Long term Operations of the CVP and SWP Fish and Aquatic Effects Analysis Peer Review Report

Central Valley Project, California

California-Great Basin Region

U.S. Department of the Interior

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The mission of the Bureau of Reclamation is to manage, develop, and protect water and related resources in an environmentally and economically sound manner in the interest of the American public.

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Central Valley Project, California

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Cover Photo: Lower Sacramento River. (Reclamation/Alex Stephens)

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Introduction

This Peer Review Report documents the independent review process and responses to comments from the Delta Science Program Independent Scientific Review Panel (Panel).The Panel reviewed the Draft Effects Analysis and evaluated the analytical approach used to assess how the LTO of the CVP and SWP affect the aquatic environment and the exposure, response, and risk to select ESA-listed species (individuals and populations), and whether quantitative and qualitative methods and risk assessment tools are used appropriately. This Peer Review followed the Peer Review Plan, which is available on the USBR website.

Reclamation appreciates the extensive comments and suggestion by the panelists. The Panel included five members including Drs. Henriette Yager (Panel Chair), Kenneth Rose (Lead Author), Nancy Monsen, Zhoajun Bai, and Emily Howe. The Panel Report is posted on the Delta Science Program website. The review was conducted between November 2023 and March 2024.

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Panel Review Questions

- To what extent do the draft analyses explain the exposure, response, and risk from project operations for individuals and populations of the ESA-listed, and physical and biological features of designated critical habitats under the approaches described by the alternatives?
- To what extent do the draft analyses provide a scientifically defensible approach for evaluating effects on listed species and their designated critical habitats throughout the action area for different alternatives?
- How well do the draft analyses use the best available scientific information in their analyses and findings?
- How well do the draft analyses address data gaps and uncertainties? Are assumptions and methodologies suitable for addressing identified data gaps?
- Of the key operations modeled, how adequate are the models for representing the effects of the different alternatives on aquatic listed species and their habitat?

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Disposition of Panel Findings and Recommendations

Bay-Delta Office staff reviewed every comment provided by the DSP panel and identified a resolution for each of the comments (Attachment 2). Generally, comments fell into 3 categories. First, there were comments that did not provide a recommendation or suggestion about improving the document's use of literature, observation, and models to inform Reclamation's assessment of biological effects of LTO impacts on ESA-listed species and no response was identified. Second, there was a set of comments providing recommendations on how to improve the technical tools and the resultant evaluation of effects, which could be addressed through revising and clarifying draft Biological Assessment text. Finally, there was a set of comments making recommendations that will require further development, recalibration, visualizations, and/or new analyses with the tools being reviewed. As part of the LTO Adaptive Management Program, Reclamation will share these comments with the Adaptive Management Steering Committee and Adaptive Management Teams to inform their development of tools, results, and analyses informing planning and evaluation of the CVP's operations. Many of these fit into specific Adaptive Management Actions such as Spring Outflow, Summer-Fall Habitat Action for Delta Smelt, Winter-run and Spring-run Chinook Salmon OMR Management, and discussion and improvement through the longer duration of the adaptive management program will ensure coordinated improvements to these tools used in the planning process.

Table 1. DSP Review comments on Draft Biological Assessment Species Chapters

Comment	Response
Comment 14. The method used to date in the species chapters for integrating effects is useful.	N/A
The Panel was encouraged by the species chapters and Reclamation's attempt to date to evaluate the effects by stressor and life stage for each species. Reclamation's approach starts with a statement of the status of the species (distribution, abundance, temporal and spatial domains) and habitat, a description of the limiting factors, threats, and stressors by life stage, and a listing of management activities. This is followed by a short summary of monitoring data and the current incidental take statement.	No response identified.

Comment	Response
The analysis of the Proposed Action consists of a systematic evaluation by stressor and life stage. Each stressor effect by life stage that cannot be deemed discountable or insignificant (i.e., stressors with potential effects) is categorized by its severity (e.g., sub-lethal, lethal, beneficial, or minor), the proportion of the population affected (e.g., small (≤2%), Medium (>2% and <70%) or large (≥70%), sometimes prefaced by "likely"), and the frequency of occurrence of the increased (or decreased) stressor (e.g., high (≥75%), Medium (25-75%), or low (<25%), sometimes with a "likely").	No response identified.
A series of appendices prepared by Reclamation enabled the linking of conceptual models, field data, and model predictions (i.e., indicators) of individual stressor effects to inform the proportion of the population affected and the frequency of occurrence of the stressor: Appendix D analyzed potential stressors for the seasonal operation of the CVP and SWP, Appendix C summarized when fish may be present in different locations based on historical monitoring, Appendix G analyzed potential stressors due to facility-specific operations, and Appendix H analyzed conservation measures to minimize or compensate for adverse effects. These appendices were valuable documentation of stressor-by-stressor impacts on the life stages of each species. The Panel recognizes the effort involved in assembling and clearly presenting the many sources of information used in these appendices and the species chapters.	No response identified.
Reclamation then stated that they used a weight-of-evidence approach to infer the proportion of the population that will be affected and the frequency of an increase in the stressor; however, the Panel did not see any actual use of a weight-of-evidence approach. In the section entitled "Effects Analysis" in each species chapter, the sources for the evidence were systematically listed for each stressor and life stage, followed by a list of Conservation Measures that are designed to reduce the impact.	We describe multiple Lines of Evidence, leaving assessment of the Weight of Evidence to the regulatory agencies.
Comment 15. The analysis stops short of providing an integration of the stressor effects that limits understanding of population-level outcomes.	*N/A*

Comment	Response
Although the approach used by Reclamation is logical, the analysis presented stops too soon and does not state any population-level conclusions. Whereas some of the strongest modeling to detect the local effects of Reclamation activities were the stressor models (e.g., TDM models), they were not integrated into a life cycle modeling framework (expanded in Comment 17). Even if such conclusions are not required, the information in the chapters on stressor-by-stressor effects should be presented so that such determinations are easy and transparent. The analysis lists the sources of the evidence under the weight of evidence statement and then stops. Appendix D has some of the information needed but it is not easy to combine this with the species chapters. Thus, the Panel recommends that Reclamation consider modifying or adding documentation to the species chapters to enable an easy extension to assessing species-level responses. The Panel also notes that the organization of the information in the species chapters would be very useful to the evaluation of all alternatives (i.e., the EIS), including the addition of a graphical presentation of the results.	Reclamation uses the effects analysis to determine if there is an adverse or beneficial effect, not the population level conclusions.
Comment 16. Some components of the Draft Effects Analysis destined for the BA should be better estimated.	*N/A*
The severity determination seems too coarse, and it does not seem informative to a population (or Evolutionarily Significant Unit (ESU) or Distinct Population Segment (DPS))-level assessment. The severity terms come from situations focused on individual organisms. It was not clear how lethality was determined for some stressors because the processes (vital rates) affecting populations (growth, mortality, reproduction, movement) are interrelated. For example, mortality is often size-dependent and so it depends on growth. Would the reduced reproduction be lethal or sub-lethal? Importantly, how was the classification used (or will be used) to inform the severity of the effect and then used with the other terms (i.e., proportion and frequency)?	Reclamation uses the effects analysis to determine if there is an adverse or beneficial effect, not the population level conclusions. Determination of effects were determined based on thresholds identifiable from the literature.
In some cases, the estimation of the exposure (proportion of the population affected) seems weak and relies too much on the percent occurrence of conditions across years rather than a more relevant measure that quantifies the degree of exposure over ecologically relevant time scales. For example, a typical statement (this is from the Delta smelt chapter) is: "In 21 out of 27 (~78%) years, spring outflow was low (Figure 9-8)." There are many data sources listed that should provide more informative exposure information and the Panel suggests this be explored.	Where more informative information is identified in species effect and individual model comments by the panel Reclamation has incorporated into the BA.
Comment 17. The life cycle models are treated separately from the effects predicted by other models.	N/A

Comment	Response
This may not be possible in the short term, but the absence of life cycle models for some species is both surprising and disappointing given their listing status. When a life cycle model is available for a species, its results are treated separately from the effects estimated from models focused on individual stressors. The estimation of effects with the life cycle models involves the simultaneous effects of multiple stressors across life stages, but the models do not represent all of the stressors. Integrating the models that looked at a single or a subset of stressors within the framework of a life cycle model is challenging but necessary. One approach is to incorporate mechanistic stressor-effects sub-models or modules into the life cycle models so they better represent and cover the important local and fine-resolution stressors. This is doable, although it likely would involve recalibration and validation.	This comment will be shared with adaptive management programs working on life cycle models supporting LTO activities for consideration on how to improve evaluation of project effects on species.
Comment 18. There is no weight of evidence analysis presented.	N/A
The analysis of each stressor and life stage in the Draft Effects Analysis always stops with a form of the general statement: "To evaluate the weight of evidence for the X stressor, multiple location- and species-specific datasets and studies have been evaluated to infer the proportion of the population that will be affected and the frequency of an increase in the stressor." The Panel recommends caution in labeling the analyses as using “weight of evidence.” Weight of evidence is not a simple listing of the sources of evidence or even a listing of the actual evidence. Weight of evidence is a formal methodology (Hope and Clarkson 2014; Linkov et al. 2009). The present sequential listing of stressor effects provided to the Panel did not include a clear and transparent weight-of-evidence analysis. The Panel found the qualitative summaries using a standard typology (e.g., severity-abundance-frequency) very useful and we recommend using a more formal weight-of-evidence approach, especially explaining how the different sources were used and how they were considered. Using such an approach could also make better use of the life-cycle modeling and would ensure that multiple stressor effects on the same life stage and the effects over different life stages and processes (e.g., reproduction, mortality, migration) were appropriately ‘weighted’ to produce population-level outcomes (see Comment 18).	Agree, "weight of evidence" was a piece of the suite of items provided to readers within stressors in BA chapters (alongside frequency, proportion, etc.). It is intended to represent all the available sources (e.g., historic data, models, literature) that can be used / were used to evaluate the stressor on the species / life stage discussed. In the future, Reclamation will consider utilization of different categorization to describe available sources and will consider taking a more formal weight of evidence approach.
Comment 19. Clarify how stressor effects were determined to be insignificant or discountable.	N/A

Comment	Response
The determination that a stressor was insignificant or discountable requires additional explanation beyond that provided. Reclamation states: "Based on best judgment, a person would not be able to meaningfully measure, detect, or evaluate insignificant effects. Discountable effects are those extremely unlikely to occur. Based on best judgment, a person would not be able to expect discountable effects to occur." It was not clear to the Panel how these criteria were applied to each stressor by life stage. The Panel notes that, in some cases, insignificant effects that were based on modeling results may be a consequence of modeling that underestimates differences among alternatives. Also, the Panel considered that Reclamation appeared to use modeling results in some places as absolute values – does this violate their philosophy about how modeling results should be used to make relative predictions? Given the recent weather patterns (drought, heavy rains), what does Reclamation consider to be "extremely unlikely to occur?" Is this referring to seismic activity causing levee failures?	Insignificant effects relate to the size of the impact and should never reach the scale where take occurs. Discountable effects are those extremely unlikely to occur. Based on best judgment, a person would not: (1) be able to meaningfully measure, detect, or evaluate insignificant effects; or (2) expect discountable effects to occur. Reclamation understand that modeling has limitations and uncertainties, modeling results help to inform effects of our action/impacts. Specifically, regarding drought and heavy rains, Reclamation's climate change methodology is described in App F.
Comment 20. Consider other related frameworks to improve and expand the approach.	N/A
In the field of Conservation Biology, there are frameworks to consider that are specifically designed to focus on threats to listed species. When available, the best available science includes the use of a Population Viability Analysis (PVA) to systematically add and remove multiple stressors (Murray et al. 2021). In fact, the best use of stochastic PVA models (i.e., stochastic life-cycle models) is to evaluate threats posed to species at risk, taking uncertainty into account (Caughley 1994). The Panel suggests Reclamation consider the ideas and concepts of PVA, if not an actual PVA, going forward.	This comment will be shared with adaptive management programs working on life cycle models supporting LTO activities for consideration on how to improve evaluation of project effects on species.

Comment	Response
In addition, the literature on how to perform weight-of-evidence (Linkov et al. 2009, Suter et al. 2017) and cumulative impacts analysis (Blakley and Franks 2021) is extensive. Of particular relevance here is how many cumulative-type analyses use a variation of the concept of the Drivers-Pressures-State Change-Impact-Response (DPSIR) framework (Patrício et al. 2016) and when quantitatively combined use a form of the index proposed by Halpern et al. (2008, 2015). Cumulative impacts have been analyzed for many ecosystems and projects (Foley et al. 2017); examples of large-scale ecosystem restoration include the California Delta (Diefenderfer et al. 2021). Whereas many of the example applications focus on spatial (often GIS-based) information (Halpern and Fujita 2013; Hammar et al. 2020), the approaches and examples also apply to non-spatial situations; in the case of the Draft Effects Analysis, the multiple species, multiple stressors, and life stages form the dimensions of the analysis matrix.	This comment provides a thoughtful setup on cumulative impacts methods. Reclamation will share these alternative approaches for weight-of-evidence and cumulative impacts analyses in adaptive management programs supporting future LTO planning and evaluate their use in those planning efforts. Tied to comment in row 15.
The approach by Reclamation could be clarified and strengthened by explaining how the analyses were applied and relating the approach used to other approaches used in similar situations. Reclamation may consider modifying their approach in light of these other analyses. For example, how multiple stressors affect each other (additive, synergistic, antagonistic - Crain et al. 2008) is often part of assessing effects when multiple stressors are present. Reclamation does not discuss this and so seemingly assumes all effects are additive, when in fact, it is likely some stressors show synergistic or antagonistic effects with other stressors. If stressors act synergistically, treating them separately would underestimate the total effect and could influence determinations of "discountable."	Reclamation used the analytical approach identified in the NMFS 2019 Biological Opinion. A description of approach for stressors is provided in App D. To complete the deconstruction, Reclamation evaluated each stressor for each key life stage of each species, in each geographic region (watershed or river reach), during each season. Reclamation took on the exercise of identifying which stressor were affected by the action. When stressors may act synergistically, we identify information sources (Lines of Evidence) to inform the total effect and determinations.
Comment 21. Some of the studies cited are possibly outdated.	N/A

Comment	Response
The Panel encourages the use of all studies with the caveat that some studies may have less relevance because of new methods becoming available and the changing ecosystem. For example, the Kimmerer (2008) analysis of entrainment of Delta smelt is relied on heavily by Reclamation. This analysis used data from 1995 to 2005, which is now more than 20 years old. Smith et al. (2021) is a more recent analysis of entrainment and is also presented. How these two studies, both of which are considered excellent analyses, can be combined is not clear from the present cumulative analysis. The Panel uses Delta smelt here because much has changed with Delta smelt in the system; similar examples can be cited for the other species.	Reclamation analysis incorporates all Lines of Evidence for the agencies to consider when evaluating the effects of the action/project. We describe multiple Lines of Evidence, leaving the Weight of Evidence to the regulatory agencies.
Comment 22. A graphical presentation of the results of species-by-stressor effects is needed.	N/A
As part of the species chapters (and also perhaps for the EIS), the Panel suggests that Reclamation develop graphical visuals to communicate the results of the analysis by species and stressor. There are many examples in the literature of ways to visualize the results so that readers can see the patterns and properly interpret the results described in the text. The systematic approach is good, but the bulleted narrative form is difficult for the reader to comprehend because it is impossible to keep track of a long list of results when presented individually as text. This is closely related to Overarching Comment 1 that applied to the entire Draft Effects Analysis.	Reclamation will consider, in future efforts, building out a visualization tool to visually convey results of analyses by species/life stage/stressor as part of the adaptive management program to be used in future consultation on the CVP.

Table 2. DSP Review comments on Draft Biological Assessment Technical Appendices

Comment	**Response**
Major Comments on Certain Models	*N/A*
Many of the individual model attachments and documents were incomplete and the Panel has commented on these to the extent possible using the provided documentation. Many analyses also used existing models and thus, while the Panel had comments, we erred on the side of assuming that these models, based on their previous use, were reasonable approaches. Exceptions, where the Panel had significant issues with the models in their current form, were: a) Appendix I OMR, Attachment I.6, Volumetric Influence Analysis – Labeled review V, b) LTO Appendix J – Spring Delta Outflow, Attachment J.1 Longfin Smelt Outflow – Labeled review Eye, c) LTO Appendix J – Spring Delta Outflow, Attachment J.2 Sturgeon Year Class Index and Delta Outflow – Labeled review J, d) LTO Appendix J, Attachment J.3 – Zooplankton Delta Outflow Analysis –Labeled review Y, and e) Ch. 11 Killer Whale – Labeled review P.	No response identified.
Panel Comments on Species Chapters	*N/A*
The Panel reviewed the species chapters destined for the BA, but written comments are not included here. These were: Ch. 05 Winter-run Chinook Salmon, Ch. 06 Spring-run Chinook Salmon, Ch. 07 Steelhead, Ch. 08 Green Sturgeon, Ch. 09 Delta Smelt, and Ch. 10 Longfin Smelt. The exception was Ch. 11 Killer Whale, which we included a written review of because the analyses provided on food effects greatly differed from the other species and the effects analysis was also quite different.	No response identified.

Comment	Response
The general format for the species chapters included a summary of the status of the species followed by a summary of the Draft Effects Analysis results, and this is an excellent start. Presently, the chapters mostly consist of a listing of effects results, generally by life stage and stressor (i.e., a long listing of stressor effects by life stage). These are the raw ingredients for a synthesis of these effects and responses to risks at the species level. Our major comments about the species chapters are covered in the Global and Overarching Comments and apply to all of the species chapters. Other relevant comments can be found in the individual model reviews below.	No response identified.
In particular, proper synthesis of stressor responses for each species remains a major challenge that must be overcome with a consistent and transparent "roll-up" of the life stage species results. The life cycle models, when available, provide a framework for the synthesis but are not always sufficiently comprehensive in representing stressor responses across all life stages and effects. Weight-of-evidence is cited by Reclamation but that requires a level of rigor and systematic methodology (not just a listing of effects) which was undefined in the documents provided to the Panel, and also not used to synthesize across life stages and stressors.	We describe multiple Lines of Evidence, leaving the Weight of Evidence to the regulatory agencies.
(A) CALSIM-3 (Multiple documents)	*N/A*

Comment	Response
Material reviewed: a) LTO – Draft Biological Assessment, Chapter 1 to Chapter 4, b) Appendix F – Modeling, main report, c) Appendix F – Sections_1-1_to_1-3_Attachments_1-1-to-2_11, and d) CALSIM-3 Report (August 2022) – not part of review material provided by Reclamation The CALSIM-3 model is a standard operational hydrology model used for the operations of the Central Valley Project. As such, this is the model that Reclamation knows the best and they provide very detailed information about the settings and assumptions used for each of the simulation runs.	No response identified.
Topic #1: Application of climate change perturbations to drive CALSIM-3 simulations	*N/A*
Data uncertainty is a great challenge to any quantitative modeling, which is particularly true for climate change. The LTO team developed model simulations to support the analysis of CVP and SWP LTO under climate change, including 15cm of assumed sea level rise. For rim boundary conditions, "historical and perturbed meteorological data were used for simulating projected surface runoff, base flow, surface water evaporation, and potential evapotranspiration variables for future periods using the Variable Infiltration Capacity (VIC) model" (Appendix F, Attachment 1-1). In another example, CALSIM-3 projected hydroclimate input data under different change scenarios using methods such as the perturbation of total flows of major watersheds (Appendix F, Attachment 1-1). Overall, the perturbation method seems reasonable but the provided documents did not describe in sufficient detail how the perturbation method was applied to simulate climate change in CALSIM-3. If it is a uniform perturbation of historical and meteorological data, then it may not be the best practice for this study. Better simulation and regression methods should be considered (e.g., Kolstad and Moore 2020).	Reviewed and ensured details of the methodology used in developing hydroclimate boundary conditions for the CalSim3 models to represent 2022+/-15 conditions are described in Appendix F, Attachment 1-1. Additional detail (scripts etc.) can be made available by request.

