Independent Review of a Draft Version of the 2009 NMFS OCAP Biological Opinion

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CALFED Science Review Panel

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I. Introduction and Background

This is the second report of the Review Panel (Panel) convened by the CALFED Science Program to review the 2009 Biological Opinion (BO) on the Long-Term Central Valley Project (CVP) and State Water Project (SWP) Operations Criteria and Plan (OCAP). The BO assesses the effects of the continued operations of the CVP and SWP on listed Sacramento River winter-run Chinook salmon (*Oncorhynchus tshawytscha*), Central Valley spring-run Chinook salmon (*O. tshawytscha*), Central Valley steelhead (*O. mykiss*), Central California Coast steelhead (*O. mykiss*), Southern Oregon/Northern California Coast coho salmon (*O. kisutch*), Southern Distinct Population Segment of North American green sturgeon (*Acipenser medirostris*), and Southern Resident killer whales (*Orcinus orca*). This review was voluntary and was initiated at the request of the National Marine Fisheries Service's (NMFS) Sacramento Office. Killer whales were not included as part of the review.

The initial BO on the CVP/SWP OCAP was issued by the NMFS, Southwest Region, in October 2004. That BO was reviewed by an earlier CALFED review panel[1] and two reviewers (Dr. Thomas McMahon and Dr. Jean-Jacques Maguire) from the Center for Independent Experts (CIE) at the University of Miami. These reviews were then considered by a team of NMFS scientists assembled from NMFS Science Centers. All reviews are available on the CALFED Science Program website under Review Panels: http://www.science.calwater.ca.gov/events/reviews/review_ocap.html

NMFS requested a review of a draft version of the 2009 BO as it was being prepared. The review process was divided into two phases: review of the analytical framework and review of the December 11, 2008 draft version of the BO itself. We refer to the December 2008 BO we reviewed as the draft BO. The analytical framework specified the technical analyses that NMFS was using to produce the 2009 BO. A review of the analytical framework was delivered to NMFS on October 31, 2008 and is available at the CALFED Science Program website. The Panel for the review of the analytical framework was composed of four (Anderson, Duffy, Rose, and Smith) of the seven members used to review the draft BO. This seven-member Panel was then convened to review the draft BO. This report is the Panel review of the draft BO.

Different questions were charged to the Panels for the analytical framework and BO reviews. Questions for the BO review consisted of one overarching question, and eight more specific questions:

1. Overarching question: Is the NMFS BO scientifically sound and are the conclusions scientifically defensible?

2. Are assumptions clearly stated and reasonable based on current scientific thinking? What uncertainties and limitations were not addressed that might impact the analysis in the BO substantively? Are any of the current

[1]Rose, Anderson, and Deas are members of this review Panel and were also members of the review panel that reviewed the 2004 OCAP BO.

assumptions and uncertainties limiting or critically influencing future analysis?

3. Does the analytical framework adequately reflect comments and issues raised in Part 1 of this review? Is the framework effectively applied? Does the application of the framework adequately assess all potential responses of each fish species to all likely effects of the proposed action (i.e., both direct and indirect effects of the project)?

4. Have any temporal or spatial aspects of fish or ecosystem needs been overlooked, exaggerated or in some other way not adequately addressed with the models and analytical approaches presented? If so, what are they and how could NMFS ensure they are adequately addressed in the BO?

5. Do the data, analyses, results, and conclusions presented in the BO lead to a thorough understanding of the risks to individuals and populations from the proposed action? If not, what risks have been overlooked and what other scientific information should be considered?

6. Were gaps in life history information considered?

7. Does the framework adequately allow future climate scenarios to be evaluated? Does the BA provide sufficient information for this climate change analysis? Does the BO adequately address likely future climate change effects on salmonid fishes and green sturgeon in the Central Valley?

8. Were statistical uncertainties in population numbers, survival, entrainment loss, etc. considered? Did the BO analyze the impacts of the proposed action in the proper perspective (e.g., the appropriate time scale, or the likelihood that an event, such as climate change, will happen)?

9. Does the analysis in the Effects of the Proposed Action section present a clear argument that supports the conclusion? Are the arguments and information from the Effects section carried through the Integration and Synthesis section into scientifically supported conclusions?

We first summarize the process for this second phase of the Panel review of the draft BO (Section II). We then answer the questions posed to the Panel (Section III). Section IV lists our major comments, which we term Tier 1. Our responses to the questions rely heavily on the issues identified, and discussed in more detail, in the Tier 1 comments. Minor and specific comments were collected from the Panel members, collated, and are listed as Tier 2 comments in Appendix A. Some of the Tier 2 comments are minor suggestions for improving the document, but others are quite important and either add to or support the issues raised in the Tier 1 comments. The Tier 2 comments were not completely vetted by all Panel members, or edited carefully, but they are included because they can assist NMFS in their revisions. Documents provided to the

Panel by NMFS and CALFED are listed in Appendix B. Appendix C summarizes Panel comments about the climate change analysis reported in the Biological Assessment (BA).

We emphasize that this review of the draft BO relies heavily on the information and model results presented in the BA prepared by the U.S. Bureau of Reclamation and the California Department of Water Resources. Our focus, however, is on the BO itself and the scientific basis and validity of how the information (most of which is found in the BA) was used in the BO to reach jeopardy-related decisions. We were not charged with, nor did we have the time, to systematically review the BA.

Our comments are intended to increase the clarity and transparency of the BO that would enable clearer links between evidence and conclusions and thus greater confidence in the jeopardy decisions. The short time period available for this review may have led to oversights or misrepresentations of the BO (or BA); if so, we apologize for them and hope they do not detract from the more general messages. Citing examples is always helpful to illustrate general ideas but the time constraints did not allow the Panel to fact-check all of the cited examples to the extent we would have preferred. Also, the document we reviewed was a draft and thus subsequent versions may already address some of our comments. We offer our comments in the spirit of improving the logical consistency and readability of the document. This review represents the unanimous opinion of the Panel.

The Panel would like to acknowledge and thank the NMFS personnel, and the staff from the other agencies, for their cooperation in the review process. Our review would not have been possible without rapid and open exchanges between agency personnel and Panel members. We also acknowledge the CALFED Science Program for conducting this review. We especially thank Rebecca Fris of the Science Program who competently handled all the logistical aspects of the review, including organizing the public workshop, panel meetings, and conference calls. The success of the review was a direct result of Rebecca's efforts.

II. Review Process for the BO

The Panel received the draft BO, the BA, and previous reviews of the 2004 BO on December 12, 2008[2]. The Panel was then involved with two conference calls in preparation for the workshop. The first conference call held on December 19, 2008, included Panel members and representatives of NMFS. As a result of the conference call, the Panel requested and received a variety of additional information including: supplements to the BA, expert testimonies, and Judge Wanger's decisions (Appendix B). The second conference call was held on December 30, 2008, and involved the Panel members only. This second conference call mostly focused on how to proceed with the review. A workshop was held January 8 and 9 in Sacramento, at which NMFS gave presentations that summarized the analyses used in the draft BO. The initial part of the

[2] Reisenbichler and Erickson joined the panel, and thus received the documents, in the last week of December.

workshop was open to the public. After the presentations, the Panel posed questions to NMFS and the public was given the opportunity to make comments. The Panel briefly met again with NMFS after the public session in order for the Panel to ask additional clarifying questions about the BO. The Panel then deliberated that afternoon and the next day to outline Tier 1 comments and agree upon rough answers to the questions posed to the Panel. Over the next 10 days, the Panel developed this report.

We strongly support NMFS in seeking peer-review and feedback on the analytical framework (Phase 1) and the draft BO (Phase 2). The BO addresses an incredibly complex situation involving complicated project operations and multiple species. We focus mostly on the big picture issues in this review, because time was not sufficient to confirm the specifics of all analyses and the data used in the BO. In addition, some of our comments may have arisen because we reviewed a draft version of the BO. We offer comments in the hopes of providing guidance to improve the science underlying the BO.

This review does not cover the RPAs (Reasonable and Prudent Alternatives), which the Panel understands will be part of the final BO. The Panel was provided with an early draft of the RPA document; however, this review does not comment on the relative or absolute merits of different specific actions that were described in the draft RPA document. We do mention the RPAs when comments about the BO are also germane to the general process of developing RPAs.

III. Panel Responses to Questions

1. Overarching question: Is the NMFS BO scientifically sound and are the conclusions scientifically defensible?

The terms “scientifically sound” and “scientifically defensible” imply the use of the best available tools. When sufficient data are available, quantitative analyses using data analysis and models generally are superior to qualitative tools because they allow more refined estimates of the overall effects of individual stressors, provide a clear logic between results and conclusions, and allow for estimation of uncertainty and the degree of confidence we have in the conclusions. In other situations with more limited information, as for steelhead and green sturgeon here, qualitative and semi-quantitative analyses are considered the best available tools.

The BO relies heavily on the analysis of project effects at the individual level by life stage, which was the strong point of the analysis, and then accumulates these effects by making a list of the individual effects to infer higher order (population, species) effects. The population and species level analyses are the weak aspect of the BO that can, and should be, elevated to next level of quantitative analysis, at least for the salmon species. Available data are likely insufficient for population and species level quantitative analysis of green sturgeon and steelhead.

The Panel cannot provide simple Yes/No answers to this two-part question. Some aspects of the draft BO analyses prevent the Panel from endorsing the draft BO as 100% scientifically sound. These include inconsistent use of the BA and other information across Divisions, poorly defined baseline conditions, the general lack of quantitative rigor and species life-cycle integration, partial implementation of the analytical framework as envisioned by the Panel and documented in the 2008 review of the framework, lack of clarity about uncertainty and degree of protectiveness, and incomplete use of the climate change information. These concerns are expanded in Tier 1 comments. A great deal of state-of-the-art science was used, and used appropriately, in the draft BO. The Panel withholds full endorsement of the science in the draft BO because we believe the analyses could have been better and more convincing.

Despite the limits to our review and the shortcomings we perceive in the BO, the Panel is of the opinion that the decisions of jeopardy for the considered species are reasonably based on the preponderance of evidence presented for the numerous individual stressors that affect the species from the upper watershed through the Delta. The Panel understands what led NMFS to their decisions, and the Panel has no basis for thinking that the jeopardy decisions are incorrect. However, without a more integrative analysis, one cannot determine the relative contribution of the various components of the proposed actions (e.g., warmer upstream water temperatures versus higher within-Delta mortality due to pumping), and thus one cannot assess which specific project actions are most responsible for causing jeopardy to the considered species. More quantitative and integrative analyses would improve our understanding of what are the drivers of the jeopardy decision, and thus increase our confidence in the conclusions of the BO. Nevertheless, the convincing accumulation of individual effects at the life stage level related to project operations, the poor general state of the populations, and the requirement to resolve doubt in favor of the species, led the Panel to judge that NMFS' conclusions were robust. The lack of integrative analyses and tools is especially important and critical in developing the RPAs because such quantitative tools enable comparison of the relative benefits of alternative actions.

We stress that our criticisms do not constitute a challenge to the validity of NMFS' final jeopardy judgments. Our criticisms should be of value primarily for increasing the rigor of analyses in the BO, and for developing the RPA's on the basis of the likelihood of success and expected population-level responses of specific actions.

2. Are assumptions clearly stated and reasonable based on current scientific thinking? What uncertainties and limitations were not addressed that might impact the analysis in the BO substantively? Are any of the current assumptions and uncertainties limiting or critically influencing future analysis?

In general, we found the assumptions to be clearly stated and reasonable, and the information and data used in analyses to be current. The presentation of summary tables in a systematic way was very helpful. However, certain relevant assumptions and how results were interpreted were not always clear because assumptions, data richness, and analyses often differed among species and among Divisions. For example, the reader often is left to infer the specifics of baseline conditions because these conditions were not always adequately explained or modeled. Some assumptions using one species as a surrogate for another should have been more clearly stated and justified. At times, fall-run salmon or late fall-run salmon were used as surrogates for steelhead, and salmonids were used as surrogates for green sturgeon. The secondary role often given to the climate change results can also impact the analyses. NMFS' assumption that "if things are bad under current conditions, they will be bad or worse under climate change" needs to be clearly described and the evidence for this better explained. The Panel also understands the logic used by NMFS in essentially ignoring likely changes in marine survival of the salmon (i.e., assuming past marine survival will continue). However, as a science-based Panel that is less constrained by administrative aspects than NMFS, we find it troublesome to have such a large, and potentially important, unknown dealt with so simply. Other examples can be listed, but the major assumptions seem reasonable and are stated in the draft BO, although their presentation is uneven and not always as obvious as they should be.

Several aspects of the draft BO currently limit NMFS if additional analyses are needed in the future. These include: lack of a simulated true baseline, lack of integrative modeling tools to go from individual effects by life stage to population-level responses, and lack of methods for characterizing the uncertainty associated with the different analyses. Each of these is expanded upon in the Tier 1 comments. Clear statement of assumptions and these current limitations assume even more importance for designing RPAs. These limitations will be especially important as questions are asked about the effectiveness and tradeoffs among alternative remedial actions.

3. Does the analytical framework adequately reflect comments and issues raised in Part 1 of this review? Is the framework effectively applied? Does the application of the framework adequately assess important potential responses of each fish species to important likely effects of the proposed action (i.e., both direct and indirect effects of the project)?

[Note the Panel substituted "important" for "all" in the two places that "all" was used in the second part of the question.]

The analytical framework that was applied in the draft BO is, for the most part, the framework described by NMFS in the document provided to the Panel for the phase 1 review. As stated in the 2008 review of the framework, the framework is a major step forward in being systematic and transparent, and is a well-considered response to the 2005 review that criticized the 2004 BO for the lack of a conceptual model and analytical framework. The 2008 review of the framework was done based on the general description of the framework without any specific analyses being reported. The framework that was ultimately applied in this draft BO does not completely address the comments of the 2008 framework review, and thus does not fully represent the framework as envisioned by the Panel.

We repeat the five major technical comments here that were expanded in the 2008 review of the framework.

> Comment 1—The Final Steps: The analytical framework does not clearly state the details of how the key final steps in the analysis — extending individual effects to population responses and then to species risks — will be accomplished.
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> Comment 2—Baseline: The analytical framework does not explicitly state what will be used as the baseline conditions.
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> Comment 3—Uncertainty: Uncertainty is always an issue with analyses such as those outlined in the analytical framework, especially when information is taken from a variety of sources. How uncertainty will and will not be dealt with needs to be part of the analytical framework.
>
> Comment 4—Climate change: The analytical framework is general enough that if the appropriate information were provided, the analytical framework should be applicable to including future conditions that include assumed climate changes.
>
> Comment 5—Clarity and presentation: There are several ways to increase the transparency of analyses emerging from the analytical framework… We recommend a flow chart type approach, whereby the inputs and outputs (including their spatial and temporal scales) of each analysis are shown.

The substance of these comments, to varying degrees, still apply to the framework that was actually implemented by NMFS in the draft BO. The five comments from the 2008 review of the framework appear again in this review of the BO as Tier 1 comments. The Panel considers these comments important and still applicable. In our consideration of whether the 2008 review of the framework was addressed, the Panel did not factor in any time constraints imposed on NMFS for preparation of the BO.

