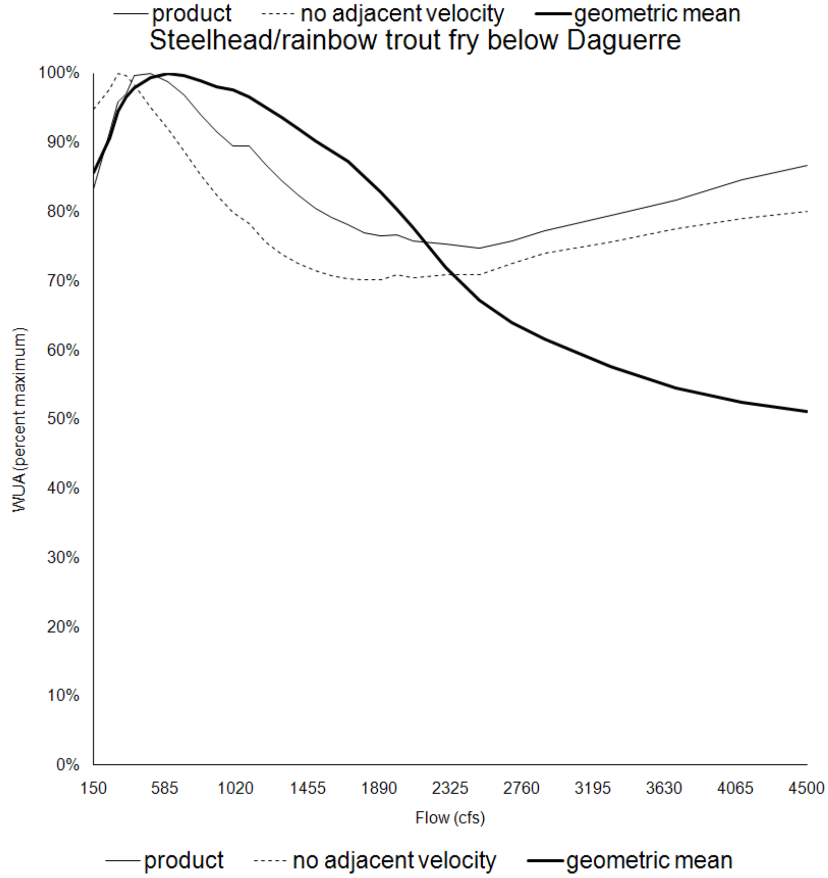
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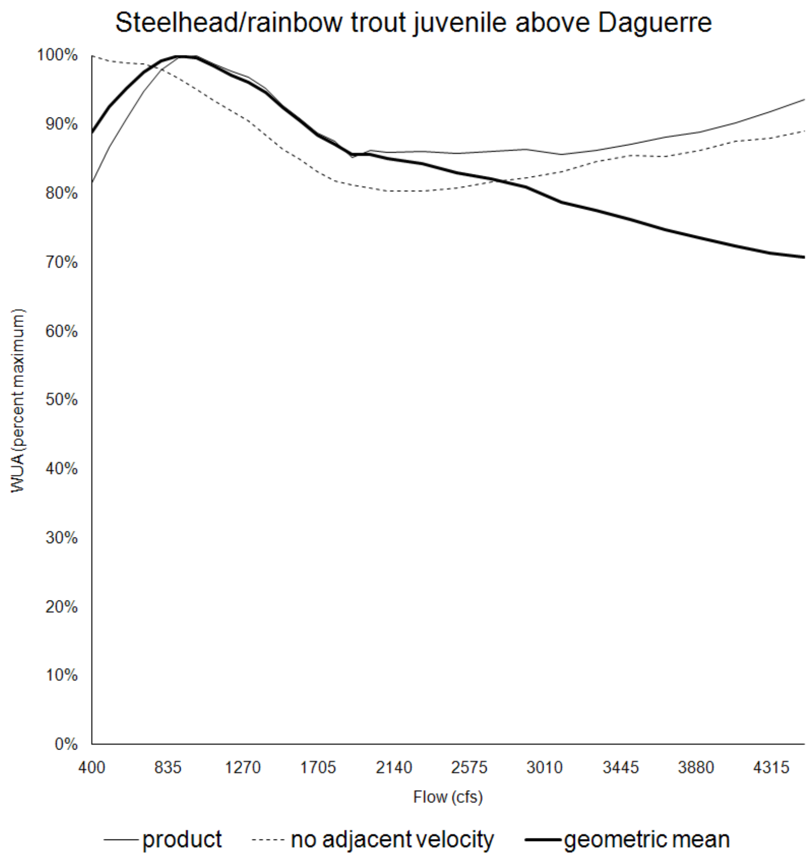
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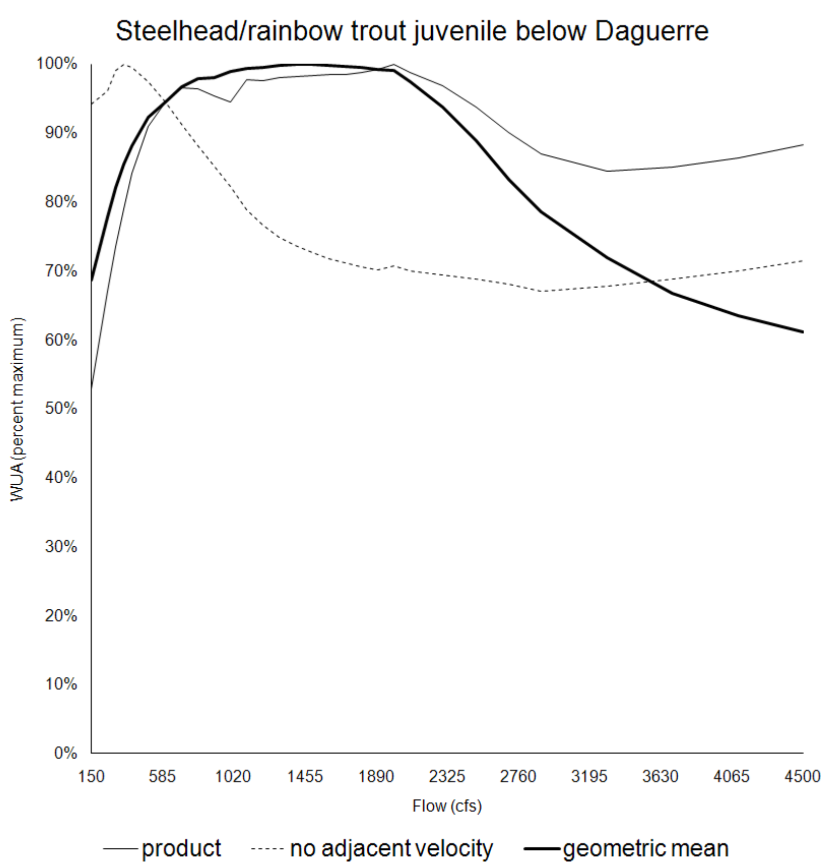
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Flow (cfs)
product
no adjacent velocity
geometric mean

near-shore habitats by fry and juvenile anadromous salmonids. As such, it does not appear that consideration of near-shore versus mid-channel habitats is warranted in the development and application of criteria and WUA-discharge relationships.

YCWA 59. Comment: *Biological validation is intended to determine if the HSCs are correctly defining habitat usability. However, review of the plots of combined suitability predictions and locations of observed fry and juveniles presented in Appendix M clearly show that the USFWS 2-D simulations generally failed to predict the location of observed fry and juveniles.*

Response: Biological verification as conducted in this study tests a combination of whether the hydraulic model is correctly predicting the depths and velocities at the fish locations and if the HSCs are correctly defining habitat usability. We agree that the data presented in Appendix M shows that the 2-D simulations generally failed to predict the location of observed fry and juveniles. The discussion establishes that the failure of the biological verification was largely due to small sample sizes and the predictive accuracy of the hydraulic modeling.

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