Comment	Response
Topic #2: Filling data gaps for CALSIM-3 simulations	*N/A*
The draft analysis numerously reported data gaps and/or missing data. Examples in Appendix F, Attachment 1-3 include: "streamflow data (for CALSIM-3) exist at the watershed outflow point for only a limited period between water years 1922 and 2021''. ''(As) evaporation data is incomplete it is necessary to develop a standard method of estimating reservoir evaporation rates beginning October 1921.'' Techniques were reported in the draft BA report to address this data gap issue. For example, since no gage data exists for the watershed, "it is assumed that runoff is proportional to the product of drainage area and average annual precipitation depth over the watershed. Outflow was determined through association of the watershed with a similar but gaged watershed and the use of multiplicative factors representing the ratio of watershed areas and the ratio of precipitation depths" (Appendix F, Attachment 1-3). The technique of filling gaps with existing data is also known as imputation. The imputation techniques described in the Draft Effects Analysis seem very limited in scope. There are various missing data imputation techniques (see Jager et al. 2021).	Details about imputation were clarified in the sections identified here and other sections identified for further review of Appendix F. Utilized data gap filling approaches are believed to be within the model uncertainty.
Topic #3: Interpolation techniques to preserve volume continuity when linking CALSIM-3 model output (monthly time step) to connecting models (e.g. HEC5Q, DMS2) that require daily or hourly time steps to drive the model	*N/A*

Comment	Response
As an overarching planning model to simulate operations of the CVP and SWP over a range of hydrologic conditions, CALSIM-3 simulates system operations for a multi-year period using a monthly time step. The model assumes that facilities, land use, water supply contracts, and regulatory (e.g., water quality, instream flows) requirements are constant over this period, representing a fixed level of development. For input of CALSIM-3 output into other models, the preprocessor aggregates various CALSIM-3 time series as well as interpolates the time series, as needed, from monthly to daily values (Appendix F, Attachment 1-3).	No response identified.
For the HEC5Q temperature model, several time series within each basin model require disaggregation from monthly CALSIM-3 inputs to a daily time series. The CAL2DOM utility program translates data from CALSIM-3 to USRDOM, including conversions from monthly to daily operations and the disaggregation and consolidation of flow data.	No response identified.
Reclamation undertook a modernization of the temperature preprocessor to improve their code transparency, understandability, and maintainability. The revised preprocessor utilizes the PchipInterpolator from the Python Scipy library to perform the spline interpolation (instead of using legacy Fortran code) for interpolating the time series from monthly to daily time steps. It is indeed a modernization. However, as noticed by the draft report,"PchipInterpolator does not preserve the monthly volumes which is necessary to prevent an unphysically realistic trough" (Appendix F, Attachment 1-3).	No response identified.

Comment	Response
To address the volume conservation issue, Reclamation explains "*volume was enforced through a preconditioning operation that incrementally adjusts the maximum monthly magnitude until the average value of the spline matches the CALSIM monthly value." Meanwhile, "to prevent an unphysically realistic trough prior to large increases in magnitude, the code shifts the date of the maximum monthly magnitude backwards in time if the months differ in magnitude by more than a factor of two. This results [in] a continuous time series that is more smooth and representative of the CALSIM monthly time series than would otherwise be produced by PchipInterpolator with the maximum flow occurring mid-month. The maximum monthly flow is limited to occurring five days before the end of the month.*"	No response identified.
Unfortunately, the methodologies used to address these two critical issues seem ad hoc. It is unclear whether these methodologies have been thoroughly validated and mathematically proven to be effective. The following plot (Figure 1) illustrates the negative flow phenomenon for the disaggregation of the monthly flow to daily flow by the PchipInterpolator for monthly volume (August 2014 to March 2015) of Cow Creek, a Sacramento River tributary in Shasta County.	Spline method is not applied to Cow Creek. There are specific checks within the workflow to prevent negative flows from occurring. We can further investigate and respond if other specific locations can be identified by DSP. The goal was not to update the approach but rather to replicate what was done in the existing Fortran processor as closely as possible.
Figure 1. Disaggregation of monthly flow to daily flows generated by the PchipInterpolator for monthly volume from August 2014 to March 2015 at Cow Creek, a Sacramento River tributary in Shasta County. The x-axis is the count of days, and the y-axis is daily flows (cfs). The plot illustrates the "negative flow" phenomenon and the peaks and troughs pattern versus the observed data. There are various studies on interpolation techniques in mathematics and engineering for mass conservation that suggest maintaining non-negativity and continuity (e.g., Hittmeir et al. 2018).	No response identified.
Topic #4: Optimization guidelines applied to the CALSIM-3 operations model to represent ESA-listed species effects	*N/A*

Comment	Response
There are two key changes to the operation of the CVP that are being proposed and modeled for the Draft Effects Analysis: (a) water management for Shasta, based on water-year type to preserve cold water pool volumes under drought conditions and (b) Sacramento River pulse flows. The water management philosophy for the CVP changed to Victorian objectives for the Shasta Reservoir: "In order to recognize and adapt to these significant changes to the system, as a whole, Reclamation is proposing a new approach to managing Shasta which changes the balance between risks of flood control releases (aka spills) and maintaining water in storage for future drought protection and temperature management. This approach, described below, places a higher priority on maintaining storage for drought protection for all project purposes while limiting the frequency of spilling water due to flood control limitations" (BA, Chapter 03 Proposed Action, p. 3-13; pdf p. 22).	No response identified.
Of particular importance for this review are operations for drought conditions (Bin 3-Protect Shasta operations): *"Under Bin 3, critically dry conditions exist, the system is stressed, and water resources are not available to meet all demands. There is low confidence to meet sufficient temperatures at the Clear Creek gage and future drought protection is at risk. The main biological objective is to protect winter-run Chinook salmon against decline. This Bin includes the widest array of potential water supply and fishery management actions to protect winter-run Chinook salmon from significant impacts and to protect against future drought risks" (BA, Chapter 03 Proposed Action; p. 3-20 – 3-21; pdf p. 29-30).*	No response identified.

Comment	Response
To increase the outmigration survival of Chinook salmon, Reclamation would release up to 150 thousand-acre-feet (TAF) in pulse flow(s) for the Sacramento River each water year, typically in the spring, to benefit Chinook salmon in the Sacramento River watershed when the pulse does not interfere with the ability to meet temperature objectives or other anticipated operations of the reservoir. Reclamation plans to schedule this pulse after coordination through the Sacramento River Group (SRG) and Shasta Operations Team (SHOT) and may include coordinating timing with natural flow events, potential storage management operations, and/or pulse flows in tributaries (BA, Chapter 03 Proposed Action pdf p. 20; 3-11). Most of the documentation about the simulations focuses on how Reclamation incorporates these two operational changes (Shasta management and Sacramento River pulse flows) into the simulations. These descriptions also include the other regulatory conditions that must be met and the Voluntary Agreements with stakeholders. The Panel will not comment on the current regulatory implementation details or voluntary agreements.	No response identified.
CALSIM-3 output is used as boundary conditions for HEC5Q and DSM2 models that then drive biological effects models, as well as directly in biological effects models. The Panel had some comments from the perspective of the end-use of the biological effects models.	No response identified.

Comment	Response
The first comment is about the use of model outputs to estimate species-specific cumulative salvage or loss threshold. Reclamation states "Since salvage or loss cannot be directly modeled in CALSIM, historic salvage data at the fish facilities at Banks and Jones Pumping Plants and other triggers for these actions were analyzed for the 2010 – 2022 period. Based on this historic data and water year type, the modeling used an OMR index of negative 3,500 CFS in a portion of each January through June" (Appendix F Section 1-1 Modeling Methodology, p. 39). The Panel requests additional evidence be provided that this use of monthly output is a reasonable way to estimate the salvage-related effects.	Reclamation used available historic data to model the OMR management actions to the best of model capability. Monthly CALSIM3 model outputs are used in a set of biological models to evaluate salvage-related effects, typically with a metric related to modeled cumulative salvage or loss thresholds. These tools have been used in previous consultations. Other metrics related to through-Delta survival and entrainment risk use daily flow data to evaluate OMR-related effects. We developed multiple lines of evidence to understand salvage-related effects for consideration of the PA. The value of further refinement of salvage-related effects modeling may be considered in the PA adaptive management program related to Chinook salmon OMR management.
A second comment is about turbidity bridge avoidance. Reclamation explains "January through March in any Sacramento (40-30-30) Index Water Year type, if first flush has already occurred and if the turbidity trigger is reached (SACRI greater than or equal to 20,000 CFS), Projects operate to OMR Index of negative 2,000 CFS for ten days" (Appendix F Section 1-1 Modeling Methodology, p. 39). Again here, the Panel questions if the use of modeling output treated in a way that enables statistically robust estimation of first flush and the trigger.	Monthly CALSIM 3 model outputs reflecting turbidity bridge avoidance measures are based on the literature, observations, and models available to us to estimate first flush and trigger conditions. The value of further refinement of first flush and trigger modeling may be considered in the PA adaptive management program.
Thirdly, Reclamation states "not all salinity requirements are included, as CALSIM-3 is not capable of predicting salinities in the Delta. Instead, empirically based equations and models are used to relate interior salinity conditions with the flow conditions" (Appendix F Section 1-1 Modeling Methodology, p. 11). This linked model uses empirical equations without evidence of its accuracy. It was unclear to the Panel from the provided text whether the Department of Water Resources' artificial neural network was modified for the Draft Effects Analysis to reflect the new empirical relationships between salinity and flow that incorporated sea level rise.	DWR uses DSM2, a 1-D hydrodynamics model of the Sacramento-San Joaquin Delta and the Bay to inform CalSim on Delta salinity conditions for a specific sea level condition and bathymetry under various operational scenarios. DWR completes a full circle analysis to ensure ANN performance is acceptable. DWR completed a retraining of the ANN for sea level rise conditions used by Reclamation. Reclamation did not update the ANN as conditions were unchanged from DWR product.

Comment	Response
(B) Attachment L.1 Coldwater Pool Storage and Coldwater Pool Exceedance Analysis	*N/A*
This appendix analyzes alternatives for the management of Shasta Reservoir for water temperatures downstream of Keswick Dam. The last remaining population of winter-run Chinook salmon is below Keswick Dam and relies upon the operation of the CVP to provide cold water for spawning and incubation over the summer months.	No response identified.
An initial alternative report (LTO 2021) developed potential options for the LTO of the CVP and SWP. Reclamation identifies 8 management questions to inform the formulation of alternatives, such as "does real-time onset and shaping of temperatures improve winter-run Chinook salmon production or does a fixed schedule based on historical observations protect fish with limited water supply impact"?	No response identified.
Reclamation solicited input from agencies and interest parties for their "knowledge base paper", and conducted full 82-year CALSIM II simulations for alternatives; followed by HEC-5Q and temperature-dependent mortality (TDM) models.	No response identified.
The initial findings provided answers to five of the 8 questions, including partial answers, while answers to the other three questions are still "under development". Among these answered questions, some answers are clear, even quantitatively. However, some findings are less formative, such as those presented for "limited effect".	No response identified.

Comment	Response
A comprehensive set of literature, datasets, and models for the development of "the knowledge base paper" are detailed in section 5, with a total of 26 pages, consisting of the main body of the appendix. Some datasets include data gaps or shorter sampling efforts than others, but overall, a large body of data is available. These datasets, in conjunction with the modeled data (i.e., CALSIM-3, DSM2, USRDOM), serve as input for models that can be used to understand and predict the effects of CVP and SWP operations on environmental conditions and fish distribution and loss. Overall, the contents are solid and informative.	No response identified.
Rationales behind different concepts and approaches to coldwater pool management strategies were documented during the development of the alternatives. These concepts are described here as lines of evidence in section 6. Reclamation stated that the analyses will be done to assess how the storage and cold-water pool criteria are met across alternatives.	Clarified that frequency analysis to assess how storage and coldwater pool criteria are met by each alternative is presented in Appendix L Attachment L.
In section 7 on Uncertainty, Reclamation developed a special study plan to answer a couple of key questions. The new models may be used alongside or combined with existing TDM models to evaluate the effects of operations and the Panel supports such efforts. At the time of this review, such information is forthcoming and not included in the document provided to the Panel.	No response identified.
(C) Climate change, including CALSIM-3 (multiple documents)	*N/A*
Material reviewed: a) Appendix F, main report, b) Appendix F, Section 1-1, CALSIM-3, DSM2 and HEC5Q Modeling Simulations and Assumptions, c) Appendix F, Section 1-2 Callout Tables, and d) Attachment 1-1, Climate Change	No response identified.

Comment	Response
Based on section F.3.1 of the main report of Appendix F, climate change impacts representing 2022±15 median climate conditions and 15cm of assumed sea level rise were analyzed by updating CALSIM-3 meteorological and hydrologic boundary conditions for LTO, including the No Action Alternative, EXP1, and EXP3. In addition, from the callout table of DSM2 in Section 2.1 of Appendix F, 2022±15 median climate conditions are also included in HEC-5Q.	No response identified.
In addition to updating meteorological and hydrologic boundary conditions, Reclamation developed further climate conditions and a set of different scenarios to review the range of uncertainty. Historical and perturbed meteorological data were used for simulating projected surface runoff, baseflow, surface water evaporation, and potential evapotranspiration variables for future periods. Inputs of CALSIM-3 for the 2022±15 median condition are described in detail in sections 2.5 and 2.6. Three sensitivity scenarios: 2022±15 hot-dry, 2022±15 warm-wet, and 2040±15 median climate conditions are described in sections 2.7 and 2.8.	No response identified.

Comment	Response
One key issue is how to represent future climate conditions. It appears that an overarching technique is perturbation. For example, for applying the 2022±15 median climate condition, rim inflows in the upper San Joaquin of CALSIM-3 were impaired before the perturbation process. The rim inflows were re-impaired after perturbing the unimpaired inflows to present future climate conditions. Missing data for some specific local project operations (impairment) was calculated as the difference between the unimpaired historical flow and the CALSIM-3 inflow time series, under the assumption that the local project operations will be the same in future climate conditions, not accounting for potential adaptation in local project operations. While all these descriptions are sound, they lack substantial techniques that are needed for the Panel to fully endorse that this approach uses best available scientific information. The approach used by Reclamation seems reasonable and may be the best available, but the Panel would need further details and some additional analysis to conclude definitively.	Reclamation does not make assumptions about third party operations. Such assumptions are speculative, so we avoid this. The analysis is meant to be comparative, not predictive. The effect of uncertain impairments is assumed be within the range of effects due to inflow changes under various climate sensitivities.
As recognized by Reclamation, climate change represents the most significant and least understood threat to Reclamation's operations in California. Section 2.10 states "limitations and appropriate use of results", essentially the same statements as used in Section 4 of the main report of Appendix F, which should be applied to all numerical models developed and applied for the LTO of a complex water resources system.	The application of these statements in Section 2.10 to other sections is clarified.
(D) Attachment M.3 American River Weighted Useable Area Analysis	*N/A*

Comment	Response
The document provided presented relatively complete methods and an initial presentation of the results. However, the results section was two paragraphs, one of which simply stated what results are found in which tables. There was little comparison among alternatives and no further explanation of why such results occurred. Several sections were identical to the text in Attachment O.1 – Coldwater Pool Clear Creek Weighted Useable Area Analysis. Both analyses were similar so some comments here (Attachment M) are repeated in Attachment O for completeness.	Revisions made.
Data used in the analysis are from Bratovich (2017). That analysis was well described and provides a solid foundation for the use in this analysis. There are only WUAs for spawning, as no reliable data on rearing WUAs exist- except for a 1980s report that was deemed unreliable. The Panel notes there are potentially usable relationships from other rivers in the region. Composite WUA curves exist for eight stretches of river and include simulations from a 93-year period of record. WUAs were estimated for spawning months for each species under each water-year type, all water-year types combined, and for each spawning month.	No response identified.

Comment	Response
Flows used in the analysis were obtained from CALSIM-3 below Nimbus Dam. The analysis assumes those flows represent flows in the downstream 10 miles where WUA was applied. An explanation that is reasonable (e.g., no major tributaries) would strengthen the justification for using these flows. Also, Bratovich created a composite WUA derived from eight different river stretches, however, it is unclear if Reclamation uses this composite WUA curve to compare across scenarios. If Reclamation does, that could be potentially problematic. While it may be defensible to roll up WUAs to create a composite WUA, it may not be defensible to work in the other direction. The Panel suggests providing a rationale for whether it would be preferable to model the flow for each of the eight sections of river under each management scenario and then rolling those results into a composite curve. A better explanation of the approach is warranted.	Additional information providing justification for using the composite curve has been added to the Methods section of Attachment M.3.
In addition, is there an assumption that the changes in flow under the alternatives must be realized and further must adhere to how the components (velocities and depth) are each assumed to affect habitat suitability? That is, if predicted values of velocity and depth were available for each alternative, and these were plugged into their habitat suitability curves (rather than flow into the composite relationship), would you get the same responses in WUA?	The WUA values derived from the flow estimates for each alternative are based on hydraulic modeling (PHABSIM/RIVER-2D) using field measurements of depth, flow velocity, and substrate at different flows and multiple sites within each river section; and their corresponding suitability’s are based on the habitat measurements at observed active redd sites.

Comment	Response
CALSIM-3 produces a monthly time step, and WUAs should be interpreted as monthly averages, which "faithfully represent the average conditions affecting fish". Therefore, Reclamation states that using monthly averages is justified as "acceptable". Acceptable for what? It is acceptable for ensuring that the data and interpretations match in terms of their time step, and that we do not assume more specificity than the models can produce. However, it is not acceptable to assume that monthly average conditions describe spawning success because stochastic events and/or daily variables, such as scour from flood events, drops in water surface elevations, etc., all operate on smaller time step than monthly, and influence spawning success. The Panel cautions against assuming monthly time steps are adequate for an effects analysis on spawning success. However, there is substantial literature on what scales are relevant to fish (e.g., Witman et al. 2023, Thompson et al. 2013, plus many others).	Further discussion of this caveat to the results is included in the assumptions sections for all WUA analyses. This discussion notes that "the channel characteristics of the river, such as proportions of mesohabitat types, during the years of field data collection for the WUA study have remained in dynamic equilibrium to the present time and will continue to do so through the life of the Project." Effects of short-term drops in surface elevation are addressed in Attachment M.1, American River Redd Rewatering.
There is very little interpretation or description of the results provided, other than to show the plots and make mention of the following notable findings: (a) for Steelhead, WUA increases from wetter to drier years, for all scenarios, (b) for Chinook, the maximum WUA occurs in the above normal water-year types, and reaches a near minimum WUA in wet water-year types, and (c) there appears to be very minimal differences among alternatives and no statistical or ecological comparisons are provided.	Interpretation is provided in the BA effects analysis and appropriate NEPA documents. See Attachment M3. Additional response will be provided in the next BA draft.

Comment	Response
While the plots provided are easy enough to compare, the Panel suggests that tables be created to help with interpretation. It would be helpful to provide tables that outline percent decreases or increases from the No Action Alternative, or whichever baseline comparison is required. It is unclear how Reclamation plans to handle tradeoffs in model output among species or river segments, etc., or how Reclamation plans to integrate WUA metrics into the assessments of impact at the population scale. This may be discussed elsewhere, but in general, the connections among separate analyses were not very clear.	Attachment M.3 has percent difference comparisons in tables for the DEIS results. Each appendix is a standalone, and results are carried into the NEPA and ESA documents to evaluate stressors, limiting factors, and threats that are affected by the alternative. The connection among separate analyses should be in the NEPA and ESA document sections related to the specific species, river, and stressors (e.g. reduction in spawning from increased flows).
There is no discussion section in this document. There should be an explanation that makes the connection between the main spawning season for the species of interest so that one can see how alternatives not only compare among year types but also across months and how that maps onto the peak or tails of the spawning season. From there, the Panel (and Reclamation) could understand whether the alternatives promote the tails or center of the spawn time distribution which has important implications for genetics and population dynamics.	Results showing differences among the alternatives in spawning WUA over the months of the spawning seasons are provided in Appendix M (see Notes) and Attachment M-3.
The results and discussion sections should both be expanded upon to provide an explanation for why the presented responses occurred across the different scenarios. As the methods currently stand, it would be impossible to know which of the habitat characteristics used in the WUA are responsible for increases or decreases in WUA under each scenario. For example, there are a lot of outliers in the Chinook figures, but no discussion as to what is going on there compared to the Steelhead plots. Why are the Chinook so much more variable? Is it a model convergence issue? Something about the Chinook input data?	Outliers are computed as a function of the interquartile differences. Therefore, the greater number of outliers in the Chinook salmon figures may result from reduced interquartile variability rather than signifying greater overall variability. Species-specific interpretation is discussed in the NEPA and ESA documents. We have revised this attachment to include more information about the ecological basis for the modeled increases or decreases in WUA under each alternative.

Comment	Response
Lastly, it would be helpful if the scenario flows used as inputs to the WUA analysis could be placed upon the composite WUA curves so we can visualize how they differ from one another. There needs to be an explicit paragraph linking the WUA responses to the Proposed action alternatives with respect to flow, not just name.	Additional information is provided in the Folsom Cold Water Pool Appendix.
(E) Attachment I.1 Negative Binomial Salvage Model	*N/A*
The document provided was a draft version that was incomplete.	No response identified.
Why was a different statistical method used here as compared to LTO Appendix I – Attachment I.2, OMR salvage-density Model Loss? This approach seems (at least appears) to be better. Results for both facilities are combined here but kept separate in Attachment I.2.	Multiple lines of evidence were completed to evaluate OMR effects of performance metrics identified by agencies and interested parties. Different methods were used, some of which provided different outputs. In this example, the salvage density model uses historic loss and independent facility-specific exports while the negative binomial model uses combined facility exports and other selected variables.
Where was catch per unit effort (CPUE) measured? This relates to how well CPUE would index abundance that is vulnerable to the pumps.	CPUE is measured at the specified monitoring sites, as documented in the Negative Binomial Appendix.
The analysis uses a clever treatment of explanatory variables to try to get more precise estimates of salvage for the alternatives. Another positive aspect of the analysis is the cross-validation using a standard sub-setting of data.	No response identified.