If one defines the framework as NMFS' vision, then the framework was effectively applied. Given that the technical issues identified in the 2008 review of the framework still largely remain, the Panel would state that the framework for salmon, as envisioned by the Panel, was not effectively applied. The Panel considers the lack of a final integrative tool as a serious omission for salmon. For green sturgeon and steelhead, the qualitative integration in the final steps is reasonable because of information and data limitations.

The framework applied by NMFS does adequately cover the likely important responses and effects, within the limits of available information. There are always more responses and effects that could be examined, but the Panel determined that the list of effects and responses analyzed by NMFS was reasonable.

4. Have any temporal or spatial aspects of fish or ecosystem needs been overlooked, exaggerated or in some other way not adequately addressed with the models and analytical approaches presented? If so, what are they and how could NMFS ensure they are adequately addressed in the BO?

The BO generally addresses the appropriate temporal and spatial aspects of fish and ecosystem needs. The analyses by Divisions help to enable a logical progression from upstream to downstream. There are examples, however, of where issues have been overlooked or not adequately addressed. For example, how project operations might affect flow and water temperature for sturgeon spawning and rearing over the entire reach between Keswick Dam and Hamilton City was not adequately addressed. An example of conflict between species (ecosystem view) is the potential conflict between salmon and sturgeon regarding optimal flow and temperature conditions. Regulated releases from Shasta Reservoir provide stable and cool flows during spring, summer, and early fall months that benefit salmonids but may be suboptimal for sturgeons. For example, the target temperature between Balls Ferry and Bend Bridge for the period 15 April through 1 October is 56^{o} F (13.3^{o} C). Although this is within the range of temperatures for optimal egg development in sturgeon, 56^{o} F is below the thermal preference of age-0 sturgeon ($60\text{-}61^{o}$ F), and may reduce the growth rate of larvae and post-larvae relative to warmer temperatures (Mayfield and Cech 2004). Although the BO briefly mentioned potential negative impacts of the regulated flows on sturgeon, there was no evaluation of the effects of artificially cool summer temperatures on sturgeon development.

Other examples that illustrate where temporal and spatial aspects may not have been adequately addressed are the use of changes in fish survival associated with closing the Delta Cross Channel (DCC) gates, affects of fish routing through the Delta on survival, use of monthly rather than daily temperature values for egg mortality effects, and the oversimplified summarization of CALSIM results. The Delta analysis for salmon relies heavily on the results of a pilot study using acoustically-tagged fish that examined how survival and routing of fish through the Delta changes with the DCC gates open versus closed. These data are directly relevant to the analysis and should be used but important limitations should be discussed. Specifically, better documentation should be supplied indicating that the increased survival measured with the DCC gates closed is based on only two sets of data from one water year (2007) and involved a total of only 144 tagged fish. The observed difference in survival due to the DCC operations is likely not statistically defensible. The ecosystem is very complicated spatially, and more attention should be given to how the routes taken by fish can affect their growth and survival. Any effect on growth should then be translated into expected effects on subsequent marine survival. Simple models of routing exist (see Tier 1 comment 3), and these can be used to estimate travel and residence times in different habitats and regions. Another example was the use of monthly averaged temperatures in certain analyses, despite the comments about the importance of daily variation in the 2005 review of the BO and the subsequent workshop that was held (Deas et al. 2008). Finally, for some of the analyses, the 82 years of monthly CALSIM output were overly reduced to single mean values. While use of mean values is valid, more information can be obtained by analyzing the 82 years individually or grouped by water year type.

The long-term or evolutionary time scale also is important for jeopardy considerations, and the BO reflected adequate consideration of this scale. For example, the BO appropriately stressed the importance of maintaining or increasing habitat, population, and life history diversity to facilitate population and species adaptation and persistence.

5. Do the data, analyses, results, and conclusions presented in the BO lead to a thorough understanding of the risks to individuals and populations from the proposed action? If not, what risks have been overlooked and what other scientific information should be considered? [The Panel interpreted the term "risk" as the general use, rather than specific use of risk as a probability.]

The analyses, results, and conclusions lead to a thorough understanding of the risks to individuals but not to populations. The analyses of the project effects on individuals are sound. The best available information is used to assess the effects of temperature and flow on survival of salmon over river reaches and within life stages. The draft BO uses the most recent information on survival of fish through individual Delta passage routes and through the SWP/CVP export facilities. However, the integration of the individual route survivals to a population level survival (defined as the number of fish passing Chipps Island and entering the Bay) is not thorough. Fundamentally, a thorough analysis would estimate the percent change of individuals by life stage due to the effect and the percent of individuals in that life stage exposed to the effect, and combine these into a population-level response. The lack of an integrative modeling tool for salmon was a common criticism by the Panel that applies to many questions and comments. A thorough analysis would include the use of an integrative tool that allowed partitioning of responses to the different effects. In particular, to understand the effects of the Delta flows and water exports at the population level, one must integrate fish routing between the main river channels, the interior Delta, and the pumps in the analysis. The effects of pump operations, exports, and operational barriers and gates can be significantly reduced when the majority of fish are routed through the main river. Consequently, without considering routing in sufficient detail, the effects of upstream actions relative to Delta actions cannot be thoroughly understood and compared. This issue is especially relevant in designing the RPAs. Without an adequate integrative tool, one cannot quantitatively evaluate the effectiveness of alternative allocations of limited water resources to upstream temperature control versus within-Delta actions.

Examples of possible overlooked risks include the impact of project operations and climate change on the adult migration and the pre-spawning life stage of salmon, effects of a sequence of multiple drought years, and project effects on early life stages of sturgeon. Summer pre-spawning mortality in spring-run Chinook salmon has been significant in recent warm years, and will likely increase with climate change. While there are sequences of drought years in the historical record simulated with CALSIM, these did not seem to be used explicitly in the BO to assess the possible cumulative effects of a sequence of drought years. The BO uses averaged conditions as inputs to the salmon survival models and could make more use of the 82-year time series of results from the CALSIM simulations. Finally, more consideration, even qualitatively, could be given to potential project operations on the early life stages of sturgeon.

6. Were gaps in life history information considered?

Gaps in the life cycle of the species were generally considered by NMFS but not always discussed to the degree preferred by the Panel. The general lack of information on steelhead has been documented by NMFS and others. Important gaps also exist for green sturgeon. Very little was known about green sturgeon prior to 2000. Although considerable progress has been made since then, gaps in life stage and life cycle information for green sturgeon are significant and make it difficult to evaluate or estimate project effects on sturgeon. Behavior, movements, and habitat needs of larvae, post-larvae, and juvenile green sturgeon are poorly understood. The effects of project operations on these life stages cannot be adequately evaluated without better information, such as the spatial and temporal distribution of juveniles in the Delta and along the Sacramento River and the effects of pumping and diversion operations above RBDD on green sturgeon larvae. Clearly, more research is needed to complete these gaps in life stage information.

The ocean phase remains a major knowledge gap for all of the species. Some information is available for salmon and adult green sturgeon, but little is known for steelhead and sub-adult green sturgeon. Growth and mortality after leaving the system can be affected by a variety of sources including climate patterns and effects on productivity and species community, harvest, trawl by catch, and predation by marine mammals and other predators. The draft BO does not directly address growth and survival during the ocean phase for any of the species. While we understand the logic, and time and knowledge limitations, the possibility exists that we may be analyzing effects that occur within the system that ultimately are overshadowed by dynamics and effects in the marine phase.

7. Does the framework adequately allow future climate scenarios to be evaluated? Does the BA provide sufficient information for this climate change analysis? Does the BO adequately address likely future climate change effects on salmonid fishes and green sturgeon in the Central Valley?

The analytical framework does allow appropriate consideration of climate change. The analytical framework works equally well with assumed future climate change as without, and thus is completely compatible with appropriate consideration of climate change. One potential issue is the lack of a clearly defined and appropriately used baseline. An assessment of species jeopardy should include a comparison of a baseline scenario that includes climate change (and none of the project operations being proposed) with a baseline scenario that includes climate change with the proposed project operations.

We found that the BA does provide "sufficient information for this climate change analysis." The BA includes impressively thorough quantitative analyses of major physical impacts of climate change, including effects on unimpaired and below-dam river flows, reservoir cold-water volumes, below-dam water temperatures, in-Delta flows, and Delta salinities. These are presented in Appendix R of the BA. The Panel considers this analysis to be state-of-the-art, although the analysis, like all climate change analyses, has important limitations (see Appendix C).

While the draft BO uses some of the climate change results reported in the BA, the BO could have made more use of the results. The draft BO used the climate change results unevenly across Divisions, and in some cases, more as an add-on to the analysis rather than fully incorporating the climate change results into analyses. The NMFS strategy was often to assume that climate change would likely make things assessed under current conditions worse. In particular, much of Chapter 6 of the draft BO makes comparisons among future scenarios (CALSIM studies 6, 7, and 8), none of which consider climate change. Subsequent chapters build upon the results of Chapter 6. Although informative, these comparisons do not include a comparison of a baseline scenario that includes climate change but no project operations to future scenarios that include both climate change and proposed project operations. Such comparisons would directly address the issue of jeopardy/no-jeopardy. Why such comparisons were not done in the draft BO should be explained.

8. Were statistical uncertainties in population numbers, survival, entrainment loss, etc. considered? Did the BO analyze the impacts of the proposed action in the proper perspective (e.g., the appropriate time scale, or the likelihood that an event, such as climate change, will happen)?

Statistical uncertainties were generally not considered in the draft BO analyses, and stochastic variation and uncertainties could have been treated more explicitly. Stochasticity is variation that cannot be reduced by more measurements (e.g., different water year types). Uncertainty includes measurement error and can be reduced with more data. In most aspects of the analysis, uncertainty is not considered. Most of the analyses in the draft BO used point estimates (e.g., means) to represent the altered environmental conditions, and then reported responses as point estimates (e.g., without confidence intervals). For example, more use could have been made of the 82 years of CALSIM output in some of the analyses, rather than using values averaged over multiple years.

Although a number of limitations and uncertainties exist, we see none that would impact the analysis in the draft BO enough to alter the jeopardy conclusions for salmon, especially when one must err on the side of protecting the species. Most likely, a thorough and rigorous treatment of uncertainty in all aspects of the problem, were this possible to perform (we do not recommend that this should have been done), would result in a wide range of possible outcomes; almost certainly some of those outcomes would result in jeopardy. By similar logic, however, inclusion of uncertainty could affect the no-jeopardy decision for Central California Coast steelhead because of the need to be protective of the species. A formal uncertainty analysis (such as Monte Carlo) is impractical, but clear documentation of the likely uncertainties and their magnitude (qualitative or quantitative, as practicalities allow) would add additional credibility to the analyses.

The long-term view taken in the BO is the appropriate time scale for analyses. One of the most important limitations to the analyses used in the draft BO is the lack of quantitative modeling of routing within the system and lack of integrative life-cycle modeling for salmon, which would enable project effects to be accumulated and be compounded over the life cycle for multiple years. The long-term view also means that possible events in the future that can be difficult to quantify need to be considered. For example, lack of specifics about how b(2) and b(3) water and VAMP flows will be continued into future decades can generate considerable uncertainty about the conditions to be expected in the long-term. Other events that should be considered include a succession of drought years, possible changes in water demands into the future, seismic and flooding events, and how to incorporate new science and new ideas about how the Delta functions and should be managed.

9. Does the analysis in the Effects of the Proposed Action section present a clear argument that supports the conclusion? Are the arguments and information from the Effects section carried through the Integration and Synthesis section into scientifically supported conclusions?

The analyses presented in the effects section of the draft BO support NMFS' conclusions. The argument is clear – when you collate all of the many individual effects from project operations by life stage, NMFS is convinced that the long list justifies their jeopardy/no jeopardy decisions. The BO is an opinion so expert judgment should and does play a major role. The Panel believes, however, that the draft BO is more opinion than is necessary or desirable. Expert judgment is appropriate, but the more that it can be supported with empirical evidence and analyses, the more transparent and defensible the decisions. This is especially true for the salmon analyses, which have a much richer database and richer modeling tools upon which to draw. The lack of a final integration tool for salmon forces NMFS to rely more on conjecture than is necessary.

Additional clarity can be achieved in the BO by making the presentation and analyses consistent across Divisions, clearly defining baseline, and dealing with uncertainty. With these improvements, the information should provide a clearer argument that supports the conclusions. Several analyses reported seemed out of place. For example, the reasoning behind comparing mortality rates among CALSIM studies 6, 7, and 8 was not clear to the Panel. If the relative effects of the different studies are of interest, then explain why. The purpose of comparing these alternatives was not clear in all sections of the draft BO, so the reader must sift through results reported as mortality rates and changes in mortality rates. The default should be comparing the CALSIM studies of future scenarios (with different scenarios for climate change) to baseline. The information used in the comparisons is the same, but the mix of presentations unnecessarily complicates interpretation.

The Panel has no basis for questioning the jeopardy decisions, but the effects can, and should be, better carried through the integration and synthesis section. List making is a valid approach to synthesis but it is the simplest and least integrative approach. More could have been done for salmon, and this would have advanced the analyses closer to the endpoint before expert opinion must be invoked.

IV. Positives

There many positive aspects and developments in the draft BO. The preparation of the BO is a very challenging and complex task that involves multiple species and complicated spatial considerations. We list some positive aspects of the draft BO below. While the text may be shorter than the more critical comments listed as Tier 1 comments, this is not a reflection of the depth of the positive developments and advances made in the draft BO. It is simply that positive things can be explained with much less text than critical comments.

Positive 1. Addressing overarching comments of the 2004 BO

The draft BO is a major step forward from the 2004 BO. The draft BO attempted to address the overarching comments from the 2005 CALFED review of the 2004 BO. The three overarching issues were: lack of a conceptual framework, need for an analytical framework, and use of a life cycle approach. The analytical framework uses the Lindley et al. (2007) paper as its foundation. Lindley et al. (2007) was prepared by the Central Valley Technical Recovery Team (TRT) and applies the Viable Salmon Population (VSP) concept (McElhany et al. 2000) that outlines approaches for conservation planning of Pacific salmonids. While the draft BO did not address all aspects of these 2005 review comments, much progress was made in preparation and review of an analytical framework. The proposed analytical framework was endorsed by the 2008 review panel, included a conceptual framework and was amenable to a life cycle approach. The 2008 review of the analytical framework stated:

> "The Panel endorses the AF [*analytical framework*] described by NMFS as a significant step in the right direction towards a comprehensive and transparent analysis of the effects of project operations on listed species of concern."
>
> "The AF directly addresses the overarching issue in the CALFED review of the need for an analytical framework, and represents significant progress towards addressing two other overarching issues: need for a conceptual framework and a life cycle approach. The current AF does not completely address all possible aspects of these issues, mostly due to its general

description and the lack of sufficient detail in the final few steps. The pieces are now nearly all present for a comprehensive and defensible analysis of the effects of operations on the listed species of concern."