Comment	Response
The results section consists of narrative text describing the results in the tables in great detail. The plots shown are very helpful and a similar format should be used in other analyses (e.g., Attachment I.2). While this is a reasonable first step and provides the basis for plotting and comparisons, synthesis of the results, especially to compare among alternatives, is needed. A strategy for synthesizing the results across analyses (i.e., operationally becomes across attachments) is also needed to effectively communicate the tabular results and compare alternatives. For example, this analysis combines the facilities while they are kept separate in Attachment I.2. There may be a logic to this but without any explanation, it adds unnecessary differences when comparing alternatives across analyses and can be viewed as a lack of coordination within the analysis team. Standardizing the results to use similar plotting across analyses would be the simplest way to increase consistency. This can be done by adding such summarizations (text and plots) to the end of the appropriate Attachments, and then using those in the synthesis.	Noted. As this line of evidence is developed in the future, we will evaluate using facility-specific salvage rather than cumulative salvage in planning efforts. The value of further refinement of salvage-related results may be considered in the PA adaptive management program related to Chinook salmon OMR management.

Comment	Response
The plots nicely show which months have high salvage, but this also tends to reduce the differences among alternatives, which is the purpose of the analysis. For example, are the differences among alternatives, which look small for a given month, important? When Alt1 is added, a different pattern is seen in Figure 2. A similar scaling of the y-axis is appropriate, and it seems another layer of presentation is needed to guide the reader. The Panel had difficulty comparing alternatives except for very dramatic differences in some plots. Reclamation should explore graphical ways to display this type of results. It would be relatively easy to select a month (March or April or whichever is highest) and make plots that highlight differences among alternatives – the same plots used in other analyses when annual values are shown (here also as a single value, but monthly rather than annually) would help with the interpretation of these results and add to the synthesis. For example, Figure 5 is scaled by the very high values in the wet year, which then compresses the values for all other water-year types. Scaling the y-axes to show patterns while also striving for as much consistency as possible across plots when so many plots are involved is difficult. Sometimes the best solution is to simply provide different versions of the same plot. Note this was done already in some attachments to display results for EIS versus the BA. Duplicate plots can be separated by putting the plots needed to compare alternatives into the synthesis section of each attachment.	In the future when this method is considered again, Reclamation will explore alternative graphical displays of results including the suggestions in this comment to allow results to be displayed for similar time steps (e.g. monthly). The value of further refinement of salvage-related results may be considered in the PA adaptive management program related to Chinook salmon OMR management.
There should be some thought and narrative given to what is the smallest difference that is meaningful. Variability is shown but does not seem to be used in the draft text in the results.	When considering future use of this line of evidence, Reclamation may consider adding the variance numbers as part of the results by determining what may constitute a meaningful difference from the literature and with agencies and interested parties.
(F) Attachment I.2 Old Middle River Salvage-Density Model Loss	*N/A*

Comment	Response
The document provided was a draft version that was incomplete. The discussion of results was limited to describing the results that were presented in the tables; no figures or plots were presented. The results consisted of 256 pages of tables that were poorly formatted (e.g., an entry of "29%" was spread over three lines). However, the Panel can offer some observations and suggestions as the information provided was sufficient for the Panel to decipher how the analyses were done and how they will likely be used. However, endorsement and definitive comments are not possible without seeing the interpretation.	No response identified.
The analysis method is straightforward. Daily salvage numbers (2009-2022) are used as number/TAF exported for a month and multiplied by the monthly export from CALSIM-3, separately for the CVP and the SWP. This resulted in monthly salvage estimates for 1922 to 2021 by species, by pump, and by alternative.	No response identified.
The results section consists of narrative text describing the results in the tables in great detail in very long paragraphs. While this is a reasonable first step and provides the basis for plotting and comparisons, there was no synthesis or plots of the results. The text mimics the tables just in text form. The Panel presumes this type of presentation is being prepared and will be added later. It was not practical for the Panel to examine the results for reasonable interpretation because of difficulties in determining how the alternatives differed. The Panel did note there were many zeroes for year-month combinations. A strategy for synthesizing the results is needed to effectively communicate the tabular results and compare alternatives. Likely, an additional variable that combines the two pumps will be helpful. Summarizing the results using plotting as in other analyses would be the simplest way to improve consistency.	Key takeaways are presented in the OMR Appendix. Reclamation will explore alternative graphical displays of results including the suggestions in this comment to allow results to be displayed for similar time steps (e.g. monthly, annually). The value of further refinement of salvage-related result presentation may be considered in the PA adaptive management program related to Chinook salmon OMR management.

Comment	Response
There should also be some thought given to what is the smallest difference that is meaningful. There were no variance estimates included anywhere in the analysis. Statistical variance is available but not reported and, further, statistical variance is only one component of the variance that can tell you meaningful differences.	When considering future use of this line of evidence, Reclamation may consider adding the variance numbers as part of the results by determining what may constitute a meaningful difference from the literature and with agencies and interested parties.
Multiple paragraphs start with the over-generalized sentence that the salvage density model, calculated across all water-year types for each month and all alternatives, has a wide range. In a few cases, it appeared to the Panel that the range shown was actually a “small” range rather than a “wide” range. The useful statement of results, when present, then occurs somewhere buried in the paragraph.	Reclamation will explore alternative methods of descriptive text for results including the suggestions in this comment.
(G) Attachment I.5 Survival, Travel Time, and Routing Simulation Model (STAR)	*N/A*
The STARS model (Survival, Travel time, and Routing Simulation) is an individual-based simulation that predicts the survival, travel time, and entrainment of juvenile salmonids migrating through the Delta. Estimates are based on acoustically tagged, hatchery-origin late-fall Chinook salmon from 2007-20116.	No response identified.
Daily flow inputs are obtained from monthly CALSIM-3 outputs (set constant across days). We were not provided with documentation for the model (refers to Perry et al. 2018, which we did not review), but we understand that the spatial structure of the model is coarse, with juveniles moved between eight polygons. One source of variation in entrainment and survival stems from the scheduling of open vs. closed Delta Cross-Channel (DCC) gates.	No response identified.

Comment	Response
The results reported show that routing of more juveniles into the interior Delta leads to lower survival. STARS seemed to distinguish different flow alternatives when used in previous analyses, showing that alternative monthly flow regimes specified by the 2019 Biological Opinion elevated survival compared to the baseline by routing fewer juveniles into the interior Delta. These regimes corresponded to winter months with greater Delta inflows, especially from Sacramento River inflows. Gate configurations also differed among alternatives on a sub-monthly basis. How gate-closure schedules differed among alternatives may be described elsewhere.	No response identified.
No interpretation is provided, so we are unsure to what extent differences in flow regimes versus gate configurations were responsible for simulated differences among alternatives. It is also possible that the timing of gate closures was responsible for pushing entry into the interior Delta away from February and toward shoulder months (December or April).	Noted. Clarification that all alternatives have similar gate operations, and thus results do not capture survival differences due to timing of fate closures.
Comment 1. As pointed out in the Appendix, unless daily variation is reflected in the inputs (i.e., not monthly average flows), the STARS model will not simulate the effects of pulse flows to stimulate migration timing or any effects of sub-monthly Reclamation operations. Thus, the effects of flow on migration and survival may not be simulated at an adequate temporal resolution, a concern with how the STARS model was implemented for the applications.	Clarified that this issue is described in the caveat and assumption section.
Comment 2. The Panel recommends updating this analysis to use more recent calibration data (see for example Hearn et al. 2014). The short time-period used in developing posterior distributions is less than one ENSO cycle and likely does not include a wide range of water-year types or climate-related shifts from more recent decades.	In the future when this method is considered again, Reclamation will explore re-calibration of the model considering ENSO cycle duration and the range of water-year types reflected in the calibration data. The value of further refinement of flow-related effects modeling may be considered in the PA adaptive management program.

Comment	Response
Comment 3. The Panel recommends updating this analysis using data from the right ESU (winter-run versus hatchery late-fall). At the very least, information regarding differences in timing and in the sizes of juveniles of the two races at the time of migration should be reported in any analysis that requires borrowing parameters from another race or species. Winter-run emigrate through the Delta from September through June and, according to Williams (2012) travel slowly, appearing at the pumps mostly in February-March. By contrast, evidence suggests that late-fall run juveniles travel in the fall (peaking in October). Williams (2012) describes great life-history variation among Chinook races in migration timing, size, development (fry vs. smolt) at the time of migration, and length of estuary residence. In addition, they claim that hatchery releases are generally larger and therefore would travel faster and (presumably) experience lower predation mortality, although they also report that winter-run juveniles at the pumps averaged 120 mm, which is large. We note that late-fall Chinook were not covered in Appendix C – Species Spatial and Temporal Domains, and perhaps should be. Clearly, modeling contingent decisions leading to variations in migration behavior related to estuary rearing and size at migration can quickly become a very complex modeling exercise, and as modelers, we appreciate the need to keep it as simple as possible, but some justification for borrowing information from hatchery late-fall run juveniles should be given.	The STARS model was run using the parameterization based on late-fall-run Chinook salmon (e.g., Perry et al. 2018), and not the parameterization based on winter-run Chinook salmon (e.g., Hance et al. 2022) as the winter-run parameterization wasn't available at the time of the analysis. In the future, when this method is considered again, Reclamation will explore using the parameterization from the winter-run Chinook salmon. The value of updating the analysis for winter-run Chinook salmon may be considered in the PA adaptive management program related to winter-run Chinook salmon through-Delta survival targets and spring pulse flow studies.
(H) Attachment F.1 Maunder and Deriso in R Model	*N/A*
The use of life cycle models is an excellent approach for combining multiple effects from the alternatives within the life cycle of Delta smelt in order to express the effects at the population level.	No response identified.

Comment	Response
The document provided was a draft version that was informative but still incomplete. There was a short discussion of results comparing alternatives, but this was limited to one paragraph that only referred to Table 3 (never refers to Tables 4-6) and did not refer to any of the figures showing results (no reference to Figures 3-6 in the text). There seems to be two sections labeled "Results" with the latter one having two bullet points that are conclusions.	Additional text and reference to the figures has been added to the results section. The duplicate results heading has been removed.
The text states "important differences between the original M&D model and the application of Polansky et al. nevertheless remain, and include model structure, surveys used, inference method, covariates tested and consideration of density dependence; these differences are summarized in Table 1." This is critical information needed to understand how to interpret the results of this model with the results of the other Delta smelt life cycle models. However, Table 1 is only a list of candidate covariates included in model selection. A key table seems to be missing.	Table has been added and numbering updated accordingly.

Comment	Response
The text is inconsistent as to whether density dependence was included or not in the transitions. At the end of the first paragraph in the Model Development section, the document states that “all transitions were assumed to be density independent.” But then results from density-dependent versions are discussed as if density-dependent relationships were developed and included. For example, the text later states: “The stochastic approach involved random selection of two covariates per transition from the complete set of candidates (Table 1) and random selection of which, if any, life stages were subject to density dependence (options for density dependence were weighted such that there was equal probability of no density dependence and any density dependence).” Also later: "The overall “best” model identified after application of the hybrid stochastic-stepwise model selection process included South Delta Secchi depth and Beverton-Holt density dependence for the sub-adult survival transition.” Additionally, Table 2 shows the best models with density dependence. Perhaps the fitting and selecting of the best model included density-dependence but then a density-independent form was used for comparing alternatives? The Panel recommends the use of the density-independent version as more appropriate for this analysis because of the high uncertainty about the strength and life stage when density-dependent mortality occurs and because the density-independent version will more likely overestimate than under-estimate stressor effects.	Density dependent models were considered during the global model selection process, but only the density-independent model was used for scenario evaluation. We've added text that clarifies this point.
Figure 2 which shows OMR values and outflow values used as inputs is very helpful. More plots like this are needed to fully understand the differences among the alternatives.	In the future when this method is considered again, Reclamation will explore additional graphical displays of results including the suggestions in this comment to visualize differences among the alternatives. The value of further visualizations of life cycle model results may be considered in the PA adaptive management program related to Delta smelt OMR management, spring outflow, and summer-fall habitat action.

Comment	Response
What is shown in the box plot (Figure 6) needs careful explanation. Is the geometric mean shown as the mean? What are the different values shown by the box, line, and points?	The components of the boxplot have been clarified in the figure caption.
An important result is that the geometric mean of the population growth rates (λs) was below one for all ALT-2 alternatives except EXP1 and EXP3. Only Alternative-3 generated a λ value close to one. The presentation of the results is well done, and the plots are clearly labeled and logical. Estimates of variability about the predicted λs would further add to a solid analysis. Table 3 is a nice summary.	Revision of figures to include uncertainty is not feasible at this stage but will be considered in the future.
The second bullet in the second Results section proposes an explanation for why EXP-1, EXP-3, and Alt-3 perform better than the others. The proposed explanation is that more positive OMR values and relatively high June-August outflows occurred during dry years. This should be explored further to confirm the reasons why these generated higher geometric mean growth rates. This would be valuable information for understanding these results and for when the modeling results are synthesized with other analyses.	The results section has been revised and discussion of interpretation and caveats has been moved to a separate section.
General Comments that Apply to the Maunder and Deriso model and the LCME model	*N/A*
The sharing of input data (e.g., covariates) with the other Delta smelt life cycle model (LCME) is appropriate. This does create issues with how to compare the predictions of the population growth rate of the alternatives between the two models. This issue of unclear use of multiple models is discussed as an overarching issue.	No response identified.

Comment	Response
The heavy reliance on the choice of model parameters for how well they fit multiple survey indices has advantages, but also limitations. This approach enables clear statistical fitting of the model and the opportunity to judge which version fits the best and how well it fits. This empirical basis for model building and fitting can provide confidence in predictions. The limitations arise from the assumption that the indices relate to each other in a consistent way over time that is well-captured by the transfer function and covariates. One can argue that the Maunder and Deriso model is a large and complicated (and advanced) correlation analysis rather than a life cycle model. While the model does simulate the entire life cycle, it does not use the standard framework of defining ecologically meaningful life stages (egg, larvae, juvenile, adults) and uses growth, mortality, and reproduction to transfer from one life stage to the next. Indices can include multiple life stages and thus can have a fuzzy relationship with classical life stages. Nevertheless, the Panel sees a valuable contribution from the Maunder and Deriso model, as long as the results are properly interpreted.	No response identified.
Population growth rate (λ) is used to compare alternatives. This is a useful model prediction for comparing alternatives because it shows population trends, but it is not sufficient alone as an index of future persistence or extinction risk. First, a tenet of conservation modeling (PVA) is that the variability in population growth rates is also very important when assessing extinction risk in addition to the trend itself. This is an important feature that should be considered in all the life cycle modeling efforts, and this is why stochastic models should be used.	No response identified.

Comment	Response
Lambda alone does not tell the reader the size of the population. In this model, λ is basically the ratio of population abundance in successive years so high or low λ can occur with high or low abundances. In addition, differences between λ values for alternatives have different implications for the population, depending on the status of the population. For example, a difference in λs of 0.2 would be different if the actual growth rates were about 0.2 (i.e., a doubling) versus if the population growth rates were close to one. Presentation of model predictions of predicted population status (i.e., indices) that accompany the λ values, as well as a risk of extinction based on variability around λ, would help with interpretation. The model was fit to these population indices (not to observed population growth rate). Although the Panel appreciates the caution in using predicted abundance indices, they do provide important context for interpreting the population growth rates. For example, it is possible to predict the same high growth rate at very low and high population sizes. Furthermore, high variability around growth rate can lead to a high extinction risk when the population is small (Staples et al. 2004).	Additional response metrics may be considered later. In the future when this method is considered again, Reclamation will explore additional response metrics. The value of further life cycle model response metrics may be considered in the PA adaptive management program related to Delta smelt OMR management, spring outflow, and summer-fall habitat action.
Despite the great similarity between this modeling and the LCME, it is very difficult to compare the results between the models. The plots are reasonable for each analysis but are not coordinated across the models. It is very difficult, and unnecessarily challenging, to determine the similarities and differences between results from the two models. This creates easily avoidable problems in synthesizing the results of both models.	A figure and caption of output from both models has been incorporated into the Biological Assessment chapter on Delta smelt.

Comment	Response
A question that will arise is how robust model predictions are to different covariates included. The authors discuss this issue in terms of collinearity among the covariates. Adding predictions from the lower-ranked versions that use different combinations of covariates but still have reasonable fits would be helpful, for example using multi-model inference (Burnham and Anderson 2002). This could be for a subset of results (those influential in comparing alternatives) and not needed for all results. This would build confidence in the primary modeling results that use the best-fit model. Given the high degree of overlapping methods and data between the LCME and Maunder and Deriso models, the Panel does not see an advantage in using both models unless additional work is done to determine how to properly interpret the models when used together. The two models are far from independent yet may have important differences. Their predictions cannot be treated as two independent predictions, but they deserve more confidence than the predictions from a single model. The use of multiple models is discussed further as an overarching comment.	Comment noted.
(I) Attachment J.1 Longfin Smelt Outflow	*N/A*
The document provided was a draft version that was incomplete. There was no discussion of results and there was no discussion about differences and similarities in salvage among alternatives. The text of the results starts with a placeholder to insert "Key Take Aways Here" indicating more interpretation is coming, and only presents figures and tables. The Panel can offer some observations and suggestions going forward as the information provided was sufficient for the Panel to decipher how the model was fit and how it will likely be used.	No response identified.

Comment	Response
The approach of the analysis seems statistically sound and attempts to quantify uncertainty using several techniques. The authors use advanced and appropriate statistical methods, including a Bayesian framework and model averaging. The overall fit is encouraging as a description of historical trends, and differences from earlier analyses are well documented. Table 3 is included for complete documentation. The figures are good, but an explanation of the violin plots is needed, as readers will not know the details that are being shown.	An explanation of the violin plots is provided in the figure captions.
Why is outflow labeled as "Sacramento River" water-year type (e.g., Figure 2_EIS)? Also, there is no text with the results. The Panel realized that the figure names indicated the similar plots were for EIS, the BA, and combined.	The categories for "Sacramento River water-year type" categories are on the CDEC website and included as a footnote
The pattern of the predicted Fall Midwater Trawl (FMWT) index shows little differences among alternatives and across water-year types, partially due the log scale used for the y-axis (e.g., Figure 2_EIS). Yet, there seems to be a much larger variation shown in the time series plots (e.g., Figure 3_BA). The time series of the two outflow explanatory variables should be shown. Is the lack of resolution among alternatives because outflows are so similar or because the statistical model is too uncertain? One conclusion could be that the model is not useful for evaluating the alternatives. This needs to be addressed in the results and discussion (when added). Perhaps illustrate the "power" of the model by showing how the predicted FMWT index would differ with specific values of outflow representative of the alternatives, including the credible intervals. This is different than showing the predicted index value for alternatives (made even more difficult to interpret by not presenting the outflow values by alternative).	A figure with the CalSim values used as input for the models has been added. Additionally, a second ("power") figure has been added showing how the predicted FMWT index (including the full probability distribution, c.f. credible intervals) would differ across a spectrum of hypothetical outflow values

Comment	Response
Acknowledging the log scale on the y-axis, the predictions of FMTE index are mostly between 100 and 1000, while the historical values are above that for the pre-Potamocorbula period. Is the explanation that the predictions for the alternatives used post-Potamocorbula conditions?	Yes, the predictions for the alternatives are conditional on model estimated parameters during the POD (post-Potamocorbula) regime. This is now noted in the text.
The stratification of years into water-year types here and through other analyses is a good framework for analyses and presenting results.	Comment noted.
How many years are in each water-year type for the model fitting? This could be deduced from Table 3 but should be made clear to the reader. Also, showing the observed versus predicted water-year type is needed, as that is how predictions are used later. The time series presentation is helpful but not sufficient.	A table has been added comparing historical and CalSim frequencies of different water year types to address this comment.
All high FMWT values are for before 1987 and so fall completely into the pre-Potamocorbula regime. Does this mean the model used to compare alternatives basically predicts a near-constant (and low) index value no matter the outflow? This would be the case if the model provided a good fit of the data.	No, the modeling is not constrained to predict near constant (and low) index values. A new figure has been added to present the modeled relationship with outflow more clearly.