NMFS' analyses and logic in reaching their jeopardy/no-jeopardy decisions are more defensible with the development of their analytical framework.

Positive 2. Use of up-to-date data and information

The draft BO (and BA) include an enormous amount of scientific information and this information appeared to be, in most cases, up-to-date. Given the complexity of dealing with four species in a complicated system, comprehensive accumulation of the information is admirable.

Positive 3. Explicit treatment of climate change

The draft BO (and BA) addressed a major comment of the review of the 2004 BO by including climate change in their analyses. Climate change was ignored in the 2004 BO, and the subsequent analyses for the draft BO (via the BA) were state-of-the-art and well conceived and executed using available data and models.

Positive 4. Use of peer review

Using peer review as part of the development of the BO is lauded and encouraged. These reviews tend to be, and should be, critical. Often peer review generates additional work for NMFS and others, but generally leads to a final product that is much improved and more defensible.

Positive 5. Ingredients are now there

The Panel strongly believes that, with this draft BO, the ingredients for a very high quality, transparent, and defensible BO are now available. Time constraints may prevent the absolutely best BO from being produced right now, but this BO will also lay a scientifically sound foundation for future assessments of the system and subsequent BOs if reconsultation occurs.

V. Tier 1 Technical Comments

Tier 1 technical comments are listed below. These identify areas and issues that would elevate the science in the draft BO and lead to a more credible and defensible BO. Where appropriate, we also note when comments made in the 2008 review of the analytical framework overlap significantly with these Tier 1 review comments of the draft BO. For space, we do not repeat the comments from the 2008 review of the framework; these can be found in the 2008 review of the framework.

Comment 1. Organizational issues

The length of the BO creates organization issues in how much common information should be repeated in different sections, and the inevitable differences among sections prepared by different lead authors. The BO would benefit from grouping information on common methods into one section. Also, a single section that documents the various data sources used throughout the BO (e.g., CALSIM output) would help streamline the presentation, and enable the reader to easily identify differences in how the information was used and why. Given the complexity of the analysis and the time constraints, it is not surprising that the materials for different Divisions are inconsistently organized. A careful review and editing, and the addition of flowcharts or similar diagrams showing the steps involved with the various analyses, would increase the clarity of the analyses and arguments and make for a more defensible BO. An executive summary should be added to the final version.

This comment relates to the Comment 5 in the 2008 review of the framework entitled “Clarity and presentation.”

Comment 2. Lack of consistency in analyses

The Panel identified a need for analyses to be more consistent throughout the BO. The analyses make use of a wide variety of data sources and model outputs, some of which are specific to the Divisions while others are used by more than one Division. The default should be that commonly used information is processed and presented the same way among Divisions. If differences are necessary, they should be documented and the rationale for the differences briefly stated. A good example is the use of CALSIM output as input to the egg mortality model. It seemed that some Divisions used the same CALSIM output differently in terms of whether temperatures were examined for all 82 years of output, by water year type, or simply averaged to obtain a single value. The same data and model outputs were also presented in tabular and graphical form differently among Divisions. These inconsistencies unnecessarily complicate interpretation of the results.

Maintaining consistency is especially important because the models used in the BO operated on different time scales. Combining output from models that have different temporal and spatial resolution is always a challenge. Thus, clarity in how the outputs were used within and among Divisions is especially important. The operational, temperature, and salmon models are presented in section 2.4.1 of the draft BO. CALSIM operates on a monthly time step and the Delta hydrodynamic model (DSM2) uses a 15 minute time step. Among the temperature models, the Reclamation Temperature Model was monthly, SRWQM used a 6 hour time step, and the Oroville Facilities Water Temperature Model was hourly. Even between the two salmon mortality models, the Reclamation Salmon Mortality Model was daily and the SALMOD was weekly. Although there was a simple schematic illustrating the relationship among the models in Figure 2-11, there was no discussion of how these various temporal discrepancies were resolved among Divisions. This lack of consistency and clarity makes an already complicated analysis more difficult to follow and decipher than is necessary.

An example of a major inconsistency in the analyses, beyond just using the data and model outputs differently among Divisions, was the apparent abandonment of the analytical framework for the Delta Division. The draft BO states that “this section does not follow tightly with the analytical approach described in section 2. However, this section also does not detract from the critical elements in our analysis of effects on the listed species and their critical habitats within the Delta Division” (page 236). There is considerable discussion of the need for an analytical framework in Chapter 2 of the draft BO, including legal and policy aspects. The Delta Division section deviates considerably from this analytical framework. The Panel understands that the Delta is a complex system with no direct effects on some life stages, but the Panel recommends, at a minimum, that the Delta Division section be presented in a manner that clearly shows the consistencies and deviations from the analytical framework.

The Panel is not suggesting that only analyses that can be done consistently among Divisions should be allowed in the BO. There are good reasons for different processing of model outputs, and there are data and information available for some Divisions but not for others. These situations simply need to be documented as to why they are different from the analyses in the other Divisions, or they need to be rectified to a common methodology of analysis and presentation. A more formal and standard approach to the presentation of data, analyses, and model outputs would increase the transparency of the BO. A section that summarizes all of the available data and models, how commonly used data and model outputs were used in each Division, and the data and model outputs only available in a subset of Divisions should be added to the BO. This would also let each Division section focus on results, rather than repeating the methods, often using slightly different descriptions.

3. Unclear definition of baseline

The draft BO states that the environmental baseline includes: “the past and present impacts of all Federal, State, or private actions and other human activities in the action area, the anticipated impacts of all proposed Federal projects in the action area that have already undergone formal or early section 7 consultation, and the impact of State or private actions which are contemporaneous with the consultation in process" (50 CFR 402.02). The environmental baseline provides a reference condition to which we add the effects of operating the proposed action, as required by regulation ("Effects of the action" in 50 CFR 402.02)."

The baseline used in the analyses sections of the draft BO did not match the definition stated earlier in the draft BO and quoted above. Defining baseline when the proposed action involves continuation of existing actions can be problematic. The baseline should be a hypothetical situation in which physical project infrastructure exists, but no project operations are performed except those mandated by prior agreements or those that are not part of the proposed actions. For example, the decline of stream habitat because the dams block gravel recruitment from upstream would be part of baseline, as would providing water to fulfill senior water rights agreements. Modeling in the Delta seemed to use recent conditions rather than an estimate of baseline conditions (i.e., recent conditions minus effects of project-related actions). This definition of baseline was described in words (although too succinctly, in the opinion of the Panel) in the draft BO but never quantified with model results. This can be a serious omission because without a proper baseline, one struggles to make straightforward comparisons of scenarios that differ only by whether proposed project operations are included or not. Much of the draft BO involves comparing results of various simulations, but we had difficulty interpreting results without direct comparisons of the correct baseline to the correct baseline with project operations. Further, NMFS seemed to view the role of climate change as an add-on to baseline, whereas one could argue that climate change should be part of baseline. NMFS must clearly define the baseline used in analyses and explain why this baseline was used rather than the baseline quoted above and seemingly required by the ESA.

Further complicating the idea of baseline was that the baseline used in the BO seemed to vary among the different project components. For example, the evaluation of RBDD seems to consider baseline conditions as those occurring prior to construction of RBDD or CVP, as suggested by the general definition above. Despite recent and proposed improvements in gate operations, the project action contributes to jeopardy because the expected mortality is greater than before RBDD. In contrast, the evaluation in Clear Creek seems to consider baseline conditions to be the more favorable conditions in the recent past, even though they may no longer be possible because of recent court-ordered reductions in the amount of water diverted from the Trinity River into the Sacramento River. The dewatering rate for steelhead redds in the American River seems to be 15% under proposed project operations, but how low is it under baseline conditions? Scouring seems to occur in one of five year under proposed project operations but what is the frequency under baseline conditions? Such inconsistencies may be appropriate, but they should be acknowledged and adequately explained.

This comment relates to Comment 2 in the 2008 review of the framework entitled "Baseline."

4. Need for integration using common measures of survival

The Panel suggests that the quantitative analyses in the draft BO used for salmon can be better synthesized than the current approach of listing individual effects. The use of effects by life stage in the draft BO is not the same thing as a life cycle approach, which involves combining effects over life stages. Also, concatenation of effects (i.e., list making) is not the same thing as integration of effects.

In a simple river system where all fish take a single route between their spawning or rearing habitat and the ocean, the effects of individual life stages on the life cycle can be estimated independent of the effects of other stages. In these "linear" rivers, the ratio of recruits, *R*, to spawners, *S*, can be expressed simply as the product of the life stage survivals. For example, with three stages (rearing habitat, river, and ocean), the recruit per spawner ratio might be expressed as $R/S = c \times l_{rearing} \times l_{river} \times l_{ocean}$, where *c* is fecundity and *l* terms are the life stage survivals in each habitat and are estimated as the numbers exiting/numbers entering each habitat (Figure 1). The effect of an action in any habitat on stock productivity, as measured by *R/S*, equates directly to the change in habitat survival from the action. Therefore, the survival rate terms are expressed in a common measure and so the effect of a fractional change in the rate in any one stage can be directly compared to the effect of a fractional change in another stage.

However, this relationship gets more complicated for the BO because fish can take different routes to the ocean; some enter the ocean via the main river and some divert into the Delta, Yolo Bypass, or other channels. In these more complicated systems, the fraction of the total migration using each route affects the impact of route survival on *R/S*. As shown in Figure 1, if a fraction *f* of the juvenile migrants enters the Delta and the remaining fraction (1− *f*) continues through the river then the equation relating recruits to spawners is $R/S = c \times l_{rearing} \times \left((1-f) \times l_{river} + f \times l_{delta}\right) \times l_{ocean}$. In this case, the effect of survival in the Delta depends on the fraction (f), of fish that enter the Delta. In the extreme case, if few fish enter the Delta, so *f* is close to zero, then actions that alter Delta survival have no appreciable impact on the total population. For Sacramento and San Joaquin fish, the fraction entering the Delta depends on, among other things, the operation barriers and diversions, flow, salinity, import/export ratios, tides, and the fish species. Thus, an assessment of the importance of actions upstream and within the Delta depends on both the changes in survival in the habitats and the routing of fish through the habitats. In actuality, salmon in the San Francisco estuary system have many passage routes so the issue is complex, and yet very important to relating a multitude of actions in different habitats to a population level response.

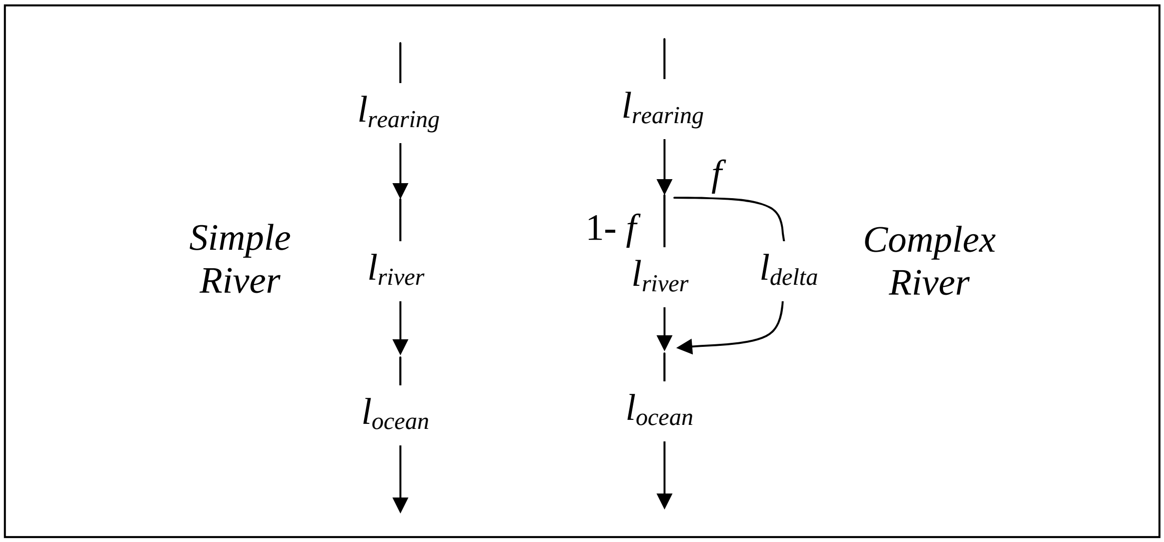

Figure 1. Diagrammatic representation of survival fractions (*l*) of successive fish life stages defined by habitat type for an hypothetical simple (linear) river and for a more complex river in which fish can either go through the river habitat or the delta habitat. *f* is the probability of a fish going through the delta.

Ideally, the best population level measure is the ratio of the number of recruits returning to the spawning habitat divided by the number of spawners producing the recruits. This is known as the spawner-to-recruit Ratio (SRR) and is often expressed in $\log_e$ form: $\log_e(R/S)$. It is the most common measure of population productivity and is the basis of many population viability analyses that are used to assess the risk of extinction. However, because SRR integrates a full life cycle estimate it involves both the freshwater and ocean habitats. If we want to ignore the ocean habitat, an alternative population measure we suggest is the adult-to-smolt ratio (ASR). The ASR is the number of young fish exiting the River/Delta system (e.g., Chipps Island) divided by the number of adult spawners that previously entered the system to produce the young. This segment of the life cycle captures the effects of freshwater environmental conditions and water operations. Included life stages are: adult upstream migration, egg to fry emergence, fry to smolt passage to the Delta, and multiple pathways through the Delta until emerging as survivors at Chipps Island. In concept, the ASR is applicable to salmon, steelhead, and green sturgeon, although it is unlikely that sufficient information exists for full implementation for green sturgeon and steelhead.

The Panel is aware of two modeling systems designed specifically to estimate both SRR and ASR for the Central Valley. The IOS model (Cavallo et al. 2008), is a detailed mechanistic model describing the full life cycle of winter-run (1975-2004) and spring-run (1965-2004) Chinook salmon in the Sacramento River. It contains a Delta routing component applying the analysis developed by Newman and Rice (2002) and Newman (2003). The other modeling system is the *Oncorhynchus* Bayesian Analysis (OBAN), developed by Hendrix (2008). OBAN is a Bayesian statistical model for Sacramento River winter-run Chinook. OBAN incorporates nine life history stages affected by environmental and anthropogenic sources, and includes the Delta routing information identified in Newman (2003). The OBAN model has simpler, and less biologically realistic, equations for mortality than does the IOS model. The IOS model was used to some degree in the BA while the OBAN model was not, presumably because the OBAN model was not available at the time. The other salmon models used in the BA and the draft BO, the Reclamation Mortality and SALMOD, are single life stage models and so do not calculate ASR or SRR. It is noteworthy that the life stages addressed by the Reclamation Mortality and SALMOD models are contained in the IOS model using similar equation forms and parameters, while the OBAN model uses simpler mathematical formulations.

NMFS made a decision to not use the IOS model in the BO based on an internal discussions and review. The Panel understands that the IOS model is relatively new and has not been extensively vetted and published, but all of these types of models are flexible and the Panel wonders if, with sufficient time and with some adjustments and modifications, whether a new version of the IOS could be used. NMFS also hopes to eventually use the OBAN model, but again availability and time constraints are limiting factors. The Panel notes that by using models such as IOS and OBAN (or hybrid versions), and having easy access to modeling expertise, NMFS could have estimated both ASR and SRR measures for salmon in the draft BO.

Estimates of ASR and SRR can be used to inform the jeopardy decision. SRR values can be used in a population viability analysis (Lindley et al. 2007). More likely, ASR would be estimated to simplify the problem (i.e., avoid the ocean phase) and to keep the analysis within the rivers and San Francisco estuary system. One approach is to express the change in productivity as the ratio of ASR values with and without the effects of project operations ($P = ASR_{action}/ASR_{baseline}$). One can then use *P*, if it is less than one, to determine if the population status would shift from its baseline category of risk to a higher risk category. In practice, a series of *P* values would be computed by considering ASR over a range of water year types and without and with climate change. With sufficient information on the likelihood of different water year types, it is possible to combine the *P* values into an overall single *P* value that reflects how water year types might occur in the future.

A critical component of these calculations, and indeed of the BO regardless of the models used, is understanding the effects of project operations on the routing of fish through the Delta. While the IOS or OBAN models represent routing, they both do so in a simplified manner relying on statistical equations. This approach, while powerful, does not reveal the mechanisms and individual effects of project operations. The Panel notes several veins of ongoing and active research for the longer-term, beyond this BO, that should improve our understanding of Delta routing of fish. These include: radio tagging studies (e.g. Perry and Skalski 2008), the Delta Passage Model (Anonymous 2008), and the Regional Salmon Outmigration Study Plan (Burau et al. 2007).

The Panel believes that the ingredients for tailor-made models for use by NMFS for integrative analysis of salmon now exist. Such a model may be impractical for this BO due to time constraints and the need for NMFS to have access to additional modeling expertise. The issue of fish routing in the system is obviously a critical aspect of this BO, including the development of the RPAs, and will form the basis for many activities in the system in the longer-term. Much has been learned and ongoing projects will contribute more. Additional modeling and data collection should be done now to ensure the information is available in the future. The routing issue certainly will be the lynchpin for many future analyses and planning efforts.

This comment relates to Comment 1 in the 2008 review of the framework entitled "The Final Steps." The comment included the statement: "The AF (analytical framework) does not clearly state the details of how the key final steps in the analysis —extending individual risks to population responses and then to species risks— will be accomplished."

5. Inadequate treatment of uncertainty

There are large uncertainties for some aspects of the analyses used in the draft BO, while other aspects are known with more confidence. As part of the BO preparation under ESA, doubts should be resolved in favor of the species. The draft BO does not explicitly deal with the various uncertainties that are associated with the analyses, and it is unclear that uncertainties were factored into how the data and model output were used and how the results were interpreted. Too much reliance on averaging data and model output (e.g., single average of 82-years of CALSIM results) can underplay uncertainty and variation, and obfuscate how protective the results are for the species.

While the BO should consider uncertainty, this need not be done by means of a thorough quantitative uncertainty analysis. Indeed, for a problem as complex as that considered here, such an analysis is not possible given constraints of time and funding, and may not be credible due to large uncertainties. However, a semi-quantitative approach that documents the uncertainties and variation in each of the major steps in the analysis should be included in the BO. We suspect uncertainties and variation were considered in the draft BO, but the thought process needs to be documented and uncertainty and variation should be considered consistently across Divisions and species.

This comment relates to Comment 3 in the 2008 review of the framework entitled "Uncertainty."

6. Need for additional modeling expertise

The Panel strongly recommends that additional and dedicated modeling expertise be made available to the NMFS team preparing the BO. The Panel was impressed with the knowledge of the team members about the system and the species. The heavy reliance of the BO on multiple models has limited the ability of the NMFS team to get rapid and exploratory model results. The Panel encourages NMFS to develop a working level capability within the NMFS team to use such analytical tools, or at a minimum develop relationships with agencies that can provide modeling support in an interactive and rapid response manner. BO analyses based simply on model output provided by a second party, often with a significant time delay in responding, is inferior to an analysis where a model is directly integrated into the process and team members can use the model to explore possibilities and to better understand the behavior of the model. Modeling is a powerful tool and has considerable merit in assessing sensitivity to assumptions and input data, examining uncertainty, and providing a mechanism to explore system or species responses to a wide range of conditions. As such, incorporating modeling expertise into the NMFS BO team would allow them to more fully explore the modeling and to better implement the analytical framework.

7. Inconsistent treatment of climate change

An important issue cited in reviews of the 2004 BO was the lack of consideration of climate change, which was not considered in either the earlier BA or the 2004 BO. Subsequently, climate change scenarios were included in the revised BA, and these were considered by the Panel to be state-of-the-science and to represent “best available” information and practices. Nonetheless, the climate change analysis in the BA does have significant limitations, which should be borne in mind when interpreting the BO. These limitations may limit the generality and validity of the ultimate conclusions of the BO. A detailed review of the climate change analyses reported in the revised BA was presented in the 2008 review of the framework, and is reproduced here as Appendix C. We emphasize that these limitations are typical of those of today’s climate change studies, and therefore do not constitute inadequacies or suggest a need to revise this aspect of the BA. Furthermore, many of these limitations are acknowledged in the BA itself (e.g. in Section 5); we repeat them here to ensure that they receive adequate attention.

The climate change analysis in the BA considers multiple future scenarios; however, because no attempt is made to assess the absolute or relative likelihoods of the different scenarios, this does not constitute an uncertainty analysis. To be specific, one of the four climate change scenarios considered (CALSIM study 9.2, “wetter, less warm”) results in a relatively small effect on salmon. This does not mean, however, that the likelihood of this scenario occurring is 1 in 4, because we do not know the probability of that scenario coming to pass; it is simply 1 of 4 scenarios considered. Hence the consideration of multiple future climate scenarios constitutes a sensitivity study, but not an uncertainty or risk analysis.

The treatment of climate change in the draft BO appears to be less thorough than that in the BA in the sense that climate change information provided in the BA is considered in some, but not all, relevant places in the BO. In particular, much of Chapter 6 of the draft BO is spent making comparisons among future scenarios, none of which consider climate change. When climate change was considered, it was treated somewhat unevenly across Divisions. More often, it seemed that NMFS used the logic that climate change would make results worse. Thus, when current conditions yielded poor conditions for a species, there was no need to rigorously repeat the entire analysis that included climate change because it was assumed that conditions would just be worse. While this argument is likely valid, more explicit inclusion of climate change, and more even treatment of climate change across Divisions, would strengthen the analyses. Furthermore, as noted above, one of the climate change scenarios considered (CALSIM study 9.2) actually had little negative impacts on species. Likely, these inconsistencies do not affect the soundness of the jeopardy/no jeopardy decisions. However, more consistent use of the climate change results would increase our confidence in the conclusions. The thinking should be that climate change is not an add-on to the analysis but is actually part of baseline.

Some examples of situations in which the BO could treat climate change in a more rigorous manner are provided below. Rectifying these with additional information and analyses would strengthen the BO. The analysis of water temperatures downstream from dams was presented in an unclear manner. At the outset (Table 2-3 of the draft BO, "Critical Assumptions"), it was stated that "We added 1-3°F to projected water temperatures to incorporate the effects of future climate change." But later (Section 6.3.1.3), the text refers to Figure 6-10 and Table 6.8 that show results based on CALSIM studies 6, 7, and 8, which do not consider climate change. The draft BO then states that climate change would increase these temperatures by 1-3°F. It is not clear that the draft BO actually did use this simple approach of adding 1-3°F to water temperatures to account for climate change. Such an approach, if it was used, is unnecessarily simple because the climate change scenario results (study 9) are available. The draft BO makes use of studies 6, 7, and 8, but not 9, in other places as well: (a) end of September storage in Shasta Reservoir (Figure 6.8), (b) water temperatures and flow rates in the Clear Creek and Whiskeytown Dam region (Section 6.2), (c) most of the draft BO's discussion of the Delta Division (Section 6.6), except for Delta flows and velocities, (d) winter-run mortality at Ball's Ferry (p. 182 and Figure 6-11). There are other examples. Consistent use of the climate change results would streamline the presentation of the BO and strengthen its conclusions.

This comment relates to Comment 4 in the 2008 review of the framework entitled "Climate change analysis."

8. Specifics about steelhead

The data limitations for steelhead compared to those for salmon present a special challenge for the BO. In addition, steelhead in the Stanislaus River was the only species analyzed for impacts in the entire San Joaquin River basin. Many of the Tier 1 comments apply to the steelhead analyses. Here we list some additional comments that are specific to steelhead; we mostly use the Stanislaus River analysis to illustrate the comments.

The draft BO (page 223) states for the Stanislaus River, "steelhead are likely to have unmet flow needs in 59 percent of years, based on recent history, and may also be adversely affected by operations that target higher flows for salmon than are appropriate for steelhead." Although "recent history" probably refers to operations since 1982, a clearer presentation of what is meant by recent years and how these relate to baseline with and without project operations would be helpful. Also, the evidence for a flow-survival relationship in steelhead is not clearly documented so a brief explanation of IFIM and its focus on habitat rather than survival would be helpful.

The draft BO goes on to state that “[I]f future conditions are drier, warmer or both, instream temperatures will be increased resulting in an adverse reduction of usable spawning, rearing and freshwater migratory habitat, and increased egg mortality of up to 25 percent. These factors will reduce the productivity and abundance of this already diminished population” (page 228). The statement quantifies egg mortality but does not discuss the other aspects of elevated temperature effects on spawning, rearing, and migratory habitats. For example, the freshwater migration corridor for juvenile steelhead is discussed on pages 227-228 and flow regimes deemed protective of steelhead are presented in Table 6-15, but temperature effects on juvenile outmigration (e.g., thermal barriers) is not highlighted. Thus, in addition to the temperature reference point near Orange Blossom Bridge, the BO should assess the need for a seasonal temperature reference point near the mouth of the Stanislaus River. Considerable temperature work has been completed on the Stanislaus River with sub-daily models (CALFED 2006; AD-RMA 2002) that could have been incorporated into the analysis. Finally, the panel suggests that the BO might benefit from a discussion explaining the relationship between VAMP flows and the potential for a temperature barrier in the lower San Joaquin River. How frequently are VAMP flows necessary for successful downstream migration?

Another issue is how the egg mortality responses were derived or applied. The egg mortality information presented in Figure 6-27 is for Stanislaus River fall-run Chinook salmon that spawn in the fall, whereas steelhead spawn in the winter. The thermal regime in the fall period is considerably different than in the winter period in the Stanislaus River, with little thermal stress occurring in the winter. Further, fall-run Chinook salmon are used as a surrogate for smolt emigration (page 235). Additional explanation and data comparing the sensitivities of spawning of the two species to temperature should be presented to support these arguments.

The BO should maintain a clearer separation between baseline and project effects on stream geomorphology. The draft BO reports that lack of gravel recruitment due to the dams has led to incision of the river channel and is part of baseline (although Table 6.17 also seems to indicate it as an effect of project operations). It seems that the BO must deal with a possible conundrum – channel-forming flows are necessary to maintain the quality of spawning and other habitats, but they also move spawning gravels to inhospitable downstream areas. If channel-forming flows, in the absence of gravel recruitment from upstream, had been provided regularly since the dams were built, would sufficient spawning gravel remain for steelhead? Would sufficient gravel have been recruited from bank erosion or tributaries during high flows? Would a sufficient amount of the gravel presently persist until well after 2030? Gravel has been added artificially to the Stanislaus River. Will the supplemented gravel persist or will supplementation continue? The Panel believes that the BO would benefit from additional clarity on this issue.

9. Specifics about green sturgeon

As for comments about steelhead, most Tier 1 comments also apply to green sturgeon. We highlight here aspects of the analyses specific to green sturgeon that deserve further attention.

Green sturgeon clearly present a challenge to cogent analysis because of the limited of life-history and related information. It is easy for the BO analyses to focus on salmon and for green sturgeon to be slighted.

The effects of the Red Bluff Diversion Dam (RBDD) on green sturgeon are clearly presented and documented in the draft BO. The RBDD is located 54 miles below ACID dam and 59 miles below Keswick dam. According to the draft BO, preferred spawning habitat for green sturgeon is located above RBDD. Keswick dam is a permanent structure with fish ladders that cannot be navigated by sturgeons, and the ACID dam blocks upstream migration of sturgeon during April to October. The RBDD directly reduces green sturgeon abundance through mortality of adults, and reduces population productivity by blocking some adults from most favorable spawning habitat (i.e., above RBDD) and by reducing the abundance or quality of spawning and larval habitat for approximately 6 miles above RBDD through the formation of Lake Red Bluff.

The exposure and responses to other stressors besides RBDD and the effects on the early life stages were not as clearly presented and documented for green sturgeon as for the salmonid species. For these other stressors and life stages, the systematic use of the exposure-response tables (similar to Table 9-12) for each stressor and sturgeon life stage would help the presentation. The life stage approach is useful even in data poor situations.

The application of the life stage approach requires careful and consistent definitions of the life stages. The definitions of sturgeon life stages need to be standardized throughout the BO. The draft BO defined four life stages based on developmental stage and habitat use: (1) mature adult (adult females greater than or equal to 13 years of age and males greater than or equal to 9 years of age); (2) larvae and post-larvae less than 10 months of age; (3) juveniles less than or equal to 3 years of age; and (4) coastal migrant (females between 3 and 13 years and males between 3 and 9 years of age). Age alone is a poor criterion for distinguishing “coastal migrant” from adult sturgeon, because age determination is suspect for sturgeons. A range of lengths should be included with the range of ages for each life stage. Furthermore, age and length at maturity criteria presented in the BO should be updated using Van Eenennaam et al. (2006) and Erickson and Webb (2007), and definitions of larvae and juveniles should follow Deng et al. (2002). Metamorphosis from larva to juvenile occurs at 45 days post hatch. Finally, defined life stages must be used consistently throughout the BO. “Coastal migrant” and “subadults” are used for the same life stage, and juvenile and larva seem to be used interchangeably throughout the draft BO.

The analysis in the BO would also benefit by comparing expected flows and temperatures to how fish responded to similar conditions in other systems. One could compare pre- and post-Shasta flows and temperatures to those of other green sturgeon spawning systems (Rogue and Klamath Rivers), and discuss the differences in temperatures and flow patterns and sturgeon responses among systems. For example, one can compare the overall shape of the monthly flow profile of pre-Shasta flows at Bend Bridge (Figure 5-16) to those for the Rogue River shown by Erickson and Webb (2007). Using sturgeon responses in other systems may indicate similarities and differences that would prove useful in supporting green sturgeon analyses in the BO.