Comment	Response
The inclusion of a variable indicating the Pelagic Organism Decline (POD) as a covariate is interesting. The assumption is POD conditions will continue into the future as they exist now. Does that seem inconsistent with the state of the system now? The predictions are what would have happened under historical hydrological conditions of 1922 to 2022, given the present configuration of the Delta and that the regimes occurred. Each year Reclamation uses outflow and looks up the regime value and predicts the index. This use of regime needs to be discussed because it seems inconsistent with other similar regression analyses applied to salvage or indices. The repeating of hydrology with the current Delta is consistent with other analyses, but the regimes may not be. Are the predicted indices for the alternative projections looking forward as they seem to be presented as? This is where interpretation of the results and a discussion would help and, when added, needs to address this.	Additional text has been added as discussion (under "Assumptions / Uncertainties") with respect to the assumptions and uncertainties for the correlation with outflow as it relates to modeling ecological regimes, vis-à-vis CalSim.
Coarse relationships between highly aggregated variables, such as the Longfin smelt index and Delta outflow, based on correlations using annual values have many potential problems. These have been repeatedly documented (e.g., Keyl and Wolff 2008, Tyler 1992, Gargett et al. 2011, Walters and Collie 1988) and provide little predictive power for new conditions not within the range of the data. Correlations fall apart with the addition of new data and as empirical models; they fail to provide a mechanistic understanding (aggregated variables result in spurious relationships). The Panel greatly cautions against the use of the LFS index and outflow correlations for comparing alternatives without careful development and a level of mechanistic understanding of the reasons for the observed relationships; useful information can be obtained but not with simple development and application.	The correlation between the FWMT Longfin Smelt index and Delta outflow has been evident over the more than fifty-year survey time series (1967–2023). Other more mechanistic (e.g., life cycle or population dynamics) models may be considered in the future when this method is considered again. Such methods may be considered in the PA adaptive management program related to spring outflow, longfin smelt science plan actions, and larval and juvenile Delta smelt OMR management.
(J) Attachment J.2 Sturgeon Year Class Index and Delta Outflow	*N/A*

Comment	Response
A sturgeon Year Class Index (YCI) is a relationship with Delta outflow, based on annual historical data, that uses White sturgeon as a surrogate species. However, the Green and White sturgeon have different life histories and little justification is given for using this information. Annual Delta outflows used in the YCI are too coarse to represent the effects of pulse flows on either the adults moving upstream or juveniles moving downstream or any seasonal upstream effects due to operations or diversions.	Text regarding the use of white sturgeon as a surrogate for green sturgeon was added to Chapter 8, Appendix J, and Attachment J2. Please also see existing text in Section J.2.2.2, Uncertainty/Assumptions in Attachment J2, for additional justification. Regarding the annual time step, while the authors agree that it does not account for effects of smaller temporal scale fluctuations in flows, the year class index is calculated at an annual time step. No sub-annual (e.g., monthly) class indices exist and may not be appropriate given the duration of the combined spawning, egg incubation, and juvenile rearing, and juvenile migration life stages.
Please see our comments above at the end of the review of Attachment J.1 regarding caution in using this type of correlation model based on annual values. Nevertheless, we provide some comments on the details of the sturgeon YCI model below.	No response needed.
The document provided was a draft version that was incomplete. The discussion of results was limited to describing the results that were presented in the tables, and there were only a few sentences that synthesized the results (labeled for the EIS); no figures or plots were presented. The Panel can offer some observations and suggestions going forward as the information provided was sufficient for the Panel to decipher how the model was fit and how it will likely be used. However, endorsement and definitive comments are not possible without seeing the interpretation.	No response needed

Comment	Response
The index is used without any variance estimates, which may be a limitation of what information is available. The index, however, shows wide fluctuations in its annual values and also includes many zeroes. This suggests other statistical methods (e.g., Poisson regression) may be more appropriate. There are standard methods for data with zeroes, and methods that accommodate rare but large values should be investigated. There were no plots showing the model fits (the Fish (2010) reference had some predicted vs observed plots but for different outflow months that suggest there is a relationship). The reference to ICF International (2016) is not very helpful for the reader in finding the details (~1,300 pages). The Panel examined the plots, which reinforced our impression that the statistical model is weak for predicting the index among alternatives within water-year types. Basically, the model predicts a decreasing index with increasing dryness and no differences among alternatives (except higher values for Alternative 3). Also, the higher values for Alternative 3 start off much different under wet conditions and, with increasing dryness, converge to the predicted index values for the other alternatives.	Please see Attachment J.2 for more full presentation of results including measures of variance. Page limitations under NEPA regulations precluded full presentation of results in the main body. Other than adding data newer than that used in Fish (2010), no attempt was made to improve model fit from Fish (2010). Page numbers have been added to the text for ICF International (2016) to help the reader.
The cross-validation analysis is a good addition and can greatly help in interpreting alternatives, although in this case there are no real differences except for clearly higher values for Alternative 3 under wet years.	Comment noted.
As presented, the use of regression modeling to assess alternatives is questionable. With better fitting that accounts for zeroes and ensures models work well for average index values and also predict extremely high values, the use of this approach can be informative. How different are the outflow explanatory variables among the alternatives?	Other than adding data newer than that used in Fish (2010), no attempt was made to improve model fit from Fish (2010). The value of further refinement of outflow-related effects modeling may be considered in the PA adaptive management program related to spring outflow.

Comment	Response
The choice of explanatory variables (i.e., how outflow is averaged) should be documented. Table 3 in Fish (2010) shows that many other months provided very similar evidence for the strength of the relationship with the index. Since the purpose is to compare alternatives, it may be important if the alternatives differ in outflow in other months than the April-May and March-July used in the regression equation.	It may be more important, however this method (Max 201, ICF International 2016, pp 5-197-205) selected these explanatory variables. More information can be found in these citations in the attachment. The value of further refinement of explanatory variables may be considered in the PA adaptive management program related to spring outflow.
Figure 2. Plot of the log of year-class index of White sturgeon versus log of the mean daily outflow (cfs) calculated from November to February. The numbered points show select year-classes. This is Figure 2 from Fish (2010). This figure is from Fish (2010) cited in this attachment. Different months were used in the Draft Effects Analysis but these data generally show the data used in the attachment.	Comment noted.
(K) Attachment O.1 Coldwater Pool Clear Creek Weighted Usable Area Analysis	*N/A*
The general approach to habitat analysis, here and elsewhere, appears solid and based on well-described studies from USFWS.	No response identified.
Data used for the analysis is from the USFWS report for field studies conducted between 2004-2009. Spawning and rearing were evaluated using CALSIM-3 outflow data for each month of the 100-year period of record. RIVER2D was the primary hydraulic model used. Spawning WUA was assumed to be a function of water depth, flow velocity, and substrate particle size. Rearing WUA was assumed to be a function of water depth, flow velocity, adjacent velocity, and availability of cover. WUAs were developed by the USFWS for 3 sections of the creek for each species/run and life stage. Figures of these curves are provided.	No response identified.

Comment	Response
CALSIM-3 flow data for Whiskeytown Lake releases to Clear Creek were used to estimate WUA for each species/run and life stage across the management scenarios. Total weighted means for spawning and rearing WUA that combine monthly WUA results from all three stream segments were computed. This seems to match the Bratovich (2017) approach on the American River, but because of the differences in presentation, the Panel was not certain if the same method for deriving the composite relationship, and then applying the same with alternative flows, was used. Given the apparent similarities across the multiple WUA analyses, it would be helpful to see a flow chart of the process to ensure the Panel is interpreting the order of operations correctly, and for Reclamation to identify whether different methodologies are being used across different river systems.	Spawning WUA results across stream segments were derived differently between the American River (Attachment M.3) and the Clear Creek (Attachment O.1. As noted by the commenter, a composite spawning WUA curve provided by Bratovich et al. (2017) that combines eight river segments was used with Nimbus Dam flow releases for the American River. For Clear Creek, WUA results were derived using the separate WUA curves for the three separate segments of the creek and weighted sums based on frequency of segment use were used with Whiskeytown Lake releases to compute composite spawning WUA results for Clear Creek. The steps taken for the American River and Clear Creek spawning WUA results were not in the same sequence, but their computations are equivalent except for the weighting of the Clear Creek segments. The American River segments were weighted equally because the segments are much closer to one another than those in Clear Creek, with more similar habitats.
The use of monthly information again raises the question of what level of aggregation is reasonable to use. While the Panel understands that is what is generated from CALSIM-3, perhaps such a coarse scale is simply too limiting? There is substantial literature on what scales are relevant to fish (e.g., Witman et al. 2023, Thompson et al. 2013, plus many others).	Assumptions / Uncertainty section of Attachment O.1 discusses use of monthly information. Note that a daily timestep flow model of Clear Creek is not available.
Is there an assumption that the changes in flow under the alternatives must be realized and adhere to how the components (velocities and depth) are each assumed to affect habitat suitability? That is, if predicted values of velocity and depth were available for each alternative, and these were plugged into their habitat suitability curves (rather than flow into the composite relationship), would you get the same responses in WUA?	Yes. The WUA values derived from the flow estimates for each alternative are based on hydraulic modeling (PHABSIM/RIVER-2D) using field measurements of depth, flow velocity, and substrate at different flows and multiple sites within each river section, and their corresponding suitability's are based on the habitat measurements at active redd sites.
A major issue that almost always arises with WUA yet goes unmentioned is problems in interpretation. WUA is an index of habitat capacity not realized spawning.	This issue is identified in the Assumptions / Uncertainty section of the attachment.

Comment	Response
The Panel suggests that more explanation be provided about the role and source for the weighting factors applied to each river segment for each scenario for Chinook. The document refers to Appendix C, Figures 35 and 36 for the weighting factors. These figures appear to be for O. mykiss, not Chinook, and are the proportion of redds by date and river mile. The Panel recommends Reclamation carefully review weighting factors for each WUA to ensure that all inputs to the analyses are sufficiently documented.	All scenarios use the same spatial and temporal weighting factors. Appendix C and the literature cited in the Attachment references are the sources of all the information used. In the future when this method is considered again, Reclamation will explore how to handle uncertainty in the context of weighting factors used. The sensitivity of predictions to these uncertainties may be considered in the PA adaptive management program related to tributary habitat restoration effectiveness for salmonids fishes.
There is an enormous amount of data that is being pulled/available in Appendix C, and the paragraph describing which data was used is incomplete.	Refer to Response to Comment in Row 119. BA text updated.
CALSIM-3 flow data at Whiskeytown Reservoir outflow is described as appropriate for application to Clear Creek's entire length because there are only minor tributaries, which only influence Clear Creek under high runoff conditions. The citation for this is weak, as it is listed as "USFWS?". Also, it is unclear what the definition for "high runoff conditions" is, and whether that should apply or affect Wet Year scenarios.	Added text justifying use of Whiskeytown outflow to characterize typical flow regime for Clear Creek to the Sacramento River confluence.
The uncertainties are the same for the American River across all WUA analyses, there is no explanation of how Reclamation plans to handle the uncertainty beyond identifying it, or how they plan to interpret the uncertainties with respect to the results. This should be included in discussion sections.	In the future when this method is considered again, Reclamation will explore how to handle uncertainty in the context of the results. The sensitivity of predictions to these uncertainties may be considered in the PA adaptive management program.
A new uncertainty listed for Clear Creek is fixed spawning periods were used to determine the effects of changes in flow on spawning. However, the time of spawning varies among years depending on flows. This feedback mechanism is not accounted for in the model, however, because spawn timing is a conservative, genetically controlled trait (Quinn 2005), the impact of this uncertainty is likely to be small. The Panel suggests a check on this rationale.	Fixed spawning periods were used for all rivers, although for spawning WUA they received different weightings in computing the WUA sums. The uncertainty is unknown, and its impact would be difficult to assess. This issue is discussed in the Assumptions / Uncertainty section of Attachment O.1

Comment	Response
The discussion section regarding the validity of WUA analysis is the same across WUA models, with a minor, one-sentence modification explaining that improvements made to WUA models exist but are not currently available for the river of interest. This is repeated for each WUA, but there is no attempt to explain how one should take these limitations into account with respect to the specific scenarios being examined. Given that new models exist, what are the biases they correct? And how should managers then think about those biases with respect to the model comparisons at hand? Does the model consistently over or underestimate WUA under high or low flows? Are the WUA models fitted in the early 2000s representative of a small subset of environmental conditions (i.e. low flow years or high flow years?), or do they capture a wide distribution of flow conditions to which fish can then respond?	In the future when this method is considered again, Reclamation will explore how to handle uncertainty in the context of the results. The sensitivity of predictions to these uncertainties may be considered in the PA adaptive management program related to tributary habitat restoration effectiveness for salmonids fishes.
Results, figures, and tables are presented. The results section was a series of tables and figures only. There is no explanation or narrative to go along with the tables and figures. Results, however, are much less clear for Clear Creek compared to the American River. Box and Whisker plots are dominated by outliers, with some plots reflecting no boxes, just points, which is confusing for the reader. Interpretation of these results and of what Reclamation plans to do with these plots is needed. It appears the models for Clear Creek are unfinished, so evaluation is difficult.	The original draft of this attachment provided much of the information requested in the comment, including an explanation of the boxplots. The box plot figures showing Clear Creek WUA results are confusing because the results include only a few discrete WUA values. There are so few WUA values because, under most conditions, outflows from Whiskeytown reservoir are identical over months due to highly regulated operations. To improve legibility of the results, the boxplots are being replaced by bar charts for the Final BA.

Comment	Response
The plots are a good idea, but they need further adjustments and supporting explanations. What is being shown? Key points on the boxes, vertical lines, and points need to be clearly defined. For example, Figure 1 seems to have the blue boxes cut off, and the different alternatives by month are difficult to see with the small similar-looking symbols. Why do Figure 1 and Figure 2 use different formats? Were all points considered "outliers" and shown as individual points in Figure 2? The only difference is which alternatives are shown, which is displayed in the legend. The Panel thought there should be 100 points shown since there were 100 years of CALSIM-3 used. Where are the missing points for Figure 2?	See response to Comment #126. Note that many of the 100 values have the same value, so they only one point is visible. Plots have been updated.
There are no plots for fry-rearing or juvenile-rearing. It appears that there is no effect of water-year type nor alternatives on the two rearing WUAs for both species. The Panel could not interpret the plots for spawning WUA due to uncertainties about what is plotted.	See response to Comment #126. Note that there are some large differences between the alternatives and the NAA in rearing WUA for both species.
Results need to be explained in relation to projected flows, and there needs to be a discussion that incorporates uncertainties and explains why there are greater effects in some conditions and lesser effects under other conditions. There also needs to be an explanation of how these results build off previous models (i.e. CALSIM-3), how uncertainties are carried forward from previous models, and how the results and uncertainty from the WUA models will be carried forward to the next model they inform (presumably life cycle models).	In the future when this method is considered again, Reclamation will explore how to handle uncertainty in the context of the results. The sensitivity of predictions to these uncertainties may be considered in the PA adaptive management program related to tributary habitat restoration effectiveness for salmonids fishes.
(L) Attachment F.5 Delta Life Cycle Model with Entrainment (LCME)	*N/A*
The use of life cycle models is an excellent approach for combining the multiple stressor effects from the alternatives with the life cycle of Delta smelt and expressing the overall population-level response.	No response identified.

Comment	Response
The document provided was complete and well-written with an excellent presentation of modeling results. The modeling reflects the long history of development and refinement of the LCME, and the diligent attention to detail by the authors. The inclusion of the memos to the Collaborative Science and Adaptive Management Program (CSMAP) on key topics related to the modeling is very helpful.	No response identified.
The conversion of alternatives to their food effects is quite complicated and can be followed with careful reading. A concern is that the multiple layers of averaging may act to eliminate important differences in food among alternatives. The authors used a bottom-up approach starting with many prey species in spatial boxes and collapsing until a single metric per year was obtained. The two final metrics used as covariates were food in January-February for late-sub adults and in March for early adults. Many steps are involved from CALSIM-3 predicted salinity changes to changes in these yearly food metrics. Another approach might be to develop relationships using aggregated zooplankton data. That is, work at the level of the LCME rather than at a more resolved level that is then collapsed into a yearly value. Other modeling has shown the importance of food to Delta smelt population dynamics and λ (Kimmerer and Rose 2018) so the apparent insensitivity of the LCME to food differences from the alternatives warrants further investigation. It may be the correct result or may be due to the need to summarize the information with so many steps and decisions that differences in food metrics among alternatives important to Delta smelt growth were greatly reduced.	No response identified.
Why are the food covariates not included in Figures 5, 6, and 7? Showing all covariates is needed.	The food covariates are labeled as "food biomass" and are already included in the figures.

Comment	Response
The general pattern of results seems to be that most alternatives generate similar growth rates and the few that differ result in higher growth rates in already good years. Figure 9 summarizes the overall result and the time series plots of growth rate show the bump in the few cases they occur, in good years.	No response identified.
The Panel had several high-level cautions about the LCME. These were already listed in the Panel comments on the other life cycle model (Maunder and Deriso) that uses a very similar approach to the LCME. The reader is referred to the comments under General Comments that Apply to the Maunder and Deriso model and the LCME model in the review labeled H.	No response identified.
Conclusions about the use the Delta smelt life cycle models	*N/A*
As stated in the review of the Maunder & Deriso model, despite the great similarity between the two modeling approaches (e.g., same years, shared covariate data), it is very difficult to compare the results between the models. The plots are reasonable for each analysis but are not coordinated across the models. It is very (and unnecessarily) challenging to determine the similarities and differences between the two models. This lack of coordination in presenting the results creates easily avoidable problems in synthesizing the results of both models.	A figure of results from both Delta smelt models has been added to look for similarities in response metrics. If these tools continue to be used in the future, we will consider further identifying important response metrics and comparing through the PA adaptive management program related to Delta smelt management actions.
Given the high degree of overlap in methods and data between the LCME and Maunder and Deriso models, the Panel does not see an advantage in using both models unless additional work is done to determine how to properly interpret the models when used together. The two models are far from independent yet may have important differences. Their predictions cannot be treated as two independent predictions, but they deserve more confidence than the predictions from a single model. The use of multiple models is discussed further as an overarching comment.	No response identified
(M) Attachment I.4 Longfin Smelt Salvage Old Middle River Relationship	*N/A*

Comment	Response
The document provided was a draft version that was incomplete. There was no discussion of results nor about differences and similarities in salvage among alternatives. The text ended with the presentation of figures and tables. The Panel can offer some observations and suggestions going forward as the information provided was sufficient for the Panel to decipher how the model was fit and how it will likely be used.	No response identified.
A rationale should be provided as to why years with high salvage were selected. Usually, the widest spread in the data is best for fitting regression models. There is no need for contiguous years to be used, so are there years from the past that can be added to the dataset? The gap between 1,000 to 5,000 cfs is particularly critical to fill as best as possible. That is where the alternatives are operating yet there is no data for the model fitting. While there is an attraction to using the same data as Grimaldi, that paper was published in 2009. Updating the analysis is needed, even if it confirms the earlier model because, at minimum, there will be more confidence in the predictions allowing for a better comparison among alternatives. Can years since 2008 be added with a covariate since they are post-Biological Opinion (2009)? Can any earlier years be added that have high or intermediate salvage?	In the future when this method is considered again, Reclamation will explore validation methods. These methods may be considered in the PA adaptive management program related to Longfin smelt OMR management.
The stratification of years into water-year types here and through other analyses is a good framework for analysis and presenting results.	No response identified.
How many years are in water-year types for the fitting? Is it 13 years spread among 5 types? The Panel suggest Reclamation show the years for fitting by type.	The analysis uses the same dataset used in Grimaldo et al. 2009. Figure 2 in Grimaldo et al. shows the differences in physical variables used to examine fish salvage for the 13-year period for different seasons. This figure includes mean monthly X2 position for the 13-year period. Added a reference to the figure from Grimaldo et a. 2009 in the text.
What is meant by "annual estimates were made for the mean upper and lower 95% prediction limits of salvage estimates"? The Panel did not know what this meant and results using this were never shown.	Clarified text.

Comment	Response
What is shown in the box plots? Annual values grouped into water-year types from 1922-2021? And what is shown on the boxes?	Figure captions for both box plot figures now read: "Predicted median, quartile and interquartile ranges of longfin smelt salvage from April-May at USBR and CDWR facilities by alternative and water year type, predicted from CalSim3 Old and Middle River simulated flows. Figure displays data given in Table I.4-1"
There was little difference among the alternatives in OMR for the No Action Alternative, versions of Alternative 2, and Alternative 4. Yet, there seem to be larger differences in predicted salvage across some alternatives with similar OMR flows. How can this occur if the OMR is the only explanatory variable in the regression equation? While there is a monotonically decreasing relationship between salvage and OMR, there seems to be an important interaction between water-year type and OMR. Is it the variability in OMR? This is where the interpretation of the results and a discussion would help and when added, needs to address this.	Table I.4-2 has also been updated. Table I.4-2 caption has also been updated to reflect that this is mean predicted salvage. Clarified the results to describe variability in OMR flows during different water years to provide more explanation.
While split sample testing is not feasible, it is possible to complete a jackknife process in which you delete 1 year at a time, however, some level of validation is needed.	In the future when this method is considered again, Reclamation will explore validation methods. These methods may be considered in the PA adaptive management program related to Longfin smelt OMR management.
Why were age-0 and adult life stages combined into one regression equation? This may be a good decision, but there needs to be an explanation. Whether age-0 or adults are entrained (indexed by salvage) can have important implications on population-level impacts.	Age-0 and adult life stages were not combined into one regression equation. Language added to clarify.
How can the fit be improved as the regression is really between two clusters of points and may underestimate salvage at negative OMR flows?	In the future when this method is considered again, Reclamation will explore other modeling approaches to improve fit. This method may be considered in the PA adaptive management program related to Longfin smelt OMR management.
Where is the tabular summary and graphic display of the fit of the new equation?	The tabular summary is already provided, the p-value has been added. Figure I.4-1 has been updated to include the graphic display of fit of the new equation.