Although the draft BO briefly described potential negative impacts of artificially low spring flows for successful sturgeon spawning, the draft BO did not describe potential negative impacts of artificially cool water temperatures (and high flows) during summer months. Summer releases from Shasta Dam ensure a somewhat constant flow and water temperature (target temperature of 56 ^{o}F or 13.3 ^{o}C) in the Sacramento River above Balls Ferry during the summer months. The resulting water temperatures in rearing areas for age-0 juveniles may be suboptimal for growth (Mayfield and Cech 2004). Indeed, summer-rearing temperatures in the Rogue River, Oregon exceed 22 ^{o}C (72 ^{o}F), nearly 9 ^{o}C (16 ^{o}F) warmer than targeted temperatures in the Sacramento River. The impacts of different rearing temperatures among natal river systems may cause different timing of metamorphosis and impact growth rates for larvae and juveniles. The panel feels this point that should be evaluated or discussed. We note that the Delta Regional Ecosystem Restoration Implementation Plan also discussed that the effects of high summer flows (relative to pre-Shasta Dam) on age-0 juveniles were uncertain.

The Panel recognizes the information limitations with green sturgeon. In such situations, a large effort is required to develop a semi-quantitative or quantitative life cycle model, and this would not be possible with the time constraints and high-level modeling expertise needed. However, neither does the Panel want NMFS to dismiss such modeling too quickly, especially since the issue of green sturgeon will persist, new data are becoming available (e.g., Israel 2006; Vogel 2008; Mora et al., submitted), future studies are planned, and additional analyses are required to prepare and evaluate the RPAs. Even though data are sparse for green sturgeon, quantitative estimates of project impacts (e.g., change in survival) at each life stage are possible. A conceptual model is being developed as part of Delta Regional Ecosystem Restoration Implementation Plan (DRERIP) (Israel and Kimley 2008). Beamesderfer et al. (2007) applied life-table models to evaluate impacts of mortality for different life stages of green sturgeon in the Sacramento River system. Heppel (2007) applied a life-cycle model to estimate population-level impacts of mortality at various life stages. These models suggest that, although green sturgeon are data deficient, with a concerted modeling effort, enough life-history information exists to model potential-project impacts to green sturgeon survival for each life stage.

10. Envisioning the future

Eventually, the idea of the future Delta being discussed in efforts such as Delta Risk Management Strategy (California DWR and DFG 2008), the Delta Vision (California Resources Agency 2008), the Bay-Delta Conservation Plan (California Department of Water Resources 2008), and other future-looking studies (Lund et al. 2008), should be incorporated at some level into the interpretation part of the BO (e.g., as part of cumulative impacts). These efforts examine possible changes in the Delta ecosystem into the future which can greatly affect project operations and the population dynamics of the endangered species. The conceptual models being developed as part of the Delta Regional Ecosystem Restoration Implementation Plan (CALFED 2007) should provide a common basis for people to discuss how project operations and other factors can affect species dynamics.

This comment relates to Comment 4 in the 2008 review of the framework entitled "Climate Change Analyses."

11. The RPA process

The Tier 1 comments for the BO also apply to the preparation of the RPAs, and in many cases, the comments become even more important or critical. One can accumulate a number of large effects of project operations on individuals and logically deduce an opinion of jeopardy or no jeopardy. With a jeopardy decision, however, the next step is to develop a suite of actions to avoid jeopardy and promote recovery that are reasonable and prudent (RPAs).

The preparation of the RPAs shifts the questions from jeopardy/no-jeopardy to questions like: Will proposed export and other modifications in the Delta provide the expected benefit for targeted species? Will water withdrawals through a new pumping facility at Red Bluff impose new mortality on downstream migrants that largely offsets the reduced mortality from lifting the dam gates at RBDD? Will remedial actions be effective or will they become expensive projects that show little improvement in species status? How will specific RPAs affect other listed species (e.g., delta smelt) and unlisted species (e.g., fall-run Chinook salmon)?

Tier 1 comments, especially related to defining baseline and lack of quantitative integrative tools, become even more important in addressing these and similar RPA-related questions. The long-term solution to this challenge is targeted research on the critical issues; careful monitoring of responses to implemented actions; and further development of models for generating baseline conditions, downscaling temporally and spatially coarse outputs, and simulating life cycle dynamics. The modeling and monitoring before and after implementation of actions is needed to highlight or test key uncertainties and to increase our understanding of the system in order to facilitate improved management in the future. We believe that lack of quantitative integrative tools will hinder the development of RPAs because NMFS cannot presently quantify the relative contributions of the different project effects to population status nor can NMFS quantitatively determine the potential benefits of specific remedial actions to population recovery. Without this information, it is difficult to rank the many possible remedial actions by their biological effectiveness relative to their fiscal and social costs in order to logically develop an optimal mix of actions.

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Appendix A. Tier 2 comments on the draft BO.

Below we provide additional comments on the BO that are more detailed and specific than the Tier 1 comments listed above. These were collated from the notes of individual Panel members, and were not reviewed by all Panel members. The entire draft BO was not necessarily reviewed evenly for Tier 2 comments and this list is not comprehensive. Some of these are more significant than others, and we suggest that NMFS consider these comments when they revise the draft BO. For the sake of brevity, only in a few instances do we comment on grammatical or spelling errors.

We have organized these comments into the categories of general and specific. The specific comments relate to the original document by page, paragraph (partial paragraphs at the top of a page are scored as new paragraphs), and sentence. For example, 5, 1, 1 refers to page five, the first paragraph and the first sentence of that paragraph where the BO states that "the production of fall-run from the Trinity River Hatchery, will be analyzed in this consultation."

General Comments

1. On the American River there appears to be little hope for temperature control as demand will increase from a maximum of about 300,000 acre-feet to approximately 800,000 acre-feet at build out. Folsom Reservoir has just under a 1,000,000 acre-feet of storage. Cold water management will be largely infeasible under this higher level of demand, a condition that would be exacerbated with climate change. This seems like a considerable challenge for steelhead.

2. The U.S. Bureau of Reclamation provides useful information in the BA on climate change effects on downstream river temperatures, but the BO does not consider this work in detail. In most cases, analyses in the BO simply increased water temperatures by 1-3^{o}F. It is unclear if this temperature increase accounts for cold water storage in upstream reservoirs. A more detailed analysis – using available models – could readily be completed for Shasta, Oroville, Folsom, and New Melones reservoirs.

3. Is climate change detrimental to all life stages of listed species? Has there been research or study regarding increased water temperature on increased primary production and potentially increased ration levels for juvenile rearing? That is, can juvenile fish perform sufficiently in warmer waters with higher ration?

4. Figure 6-15 (Page 199) and Figure 6-27 (page 219) present conceptual models for project related stressors of steelhead and habitat on the American and Stanislaus Rivers, respectively. These two diagrams are not consistent even though many of the processes at work in one system are also present in the other. Further, this is more of a relational diagram than a conceptual model. Certain boxes have a response description (e.g., passage blocked…, loss of natural river function), while others are simply state variables (e.g., water temperature, or migration flows). Recommend

reviewing diagrams, ensure proper relationships are represented, and provide consistent treatment among rivers (e.g., there is no such diagram on Clear Creek or the Sacramento River).

5. The following are conflicting statements:
    - "If future conditions are drier, warmer or a combination of both, temperature caused egg mortality will increase by 5 percent in wet years to 19 percent in critically dry years" (page 224).
    - "If future conditions are drier, warmer or both, instream temperatures will be increased resulting in an adverse reduction of usable spawning, rearing and freshwater migratory habitat, and increased egg mortality of up to 25 percent" (page 228)

6. The cumulative impacts lack a quantification of the cumulative effects. Even if conceptualization and qualitative arguments suggest negative impacts, there is typically a significance threshold (a clear definition of "significant impact) and an analysis that identifies if the cumulative effect is significant or not.

7. Other than a brief treatment on page 338, the BO does not appear to address the potential role of strays, particularly the potential value of strays in repopulating areas where very depressed populations exist.

8. Many sections of the BO that deal with green sturgeon are incomplete or missing (see, for example, page 100). Other important sections are quite rough.

9. Terminology regarding green sturgeon needs to be better defined and consistent. For example, green sturgeon life stages were poorly defined and inconsistently applied. The life stages were categorized by age only (not length). Ages for green sturgeon are suspect. The BO defined one life stage (Coastal Migrant) as males 3 – 9 yrs and females 3 – 13 years, whereas the term sub-adult is applied throughout the text without being defined.

10. The organization of sections in the BO is not always consistent and is at times confusing. An example of inconsistent organization within sections is illustrated on pages 190 – 191 (6.3.2. Assess the Risk to the Individuals) relative to pages 193 – 194 (6.3.3. Assess the Risk to the Population). Section 6.3.3 is well organized and easy to follow; risks are shown by species. Section 6.3.2 is poorly organized and difficult to follow; it uses a bullet list of risks with species mentioned for each risk, which is at times confusing. There also are many sections in the BO where impacts on sturgeons are "tucked" into sections on salmonids.

11. Assumptions are inconsistently stated (e.g., adult sturgeon emigration dates from the Sacramento River; lethal water temperature limits for sturgeon). More details are provided in the specific comments below.

12. A better understanding of the behavior, density, and movement patterns of juvenile green sturgeon in the Delta is needed to understand project implications, especially entrainment.

13. Fishing mortality (harvest and release) for green sturgeon in rivers, bays, and the ocean needs to be discussed and presented in more detail in the BO.

Specific Comments

1. ii, Table of contents: everything between page 39 and page 103 is missing.
2. 5, 1, 1: The BO says, "… the production of fall-run from Trinity River Hatchery, will be analyzed in this consultation;" however, a search of the document in Adobe Reader shows no subsequent occurrences of "Trinity River Hatchery." Furthermore, the panel does not recall any further treatment. Is the BO in error here?
3. 14, Table 2-1, Step A: "… direct of indirect …" should be "direct <u>or</u> indirect"
4. 18, 1, 2: add word -- "identify the probable risks <u>that</u>"
5. 18, 1, 3: make singular: individual**<u>s</u>**
6. 20, 1, 6: The BO says, "Absolute changes in the number of smolts that survive their migration to the ocean have the largest impact on Chinook salmon population growth rate (Wilson 2003) followed by the number of alevins that survive to fry stage." This statement seems to downplay pre-spawning mortality which has an effect equal to smolt mortality (e.g., because limiting density-dependant mortality typically occurs before ocean entrance, reducing survival by 50% at either life stage will have the same effect on subsequent egg deposition). The panel mentions this primarily because we recall seeing little consideration of temperature effects on pre-spawning mortality. NMFS should consider whether they have given this subject adequate treatment in all relevant areas.
7. 20, 2, 1: In this sentence, the BO states that population health ("sturgeon species growth rate") is "most sensitive to young-of-the-year and juvenile survival and less sensitive to annual adult fecundity and survival (Caswell 2001)". This is not true when assessing anthropogenic impacts to long-live fishes, such as sturgeons. Should cite Heppel (2007) who, by using elasticity analysis, showed that health of green sturgeon populations is most sensitive to survival of adults and subadults. Heppel (2007) states "a dramatic increase in the survival of young of the year sturgeon or annual egg production is required to compensate for relatively low levels of fishing mortality……..Elasticities were highest for adults (50% mature) and subadults (> 107 cm TL) than for other life stages."
8. 20, 2, 1-2: The references of Caswell (2001) and Gross et al. (2002) are not listed in the "Literature Cited" section.
9. 28, 2, 1-2: use spell checker – u**<u>r</u>**ilized; through**<u>t</u>**
10. 33, 1, 2: Should this be, "By regulation, the baseline includes the impacts of past, present, and future actions [excepting the proposed action (OCAP)] on the species and critical habitat?"
11. 43, 4, 1-2: Supporting references are missing from these sentences describing the adverse environmental conditions created by Delta water export operations on

outmigrating juvenile salmonids. It should be made clear whether these are documented effects, or just assumptions.

12. 44, 3, 1-5: This entire paragraph describing the potential impacts of the ACID dam to sturgeon is inserted into a section on salmonids (Section 4.2.1.2.2 Factors Responsible for the Current Status of Winter-Run, Spring-Run, and CV Steelhead). This paragraph does not belong in this section. It creates a fragmented document for sturgeon, making it difficult track impacts to sturgeon. As noted in the general comments above, discussions about sturgeon are inappropriately "tucked" into sections on salmonids.
13. 61, 1, 1: This seems to be the first reference to EWA; spell it out the first time.
14. 67, 5, 5: Fix this sentence after the second comma – "In addition, data used for Lindley *et al*. (2007) did not include the significant decline in escapement numbers in 2007 and 2008, which are reflected in the population size and population decline, nor the current drought conditions."
15. 92, 2-3: Provide references in these paragraphs so that the statements do not appear as merely assumptions.
16. 92, 5, 3: Change reference from (NMFS 2005) to (Erickson and Hightower 2007).
17. 92, 5, 4: Sturgeon "subadults" are referred to in this sentence but the term is not defined anywhere in the BO. The BO uses the term "coastal migrant" elsewhere for this life stage. Consistency is needed.
18. 92, 5, 4: Change Moser and Lindley (2006) to Moser and Lindley (2007).
19. 93, 1, 1: Substantial concentrations of adult and "coastal migrant" green sturgeon also are present in Winchester Bay, Oregon (Erickson 2006)
20. 93, 2, 2: The BO says, "The life cycle of Southern DPS of green sturgeon can be broken into four distinct phases based on developmental stage and habitat use: (1) adult females greater than or equal to 13 years of age and males greater than or equal to 9 years of age; (2) larvae and post-larvae less than 10 months of age; (3) juveniles less than or equal to 3 years of age; and (4) coastal migrant females between 3 and 13 years, and males between 3 and 9 years of age (Nakamoto *et al*. 1995, McLain 2006).
    - These life-stage definitions need to be improved. Use of life-stage terms need to be applied more consistently throughout the text.
    - Descriptors for life stages should also include length ranges for two reasons. First, aging is not accurate for green sturgeon, especially for coastal migrants and adults. Second, the lengths assist with the interpretation of certain figures and tables (e.g., size of sturgeon caught in screw traps).
    - Although the term "coastal migrant" is defined here, the BO uses the term "subadult" most frequently (10x versus 4x) for this life stage. This should be consistent as noted in another comment included above.
    - Use Deng et al. (2002) for defining larvae and juveniles.
    - Metamorphosis from larvae to juvenile occurs at ~ day 45 (Deng et al. 2002). The term post-larvae (instead of juvenile) is confusing here. Also, juveniles (~ day 45 – 10 months) exhibit much different behavior than larvae, and should not be grouped together. Finally, the term "post-larvae" was referred to only once in the text.

- Descriptions of juveniles, coastal migrants, and adults were from Nakamoto et al., (1995). Adults should be defined using more recent references, such as Van Eenennaam et al. (2006) and Erickson and Webb (2007).
- McLain (2006) is missing from the reference list of literature cited.