Comment	Response
Are there other variables that would be affected by the Proposed Action that are not captured with OMR? For example, turbidity is mentioned. The analysis is really about how OMR, under the alternatives, will affect salvage and should not be over-extended to how the alternatives will affect entrainment. In addition, salvage occurs throughout most months of the year, so the predictions provided include only April-May salvage and not annual salvage.	The attachment has an assumptions/uncertainty section which has a brief discussion of other variables that may affect salvage such as turbidity, population size, and different life stage behaviors. This section is meant to provide context for what this analysis is missing but does not make conclusions about entrainment.
An explanation is needed for how absolute salvage can be lower in dry years when OMR flows also seem more negative.	After transmission, a new table was added showing mean April-May OMR flows, the figure in the attachment of OMR flows are boxplots. The table values show that OMR flows are LESS negative during drier years (Table I.4-3)
(N) Appendix F Attachment 2-5 DSM2 Salinity	*N/A*
Additional material reviewed: a) Main Report, b) LTO Appendix F: Attachment 1-3 Model Updates	No response identified.
Main Report - The use of the DSM2 Delta Hydrodynamic model is fairly limited in scope for this Draft Effects Analysis. Only basic salinity modeling has been reported in the documents provided to the Panel and therefore included in this review. No major changes were made to the DSM2 model for this application. The model is appropriate for representing Delta hydrodynamics and salinity intrusion. The application of the DSM2 results for the Zone of Influence analysis is discussed in a separate review (labeled "X"). The DSM2 particle tracking simulations were not available at the time of this review.	No response identified.

Comment	Response
To account for sea level rise and the associated salinity intrusion, the western boundary of the DSM2 model (at Martinez) is driven by a tidal stage boundary and salinity. Salinity measurements were based on a flow-salinity correlation developed from simulations with a multi-dimensional hydrodynamic model of San Francisco Bay and the Delta that incorporated the assumed level of sea level rise. That multi-dimensional model has a tidal boundary on the ocean side of the Golden Gate Bridge with a constant ocean salinity value applied at that boundary. This approach to account for sea level rise and associated salinity at the DSM2 western boundary is well known and has been used in other studies.	Comment noted.
For the Draft Effects Analysis, "the DSM2 models assume a 15 cm increase in sea level rise. The Martinez electrical conductivity (EC) boundary condition is modified to account for the salinity changes related to the sea level rise using the regression equation derived based on the three-dimensional (SCHISM) modeling of the Bay-Delta under the future conditions with 15 centimeters (0.5 feet) sea level rise. The hydrodynamics and salinity changes in the Delta due to sea level rise were determined from the SCHISM three-dimensional Bay-Delta model simulations based on 2009 through 2010 historical hydrology. SCHISM results for changes of stage at Martinez were dominated by a scalar shift of about 0.5 feet. Given that the magnitude of the phase shift is very small relative to the DSM2 time step, it was assumed that 0.5 feet sea level rise would lead to 0.5 feet incremental change at Martinez with no phase shift" (Appendix F Modeling Attachment 1-3, Model Updates, p. 13).	No response identified.

Comment	Response
DSM2 flow boundaries are based on the monthly flow time series from CALSIM-3 (Appendix F Modeling Section 1-1, CALSIM-3, DSM2 and HEC5Q Modeling Simulations and Assumptions, p. 14). The timing operation of the DCC gates matters for salinity intrusion. Specifying that the DCC is closed for a certain number of days within a month is rather vague. DSM2 is a hydrodynamic model for an estuarine system. Therefore, tides matter and results are important on a tidal timescale. Yes, the flow inputs will be monthly values specified by the CALSIM-3 simulations with climate change adjustments. But the valuable information is at the tidal timescale. The anticipated particle tracking simulations need to be reported on a timescale of less than 1 month.	Numerical models developed and applied for the LTO are generalized and simplified representations of a complex water resources system. The models are not predictive models of project operations and results cannot be considered as absolute with a quantifiable confidence interval. The model results are only useful in a comparative analysis and can only serve as an indicator of conditions. Due to the assumptions involved in the input data sets and model logic, care must be taken to select the most appropriate timestep for the reporting of model results. Sub-monthly (e.g., weekly, or daily) reporting of raw model results is not consistent with how the models were developed, and results should be presented on a monthly or more aggregated basis. Models are supposed to be comparative, not predictive.
Issues to consider related to DSM2 modeling are: (a) appropriate use of the model in the Zone of Influence modeling (separate review), (b) reporting model results on a subtidal time step is appropriate for particle tracking and used to estimate entrainment (PTM not include in review material), and (c) binning results based on the volume of water entering at the boundary of the Sacramento and San Joaquin rivers (low, medium, high) is appropriate and recognizes that the Sacramento and San Joaquin rivers are distinct parts of the Delta system with unique water quality characteristics. For example, see the Zone of Influence modeling where high/high, medium/high, medium/medium binning was applied (Appendix I, Old and Middle River flow management, p. 3).	No response identified.
(O) Appendix F Attachment 2-11 HEC5Q	*N/A*
Additional material reviewed: a) Appendix F Modeling, Section 1-1, CALSIM-3, DSM2 and HEC5Q Modeling Simulations and Assumptions Although HEC5Q has been widely used for these California reservoirs, this model was significantly changed to operate the model in forecasting mode for this Draft Effects Analysis.	The model was not used in a forecast or predictive mode. It was intended for relative comparison between operational scenarios.

Comment	Response
To date, an impressive amount of programming effort has been made to convert the HEC5Q model into a tool that can be applied to this application, which is very different from the standard operational use of this model. This effort included extending input data, filling in data gaps, creating a more realistic representation of Shasta Temperature Control Device operations, and changing techniques for iterative processes that were previously done manually. The old manual approach involved making adjustments that were dependent on the modeler's judgment. While making these changes, the modelers have also found calibration errors that prompted a major review of the calibration of the model. Because of this major calibration, a calibration review document should be cited and available for reference.	Reclamation did not recalibrate the model. HEC5Q was calibrated by RMA in 2003, this calibration is referenced in attachment 1-3 Model updates.
The temperature modeling is a critical component of the overall analysis for both the BA and the EIS. Simulating Shasta Coldwater pool volume in addition to downstream temperatures in key spawning habitats is critical information. HEC5Q is a powerful modeling tool that produces an enormous amount of valuable output data at a 6-hour time step. It is vital that this information is synthesized in a format that best supports the needs of the ESU evaluation. As an example, the model output available to support evaluating key spawning habitat includes the maximum temperature experienced and the exposure duration exceeding a maximum temperature criterion.	No response identified. Operational decisions are made on a monthly time step. HEC-5Q can run at 6-hour time steps for scenarios where operational and temperature logic requires. For scenarios where this is not required, it is not included to maintain computational efficiency.
To follow the "Appropriate Use of Model Results" guidelines (Appendix F Modeling, p. F-8; pdf p. 12), the HEC5Q model results are presented as mean monthly temperatures with the probability of percent exceedance at sampling stations. Other approaches to statistically present the data need to be considered. In addition to the mean temperature, please report the range of the temperature output (10% and 90% range) as well as the number of successive days of exceedance per month.	Additional analyses and graphics

Comment	Response
The modelers state that temperature modeling for the EXP1 temperature simulations is faulty for significant numerical modeling reasons. This scenario created numerical challenges due to the very low storage utilized by the HEC5Q basin models outside of their intended range of inputs. The Panel recommends that Reclamation not plot the EXP1 temperature simulation output data alongside all the other alternatives. The EXP1 temperature output is not in the same range as the other alternatives and the differences have already been identified as errors from the model representation of this unique configuration.	Differentiate EXP1 trace to indicate uncertainty.
The Panel notes the following quote: "*The only definitive method to fully resolve the HEC5Q numerical issues under the EXP1 operations logic would be to re-architect the HEC5Q model engine itself to correct the problematic algorithms. Such an undertaking is not within the scope of the 2021 LTO and would require full revalidation/recalibration of the HEC5Q basin models. It was therefore decided to utilize an approach to minimize the numerical issues within the current HEC5Q model engine*" (Appendix F Modeling, Attachment 1-3 Model Updates, p. 25).	No response identified.
Much more synthesis of results is needed. For example, when a critically dry year fails to meet the set criteria, what are the underlying mechanisms? For example, Was this year part of a multi-year critically dry event? Was there a carry-over in stratification from the previous year? Was this a simulated year where there was missing data and a default value was applied? The Panel recommends looking for themes to identify how to improve both the temperature model and the operations guiding criteria.	Such cause-and-effect analyses are done when developing the alternatives analyzed, however for the comparison of used alternatives summary statistics are provided.

Comment	Response
The preprocessing of CALSIM boundary data needed to be changed with the switch over to CALSIM-3 format. The time series interpolation was changed and temporal downscaling using spline interpolation was applied to the monthly time series. The Panel noted that the spline interpolation did not preserve monthly volumes and had to be done with a preconditioning operation that adjusts the maximum monthly magnitude until the average value of the spline matches the CALSIM monthly value. In addition, to prevent a physically unrealistic trough, the code shifted the date of the maximum magnitude backward if the magnitude changed more than a factor of two. Giving an example showing how the data was processed would greatly help the reader. How did these changes affect the original simulation results?	In the future when this model is considered again, Reclamation will explore this issue. These methods may be considered in the PA adaptive management program related to Shasta Coldwater Pool Management.
A second issue was the Meteorological Data Extension for these simulations. The Panel noted the extension of the HEC5Q to 2022, which covers recent conditions. Because California Irrigation Management Information System (CIMIS) does not provide coverage back to 1921, the period CIMIS data has been augmented to is based on water-year types to backfill for the full CALSIM period. If you are doing the simulation by individual year and not looking at year-over-year carryover, what is the purpose of going back to 1921 when there is no driving meteorological data available? Is it so you have a statistical range of temperature values to use in your wet/dry/critically dry classification? Do the water-year classifications change as climate change shifts the hydrology or do you use the historical water-year classification for binning data? How is climate change incorporated into these simulations? It has been suggested that water-year types be updated regularly to account for non-stationary future conditions (Rheinheimer et al. 2016).	The HEC5Q period of record is taken back to 1921 to align with the CalSim period of record. HEC5Q is solved per year 1922 through 2021, with the reservoir storage and thermal profile from the end of the previous year being used as the initial condition for the next year. Solution by individual year is done to converge TCD operations based on downstream compliance conditions. After every individual year has been solved, the TCD operations are combined and the fully 1922-2021 period is solved continuously to eliminate any errors introduced by the individual year solutions. TCD operations are assumed to be converged over the continuous, full period solution. Water year types are determined by hydrologic conditions across the Sacramento basin, not within the temperature modeling. Water year types are therefore fixed prior to temperature analysis. Adjustments for climate change are done by regressing air temperatures to the HEC5Q input equilibrium temperatures. Future air temperatures from the VIC hydrologic model at the utilized CIMIS stations are used to perform the adjustment.

Comment	Response
A third issue was the use of Gerber, Nicolaus, and Modesto stations. "Solar radiation, the primary variable used to calculate equilibrium temperature and the heat transfer coefficient, and the wind speeds were markedly different in both trend and magnitude between the CIMIS values and the existing HEC5Q meteorology" (Appendix F Modeling, Attachment 1-3 Model Updates p. 23). This triggered a review and the primary finding of the Reclamation review was that "the total solar radiation as measured at the CIMIS station was not being utilized in favor of top of atmosphere short wave radiation .... The differences between the CIMIS station information and the existing HEC5Q meteorology were significant enough to warrant additional consideration during the present extension. The difference due to geometric factors and wind speed could not be satisfactorily resolved" (Appendix F Modeling, Attachment 1-3 Model Updates p. 23). The solution was "revised geometric correction factors were applied to the top of atmosphere solar radiation estimates and the reduction factors were eliminated" (Appendix F Modeling, Attachment 1-3 Model Updates p. 24). The Panel understands the necessity of these adjustments and suggests Reclamation better document how they influenced predictions.	The geometric factors were qualitatively calculated such that the peak top of atmosphere solar radiation remained continuous between the existing dataset and extension period. The previously calculated solar radiation component had been adjusted upward to also include the effect of longwave radiation not other otherwise included. This is not uncommon for computational models of this vintage to be parsimonious. The remaining equilibrium temperature and heat transfer coefficient used in the model utilize solar radiation as a primary input, making them contiguous as well. Wind was maintained as provided at the CIMIS stations without modification. Given that the extension was intended to be continuous with the existing period, there is limited anticipated impact.

Comment	Response
The current presentation of the data makes extensive use of exceedance probability charts. Only one temperature per month is reported for each simulation; multiple years are combined by month. How is that temperature determined? Was it the maximum or median temperature of the time series? How many times during the month was that temperature exceeded? Importantly for fish, for how many days in a row are the criteria exceeded? A discussion of the exceedance charts in locations where there is exceedance should be discussed. What was the mechanism that caused relatively long simulated exceedances?	Numerical models developed and applied for the LTO are generalized and simplified representations of a complex water resources system. The models are not predictive models of project operations and results cannot be considered as absolute with a quantifiable confidence interval. The model results are only useful in a comparative analysis and can only serve as an indicator of conditions. Due to the assumptions involved in the input data sets and model logic, care must be taken to select the most appropriate timestep for the reporting of model results. Sub-monthly (e.g., weekly, or daily) reporting of raw model results is not consistent with how the models were developed, and results should be presented on a monthly or more aggregated basis. Models are supposed to be comparative, not predictive.
The time series of simulated temperatures for an example year should be displayed and the data processing steps used to determine the representative temperature for the month should be explained. In addition, a map of key stations (similar to BA 4-11) would also be useful. Starting with the time series, the aggregation into the final figures can be described. Even if the results do not match the observed temperature exactly, the model should still represent basic trends. Are predictions staying within a realistic range over time or are there concerning, consistent deviations? We know that the time series was broken into year-long sequences to reduce any accumulation of errors. However, what are the circumstances under which the model becomes numerically unstable? Can the model simulate 2-3 years of drought in a row when there is a carryover stratification?	Yearlong timeseries were only utilized for EXP1. All other simulations utilized yearly solutions for TCD operation convergence followed by full period of recorder runs. In 2020, a calibration memorandum for the Hec-5Q Model was developed and provides information regarding prediction accuracy and accumulation of error. This document is Reclamation 2016. Surface Water Temperature Modeling – HEC-5Q Model Update. Coordinated Long-Term Operation of the Central Valley Project and State Water Project. Final Environmental Impact Statement, Appendix 6B, Section C.

Comment	Response
HEC-5Q modeling analysis enumerates the frequency at which mean monthly simulated water temperatures exceed water temperature criteria for winter-run Chinook salmon obtained from the scientific literature. Reliance on exceedance based on monthly values seems highly questionable. While this is in keeping with the guidance on the proper use of model outputs, this analysis ignores valuable model data by not reporting daily values.	Numerical models developed and applied for the LTO are generalized and simplified representations of a complex water resources system. The models are not predictive models of project operations and results cannot be considered as absolute with a quantifiable confidence interval. The model results are only useful in a comparative analysis and can only serve as an indicator of conditions. Due to the assumptions involved in the input data sets and model logic, care must be taken to select the most appropriate timestep for the reporting of model results. Sub-monthly (e.g., weekly, or daily) reporting of raw model results is not consistent with how the models were developed, and results should be presented on a monthly or more aggregated basis. These models are comparative, not predictive.
Because biological effects of temperature are non-linear, the average of the function applied to daily values does not equal the function of the average, especially when compared to thresholds, such as maximum temperature tolerance. In our view, modeling thermal risks at a monthly resolution is not adequate for making decisions based on the potentially lethal effects of short-term extreme temperature exposures. The Anderson et al. model seems to be a reasonable approach because it uses a daily time step (i.e., realistic high-resolution variability in water temperatures).	Numerical models developed and applied for the LTO are generalized and simplified representations of a complex water resources system. The models are not predictive models of project operations and results cannot be considered as absolute with a quantifiable confidence interval. The model results are only useful in a comparative analysis and can only serve as an indicator of conditions. Due to the assumptions involved in the input data sets and model logic, care must be taken to select the most appropriate timestep for the reporting of model results. Sub-monthly (e.g., weekly, or daily) reporting of raw model results is not consistent with how the models were developed, and results should be presented on a monthly or more aggregated basis. Models are supposed to be comparative, not predictive.
(P) Zooplankton – Killer Whale (Chapter 11 Killer Whale, Appendix D Seasonal Operations Deconstruction)	*N/A*
In general, the Draft Effects Analysis for Killer Whales relied on qualitative information more than the other species. Because it differed from the other species, we review the assumptions here.	No response identified.

Comment	Response
Some key assumptions used in the analysis: a) Central Valley Chinook is 22% of sampled Chinook off Oregon coast, and 50% of Chinook of California Coast (both areas within designated critical habitat), b) 40% of Southern Resident Killer Whale (SRKW) diet when whales are off the coast of California, and 18% of their diet when whales are off the Oregon coast, c) Coded wire tags indicate 21% of returning Chinook to the Central Valley are natural origin fall-run Chinook salmon, and d) 21% is used to represent the percentage of natural-origin Chinook in the ocean from the Central Valley that can be available for SRKW diet.	No response identified.
Assumptions about prey availability as a stressor included: a) Prefer Chinook. Central Valley Chinook identified in prey of SRKW; 19% of SRKW prey collected in outer coastal waters and 5% or prey/diet items collected in Puget Sound, b) The relationship between SRKW and Chinook is getting weaker. SRKW demographics decrease regardless of varying Chinook levels (likely due to multiple interacting stressors that need to be taken into account); whales can exhibit stress when they have less access to food and multiple stressors accumulate, c) High uncertainty for winter, as data are biased towards summer and early fall months when whales are in inland waters and boat-based research activities can take place.	No response identified.

Comment	Response
Assumptions about other stressors included: a) Whales exposed to persistent organic pollutants (POPs) through prey, which they then pass on to offspring or release/metabolize when hungry, b) Vessel effects caused sound interference with hunting success, energy expenditure, social cohesion, communication, foraging efficiency, etc., and c) Oil spills would result in exposure to petroleum hydrocarbons, with resulting serious health impacts to SRKW. The Proposed Action is stated as not anticipated to change these stressors: Pollution and Contaminants, Vessel Effects, Oil Spill, or Acoustic.	No response identified.
The stressor of prey availability was anticipated to change at insignificant or discountable levels. Prey availability would be impacted by about 10% for 5 months of the year by the Proposed Action. The Panel notes that a 10% shift in the diet of an organism needs to be considered in the context of whether or not prey availability is near a starvation threshold or not. Simply saying only 10% of their diet may be affected, and therefore the effect on the whales is likely insignificant needs further confirmation. However, the analysis suggests that the Proposed Action will not affect the production of natural origin Chinook (which make up that 10% of the diet). The Panel advises that if there is any uncertainty as to the Proposed Action's impact on the natural origin Chinook, then this is a place where caution and further analyses may be needed. SRKW are surviving on near starvation thresholds, which can lead to malnutrition that causes them to metabolize fats wherein POPs are bioaccumulated. The release of these POPs reduces fecundity and survivorship of their young, leading to declining population rates. So, while the proposed action may not directly increase pollutant exposure, if it limits food resources, it can indirectly expose SRKWs to increased pollutant levels.	The quantitative assessment of change in Chinook abundance was not part of the panel review materials and is available in the final Biological Assessment.

Comment	Response
Under Critical Habitat Area the Draft Effects Analysis states that the "*final rule maintains the previously designated, but not in the action area, critical habitat in inland waters of Washington, and expands it to include certain coastal waters off Washington, Oregon, and California*". While it is true that SKRWs do not inhabit the Delta or rivers upstream, this missed the point. It is a connected hydrological system, and the key food source, Chinook Salmon, crosses between both realms as part of their life cycle. The range of Chinook consumed by SRKW includes the designated waters off the coasts of California and Oregon and is impacted within the action area. This connection should not be discounted.	Quantitative analysis of change in prey availability is available in the final Biological Opinion. Critical habitat analysis did include Chinook salmon critical habitat (winter-run and spring run) in the Central Valley.
The Draft Effects Analysis also makes the following conclusions about impacts on habitat: (a) no impacts to water quality because essential physical and biological features (i.e. ocean water quality) are not affected by the Proposed Action, and (b) no impacts are expected from Proposed Action on passage conditions to allow for migration, resting, and foraging because these actions occur in the coastal ocean.	No response identified.
(Q) Attachment F.2 CVPIA Winter and Spring-run Life Cycle Model	*N/A*
Additional material reviewed: a) 01_LTO Appendix F Modeling, b) 01_LTO Appendix F Modeling, Section F.4, and c) Appendix O – Tributary Habitat Restoration.	No response identified.

Comment	Response
Life cycle models offer the most suitable tool for scaling from operations to population-level effects. Two LCMs were reported for winter-run Chinook salmon, the CVPIA Winter-run and Spring–run life-cycle models. The CVPIA LCM is a spatially discretized aggregated state-space model with a monthly time step. The use of a stochastic PVA/life cycle model is necessary when estimating population growth and persistence, and we support the decision to use the peer-reviewed CVPIA LCM. It is impressive that such a large area has been represented in a way that incorporates available data and with potential for higher-resolution linkages to relevant drivers in the Proposed Action. The model is stochastic at the level of hydrologic year types, which makes it more suitable for representing population trends and low spawner abundances, i.e., 100, 1,000 females for Winter-run Chinook salmon.	No response identified.
The model also considers straying or diversity of spawning habitats, but only for Spring-run Chinook salmon. However, evidence suggests spawning may be occurring elsewhere and this may increase in the future with access to new spawning areas. In addition, limited information was provided about hatchery operations and how they were represented in the model and used to distinguish trends in hatchery-origin fish, wild fish, and combined populations.	The model characterizes straying for both winter-run and spring-run Chinook salmon (Peterson and Duarte 2020). Hatcheries are not a part of the Proposed Action. Consult Peterson and Duarte (2020) for more information on the treatment of hatchery and wild fish in the model.

Comment	Response
However, the ability of the CVPIA model to examine impacts for a listed species is only as good as its ability to represent and discriminate among different alternatives. An important limitation of the CVPIA LCM is that the monthly time-step does not represent linkages between flow and temperature controls and survival and growth of early life stages well enough to capture the effects of ramping, pulse flows, or short-term temperature excursions. The calibrated model also produced migration survival in the main-stem Sacramento that was high across scenarios (pinned against 1), so that processes in the Delta, by default, control estimates of population growth (Appendix F, Attachment F.2). Because neither egg-fry survival nor migration survival respond to flow and temperature, the calibrated model would not appear to be designed to evaluate the effects of seasonal changes in flow releases (i.e., from spring to summer) or changes in ramping rates.	Comment noted.
We note that there is a discrepancy in how early-life stage survival in the CVPIA Decision Support Model (DSM) is described in Appendix F and O, possibly because different versions are used. In Appendix O, section 4.7.3, CVPIA SIT DSM is used to model redd dewatering using WUA and monthly flow drivers, but in the version used for Appendix F, egg-fry survival, juvenile growth, and the temporal distribution of spawners are not allowed to vary (page 5).	As you correctly assume, two different versions are used. Appendix O (now 4.7.2 and see O.5.2) relies on the publicly available version of the DS habitat package. Appendix F relies on the version of the CVPIA SIT DSM associated with Peterson and Duarte (2020).
The Panel notes that some of the short-term variability is proposed to be controlled by Conservation Measures; however, it is important to note the limitations of the models for representing these important effects from short-term variation because it questions the accuracy of their predictions that ignore this variation. It is also unclear whether the beneficial aspects of variable flows (e.g., pulses that attract up-migrating spawners or push juveniles downstream) were included in the modeling.	To clarify, the purpose of these modeling efforts is to compare relative effects of alternatives over the temporal scale of water years rather than to create or assess accuracy of predictions of salmon population demographics. The CVPIA DSM tools use the *proportional pulse* data object as a proxy for pulse flow effects.

Comment	Response
Reclamation also produced a modified version of the Peterson & Duarte (2020) model7. The model was designed to address questions related to passage and screening of diversions and habitat restoration. It is highly discretized at a coarse temporal resolution. The published version was used by Reclamation with some modifications that required recalibration. The LTO decision model by Peterson and Duarte was not described in the information that we reviewed; however, the paper provided valuable information and we considered the paper in our review. We infer that hatchery populations may have been added and that the Chinook runs may have been run separately. In the Peterson & Duarte published analysis, simultaneous fitting of the three Chinook runs was reported. This seems like a good idea. However, the analyses reported seem to treat them separately and they have separate models on GitHub8.	There are three variations of the same model for each run of Chinook salmon. Winter-run and spring-run Chinook salmon variations were therefore run separately for the LTO analysis. The Peterson and Duarte (2020) paper describes the overall structure of the model which is the same for all three with specific differences listed. Functionally, these are separate models.
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Comment	Response
It is unclear how and whether minimum viable population size is treated by the life cycle models; is there an extinction threshold? Responses of spawning and rearing habitat to flow are represented either through WUA relationships or through more-detailed floodplain hydraulic models between Keswick Dam and Battle Creek on the Sacramento River (Appendix O Tributary Habitat Restoration, section 4.7.3). However, the fact that, in the modeling, migration survival is high and egg-smolt survival did not vary among alternatives reduces any possibility of differences among alternatives. Additional explanation is needed to understand and confirm this representation because it can force similarity among the alternatives.	No extinction threshold is present in the models and there is no treatment of minimum viable population size. Reclamation would contend that limited possibility for differences among alternatives remain because there are tens of parameters included in the models, many of which were related to survival and were not high like migratory survival. For example, across deterministic runs, monthly rearing survival for small juveniles (i.e., <42 millimeters) in the Upper Sacramento River varied from a low of approximately 0.01 to a high of approximately 0.2. Some of the similarity may also be because the alternatives being compared are similar themselves given the narrow operating space for this complex system. The EXP1 alternative emphasizes this, as in many demographic parameters it stands out as unique compared to the other alternatives. Even though the constant nature of the parameters in these DSMs is a disadvantage in some ways, it lends itself to a fair comparison of alternatives management options as is the case here.
An optimization framework (dynamic programming to select restoration options) limits the number of restoration actions possible at each time interval. Notes from the CVPIA Integration Team indicate that restoration actions in the Upper Sacramento were optimal at every time step. A sensitivity analysis was reported by Peterson & Duarte (2020). The most influential inputs for all Chinook runs were existing habitat, median discharge, and temperature. In addition, winter-run predictions were sensitive to initial abundance and total water diverted for the winter run.	No response identified.

Comment	Response
Some uncertainties include how gaps in parameters and among species are filled from similar tributaries and runs, respectively. It is unclear whether this borrowing of parameters could potentially result in a poor representation of the seasonal and spatial effects of operations. Uncertainty in the environmental drivers is another concern. The temperature interpolation decisions for the CVPIA LCM seem reasonable as long as the effects of changes in operations are not impacted. This involved imputing equilibrium tailwater temperatures based on air temperature and distance downstream (note: this should also include flow because the distance traveled is higher at high flows), and basin matching. This is an important thing to check. Because temperature monitoring is widely available, empirical data should be able to be used for model testing and improvement.	Comment noted.
The Panel questions the ability of the currently calibrated CVPIA LCM to represent different alternatives because of its insensitivity to freshwater processes. This is caused by very high juvenile survival during river migration and a constant egg-to-fry survival rate. Therefore, we question whether it is really possible for Reclamation to assess future effects of Shasta operations that affect egg-to-fry survival of the Chinook salmon runs or any influences during migration. By extension, this makes the use of the model questionable for evaluating the relative importance of river versus Delta versus ocean phases. We note that these issues can be easily fixed by calibrating against monitoring data in the river to produce more reasonable migration survival estimates and by linking egg-fry survival to the TMD indicator in SAIL. We encourage using an existing approach that can represent the effects of daily temperature on egg-fry survival, the effects of pulse flows, and moderation by minimum flows and ramping rates that influence stranding and dewatering mortality of freshwater life stages.	We acknowledge limitations of the CVPIA LCMs (see Attachment F.2 and F.3 for full discussion of the model limitations). Reclamation have therefore included additional lines of evidence. Please see the line of evidence related to temperature-dependent mortality for more specific treatment of this life phase, as well as STARS and OBAN modeling for additional lines of evidence related to freshwater processes.
(R) Stanislaus River WUA (Appendix O Tributary Habitat Restoration)	*N/A*

Comment	Response
Draft EIS Stanislaus models appear in Appendix N; however, there is no WUA analysis presented in either document in Appendix N. One analysis is on water temperature. The other focuses on the stepped release plan, The New Melones Stepped Release Plan.	Attachment N.2. to be provided in Final Biological Assessment
It is mentioned that pulse flows are needed to support different life stages of steelhead. This work does not appear to be linked to the WUA analyses. Performance metrics for fish appear to be linked to migration cues and temperature.	Improvements to evaluating pulse flows on the Stanislaus may be considered in the PA adaptive management program related to the steelhead JPE.
In Appendix O Tributary Habitat Restoration, however, there is a section on the Stanislaus River describing that WUA curves exist for fall-run Chinook, for 23 miles of river below Goodwin Dam. Data are from 1993 (USFWS).	No response identified.
The curves are presented for the alternatives and baselines being considered for this review as tables in Appendix O Tributary Habitat Restoration, but would be better presented as figures.	Figures of spawning and rearing WUA curves are available in latest Stanislaus WUA attachment (Attachment N.2, *Stanislaus River Habitat Availability Analysis*). Results are presented as both tables and figures.
(S) Attachment L.3 Egg-to-fry Survival and Temperature-Dependent Mortality	*N/A*
Additional material reviewed: a)Appendix L Shasta Cold Water Pool, Attachment L.2 Sacramento RiverWater Temperature Analysis,b)Appendix L Shasta Cold Water Pool, Attachment L.3 Egg-to-fry Survivaland Temperature-Dependent Mortality,c)LTO Appendix F, Attachment F.2 CVPIA Winter-run and Spring-run LifeCycle Models,d)Appendix M Folsom Reservoir Flow and Temperature Management, ande)Appendix N Stanislaus Stepped Release Plan	No response identified.

Comment	Response
Temperature modeling below the Shasta and Folsom Dams may be important to evaluate alternatives. Two aspects include physical modeling and modeling of biological effects. Simulated temperatures below Keswick Dam and the Upper Sacramento River are used as inputs to models of biological effects produced using various approaches. Likewise, temperatures are modeled below Folsom Reservoir on the American River and below New Melones on the Stanislaus River. The approach is similar for the two tributaries and below Shasta/Keswick, so the rest of the Panel review focuses on the Sacramento River.	No response identified.
The results presented in the Draft Effects Analysis used HEC-5Q, a 2D model (width averaged) that represents leakage zones associated with the temperature control device (TCD) in Shasta.	No response identified.
The modeling, done in 2003, is described online on the waterboard's webpage.	No response identified.
A previous Delta Science Program review of Temperature modeling recommended some changes.	No response identified.
Another joint agency temperature modeling effort not included in our review is the Central Valley Temperature Mapping and Prediction (CVTEMP) model found on the agency website.	No response identified.
Integration of this temperature model with CALSIM-3 has not yet been done, but simulated operation by ResSim-TCD was successful in demonstrating that meeting a downstream temperature target would be possible, provided sufficient cold water was available in Shasta Lake.	No response identified.

Comment	Response
Below, we review approaches to modeling temperature-dependent mortality (TDM) using three different modeling approaches: a) monthly multi-species TDM indices available for all species and life stages (described in Appendix L Shasta Cold Water Pool Attachment L.2 Sacramento River Water Temperature Analysis), b) daily TDM models for the early life stages of Chinook runs (described in Appendix L Shasta Cold Water Pool: Attachment L.3 Egg-to-fry Survival and Temperature-Dependent Mortality (based on Anderson et al. 2020; Martin et al.), and c) monthly TDM modeling in LCMs for all life stages of Winter- and Spring-run Chinook salmon (described in LTO Appendix F – Attachment F.2 based on Peterson & Duarte 2020).	No response identified.
The modeling of monthly mean temperatures for the CVPIA model is described on the SIT model website. The Panel was unsure how climate change that was included in CALSIM-3 runs was included in these HEC-5Q runs. An important uncertainty could be the availability of tailwater temperature gages and models of reservoir release temperatures, but it is difficult for the Panel to evaluate this. The provided documents did not describe performance metrics measuring how well HEC-5Q represented reservoir stratification and cold-block storage nor an evaluation of the predictions from the linked reservoir–tailwater simulations.	Reclamation's evaluated performance metrics for how Hec-5Q stream temperatures are affected under climate scenarios as part of previous programs (CH2M. 2017. Development of WSIP Climate Scenarios for use in HEC5Q, Technical Memorandum. 30p.). This can be provided to the panel for review.

Comment	Response
For the purpose of estimating TDM, the Panel favors the Martin et al. (2017) and Anderson et al. (2022) models of egg-fry survival based on a daily time step. These were used to predict egg-to-fry survival for winter-run Chinook salmon as a function of temperature-dependent egg mortality, background mortality, and density-dependent mortality (but not redd-dewatering mortality, which is estimated separately). Temperature-dependent mortality (TDM) is largely understood to be caused by dissolved oxygen depletion. A stage-dependent model (Anderson et al. 2022) accounts for accumulated thermal units (ATU) to represent ATU-dependent development and to identify a critical period for thermal mortality for winter-run Chinook salmon. The Anderson et al. model was used with modifications to estimate TDM, but it's not clear that these results are fed into a life cycle model or what they were used for. The model was originally applied for all three Chinook runs together, assuming identical parameters across runs. A problem identified in the report is the covariance among parameters that makes it hard to distinguish the effects of temperature conditions from habitat limitation (e.g., superimposition) and baseline survival. It is unclear whether baseline mortality was allowed to vary among Chinook salmon runs. The Bayesian estimate of critical temperature in the Martin et al. model, Tcrit = 11.8$_{o}$ C seems quite low and the published value to which it is compared does not have a citation. It would also be useful to see the equation.	Thank you for your thorough review and insightful comments regarding the TDM modeling. Both Martin et al. and Anderson versions of the model were used without uncertainty. The Bayesian version of the model incorporating uncertainty was a modified version of the Martin et al. model. Please refer to Appendix L Attachment L.2 for more information on the models without and with uncertainty. In addition, TDM was only modeled for winter-run Chinook Salmon. Please refer to Appendix F Modeling for a brief description of the nature of the modeling. The models are consistent with Martin and Anderson's versions of the models. For Martin, background mortality is a function of the expected egg to fry survival probability in the absence of temperature or density-dependent survival S0, a capacity parameter K (# spawners), and female spawner abundance (A). Equation four in Martin et al. (2017) describes background mortality with the equation which is the same equation we used for the TDM modeling. For the TDM modeling following Anderson et al. (2022), background mortality, "...is a free fitted parameter that is assumed to be constant for all years and redd locations." Refer to Anderson et al. (2022) for more information on modeling related to Anderson et al. (2022). Furthermore, the published Tcrit value that was compared to the Bayesian Tcrit estimate is from Martin et al. (2017), referenced in the sentence referencing the table that compares the values.
Daily TDMs were used to estimate early life history survival for the three Chinook salmon races (described in Appendix L Attachment L.3). Advantages of these models include that the analyses are conducted using daily-resolution temperature drivers and that they provide stochastic results. The disadvantage is that TDMs may not represent all competing risks.	No response identified.

Comment	Response
Life cycle models can represent competing risks and incorporate monitoring data, but at the current time resolution, they are not the best option for representing temperature-dependent mortality. In our view, modeling thermal risks at a monthly resolution is not adequate for making decisions based on the potentially lethal effects of short-term extreme temperature exposures. The CVPIA LCM acknowledges that it does not represent differences in egg-fry survival due to operations. It does not appear that survival effects of daily-scale variations in flow and temperature from Shasta releases were integrated into the life cycle models to inform state transitions. Therefore, it is not really possible for LTO to assess the future effects of Shasta operations that affect egg-to-fry survival.	The monthly time-step is a component of the existing CVPIA DSM tools. However, we have additionally included more specific modeling to represent temperature-dependent mortality as another line of evidence (see Appendix F and Attachment L.2 of Appendix L).
However, because egg-fry survival involves competing risks such as redd dewatering, scouring, and superimposition, the Panel favors the use of a life cycle model to integrate risks associated with temperature and other factors and to allow comparison to available monitoring data. Ideally, the strengths of both approaches would be combined.	No response identified.
The best available models estimate risk at a daily resolution and by estimating physiological risk as a function of duration (see approaches by Martin et al. 2017, Anderson et al. 2020, Bowman et al. 2020, and Troia et al. 2023), represent competing risks, and make predictions that can be compared to monitoring data. Because biological effects of temperature are non-linear, the average of the survival function applied to daily values does not equal the function of the average, especially when compared to thresholds, such as maximum temperature tolerance. The Panel recommends estimating daily egg-fry survival from the highest resolution of thermal exposure data available (e.g., daily maxima), and thresholds based on laboratory studies. Daily TCM models could be refined by modeling risk using magnitude-duration relationships based on laboratory studies (see Troia et al. 2023).	This comment can be used to improve existing methods for evaluating effects of CVP operations on temperatures in future efforts as part of the LTO adaptive management program related to Shasta Coldwater pool management.

Comment	Response
Another important consideration is the role of dissolved oxygen. Because both temperature-dependent mortality and redd-dewatering mortality are thought to be driven by dissolved oxygen (DO), it would be helpful to have some idea of how Shasta/Keswick operations (including the use of the temperature control device and spill) influence tailwater DO. Whereas the focus on temperature is important, it is possible for DO and temperature to be decoupled in hypolimnetic releases. If the proximate driver for TDM and redd dewatering mortality is low DO, then further justification is needed for assuming that lower temperatures mean higher DO (due to saturation) in a tailwater setting and we are not sure whether Keswick provides aeration. In short, the Panel is asking Reclamation to justify modeling biological effects in response to long-term equilibrium temperature rather than as responses to transient dynamics in water quality based on the ability to discern differences in lethality among alternatives.	TDM modeling is based on HEC-5Q daily water temperature modeling which is based on monthly CalSim 3 flows. In addition, the spatiotemporal distribution of redds is included. Given dissolved oxygen is inherently highly variable at fine scales, even at the scale of individual redds, we would need redd scale DO measurements over long periods of time to justify using it as well as interstitial flow modeling to be able to approach including DO in the modeling approach. This might be a future uncertainty to address. The assumption that colder water has higher DO is a necessary one for these types of coarse scale comparisons, as one would assume colder water will on average have more DO than warmer water. Violations of this assumption certainly exist at finer scales. However, we would argue that this assumption and the use of equilibrium temperatures to compare TDM across alternatives constitutes an appropriate use in this case.
(T) Attachment O.2 Science Integration Team Life Cycle Model Habitat Estimates	*N/A*
The analysis attempts to answer a series of management questions. The overall question is where habitat is a primary factor influencing survival. The Appendix is not complete and points to two attachments ('knowledge base papers') for the methods, which are standard Instream Flow Incremental Methodology (IFIM)/WUA analyses. However, the CVPIA salmon life cycle model description also refers to the use of hydraulic models. These are not described here but rather summarized in Appendix O Tributary Habitat Restoration.	Reviewer notes that Appendix O is incomplete that the use of hydraulic models is not described in the Appendix but rather in an attachment to the appendix. Since the desired information is in the attachment, this is cited in the methods.
Table 1 provides a nice summary of the locations where habitat suitability criteria (HSC) (depth, velocity, substrate preference, or habitat suitability curves) were estimated for each species. We recommend a) collecting the habitat modeling into one place (or cross-referencing) and b) adding a column or map to show where the models were applied for each species.	The information being requested is available as a link to the cvpia github in the first paragraph of 0.2.1.1 Model overview.

Comment	Response
This approach, while well-established, could be refined by representing the temporal distribution of spawners. It has also been pointed out by the Panel that modeled hydraulic (e.g., velocity and depth) information is directly available for use, which might improve the transferability of the IFIM modeling.	No response identified.
The Panel notes the following: Responses of spawning and rearing habitat to flow are represented either through WUA relationships or through more-detailed floodplain hydraulic models, where available. Based on looking at the code, it appears that modeling is simply based on the number of weeks that the floodplain is inundated. Additional explanation of the roles of hydraulic models and WUA relationships within the life cycle modeling is warranted.	Details of hydraulic models and WUA relationships are included in the publicly available DSMhabitat package's documentation, which can be accessed through the hyperlink in O.5.2.
(U) Attachment O.3 Sacramento River Weighted Useable Area Analysis	*N/A*
In general, the Weighted Usable Area analysis completed for the Upper Sacramento River winter-run Chinook salmon, fall-run Chinook salmon, and Central California Steelhead is based on a strong history of use and methods, and relies on data obtained from three USFWS reports, dated from 2003-2006. The spawning and rearing WUA estimates presented for the BA and EIS modeled scenarios are based on CALSIM-3 flow data for each month of the 93-year period of record.	Comment noted.
Models were developed separately for the different races and species of salmon and assume that habitat suitability for spawning is based on substrate particle size, depth, and flow velocity. The habitat suitability characteristics for spawning were developed by taking observational data for these three metrics at active redds. Hydraulic modeling was then used to quantify the amount of suitable habitat available at different river flows, for different Habitat Suitability Criteria (HSC) levels. These results were combined to develop the WUA curves and tables, which are used to look up the amount of habitat available at different flows during the spawning periods.	Comment noted.

Comment	Response
Generally, the reports from which the WUA curves were developed are thorough, robust, and well-documented. Reclamation has pulled an extraordinary amount of data together and appears to have a strong, systematic approach to connecting WUA curves to the alternatives and baselines. However, without a stronger narrative that synthesizes all the WUA analyses and the methodology behind them, the Panel found it difficult to track methodologies across river systems and species to understand where, if any, differences occur and how that should be handled in assessments of effects on ESA-listed species.	Reclamation defers to NMFS and USFWS to decide regarding weight of evidence. Please refer to uncertainties and assumptions sections of the attachments.
The Panel recommends that a table that outlines whether the WUA curves for a specific river/species/life stage and reach were developed using PHABSIM vs. RIVER2D would be helpful, as well as any substitutions of data from one species or river system to another. Limitations, uncertainties, and tradeoffs between the underlying models should be discussed (i.e. for systems using PHABSIM, an increased level of uncertainty should be identified since this model has multiple identified issues).	USFWS's (2006) most recent spawning and rearing WUA curves (as well as redd dewatering and stranding analyses) for all anadromous salmonids in the Sac River were developed using the RIVER2D model. The original WUA curves were developed using the PHABSIM model (USFWS 2003), but these curves were not used for the LTO analyses. However, to avoid confusion, it should be noted that the PHABSIM model was used to prepare some of the inputs for the RIVER2D modelling.
No WUA curves were developed for spring-run Chinook spawning, so fall-run Chinook salmon WUA curves were used to quantify spring-run spawning habitat. This gives the Panel some cause for concern, as there is no discussion or evidence presented that discusses the degree to which this surrogacy is appropriate. Reclamation cites a personal communication to Mark Gard via email that USFWS staff endorsed this practice, and it has since been adopted into subsequent studies. Without further information, the Panel cannot comment as to the appropriateness of this endorsement. Interestingly, it seems spawning peaked at 700-900 cfs for spring-run but at 5,000 cfs for fall-run Chinook salmon. Does this reflect the preference of fall-run Chinook for deeper mainstem habitat and of spring-run Chinook for tributary habitat?	The information provided by Marc Gard, who has led all the anadromous salmonid WUA studies in the Sacramento River, is the most reliable available information on the subject. Anticipate tackling flow relationships in the LTO adaptive management program related to tributary habitat restoration effectiveness for salmonids fishes.