21. 93, 3, 1: Add Adams et al. (2007) to the list of manuscripts describing historical spawning ranges.
22. 93, 5, 5: The BO says, "Both white and green sturgeon likely utilized the San Joaquin River basin for spawning prior to the onset of European influence, based on past use of the region by populations of spring-run and CV steelhead." This sentence should be removed from the BO. It cannot be assumed that sturgeon used the San Joaquin River because steelhead utilized it. We suggest NMFS use Mora et al. (submitted) to help with a discussion of likely historical spawning areas.
23. 93, 6, 2-4: We suggest that NMFS also cite Lindley et al. (2008) and Erickson and Hightower (2007) regarding ocean migration for green sturgeon.
24. 93, 6, 3: The BO says, "It appears North American green sturgeon are migrating considerable distances up the Pacific Coast into other estuaries, particularly the Columbia River estuary." This sentence is misleading. It suggests that the Columbia River estuary is the primary estuary utilized by green sturgeon. The BO listed other estuaries utilized by subadults and adults for feeding earlier. These should be listed here as well. The Columbia River, Willapa Bay, Grays Harbor (Israel 2007; Moser and Lindley 2007,) and Charleston Bay (Erickson 2006) are all extensively utilized by green sturgeon. Most of this information was shown in the first paragraph on page 93. We suggest combining this information into a single paragraph on long-range movements.
25. 94, 2, 8: The BO says, "The documented presence of adults in the Sacramento River during the spring and summer months, and the presence of larval green sturgeon in late summer in the lower Sacramento River, indicate spawning occurrence, and it appears adult green sturgeon could utilize a variety of freshwater and brackish habitats for up to 9 months of the year (Beamesderfer 2006)." This is inaccurate and the reference should be updated. Beamesderfer (2006) is a personal communication. Adults are documented in the Sacramento River during spring, summer, and fall months (as late as December) (Heublein, 2006; Vogel, 2008). Larvae also are present during late spring and early summer months. We suggest adding references, such as Brown (2007), and state the source of data (e.g., screw traps), etc.
26. 94, 4, 1: The BO says, "Adults of the Southern DPS of green sturgeon begin their upstream spawning migrations into the San Francisco Bay by at least March, reach Knights Landing during April, and spawn between March and July (Heublein *et al*. 2006)". Is this sentence all based on a presentation? This reference is not appropriate for this background information. There are better references that should be used. For example: Use Heublein's thesis (2006) instead of his presentation; add that Vogel (2008) showed females with eggs in July; to demonstrate spawning months, add references demonstrating egg (Brown, 2007) and larvae catches. Most of these references are shown later in the paragraph. The BO should do better with placement of references in the text.
27. 94, 4, 5: The BO says, "They are believed to reach sexual maturity only after several years of growth (10 to 15 years), and spawn every 3 to 5 years, based on sympatric

white sturgeon sexual maturity (CDFG 2002)." Length and/or age at maturity have been described by (Erickson and Webb, 2007; Van Eenennaam et al., 2006; and Nakamoto et al, 1995). We suggest using these references and correct age at maturity (which is not 10 to 15 years). Add length as well, which is different for males and females. Also, we know green sturgeon return to natal rivers to spawn at least every 2 to 4 years (Erickson and Webb 2007; Erickson unpublished data), possibly to every 5 years.

28. 94, 4, 9: The BO says, "According to Heublein (2006), all adults leave the Sacramento River prior to September 1." This is incorrect. Heublein (2006) showed green sturgeon emigrating from the Sacramento River as late as December. Vogel (2008) also showed green sturgeon emigration through November.
29. 95, table 4-8: Some adult green sturgeon are in the upper river as late as December (the table shows through November). If adults leave the river and enter the ocean ever year (as they do), then this table should also show that adults are present in the Delta through December. Also, haven't larvae been collected in April? NMFS should check references associated with this table and update per other comments above.
30. 95, 1, 1-8: NMFS should study the references by Deng et al (2002) and Van Eenennaam et al. (2005) carefully. This entire section should be made more consistent and accurate. Early development staging, thermal optimum, etc. for green sturgeon are extremely important for this Opinion. This section should be carefully written. Stages should be clearly defined before making adjustments to this section. For example: the upper limit for thermal optima for green sturgeon embryos are 17-18 degrees C (Van Eenennaam et al., 2005). In sentence 6, juveniles are mentioned as first appearing in screw traps at 24 to 31 mm FL. Were these juveniles or larvae? The life-stage definitions make this unclear. We suggest following the definitions of Deng et al. (2002): metamorphosis occurs at day 45 and juveniles at metamorphosis are 62.5 – 94.4 mm TL.
31. 96, 1, 8: The BO says, "Smallmouth bass (*Micropterus dolmoides*) have been recorded on the Rogue River preying on juvenile green sturgeon...." The panel believes this was the Umpqua River and should be cited (Ruth Farr, ODFW, Personal communication?).
32. 97, 1, 7: The BO says, "Recent spawning population estimates using sibling-based genetics by Israel (2006b) indicate spawning populations of 32 spawners in 2002, 64 in 2003, 44 in 2004, 92 in 2005, and 124 in 2006 above RBDD (with an average of 71)." Are these numbers females only, or males and females? This should be made clear in the text.
33. 97, 2, 1: The BO says, "Based on the length and estimated age of post-larvae captured at RBDD (approximately 2 weeks of age) and GCID (downstream, approximately 3 weeks of age), it appears the majority of Southern DPS of green sturgeon are spawning above RBDD." It would help to give the length range in the text. Are these larvae or juveniles? We suggest the term "post-larvae" be dropped. If these fish are two weeks old, then they are probably larvae. Overall, this demonstrates the need to more carefully define life stages, and include both lengths and ages.

34. 98, table 4-9: If possible, it would help to report the size ranges for the juveniles found in the salvage at the fish facilities in the figure legend. These data may be available from the BA (Figure 8-10?). This is important because juveniles range from 45 days to 3 years.
35. 99, figures 4-5 and 4-6: It would help to show the length range of salvaged fish in the figure legends.
36. 100, 2, 1: The BO says, "Substrate suitable for egg deposition and development (*e.g.*, bedrock sills and shelves, cobble and gravel, or hard clean sand, with interstices or irregular surfaces to "collect" eggs and provide protection from predators..." It has been reported that 'hard clean sand" is a suitable substrate for sturgeon egg deposition. The panel's sturgeon expert (Erickson), however, has never seen this demonstrated. It is unlikely that sand is a suitable substrate for green sturgeon eggs. There is nothing for the adhesive eggs to adhere to. The eggs would also be vulnerable to predators (e.g., sculpins). NMFS might want to check with others, but we recommend dropping "hard clean sand" from this list of suitable substrates for egg deposition.
37. 101, 4, 1-2: "According to Heublein (2006), all adults leave the Sacramento River prior to September 1. Those that migrate upstream past RBDD prior to May 15 would not be able to migrate back downstream until after the RBDD gates are pulled on September 15." These sentences are not accurate. Heublien (2006) and Vogel (2008) demonstrate green sturgeon emigrate from summer months through December. Typically, peak emigration is during fall months.
38. 120, table 5-2: July is probably not a typical spawning month for green sturgeon, even though a few females have been found with ripe eggs in July. In the table, egg incubation months are shown as Apr – Jun, even though spawning months are shown as Mar – Jul. If green sturgeons do successfully spawn from March through July, then egg incubation months should probably include March through July months. Also, what does the table column heading "Juvenile Emigration" mean? Emigration from where to where? Is this just downstream migration? Juvenile emigration from freshwater into the Delta? This is unclear.
39. 125, Section 5.2.4 Southern DPS of Green Sturgeon**:** There are almost no references included within this entire section. References should be cited.
40. 125, 2, 3-6: The BO says, "Adults prefer deep holes at the mouths of tributary streams, where they spawn and rest on the bottom. After spawning, the adults hold over in the upper Sacramento River between RBDD and GCID until November (Klimley 2007). This type of behavior has been observed in spawning populations in other rivers. Post-spawn adults migrate downstream with the first significant increase in flows and turbidity following storm events." These sentences are an example of a lack of references. The only citation is not the most appropriate one and is not in the reference list. Where did "mouths of tributary streams" come from? We have never seen this as a preference for holding or spawning. Sturgeons also spawn and hold in reaches where there are no tributaries. Spawning and holding habitats are different. Sturgeon also may not "rest" on the bottom.
41. 125, 3, 1: The BO says, "During the spring and summer, the main processes influencing green sturgeon are in the freshwater environment (Figure 5-13)." This sentence is misleading. Juveniles and adults are also in the freshwater during fall

months. In fact, juveniles are in freshwater during winter. In addition, the majority of green sturgeons (age 4 and above) are in marine waters during spring, summer, fall, and winter. Note that because spawning periodicity is 2 to 4 (or 5) years, most adults are in the ocean during each spawning season.

42. 125, figure 5-13: This green sturgeon conceptual life history figure is confusing and incomplete. There is not enough time to create a better figure. However, one eventually needs to be made.
43. 127, 1, 3-4: The BO says, "Juvenile green sturgeons migrate downstream and feed mainly at night. Larvae and young-of-the-year are small enough to be entrained in water diversions, although their benthic behavior likely limits this impact." The "young-of-the-year" life stage referred to in this sentence was not defined. Also, readers should be reminded that at the size/age green sturgeon become benthic, the larvae still feed in the water column at night.
44. 127, figures 5-14 and 5-15: It would help to show sturgeon size ranges in the legends for these figures.
45. 128, 1, 1-2: The BO says, "The historical pre-Shasta Dam hydrograph shows a much different flow pattern than the current hydrograph (figure 5-16). Average monthly flows decreased 25 percent in the winter/spring (16,000 cfs to 12,000 cfs), and increased 58 percent during the summer/fall (5,000 cfs to 12,000 cfs)." Note that the shape of the pre-Shasta flows emulates the shape of flows in the Rogue River (one of three spawning rivers that, is not <u>as</u> affected by regulated flows as the Sacramento River.
46. 128, 2, 1: The BO says, "The current hydrograph shows reduced springtime flows and much higher summer flows. This pattern is necessary to support winter-run and green sturgeon spawning…" We are not convinced that lower spring flows is necessary to support green sturgeon spawning. The BO briefly mentions, later in the document, that lower spring flows could negatively impact green sturgeon spawning. The impact of higher summer flows on larvae and juvenile growth and development is unknown.
47. 144, table 5-25: This table shows NO adult green sturgeon in the Delta during September, October, and November. This is inconsistent with adult emigration dates (as late as November and December) and recreational catch records.
48. 145, 4, 5: The BO says, "Radio-tagged adult green sturgeon have been tracked moving downstream from the GCID aggregation site past Knights Landing in November and December, following their upstream migrations the previous spring." Movement downstream is not only in November and December, but in earlier months.
49. 146, 2, 1-5: Steve Lindley is lead author on a recently submitted manuscript describing movements among bays. Contact him for updated information that can be cited.
50. 146, 2, 5: The BO says, "Sub-adults are believed to reside year round in these estuaries prior to moving offshore as adults." This statement is likely false, especially if "sub-adults" are defined as "coastal migrants." Little telemetry work has been conducted with sub-adults.
51. 149, 4, 4-5: The BO says, "The draft report on the 2007 CDFG Sturgeon Fishing Report Card (CDFG 2008) indicates that 311 green sturgeon were reported caught by

sport anglers during 2007. Green sturgeon were caught in both the mainstem of the San Joaquin River between Sherman Island and Stockton (48 fish) and between Rio Vista and Chipps Island (62 fish), with most catches occurring in the fall, although fish were caught throughout the year in both reaches." Within the Sacramento River, most fish were caught by angling between Colusa and Red Bluff. This was not shown here.

52. 159, 1, 3: The BO says, "Downstream of Freeport, small natural channels branch off of the main channel of the Sacramento River and carry a small proportion of the river's discharge through several farmed Delta Islands." This sentence is not accurate. Sutter and Steamboat Sloughs branch off the main channel of the Sacramento River and carry a large proportion of the Sacramento River flow that passes Freeport (approximately 40 percent of the flow when the DCC gates are open and 45 or 50 percent of the flow when the DCC gates are closed). Figure 1 shows the approximate flow splits for the channels in the north Delta as a percentage of the flows at the Sacramento River at Freeport. The numbers in the figure are only approximate. The latest equations for predicting these flow splits are available in Appendix A of the North Delta Regional Salmon Outmigration Study Plan (see: http://www.science.calwater.ca.gov/pdf/workshops/workshop_outmigration_reg_study_plan_011608.pdf). It is important to note that flows into Sutter and Steamboat Sloughs increase when the DCC gates are closed. If predation in these sloughs are high, this may counteract some of the benefits assumed for outmigrating salmon related to the closing of the DCC gates.

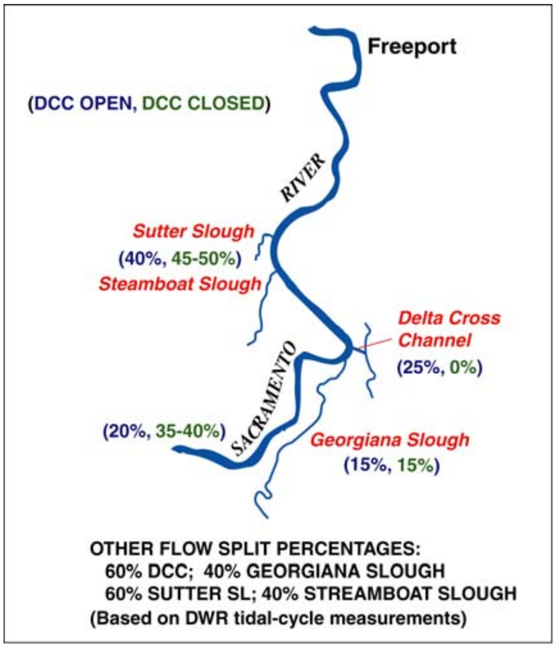

Figure 1. Flow split percentages in the north Delta based on flow in the Sacramento River at Freeport for conditions with the Delta Cross Channel (DCC) gates open and closed.

53. 159, 1, 4: The BO says, "Elk Slough branches off the mainstem near the town of Clarksburg and flows in a southwesterly direction, separating Merritt Island from Prospect Island." NMFS should correct this sentence even though it is a minor issue. Elk Slough does not branch off the Sacramento River just downstream of the Clarksburg Bend. Although there are culverts connecting Elk Slough to the Sacramento River that are opened on occasion, it mostly functions as a dead-end channel.

54. 161, 3, 2: We suggest leaving out the word "strongly." When Delta outflow is low, flows on the San Joaquin River in the western Delta (at Jersey Point, for example) are usually not "strongly positive," and actually can frequently be negative. In fact, at low-flow/high export conditions in summer and fall it is common for the entire measured delta outflow (not the DAYFLOW estimate) to become negative during

part of the 14-day spring-neap tidal cycle as the Delta goes through a filling and draining cycle.

55. 161, 3, 3: We suggest re-wording this sentence to: "However, under certain conditions, such as low Delta outflow, high pumping rates and negative QWEST, particle-tracking models have demonstrated that a significant portion of the water in the west Delta can be drawn to the pumps over a period of from 10 to 30 days."
56. 161, 3, 5: This sentence should also mention that water enters the San Joaquin River system (from the Sacramento River) through Sherman Lake. The net flows through Sherman Lake are significant. They are approximately equal to the net flows through Three Mile Slough. (Note: Sherman Lake flows were measured in a joint USGS/DWR study during fall 1998. A short paper is available on this study from 2002.)
57. 168, 3, 1-8: Why does this paragraph (section 6.2.3) assess only the risk to spring-run salmon and the following paragraph (section 6.2.4 on page 169) assess only the risk to steelhead populations? This is inconsistent.
58. 168, 3, 6-8: Do you mean to predict <u>no</u> reproductive success for dry years, or <u>reduced</u> success? You imply the former. Isn't this spatial effect from Whiskeytown Dam part of the baseline? The last sentence seems to be dragging it into the OCAP.
59. 169, 1, 4: The BO says, "Modeling shows that flows … " Give a source (BA, table, other document?). End this paragraph with a summary sentence – is risk <u>significantly</u> increased?
60. 170, 3, 1: The BO says, "Based on recent RBDD ladder counts the percentage of adults encountering delays would be approximately 15 percent for winter-run, 70 percent of spring-run, 40 percent for CV steelhead, and 35 percent for green sturgeon (TCCA 2008 Appendix B1, figure 6-5)". Sturgeon don't swim up the ladders, so this percentage was based on something else. How was the 35 percent calculated? Please show in text.
61. 171, figure 6-5: Run timing by month at RBDD should also show green sturgeon moving back downstream. This figure only shows up-stream migration. Otherwise, the figure legend should indicate this represents only the upstream run.
62. 172, 1, 1: The BO says, "Green sturgeon adults migrate upstream from March through July, with the peak of spawning occurring from April through June (September 8, 2008, FR 52084)." This sentence is inconsistent with figure 6-5, which shows no sturgeon migrating upstream in July.
63. 172, 2, 10: The BO says, "Regardless of whether the opening is large enough to avoid impingement (since adults can reach a length of 5-6 feet they have to be perfectly lined up to pass through a 12 inch opening) the gates would still injury fish due to the turbulence after they pass through." Adult green sturgeon are caught at lengths larger than 5 – 7+ feet, not 5-6 feet. Also, change "injury" to "injure."
64. 174, Table 6-3: Footnote 2 is used in the table but missing. Spell out "E.A." in the caption for the table.
65. 183, figure at top of page: This figure is not numbered correctly.
66. 183, 1, 2: This sentence is not accurate. Ambient air temperatures alone do not cool the river, and most likely have only a modest effect. Overall meteorological conditions control the complete heat budget at the air water interface (e.g., short-wave

(solar) radiation, long-wave radiation (+/-), conduction, and latent heat transfer, with bed conduction probably playing a minor role also.

67. 184, figure at top of page: This figure is either numbered incorrectly (it is numbered as figure 11-41) or misplaced in the document.
68. 184, 1, 1: Can NMFS provide any temperature tolerance data for both species to support using late-fall salmon as a surrogate for steelhead?
69. 185, 1, 1-3: These observations or conjectures seem to be baseline effects, not OCAP effects. Furthermore, these statements should be identified as inferred or documented (give references)
70. 185, 2, 6-7: Again, aren't these low survivals largely baseline effects? What are the baseline conditions?
71. 185, 3, 1: The BO says, "Based on figure 6-4 and table 6-9, water temperatures are suitable for green sturgeon spawning and rearing as far downstream as Hamilton City, which is also the location of the GCID diversion." Is this sentence meant to refer to figure 6-14 on page 187? There is also a figure 6-14 on page 186. This should be fixed. Page 186 should be considered a table.
72. 185, table 6-9: There are numerous problems with this table. The reference to NMFS (2006) in the caption is not included in the reference list. The table caption also references Mayfield and Cech (2004), but the table did not appear in that paper, although it could be based in part on interpreting results from that study. Mayfield and Cech (2004) only studied juveniles. Van Eenennaam et al. (2005) show temperature impacts on incubation. The table needs to be clear about where adult immigration "lethal" temperature limits come from. The table also needs to define "suitable" and "tolerable" temperatures. Mayfield and Cech (2004) show that temperatures up to 66 ^{o}F are optimal for juveniles. This table suggests that 66 F is above tolerable limits. In general, lab results should be discussed with ex-situ observations. For example, the average water temperature in the Rogue River during the month of July is 23 C (= 73.4 F), when juveniles and adults are present (possibly larvae). Lethal and/or "intolerable" limits for these stages in the Rogue River would be exceeded. Because interpretation of project impacts rely on this table (and definitions of life stages), it is imperative that this table is corrected and results discussed (in-situ versus ex-situ).
73. 189, 2, 1: NMFS probably should explain their logic here more clearly. If the panel understands this correctly, it is previously established (through comparisons with baseline conditions) that OCAP poses jeopardy without considering climate change. Here NMFS simply points out that things would be even worse although baseline conditions (i.e., with project but without OCAP) aren't considered explicitly. The argument probably is okay, but somewhat indirect and should benefit from a more detailed definition of baseline conditions.
74. 191, 1st bullet, 1-3: Although these sentences on pulse flows in April and May are written for salmon, it is possible they could also apply to sturgeon.
75. 191, 3rd bullet, 1: delete "cause" from "cause reduce"
76. 191, last bullet, 1-2: The BO says, "Operations of ACID and RBDD will block or delay adult winter-run, spring-run, CVsteelhead, and Southern DPS of green sturgeon. Adults will either spawn below these diversions or experience a reduction

in fecundity from delays." Should this be reduction in fecundity or spawning success?

77. 192, 1st sentence at top of page: The BO says, "Adults that spawn below RBDD would be exposed to water temperatures >60°F, therefore, these individuals would experience a complete loss of eggs and pre-emergent fry." This sentence is not supported by the current table 6-9, or by Van Eenennaam et al. (2005). If reference is made to figure 6-14, although water temperatures may be suboptimal to Hamilton City if increased by 1-3°F (due to climate change), there would likely be spawning and some spawning success at higher temperatures to Hamilton City. This illustrates the importance of improving the accuracy and clarity of table 6-9. We recommend that NMFS contact Joel Van Eenennaam to assist with the preparation of table 6-9.
78. 192, 7th bullet, 2: The BO says, "These losses would be significant for spring-run and winter-run, but not for steelhead." Where is the reader told that an increase in mortality of 2% isn't significant for a threatened species but 10% is? The panel doesn't necessarily argue with your statement, but the logic should be explained or cited. (Would 5% increase be significant? 4%?)
79. 193, 1st bullet, 1: This statement probably is true but NMFS should provide some support for it or label it as opinion.
80. 193, 2nd bullet, 1-2: Are these mortalities over the entire period in freshwater? -- i.e., over what life stages? Including pre-spawning mortality? It may be sufficient to do nothing more than cite the appropriate tables in BO or BA.
81. 193, 2nd bullet, 3: The BO says, "For green sturgeon climate change would limit spawning to the upper most reaches of the Sacramento River where habitat is blocked by ACID (5 miles) and RBDD (60 miles)." This statement is not necessarily true. See comments above regarding figure 6-14 and table 6-9. Table 6-9 needs to be improved before predictions such as this can be made. Also, warmer-water discussions should include current spawning and rearing temperatures in other river systems (such as the Klamath and Rogue).
82. 193, 4th bullet, 1: Isn't hybridization/introgression of spring-run with fall-run a consequence of dams blocking the river, hence part of the baseline? If so, it shouldn't be considered as an effect of OCAP. The BO should provide a good understanding and portrayal of baseline.
83. 194, 1st (full) bullet, 1-2: The BO says, "Project operations can negatively impact green sturgeon in the Sacramento River by restricting seasonal spring flows necessary as triggers for spawning and juvenile outmigration. Seasonal flows during the spawning migration seem to be correlated with the number of adults spawning in the Sacramento River (Israel 2008)." Erickson and Webb (2007) show spawning migration distance by green sturgeon in the Rogue River is positively correlated with spring flows. Others have shown that spawning success for white sturgeon was correlated with higher spring flows.
84. 195, 1, 1-7: But is this affected population viable? Here is one of the places where the baseline or its rationale is unclear. Is the baseline best possible conditions given the dams, and "senior" water rights and agreements? The BO should provide a good understanding and portrayal of baseline.
85. 195, 2, 8: The BO says, "…truncates the juvenile emigration timing by 2-3 months." This may be true for salmon (although supporting documentation would be good) but

is it true for steelhead? Without data indicating otherwise, we expect most steelhead to rear in "upstream areas" until they reach their critical size for smolting (~15-16 cm) and then to move rapidly to the estuary and ocean based at least partially on photoperiod. Once they start migrating rapidly, the difference in transit time would be expected to be weeks not months. What data are available to address this question?

86. 195, 2, 9: Provide a reference (Cramer? McEwan?) or explain the logic.
87. 195, 3, 2: Steelhead also feed on salmon eggs aggressively – presumably they return in the fall rather than the winter to take advantage of the bonanza of salmon eggs! Isn't this overlap of spawning areas a project effect, not OCAP? It seems to be inappropriately treated as the latter here. As we mention in many instances above, the BO must provide a good understanding and portrayal of the baseline.
88. 197, 2, 1: "RBDD backs up water on the Sacramento River to form Lake Red Bluff during the summer months when juvenile winter-run are migrating downstream. This action adversely modifies 6 miles (or 15 miles of shoreline) of critical habitat for winter-run, spring-run and CV steelhead (RBDD EIS/EIR 2007)." This action also adversely modifies rearing habitat for green sturgeon larvae and juveniles. This is atypical habitat for these green sturgeon life stages.
89. 198 (general comments on this page): We suggest that NMFS revisit and rewrite impacts of temperature on green sturgeon after making adjustments to table 6-9. Also, how many holding pools are above or below RBDD? How many holding pools do green sturgeon need?
90. 202, footnote 6: The panel does not believe that "it is reasonable to assume that releases from Nimbus Dam likely contribute to the entrainment of juvenile salmonids in the Delta, including American River steelhead." This is extrapolating the science too far. The paper by Kimmerer (2008) found no relationship between salvage of juvenile salmon and Sacramento River flow rate. (It is relevant, and contradictory, to other conclusions in the BO that the Kimmerer paper also found no relationship between juvenile salmon salvage and the gate position for the Delta Cross Channel.) If a correlation does exist between entrainment and upstream reservoir releases it should mostly be due to operational coordination and could (in theory) be easily changed if desired. In other words, if all other releases and exports remain constant but flows from the American River are increased, we would expect no correlation or a slight negative correlation because a smaller percent of inflow would be diverted. We believe this footnote should be removed so as not to tarnish the credibility of the BO.
91. 205, 2, 1: The BO says, "Myrick and Cech (2001) examined the effects of water temperature on steelhead (and Chinook salmon) with a specific focus on Central Valley populations and reported that steelhead egg survival declines as water temperature increases past 50^{o}F." This sentence is followed by extensive discussion of implications for higher temperatures. However, the reader is not left with a firm conclusion regarding an acceptable upper range for temperatures. Water temperatures in April and May (and sometimes March) in the American River at Watt Avenue are clearly above 50^{o}F (page 206, 3rd paragraph). Flexibility is recommended in this analysis because, particularly as one moves south through the study area, equilibrium temperature (that temperature which a water body is in equilibrium with meteorological conditions) during winter and early spring will range in the mid to

upper fifties (^{o}F). These are naturally occurring conditions. Thus, for example, consistently seeking to maintain water temperatures less than 51^{o}F on the Stanislaus River at Oakdale (as mentioned on page 226) during December through February is not feasible under current conditions.

92. 219, 1, 4: Isn't this part of baseline? "the dam captures sediment that would otherwise be transported downstream". The panel understands the baseline to be the existing configuration of the watershed with dams and reservoirs. Are we correct? If so, these words should be re-written or deleted to avoid the implication that sediment captured behind New Melones Dam is an OCAP effect. As noted previously, the BO should provide a good understanding and portrayal of baseline.
93. 223, 1, 3: Explain how it is known that > 98.3 TAF are needed for steelhead. This is unclear.
94. 223, 2, 3: The BO says, "… steelhead are likely to have unmet flow needs in 59 percent of years, *based on recent history* …" Was this statement based on the past 10 yrs of data? 82 yrs? More explanation would be helpful.
95. 224, 2, 1: Figure 6-27 (p. 239) is for an average below normal year not an average critically dry year. Perhaps the sentence should reference figure 6-29 which is for an average critically dry year. Also, are the 5% and 19% values means over the various climate studies? Does your sentence mean, "by 5% in wet yrs and by 19% in … ?" Recast sentence for improved clarity.
96. 225, 2, 1: Drop the first "if."
97. 226, 2, 2: The BO suggests a temperature criteria of 35ºF to 51ºF be met for the Stanislaus R. at Oakdale for Dec-Feb. Is 35 ºF a typo?
98. 226, 3, 3: "Flows that fall below …:" Where does the 50% come from? The BA? Earlier the BO gave the impression that available flow predictions were inappropriate because b(2) & b(3) water wasn't guaranteed or likely.
99. 227, 1, 1: Specify the source of data for the flow frequency analysis on the Stanislaus River below New Melones Dam. Here the BO indicates the recurrence interval for a flow of 5,000 cfs has increased from 1.5 years to 5 years; above the BO implied a much longer interval (perhaps never); hence lack of channel-forming flows.
100. 227, 2, 4: Is it necessary to mention that strategic management must include gravel supplementation so that high flows alone will not ultimately eliminate gravel?
101. 227, 4, 1: Consider recasting this sentence to avoid preachiness. Something like: "Continued habitat improvement … is necessary for … because …" would be better.
102. 228, 4, 1: This bullet list of effects that are likely to reduce steelhead survival and recovery seems to mix OCAP and other effects. The ancillary effects should be presented as such to avoid confusion. Again this seems to be an example of our confusion because the BO does not provide a good understanding and portrayal of baseline. It would also help to better explain the consequences of no VAMP (assuming we did not miss that somewhere else).
103. 229-235, table 6-17: Shouldn't this table list OCAP stressors without baseline stressors? E.g., the first entry, "no access …" clearly is baseline.
104. 254, 1, entire paragraph: The reader needs a depiction of <u>baseline</u> – What exports (and when) are "senior" or required, and how does the system (specifically fish mortalities) "look" under that condition? We don't believe the baseline can be zero exports, at least for the CVP. What is it? We know that this is oversimplified but our

question conveys the issue. The BO may support the jeopardy conclusion, but the full magnitude of the OCAP effect (relative to baseline) seems unclear, hence the target for full mitigation with RPA's remains unknown. We recognize that this suggestion is for no small effort and perhaps can only be done qualitatively but it seems necessary to try.

105. 255, 3, 1: This sentence is not correct. Historically, the TFCF has sampled only for 10 minute intervals every 2 hours, not every 20 minutes. It is common (approximately 40 percent of the time) for the sampling interval at the Skinner Fish Facility to be 20 minutes.

106. 255, 4, 5: Recent experiments by Mark Bowen (USBR) at the TFCF has indicated that the efficiency of the secondary louvers for small fish is strongly affected by how often predators are removed from in front (upstream) of the louvers. We believe (but are not certain) that the present practice is to remove predators once a week, typically on Friday mornings. Removing predators more frequently might be one way to increase the efficiency of the secondary louvers. During times when predators have not been removed for several days, we suspect it is possible that the louver efficiency might be significantly lower than the 80 percent that is assumed for salmon loss calculations.