Comment	Response
The HSC data (responses to depth and velocity) used to develop WUA for Steelhead trout were obtained from the American River. Local HSC data for Sacramento Steelhead were not available. The Panel suggests that Reclamation provide evidence that this space substitution is appropriate.	Validity of substituting American River HSC for steelhead in the Sacramento River is discussed in Section O.3.2.3.1 of the attachment: Important Uncertainties and Assumptions of the WUA Analyses Conducted for the Effects Analyses. Reclamation has no evidence concerning the validity of substituting American River steelhead spawning HSC for Sac River steelhead WUA development. The USFWS (2003) study that developed the spawning WUA curves for Sacramento River steelhead, states: "Since we were unable to conduct a transferability test to determine whether the lower American River steelhead trout HSC would transfer to the Sacramento River, we suggest that the habitat modeling results for steelhead trout should be considered with caution." Reclamation is not aware of any such transferability test having been done more recently. Gard's 2023 review of Central Valley anadromous salmonid HSC, shows similarity in spawning HSC for steelhead in most rivers, but substantial differences in a few, including the American River.
There is a section that states that the upper and lower limits of the range in WUA values are determined by the ranges of the fry-rearing WUA curves from which they are estimated (i.e. the curves differ across sections of the river for the same flow). However, there are no results that show the upper or lower ranges – only one WUA value is shown, and there is no explanation of how the upper and lower limits were incorporated into the modeling framework or represented in the output tables and figures for each alternative scenario.	WUA values for the lowest and highest flows in the WUA curves (See Figures 0.3-2 through 0.3-11 in the attachment) are assigned to all flows below or above these flows, respectively. Flows below the lowest flows of the curves (3,250 cfs) are rare and flows above the highest flows of the curves (about 30,000 cfs) generally occur in less than 5% of the monthly flows in the CalSim3 record for all scenarios.
The Panel notes that the analyses tend to focus on the peak of the spawning season; care should be taken to ensure the tails of the spawn timing distribution are carefully considered, as phenotypic variation in run-time is a specific objective of Recovery Planning. When effects analyses and resulting management actions focus on the spawning peak, it may have the effect of reducing genetic or phenotypic diversity.	Discussion of this source of uncertainty has been added to Section O.3.2.2.1 of the Attachment.

Comment	Response
The data used to inform spawning and rearing WUAs is from the late 1990s and early 2000s. The Panel questions whether the years in which these data were collected are representative of the range of environmental conditions experienced in the river systems presently, or whether the conditions are representative of wet, dry, etc. year types, or whether the river-scape is similar enough now. A source of uncertainty is whether or not the placement of redds and the biological effects of stressors on early life stages are similar across water-year types.	Uncertainty related to changes in river conditions over time is addressed in Section O.3.2.2.1 of the Attachment. WUA models assume that choice of redd placement is a function of HSC conditions. These conditions change with flow, which is captured by the WUA curves, but there is no evidence that HSCs change with water year type. While this circumstance cannot be ruled out, it is beyond the scope of this effort. Other analyses, such as water temperature and redd dewatering, capture conditions that may be affected by water year type and are not included in the WUA analyses.
Attachment O.3 provides a map of the section of the Sacramento River wherein WUA curves were developed. However, in this attachment, and all other WUA attachments, it is difficult to discern the spatial extent to which the WUA curves are being applied. Are they extended beyond the original stream reaches for comparison among scenarios? If so, is that appropriate in terms of similar channel shape and flow magnitude? If not, can the HSC relationships be applied instead to ensure that the effects of alternatives at the population (or sub-population) level are fully understood?	The spatial extent to which the WUA curves are applied is described in the text and is the same as that depicted in the map in Figure O.3-1. The spatial extent of the curves does not extend beyond the original stream reaches for which they were developed and are identical for all scenarios. An analysis based solely on the HSC relationships would be very complex and would require information not available in any of the USFWS and other studies on which the WUA analyses are based. Note that the WUA curves are essentially the combined HSIs produced by a given flow.
Differences in mean spawning WUA curves across the management scenarios are presented by month, water-year type, and for all water-year types combined (for each species/race), and account for differences in density across different river segments. This is a careful, strong effort that ensures temporal and spatial differences are considered. The Panel suggests an explanation of how the Reclamation plans to integrate this degree of complexity into the life cycle models.	The DSMhabitat package is a component of the DSM life cycle models and therefore the WUA curves degree of complexity are already incorporated in the life cycle models.
For months when the Anderson-Cottonwood Irrigation District (ACID) Dam boards (barriers to movement) are installed, how is WUA adjusted other than by flow?	The boards result in upstream ponding of water, creating very different depths, flow velocities, and available substrates for a given flow. The hydraulic models (PHABSIM and RIVER2D), therefore, create different WUA curves when the boards are installed.

Comment	Response
Reclamation acknowledges that many fry and steelhead rear downstream of Red Bluff Diversion Dam, where no WUA curves have been developed.	If this method continues to be used for evaluating CVP operations of flow-habitat availability, Reclamation can use this recommendation to further improve this modeling through the LTO adaptive management program related to tributary habitat restoration effectiveness monitoring for salmonid fishes.
The report repeats a section (Sacramento River Rearing) starting on page 11.	Thank you for noting the duplication. It has been deleted.
A limitation identified for all the WUA curves is that all of the habitat-based studies assume the channel characteristics of the river during the time of field data collection are in dynamic equilibrium. This report confusingly states the field data collection is by USFWS from 1995-1999, but also discusses redd surveys through 2021. This statement should be clarified as to whether it refers only to the rearing data, or also to the redd data.	The latter years are not related to USFWS's field data collection for the WUA analysis, but rather are years that CDFW conducted aerial redd surveys that Reclamation used to estimate spawning periods of the salmonids.

Comment	Response
The report also identifies that WUA curves were applied as far downstream as Red Bluff Diversion Dam, although known rearing occurs below this point. It is unclear how this data gap is handled in the integration of the effects for Chinook and Steelhead, but this is a significant portion of the Sacramento River wherein rearing habitat appears to be unevaluated with respect to the alternatives. This may also affect how the benefits of floodplain habitat are represented, which growth, food web, and bioenergetics studies suggest is critical to early marine survival. The Panel suggests that this omission be addressed in the text.	The DSMhabitat package assess spawning habitat as far downstream as Red Bluff Diversion Dam. Rearing habitat and floodplain habitat are assessed separately. With regards to the spatial extent of rearing habitat, the DSMhabitat package documentation explains: "Instream and floodplain rearing habitat areas are based on habitat modeling conducted by NOAA NMFS for their Winter Run Chinook Salmon life cycle model. The entire mapped rearing extent of the Sacramento River was modeled using the Central Valley Floodplain Evaluation and Delineation (CVFED) HEC-RAS hydraulic model, refined for use in their Winter Run Chinook Salmon Life Cycle Model. NMFS provided tabular suitable rearing habitat area data for three reaches of the Sacramento River (Keswick to Battle Creek, Battle Creek to Feather River, and Feather River to Freeport). The high-quality depth and high-quality velocity criteria ("Pref11") "ChanArea" value was used for instream habitat, and "BankArea" result was used as floodplain area." (See http://cvpia-habitat-docs-markdown.s3-website-us-west-2.amazonaws.com/watershed/sacramento_river.html). Details of the publicly available DSMhabitat package's documentation can be accessed through the hyperlink in O.5.2.
Estimated WUA values for the Draft Effects Analysis are similar for May, June, and August, but are lower and more variable in July, when CALSIM-3 flows are higher. The higher July flows are described as substantially higher, and possibly contributing to lower and more variable spawning WUA results. The Panel is unable to evaluate this rationale without a better explanation in the document. The July flows should be apparent in the document so that a reader could go back to the WUA curve and identify whether this statement is reasonable, and also determine which sections of the river are likely driving the reduction in spawning WUA.	Identifying potential revisions through future iteration in LTO adaptive management program related to tributary habitat restoration effectiveness monitoring for salmonid fishes.

Comment	Response
Similar to spawning, the highest WUA for rearing habitat (fry and juveniles separately) were generally higher under critically dry years and lower in above-normal or wet water years. The largest difference between the No Action Alternative and the scenarios is a 0.7% increase for Alternative 4 in critical water years. The largest reduction is 0.5% for Alternative 2 without UCP Systemwide VA in below-normal water years. Across months, estimated fry-rearing WUA curves are similar (August through December), except extreme WUA results, particularly high values, which are more prevalent for December, and somewhat prevalent in November. This is presumably due to more frequent high flows, which correspond to higher WUAs on the winter-run Chinook WUA curve. Winter-run Chinook is the only Sacramento River salmon race to show this pattern.	Comment noted.
There is a section that states that the upper and lower limits of the range in WUA values are determined by the ranges of the fry-rearing WUA curves from which they are estimated (i.e. the curves differ across sections of the river for the same flow). However, there are no results that show the upper or lower ranges, only one WUA value.	The results combine the WUA results from the three river segments (four segments for fall-run Chinook salmon spawning), as described in the attachment.
Perhaps there is a module within the RIVER2D model that accounts for this range in WUA values, but it would seem to be better to apply the appropriate HSC derived for each river section to the river sections under each scenario, and then roll those up into a composite value. This section is confusing to understand, and clarity is needed about how different sections of the river were handled in the modeling process.	An analysis based solely on the HSC relationships would be very complex and would require information not available in any of the USFWS and other studies on which the WUA analyses are based. Note that the WUA curves are essentially the combined HSIs produced by a given flow. Note also that different combinations of the values of the three habitat attributes (flow velocity, depth and substrate type) will often provide the same WUA values.
(V) Attachment I.6 Volumetric Influence Analysis	*N/A*

Comment	Response
This analysis is too coarse and based on a simplistic, flawed assumption. The Panel's primary concern is that decision-makers will look at these results and conclude that there are no significant differences between the alternatives. As an example, one of the stated EIS Key Takeaways is: "*Among the other alternatives there is great overlap in the distribution and variation among where the peaks in the distribution among the water types are observed*" (p. I.6-3).	Comment noted.
Output from CALSIM-3 is not the correct source of data for this analysis. More appropriate models of water circulation within the Delta exist and should be used when analyzing issues related to Delta hydrodynamics.	Comment noted.
The Volumetric Influence Analysis assumes that the full inflow from the Sacramento and San Joaquin Rivers is available for export. The basic assumption of the analysis that the Delta can be represented as a giant Continuously Stirred Mixed Reactor is hydrodynamically wrong. In reality, the majority of Sacramento water stays in channels of the Sacramento River. The amount of Sacramento sourced water available for export is limited to the amount that is transferred to the San Joaquin River and the South Delta via the San Joaquin/Mokelumne junction, the Three Mile Slough junction, and the Sacramento/San Joaquin confluence.	Comment noted.
To estimate the percent of Sacramento-sourced river that is transferred to the San Joaquin and South Delta and available for export, one would take any Delta hydrodynamics model (DSM2, etc.) and calculate how much Sacramento-sourced water is going through the three above-mentioned junctions. As an extremely rough estimate, calculate the Sacramento flow just below the Sacramento/Georgina Slough junction as the amount that is not transferred to the San Joaquin and South Delta region.	This is an alternate calculation method, which could be implemented if this method is further advanced through the LTO adaptive management program related to OMR management.

Comment	Response
For this analysis, the total volume of inflow water available for export is not the full Sacramento and San Joaquin flow. It is the San Joaquin inflow plus a percentage of the Sacramento flow based on Delta hydrodynamics and the Delta Cross Channel operations.	Full list of components used to calculate inflow are provided in the attachment.
Many conclusions were stated at the end of the Volumetric Influence Chapter. We will discuss here the validity of several key conclusions and how the assumptions underlying the analysis contributed to the conclusions.	No response identified.
First, the statement is made: "the frequency of specific observations can be used to qualitatively assess which alternatives have the most observations of low percent Delta inflow" (p. I-6-3). The Panel asks: Is it a low percent of the actual inflow that is available to be exported at the pumps? A large amount of water can be headed through the Sacramento system, creating a large total Delta inflow. However, that large Sacramento flow is not hydrodynamically available at the export pumps.	Analysis does not attempt to examine small scale flow dynamics. DSM2 would be a better tool.
Second, the statement: "Among the other alternatives there is great overlap in the distribution and variation among where the peaks in the distribution among the water types are observed" (p. I.6-3). The Panel reads this to mean that it was hard to tease apart differences in these different simulations. That is because the basic premise of the analysis is incorrect. Only a limited percentage of the Sacramento-sourced water crosses over to the South Delta and is available for export.	This analysis did not attempt to delve into the nuances of how much water from each inflow source might be available to the exports. This could be explored in other analyses.
Third, the statement is made: "*Inflow groups with high Sacramento River flows have a large amount of overlap in their distribution among the alternatives*" (p. I.6-19). The Panel notes that this is because high Sacramento flow is the case where the Total Sacramento plus Total San Joaquin inflow assumption works the least.	This analysis did not attempt to delve into the nuances of how much water from each inflow source might be available to the exports. This could be explored in other analyses.

Comment	Response
First, the statement is made: "the frequency of specific observations can be used to qualitatively assess which alternatives have the most observations of low percent Delta inflow" (p. I-6-3). The Panel asks: Is it a low percent of the actual inflow that is available to be exported at the pumps? A large amount of water can be headed through the Sacramento system, creating a large total Delta inflow. However, that large Sacramento flow is not hydrodynamically available at the export pumps.	Analysis does not attempt to examine small scale flow dynamics. DSM2 would be a better tool.
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Fourth is the statement: "A similar pattern occurs in the lolo and lomed groupings, however medmed (medium-medium), and medhi (medium-high) have distinctly high frequencies between 20% and 30% in Alt2woTUCPwoVA" (p. I.6-19). The Panel notes that the medhi category means that Sacramento is a medium-level flow, and the San Joaquin is a high-level flow. So, the San Joaquin-sourced water has a high influence on export in these cases. Therefore, the basic assumption of the analysis is not swayed as much compared to when Sacramento flow is high. In the case where San Joaquin water dominates inflow volume, signals that distinguish between the alternatives show up.	An informative observation but would require further analysis of inflows to rigorously examine.

Comment	**Response**
Fifth is the statement: "*The lohi group is unlike any other group by having its highest peak in Alt2woTUCPAllVA, but this is likely driven by the sample size. The NA group is introduced because of some of the observed values falling outside of the delta inflow group definitions*" (p. I.6-19). The Panel thinks that this finding could also be related to the assumptions of the analysis rather than sample size. This is the only case where the dominant water source is the San Joaquin. Therefore, the results are not diluted by the wrong assumptions related to Sacramento water volume. Here, a signal that distinguishes between different alternatives shows up.	This analysis did not attempt to delve into the nuances of how much water from each inflow source might be available to the exports. This could be explored in other analyses.
The Panel also had comments on some of the conclusions (EIS, p. 3). One conclusion was that the lowest percent diverted occurred in an above-normal year. The Panel interprets this as simply that if you have more water in the system, the percentage of the Delta inflow exported would be lower. Another conclusion was that the maximum value of 65% Delta inflow exported in multiple years. The likely explanation is that this is because the regulations that limit the CALSIM-3 export operations kicked in. Finally, the observed lowest (non-zero) mean percent Delta inflow exported was in Alt 3 and observed in the hihi inflow group at 6.7%. This made sense to the Panel because if both rivers have a high volume, you would expect that the percent diverted would be the lowest.	These comments are address in the attachment.
Hydrodynamic models (and associated PTMs) need to be used to tease apart the results. Even the basic DSM2 (1-D channel assumption) puts in necessary detail. The CALSIM-3 hydrology model can be used to drive the flow boundaries of Delta hydrodynamic models. However, CALSIM-3 should not be used to make conclusions about the influence of South Delta export facilities.	The analysis was meant as a course illustration and other analyses in the document address this with more nuanced models.
(W) Attachment F.3 CVPIA Winter-Run Life Cycle Model	*N/A*

Comment	Response
Additional material reviewed: a) Appendix I – Old and Middle River Flow Management, and b) Model Description for the Sacramento River Winter-run Chinook Salmon Life Cycle Model is found on the model's website c) The Winter-run Chinook Salmon Life Cycle Model (WRLCM) is a spatially and temporally explicit stage-structured, stochastic simulation model that estimates the number of winter-run Chinook salmon at each geographic area and time step for all stages of their life cycle. Hatcheries have been added (although we did not see information about the assumptions used, which could be important). The model uses the Newman (2003) Bayesian state-space model to estimate juvenile Delta survival based on biological and environmental covariates. The WRLCM has been reformulated into a state-space model. Many of the stage transition equations describing the salmon life cycle are direct or indirect functions of water quality, depth, or velocity from DSM2 outputs, thereby linking management actions to the salmon life cycle. The approach to modeling early survival in response to temperature effects is more sophisticated than that in the CVPIA model. Although the model represents linkages between the survival of each life stage and flow-temperature drivers, in some cases the temporal resolution is too coarse (e.g., egg-to-fry survival is based on 3-month temperature averages).	Comment noted.

Comment	Response
The WRLCM now uses the enhanced particle tracking model (ePTM) to estimate the survival of out-migrating smolts originating from Lower Sacramento River, Delta, and floodplain habitats. ePTM represents two stressors: predation and diversions. Calibration of ePTM is based on the survival of released coded-wire-tagged (CWT) juvenile hatchery fall-run Chinook salmon and their recovery at Chipps Island, which do not leave the system at the same time as winter-run juveniles. The WRLCM is calibrated against spawners below Keswick Dam, juveniles collected at Red Bluff Diversion Dam, juveniles collected in Knights Landing catches and rotary screw traps, and Chipps Island abundance. The Panel would like to see more justification for using hatchery fall-run Chinook as a surrogate.	Reclamation will consider incorporating this in future efforts. However, the version of the WRLCM used Perry et al. (2018) for almost all Delta out-migration survival estimates, which used a multi-state mark-recapture model based on acoustically tagged late-fall run Chinook Salmon. We acknowledge migration mechanics, which in turn affect survival, will vary based on run, hatchery v. wild, and fork length. However, the model made use of the best estimates for outmigration survival at the time.

Comment	Response
How well does the model represent project operations? An earlier review, found on the model's website, concluded that WRLCM incorporated the needed linkages between project operations and population dynamics, as determined by the distribution, survival, and movement of salmon within the river system. However, the review also questioned the sensitivity of the model to flow alternatives, and covariation between outflow, temperature, and spawner returns, which was a concern of the earlier Newman model, a predecessor to this model. One concern is that historical correlations may be broken in the future, changing the ability of the model to predict population-level effects of CVP decisions. The WRLCM model was used to compare scenarios releasing colder water in spring. The analysis found an important seasonal tradeoff between early pulse flows and maintaining the cold-water pool at Shasta until September. Elevated September temperatures resulted in poorer simulated outcomes for winter-run Chinook salmon (Appendix L Shasta Cold Water Pool Management). Hendrix suggests that, because of its design, the model may be overly sensitive to operations.	Thank you for your detailed feedback on the WRLCM representation of project operations. We appreciate the reference to the earlier review from NMFS, which acknowledged the model's incorporation of linkages between project operations and population dynamics of salmon, including distribution, survival, and movement within the river system. We understand the concerns regarding the model's sensitivity to flow alternatives and the covariation between outflow, temperature, and spawner returns. These issues, also noted in the predecessor Newman model, highlight the complexities inherent in modeling such dynamic systems. It is indeed important to consider that historical correlations may not hold in the future, potentially impacting the model's predictive capability regarding population-level effects of CVP decisions. The use of the WRLCM is not intended to be predictive but rather to compare the alternatives. The concern raised by Hendrix in relation to the scenario comparison of Shasta cold water pool regarding the model's potential over-sensitivity to operations is noted. We also not that in some contexts the model is not sensitive to changes in parameters, where the same outcomes are produced for different sets of parameters. Overall, because of the model's intended use as a comparison tool, hope is that while specific quantities of water may not be accurately accounted for, the model does a decent job at representing the overall decision framework around winter-run Chinook salmon and, should therefore, be suitable to compare management alternatives within the context of winter-run Chinook Salmon.