107. 257, 2, 4-8: The BO says, "However, some adverse effects have been observed in association with the trash racks in front of the screens. Adult fish cannot fit through the narrow gap between the steel slats on the trash rack. This serves as a physical barrier to their passage. Observations of sea lions "corralling" adult fall-run in front of the TFCF trash rack have been observed by TFCF staff and a NMFS biologist. In addition, adult sturgeon in moribund conditions have been observed impinged upon the trash rack." Is there evidence of sea lion predation on green sturgeon? This has become a major issue in the Columbia River for white sturgeon. One panel member (Erickson) has observed marine-mammal bites on the stomach of green sturgeon. People have witnessed adult green sturgeon being preyed upon by sea lions in the Rogue River.

108. 258, 3, 3: This sentence should probably be reworded. It can be misinterpreted to imply that Delta water is screened before entering Clifton Court Forebay.

109. 261, 2, 6: NMFS might want to check whether it has been verified that any tagged steelhead in the recent CCFB study were actually able to swim out of the forebay against the flow through the radial gates and into the Delta. The field technicians on this study have indicated that the only tagged steelheads that exited the forebay were in the bellies of large striped bass. This was all discussed publically at a CalFed-sponsored predation workshop a few years ago in Tiburon.

110. 262, 3, 8: Same comment as on 255, 3, 1 from above. TFCF samples only for 10 minutes every 2 hours.

111. 268, table 6-16: The numbers presented in this table for Old and Middle River (OMR) flows are a little confusing. What was the reasoning behind selecting the time period 1975-1991 for analysis? Even after reading the OCAP BA Appendix G, it is difficult to figure out how the values in the table were derived. Some of the numbers in the table (which are supposed to be median values) seem hard to believe. In particular, the values for February and March for wet and above normal years seem too high (not negative enough) and are not consistent with the values for exports

presented in table 6.15. For example, if February exports (in an above normal water year) are over 10,000 cfs the flow on the SJR at Vernalis would have to be very high (certainly in excess of 20,000 cfs) for OMR flows to be less negative than -2,000 cfs. This is possible, but unlikely for a median value. We realize that the main purpose for the numbers in Table 6-16 is to show differences in OMR flows for different year types, but the actual magnitudes that are presented should be reasonably correct so as to avoid confusion.

112. 275, figure 6-51: Because of the significant temporal trend that occurs each year in salvage of salmon, has anyone plotted exports versus salmon loss for particular months only. For example, plot only the April loss data for spring run each year against the April exports. Perhaps use Jan and Feb for winter run. This seems like the only way to discover a relationship, especially considering the export reductions that occur during April/May because of VAMP when salmon salvage is typically highest.

113. 275, figure 6-51: Given all the discussion in the text regarding OMR flows, these graphs should also be plotted using OMR flows for the *x*-axis in place of exports. Just like for delta smelt, OMR flows may make more sense than exports in finding a relationship with salmon salvage or loss. Especially if OMR flows are going to be used in RPAs, the relationships should be explored.

114. 278-279: The discussions on these pages regarding Newman's and Hanson's statistical analyses of the field data are important and relevant. It points out the high degree of uncertainty that is present in the findings from the field experiments carried out so far. Hopefully, studies using acoustic tagging will reduce these uncertainties.

115. 281, 1, 15: The reference by Perry and Skalski (2008) is not in the reference list. We believe this reference is to a September, 2008 (unpublished) report prepared for the U.S. Fish and Wildlife Service. The BO should point out that the data analyzed by Perry and Skalski was for acoustic tracking of only 144 fish (64 with the DCC open and 80 with the DCC closed). This is a very small sample size to estimate survival and route selection accurately through the north Delta down to Chipps Island.

116. 282, 2, 4: Is the reference MacFarlane (2008) different than MacFarlane, Hayes, and Wells (2008)? If so, MacFarlane (2008) is not in the reference list. How many pages is the MacFarlane, Hayes, and Wells (2008) report? Is it available online?

117. 285, 2, 4: "POD phenomen***on"*** Make this singular, or the following verb plural.

118. 296, 4, 6-7: The estimates of the percentage flow splits for the DCC and Georgiana Slough seem a little off from the measured data (see figure 1 above). In defining these flow splits what is meant by Sacramento River flow should be defined. Is it the flow at Freeport? Is it flow at Walnut Grove immediately above the DCC? There are now a sufficient number of years of measured data in the channels surrounding the DCC that the flow splits are known accurately. The models may not accurately represent the data. For example, we don't believe there has ever been a situation where only 5 percent of the Sacramento River at Freeport flow is diverted through the DCC (when both gates are open). We also don't believe net flows through Georgiana Slough are ever as high as 30 percent of the Sacramento River flow at Freeport. These numbers should be checked.

119. 305, last paragraph, 1-4: Can you integrate your conclusions ("In other words, in the drier …") here (or in the following summary section) with comparison to the "actual baseline" which presumably differs from recent or current conditions. Do you

know that CVP/SWP has the latitude to "be better" under all climate scenarios – i.e., that the <u>baseline</u> conditions are always better?

120. 312, 2, 6: Do you really mean "Figure 3?"
121. 313, 2, 9-10: In these sentences the BO says that the winter-run salmon rear for approximately 3 months in the Delta before appearing in the export salvage during February and March. Figure 6-28 in the BA shows that the actual peak in salvage for winter run has more typically been January in recent years (2002-2005). It might be helpful to report this in the BO.
122. 313, 2, 10: (In general, this is a very long paragraph). Provide citations for "This life history strategy … several months later in the Chipps Island trawls …".
123. 326, 1, 2: Naiman and Turner (2000) is referenced here and at 4 or 5 other places in the BO. It does not appear in the reference list of literature cited.
124. 334, 1, 2: "Mortality … is most likely lower than the survival …" This sentence mixes apples and oranges. Stick to "mortality" or "survival" but don't mix them here.
125. 357, 1, 8: Change "steelhead" to "*Oncorhynchus mykiss.*" The "resident form" may well be rainbow trout, not steelhead, but certainly is *O. mykiss*. Cite your source – CDFG? USFWS? Pers. comm.?
126. 372, 1, last sentence in paragraph: The BO says, "It is highly likely that only a small proportion of those fish passing the location of the RBDD prior to April would move all the way up to the location of the ACID dam." Is this based on telemetry studies? More should be done to estimate the proportion of the run that would otherwise move above ACID dam. What is the habitat like in this 5-mile stretch?

<u>Additional References for BO</u>

Adams, P. B., C. Grimes, J. E. Hightower, S. T. Lindley, M. L. Moser, and M. J. Parsley. 2007. Population status of North American green sturgeon, *Acipenser medirostris*. Environmental Biology of Fishes 79:339-356.

Brown, K. 2007. Evidence of spawning by green sturgeon, *Acipenser medirostris*, in the upper Sacramento River, California. Environmental Biology of Fishes 79:297-303.

Erickson, D. L. 2006. Marine Migration and Estuary Use of Green Sturgeon - Third (and Final) Report of Activities for the Central and Southern Oregon Region. Report Submitted to: NOAA Fisheries, Southwest Fisheries Science Center, 110 Shaffer Rd., Santa Cruz, CA 95060. 19 pp

Erickson, D.L. & J.E. Hightower. 2007. Oceanic distribution and behavior of green sturgeon. American Fisheries Society Symposium 56:197-211.

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Heppel, S. S. 2007. Elasticity analysis of green sturgeon life history. Environmental Biology of Fishes 79:357-368.

Mora, E. A., S. T. Lindley, D. L. Erickson, and A. P. Klimley, Submitted. Do impassable dams and flow regulation constrain the distribution of green sturgeon in the Sacramento River, California? Journal of Applied Ichthyology.

Appendix B. List of materials specifically made available to the Panel by NMFS and CALFED for the review of the draft BO. Panel members also referred to other documents and papers during the review process.

BO and BA

Draft Biological Opinion, including appendices, and a draft RPA document (on CD).

Long-term Central Valley Project and State Water Project Operations Criteria and Plan – Biological Assessment, including appendices. U.S. Bureau of Reclamation. August 2008 or burned September 30, 2008.

Reviews of the 2004 Biological Opinion (4 documents)

CALFED review and NMFS Response:
www.science.calwater.ca.gov/events/reviews/review_ocap.html

Two CIE Reviews.

Report on the 2004 National Marine Fisheries Service's (NMFS) Biological Opinion (BO) on the Long-Term Central Valley Project and State Water Project Operations, Criteria and Plan (OCAP).

Review of NOAA-Fisheries Biological Opinion on Effects of Proposed Central Valley Project Changes on Listed Fish Species.

2008 OCAP Related Reviews

Temperature Management and Modeling Workshop in support of an Operations Criteria and Plan Biological Assessment and Biological Opinion (April 1, 2008).
Workshop Presentations and materials at:
www.science.calwater.Ca.gov/events/workshops/workshop_tmm.html
Report at:
www.science.calwater.ca.gov/pdf/workshops/workshop_tmm_final_report_4-1-08.pdf

Independent Review of the 2008 NMFS Analytical Framework for its Biological Opinion (Oct 31, 2008)
Report at:
www.science.calwater.ca.gov/pdf/reviews/OCAP_NMFS_AF_review_final.pdf

VSP documents

McElhany, P., M. Ruckelhaus, M.J. Ford, T.C. Wainwright, and E.P. Bjorkstedt. 2000. Technical Memorandum, NMFS-NWFSC-42. www.nwfsc.noaa.gov/assets/25/5561_06162004_143739_tm42.pdf

Lindley, S.T., et al. 2007. Framework for Assessing Viability of Threatened and Endangered Chinook Salmon and Steelhead in the Sacramento-San Joaquin Basin. San Francisco Estuary and Watershed Science. Vol. 5, Issue 1 (February), Article 4. http://repositories.cdlib.org/jmie/sfews/vol5/iss1/art4

Lindley, S.T., et al. 2006. Historical population structure of Central Valley steelhead and its alteration by dams. San Francisco Estuary and Watershed Science. Vol. 4, Issue 1 (February), Article 3. http://repositories.cdlib.org/jmie/sfews/vol15/iss1/art3

Background Information

Background information on the ESA and NMFS' responsibilities for implementing the ESA is available from the NMFS Office of Protected Resources web site at: www.nmfs.noaa.gov/pr/laws/esa.htm.

Background information on consultations under section 7 of the ESA is found at: www.nmfs.noaa.gov/pr/consultation.

Fall-run Essential Fish Habitat assessment for BA provided by Reclamation to NMFS-supplement to BA.

Green Sturgeon Critical Habitat Analysis - supplemental info to BA provided by Reclamation to NMFS.

Technical Review of the Draft Long-Term Central Valley Project/State Water Project Operational Criteria and Plan Biological Assessment (BA).

Independent Peer Review of Two Sets of Proposed Actions for the Fish and Wildlife Service OCAP Biological Opinion

Judge Wanger's decisions April 16, 2008 – (2004 BO is deficient); July 18, 2008 (Species are in Jeopardy, but interim remedies requested are not required based on expert testimony)

Expert testimony of Sheila Greene (DWR).

Expert testimony of Christina Swanson (Plaintiffs).

Expert testimony of Chuck Hansen (Defendant Interveners).

Expert testimony of Bruce Oppenhiem and Jeff Stuart (NMFS).

Example Biological Opinion - Consultation on the Approval of Revised Regimes under the Pacific Salmon Treaty and the Deferral of Management to Alaska of Certain Fisheries Included in those Regimes.

Appendix C. Some comments on the limitations of the climate change analyses reported in the BA.

- The approach of basing analysis on a limited number of climate projections that essentially span the range of the full set of available climate projections is reasonable, and it would be impractical to perform a full analysis on a larger set of projections. However, although this approach illustrates the range of possible outcomes, it does not allow rigorous quantification of uncertainties. To do that would require analysis of a larger suite of scenarios and, more importantly, assessment of the relative likelihood of the different scenarios. Thus, the present analysis is a sensitivity study, not a probabilistic assessment.

- All the climate-based modeling described in the BA is based upon coarse-resolution global climate projections that have been "downscaled" (translated to a fine spatial grid) using an approach known as Bias Correction/Spatial Downscaling (BCSD). This approach essentially adds spatial detail to projected surface temperatures and precipitation, based on historical observations of these quantities. This approach is widely used and has proven skill. However, a limitation of this and related approaches is that it cannot represent a "snow-albedo feedback," in which warming causes loss of snow cover, which in turn amplifies warming because a bare surface is darker than a snow-covered surface and absorbs more sunlight. Omission of this effect apparently can reduce substantially changes in river flow timing that result from a greater proportion of precipitation as rain and from earlier snow melt. Estimates based on climate projections downscaled using BCSD typically demonstrate that these effects shift the timing of river flows to about 2 weeks earlier. Similar results downscaled using dynamic models that represent the snow albedo feedback indicate a shift of about 4 weeks. Hence this shift may be significantly underestimated in the BA and subsequent analyses.

- The BCSD approach has additional limitations as well; perhaps the most important of these involves the bias correction (error removal) applied to future climate projections. The BCSD method assumes that a bias correction developed based on comparing model simulations of the historical period to observations applies to future-climate projections as well. Thus, the method assumes that model biases are invariant under climate change. There is no way to assess the validity of this assumption. (This is discussed in pp. 37-38 of the BA, Appendix R.)

- The BA considers only a single sea-level rise scenario (1 foot increase by 2030); this choice was dictated by availability of existing model results for CVP/SWP operations and Delta hydrodynamics. As noted in the BA, consideration of a single sea-level rise value does not fully illustrate the range of possible effects of sea-level rise. Furthermore, using the same sea level rise value with different climate change scenarios introduces a degree of internal inconsistency, since in reality each climate change scenario should have its own unique value of sea-level rise. So, again, the approach taken here must be regarded as a sensitivity study rather than a prediction of effects of sea level rise.

- Effects of changing air temperatures on inputs to the Reclamation water temperature model were handled simplistically (Appendix R, p. 48). As noted in the BA, the simplifying assumptions tend to overestimate the effects of increasing air temperatures on water temperatures.

- All results are based upon an assumption of no change in demand for water between the historical period and 2030. Sensitivity studies illustrating the uncertainty introduced by this assumption would be helpful.

- Similarly, all results are based upon an assumption of no change in flood-control rules. As noted in the BA, changing societal priorities regarding the relative importance of flood control and water supply reliability could undermine this assumption.

- Expected changes in ocean temperatures are predicted by climate models, and are therefore incorporated into all the results that are calculated based upon climate model output (e.g. impaired and unimpaired river flow rates, etc). However, increasing ocean temperatures and other ocean conditions may have direct impacts on listed species; these are of unknown importance, and are not accounted for in the analytical framework of the BO.

- The presence of sediment may affect salmon, and climate change could influence future sedimentation rates through changes in vegetation, soil moisture, frequency and magnitude of extreme precipitation events, etc. The BA does not consider possible future changes in sedimentation, so any effects of changes in sediment loading on listed species are ignored.