Comment	Response
The WRLCM model is not uniquely specified in its parameters, reflecting the reality that multiple combinations can produce the same outcomes. It may therefore be unable to distinguish between different combinations of management decisions leading to the same fit against downstream monitoring data. One solution to this is to use Bayesian multi-parameter modeling (see Piou et al. 2009; Jager 2013). To avoid over-specification, we also agree with the decision to minimize the complexity of spatial representation, for example, in ePTM. The best solution to the model-identification issue is to find ways (data) to distinguish hypotheses about the response of salmon to specific operations higher up in the system, so this is an important data gap that should be addressed.	Reclamation agrees that the non-uniqueness in parameter combinations can complicate the interpretation of management decisions. The suggestion to employ Bayesian multi-parameter modeling, as outlined by Piou et al. (2009) and Jager (2013), is well-taken. This approach looks like a promising path to consider, especially when evaluating alternative management approaches. To address the model-identification issue, we agree it is important to gather additional data that can help distinguish between hypotheses about salmon responses to specific operational changes upstream.
(X) Attachment I.3 Delta Export Zone of Influence Analysis	*N/A*
Additional material reviewed: Chapter 5 Winter-Run Chinook Salmon (p. 5-77 to 5-89) The Panel identified three issues for Reclamation to consider:	No response identified
The first issue is the approach for the channel length analysis dilutes the results to the point that the differences between alternatives are obscured. The base channel length for the calculation of the percentage should be the length of the channels in the south Delta and the San Joaquin River. Including the length of the Sacramento River and associated tributaries north of the San Joaquin River unnecessarily dilutes the results.	The analysis was initially based on seeing the effect of exports (zone of influence) on the whole Delta. Given the scale of these results, though, it makes sense to investigate reducing the spatial limits for base channel length. We will consider this for future analytical revisions in the LTO adaptive management program related to salmonid and Delta Smelt OMR management.
Second, the Panel agrees that future analysis should anchor the results to export volume rather than OMR values. “Future work may visualize results by exports instead of OMR to better understand if exports are a more direct driver of the spatial extent of the zone of influence” (LTO Appendix I Old and Middle River Flow Management, Attachment I.3, Delta Export Zone of Influence Analysis, p. I.3-36).	Reclamation agrees and will consider using exports instead of OMR for future analyses. We will consider this for future analytical revisions in the LTO adaptive management program related to salmonid and Delta Smelt OMR management.

Comment	Response
The third issue is that there are multiple placeholders in this section for particle tracking results. It is unfortunate that the Panel did not have an opportunity to review those sections. Placeholder Sections include: Flow into Junctions, Particle Tracking Models, ECO-PTM (BA Chapter 05, p. 5-90).	No response identified
The introduction of the topic and approach is much better in Appendix I than in the BA – Winter Run Salmon section. Much of this introduction is needed in the BA Winter Run Salmon section to explain the results. To enhance understanding of the concept, provide a map of the stations for Figures 20-22. Also, make sure the x-axis in the figures is consistent – the direction for Figure 22 is flipped. (BA-Winter Run Salmon p. 5-79 through 5-81).	Added map to winter run Chinook salmon Biological Assessment ZOI section and Attachment I.3 ZOI. Axes are consistent in terms of velocity value (from negative to positive going left to right). The additional labels "away from" and "to" pumps become inconsistent between stations that are upstream versus downstream of the pumps and is meant to add to understanding for the reader.
Appendix I did not have definitions of what Alt2wTUCPwoVA, Alt2woTUCPAllVA, Alt2woTUCPDeltaVA stand for. This information may be buried in other documentation. However, since these simulations are trying to compare the results of combinations of alternatives and climate change, it would be helpful for the reader to get a one-paragraph summary with a reference to the expanded simulation documentation at the start of this discussion.	The main modeling of Appendix F defines the phases considered in the Biological Assessment see Sections 1-1 1-2 of App F, prior to Appendix I OMR. Phases are intended to further demonstrate the flexibility and impacts of these components, some of which are outside Reclamation’s direct control. It should be noted that the phases of the Proposed Action could be utilized under its implementation. All four phases are considered in the assessment of the proposed action to demonstrate the range of potential impacts.
The interpretation of the contour plots raised some questions for the Panel. The panel agrees that the Sacramento and San Joaquin inflow categories (hi, med, lo) should be used to divide the results into flow groups. The information in the contour plots (Figures 6-12) is presented well and makes intuitive sense. All the figures show the same general patterns and provide a basic lesson on how the export pumps influence the South Delta region.	No response identified.
Note that the region of influence is always on the export pump side of the San Joaquin River. This is important evidence supporting the Panel’s comments regarding the Volumetric Influence analysis and the Channel Length analysis.	No response identified.

Comment	Response
Gaussian Kernel Density Estimation (KDE) Proportional Overlap Contour Maps: Since the Delta is a tidal system, for fish entrainment, the range of velocity magnitude the fish experience over a tidal cycle matters. The changes throughout the tidal cycle influence how fish navigate through the connected labyrinth of channels in the South Delta. For example, Figure 4 (p. 12) shows a very distinct difference in the velocity fields over the tidal cycle at Station Old River and Middle River in the ‘with pumping’ and ‘no pumping’ scenarios. Note that the average median velocity over a tidal cycle may change very little (e.g. Figure 4, Velocity Differential = 0.14 fps) between the pumps on vs. pumps off scenario. But, fish entrainment is a tidal process, not a tidally-averaged process.	Reclamation would like to consider velocity differences in future analyses. Reclamation will consider this for future analytical revisions in the LTO adaptive management program related to salmonid and Delta smelt OMR management.
Sacramento River channel lengths of the Delta should not be included in a calculation of the zone of influence. The “Channel Length of Delta” figures of the Proportion of total channel length in the Delta (e.g. Figures 13-21) are diluted significantly from the actual results. There is no physical basis for including channels all the way up to the I Street Bridge on the Sacramento, Mokelumne System, Sutter Slough, Georgiana Slough, Yolo Bypass, and the Deep Water Ship Channel in a “Zone of Export Pump Influence” calculation. Likewise, it is not clear whether other channels in the Suisun Bay region were also incorporated into this calculation.	Reclamation will investigate reducing the spatial limits for base channel length in future analyses. Reclamation will consider this for future analytical revisions in the LTO adaptive management program related to salmonid and Delta smelt OMR management.
The results of all the zone of influence charts clearly demonstrate that the zone of influence of pumping rates is associated with all the channels on the export pump side of the San Joaquin River. The Panel would argue that there is also an influence of pump operations at channel junctions along the San Joaquin River. However, these results clearly show that Sacramento channels (all tributary channels north of the San Joaquin River) are not part of the zone of influence of the export pumps.	No response identified

Comment	Response
The modeling group responsible for the Gaussian KDE analysis needs to do much more synthesis of results. A map is necessary to show where the stations are for Figures 20-22. Figure 22 (p. 5-80) has the x-axis scale flipped compared to the other two figures. This axis should be consistent to promote understanding of the underlying mechanisms causing the velocity shifts.	Added map to winter-run Chinook salmon Biological Assessment ZOI section and Attachment I.3 ZOI. Axes are consistent in terms of velocity value (from negative to positive going left to right). The additional labels "away from" and "to" pumps become inconsistent between stations that are upstream versus downstream of the pumps and is meant to add to understanding for the reader.
Proportion of total channel length analysis: Fix Figure 24 (which channels should be included) before doing the analysis in Figure 29. The signal has been too diluted to give meaningful results.	Reclamation will investigate reducing the spatial limits for base channel length in future analyses.
(Y) Attachment J.3 – Zooplankton-Delta Outflow Analysis	*N/A*
Additional material reviewed: a) BA Chapter 9 Delta Smelt (9-20 to 9-95), and b) LTO Appendix J – Winter and Spring Pulses and Delta Outflow (Section 6.7, p. 55-57; pdf p. 63-65)	No response identified
The Panel identified multiple issues regarding the Zooplankton-Delta Outflow analysis. There are issues both with hydrodynamics and biological aspects of the analysis.	N/A

Comment	Response
While Reclamation included appropriate caveats on how to interpret these results in the texts, the Panel wants to emphasize these are major uncertainties and not just the usual cautions. They go to the foundation of the analysis. Some examples are: *"Yet the mechanism for why CPUE increases in the low salinity zone during higher outflow has not been clearly and definitively established. Kimmerer (2002) found lower tropic level taxa (zooplankton) responded inconsistently with flow across seasons and historical periods. Kimmerer also found that chlorophyll showed little response to flow, suggesting a bottom up, "agricultural model" explanation for increased CPUE with higher flows is unlikely" (LTO Appendix J, Attachment J.3, p. J.3-5). "Another possible mechanism is that increased flow also increase subsidies of zooplankton from higher abundance freshwater regions into the LSZ (Hassrick et al. 2023, Kimmerer et al. 2019)" (LTO Appendix J, Attachment J.3, p. J.3-6). "A historical regression of zooplankton CPUE with flow may be too simple and including other factors such as salinity, temperature, chlorophyll-a, residence time, etc. may have more explanatory power" (LTO Appendix J, Attachment J.3, p. J.3-3 – J.3-4).* The Panel wants to be sure that these caveats and others permeate throughout the entire document, especially in the interpretation of results. The Panel suggests that an important question to answer as part of the analysis is: What magnitude of Delta outflow is needed before a significant shift in zooplankton response can be observed?	N/A

Comment	Response
The variability in the simulation flow data from CALSIM-3 is averaged out of the analysis. This analysis should use a seasonal averaging approach rather than an annual averaging approach. As a result, the differences between the different Baseline Conditions and Alternatives are muted in results.	A seasonal averaging approach was used in the analysis. Added text to clarify. Now reads as: "*For each taxon, mean seasonal loge-transformed catch per unit effort + 1 for each taxon for each year was regressed against mean loge-transformed Delta outflow for each seasonal period for each year. Statistically significant regressions (Table J.3 1 through Table J.3 4) were then applied to the seasonal 1922-2021 CalSim 3-modeled data for Baseline Conditions and Proposed Action scenarios for each season (e.g. only CalSim 3 modeled outflow from Dec. to Jan. was used for the winter analysis), with predictions back-transformed to the original measurement scale (catch per unit effort, number per cubic meter) for summary of results.*"
To create the regression curves, Delta outflow historic data from 2000-2021 from the DAYFLOW database was used. Delta outflow is available on a daily time step in this database. For this analysis, "for each taxon, mean annual log-transformed catch per unit effort + 1 was regressed against **mean annual loge-transformed Delta outflow** for each season period" (LTO Appendix J, Attachment J.3, p. J.3-2, bold added).	A seasonal averaging approach was used in the analysis. Added text to clarify. Now reads as: "*For each taxon, mean seasonal loge-transformed catch per unit effort + 1 for each taxon for each year was regressed against mean loge-transformed Delta outflow for each seasonal period for each year. Statistically significant regressions (Table J.3 1 through Table J.3 4) were then applied to the seasonal 1922-2021 CalSim 3-modeled data for Baseline Conditions and Proposed Action scenarios for each season (e.g. only CalSim 3 modeled outflow from Dec. to Jan. was used for the winter analysis), with predictions back-transformed to the original measurement scale (catch per unit effort, number per cubic meter) for summary of results.*"
Instead of regressing the catch per unit effort +1 values against the Delta outflow representative of the months of the season of interest (Spring, Summer, Fall), Reclamation used the average Delta outflow over the entire year. This means annual Delta outflow may or may not be representative of the Delta outflow during the season when the sampling took place. A better alternative would be to find a seasonal mean Delta outflow for Spring (March-May), Summer (June-August), and Fall (September-November).	*A seasonal averaging approach was used in the analysis. Added text to clarify. Now reads as: "For each taxon, mean seasonal loge-transformed catch per unit effort + 1 for each taxon for each year was regressed against mean loge-transformed Delta outflow for each seasonal period for each year. Statistically significant regressions (Table J.3 1 through Table J.3 4) were then applied to the seasonal 1922-2021 CalSim 3-modeled data for Baseline Conditions and Proposed Action scenarios for each season (e.g. only CalSim 3 modeled outflow from Dec. to Jan. was used for the winter analysis), with predictions back-transformed to the original measurement scale (catch per unit effort, number per cubic meter) for summary of results.*"

Comment	Response
The analysis states that the regressions "*were then applied to the 1922-2021 CALSIM-3-modeled data for Baseline Conditions and Proposed Action scenarios, with predictions back-transformed to the original measurement scale for summary of results*" (LTO Appendix J, Attachment J.3, p. J.3-2). The CALSIM-3 produces Delta outflow results on a monthly time step but the regression analysis does not incorporate this flow variability available in the dataset. Instead, for each year in the simulation, the CALSIM-3 Delta outflow results are averaged over the entire year to produce a mean annual Delta outflow. If the regressions in the pre-processing step used seasonal mean Delta outflow instead of mean annual Delta outflow, the flow variability in the CALSIM-3 results could be represented.	*A seasonal averaging approach was used in the analysis. Added text to clarify. Now reads as: "For each taxon, mean seasonal loge-transformed catch per unit effort + 1 for each taxon for each year was regressed against mean loge-transformed Delta outflow for each seasonal period for each year. Statistically significant regressions (Table J.3 1 through Table J.3 4) were then applied to the seasonal 1922-2021 CalSim 3-modeled data for Baseline Conditions and Proposed Action scenarios for each season (e.g. only CalSim 3 modeled outflow from Dec. to Jan. was used for the winter analysis), with predictions back-transformed to the original measurement scale (catch per unit effort, number per cubic meter) for summary of results."*
For the figures in the document (LTO Appendix J, Attachment J.3, p. J.3-17 – J.3-34), the 1922-2021 data was binned by water-year type. There is only one CPUE value per year of the water-year type represented. The range bars represent the variability over the entire simulation of 1922-2021. Here again, there is more variability in the flow dataset that is not being incorporated in the synthesis charts.	The tables present mean values for each scenario by water year type. Rather than present numerical values for the variability, it is presented graphically in the boxplots (see figures J.3-1 through J.3-28).
Chapter 9 discusses how the Proposed Action *may increase food availability stressors*. The mechanism is that the proposed storage and diversion of water associated with the Proposed Action will reduce Delta inflows and outflows. Delta smelt feed on calanoid copepods (E. affinis and Sinocalanus doerrii), which exhibit a positive correlation with Delta outflow in spring. Pulse spring flows in dry water years can increase copepod biomass near Suisun Bay. Thus, if the Proposed Action reduces pulse spring flows or Delta outflows, there will be a subsequent reduction in zooplankton prey. This needs to be discussed in some detail. For example, the frequency of an increase in the stressor (low food availability/use Delta outflow as a proxy) is likely high (78% of years were low spring outflow; 81% of years were low winter outflow).	Discussion of the Proposed Action effects on food availability stressor has been expanded in the Biological Assessment using the zooplankton Delta Outflow analysis in Section 9.2.1.1.

Comment	Response
The two studies cited as “multiple studies” in Chapter 9 are Merz et al. (2011) in the white literature/non-peer reviewed and the other is a non-species specific zooplankton flow analysis model. Given that Delta smelt are specific in their prey selection, the analysis should try to be as species-specific as possible to accurately assess food as a stressor.	The language has been clarified; the word "multiple" has been removed. Additionally, the language for the Zooplankton Flow Analysis Model has also been clarified and the text "not species specific" has been removed. The use of "not species specific" refers to the fact that this analysis is not just for Delta smelt but also applicable for longfin smelt as well too since the analysis looks at multiple zooplankton species. In the attachment for the Zooplankton Flow Analysis Model it states: "*Zooplankton examined in the analyses were based on taxa (species or species groupings, split by life stage where appropriate) included in recent modeling and diet studies of both Delta smelt and longfin smelt (Slater and Baxter 2014, Smith 2021:45; Barros et al. 2022; Smith and Nobriga 2023).*", so the analysis did try to be as accurate as possible with the zooplankton species examined.
The food availability stressor links to Delta outflow-zooplankton regressions. The Panel notes that all life stages in the Delta Smelt Chapter 9 are treated the same: same rationale, data, and explanation is given linking Delta smelt to zooplankton. The Panel suggests that more description and rationale be provided about the datasets used for each life stage, as some datasets are more complete and relevant than others. It is important to indicate why certain data sets and zooplankton species were considered. The documentation should be sufficient so that the reader can determine that the species and life stages of the zooplankton are appropriately applied to the life stages of the Delta smelt.	For each life stage under the Food Availability Stressor (9.2.1.1 for Adults, 9.2.2.1 for Eggs and Larvae, 9.2.3.1 for Juvenile) the first paragraph describes prey items the particular life stage feed on and the time period being examined. Further down in the respective sections the species that had a significant relationship with outflow that were examined as part of the Zooplankton-Delta Outflow analysis are listed. Further information on the individual zooplankton species examine can also be found in Attachment J.3 Zooplankton-Delta Outflow.

Comment	Response
A table is needed to show Delta outflow and % change in Delta outflow with corresponding species-specific zooplankton CPUE and % change in zooplankton CPUE. The Panel was unable to gauge the sensitivity of the zooplankton response among scenarios without a table of this type. Reclamation provides zooplankton output tables, but not in the context of the Delta outflow – just the water-year type and the scenario type. The reader must assume there are differences in the amount of Delta outflow, but this information is not explicitly available in the Zooplankton-Delta Outflow Appendix J Attachment J.3 methodology, or in Ch. 9 (Delta Smelt).	New tables added showing mean modelled CalSim3 outflow for each season across different water year types by scenario. New figures also added showing mean, inter and intra quartile ranges of modelled CalSim3 outflow for each season across different water year types by scenario.
For simulated Delta outflow from CALSIM-3, is there one Delta outflow metric per season that holds steady (Appendix J, Attachment J.3)? What are the 95% CIs of that flow per season, and how sensitive are the zooplankton responses across the range of CALSIM-3 Delta outflow results?	New tables added showing mean modelled CalSim3 outflow for each season across different water year types by scenario. New figures also added showing median, inter and intra quartile ranges of modelled CalSim3 outflow for each season across different water year types by scenario.
The methodology appears to only use ONE parameter (Delta outflow) to explain Delta zooplankton. Multiple papers (mentioned in the materials) identify other factors that are predictive of Delta zooplankton production. As a result, many prevalent prey items are excluded from the analysis because they do not have a significant relationship with Delta outflow. The Panel would like to see how Delta outflow relates to these factors (e.g. turbidity, spatial location, temperature, actual salinity residence time, etc.). While Delta outflow is likely correlated with these factors, including each parameter and being able to test the sensitivity of the zooplankton response to those parameters may be valuable. Choosing the parameters to include in the best-fit model would require running an Akaike Information Criterion (AIC) analysis.	Added additional detail to acknowledge this in the text. In section J.3.2.2 Assumptions/Uncertainty, added, "For example, Hartman et al. (2024) found regional differences in the effects of drought on both chlorophyll and total zooplankton biomass due to differential impacts of residence time and top-down control by predation (especially the overbite clam, *Potamocorbula amurensis*). "

Comment	Response
Zooplankton export from productive ecosystems near Liberty Island, Cache Slough Complex, Barker Slough, and the Deepwater Ship Channel has been shown to influence zooplankton densities in the Delta and is positively correlated with Delta outflow. This is mentioned in the text and seems like it could be incorporated into the modeling effort herein to understand how upstream zooplankton production might be conveyed to the Delta low salinity zone (LSZ) habitat for consumption, and how that production and conveyance might change across the Proposed Action and scenarios. This linkage is missing and would require linking an upstream/freshwater habitat zooplankton/flow model to Delta outflow.	N/A
Plankton production is often linked with shallow shoals and marsh edge habitats. How do Delta outflows for each alternative map the preferred salinity and turbidity ranges of Delta smelt onto the preferred habitats of smelt at different life stages? What percentage of the shallow water and fringing marsh habitats available in the Delta, which produce zooplankton, are captured in the low/preferred salinity zone?	N/A
The Panel was confused by the tables (Tables 1-17, Appendix J, Attachment J.3). For each set of paired tables per species, there needs to be a measure of Delta outflow.	New tables added showing mean modelled CalSim3 outflow for each season across different water year types by scenario. New figures also added showing mean, inter and intra quartile ranges of modelled CalSim3 outflow for each season across different water year types by scenario.
No measures of uncertainty are reported in these Tables (i.e. % change in CPUE in E. affinis). Each prey species is reported individually, but fish can consume a range of prey types. It would also be good to understand the total zooplankton CPUE response. Overall, if some are increasing and others decreasing, is the general population holding steady across scenarios? Decreasing? Increasing?	Reclamation will consider ways of presenting these data in the LTO adaptive management program related to spring outflow and Delta smelt summer fall habitat action.

Comment	Response
Figures show a high degree of uncertainty with error bars. There is no explanation provided on whether they are 95% CIs or something else. Given the error bars, it would appear that most scenarios are not significantly different from one another. That said, the summary suggests that in Critical Years, alternative action is better for zooplankton action than No Action. This should be clarified, as Alt2woTUCPwoVA does not improve food web conditions in the spring (but you can only really see that for "other calanoid copepods", and this size class is actually the one to pay attention to for larval-juvenile Delta smelt).	The figure captions have been clarified, the text now reads: "*Median, quartile and interquartile ranges of Box Plots of CPUE of …*". The summary portion has been reworked and rewritten, the statement regarding Critical Year CPUE has been removed.
Modeling zooplankton species only represents a proportion of Delta smelt diets (i.e. only those with a significant relationship to Delta outflow). It would be good to have a table showing what % of Delta smelt diets are represented by the prey items in the models. For example, a 15% decrease in a species rarely consumed has a very different impact than a 15% difference in a preferred prey species.	N/A
Note on Table 13 (p. J.3-14 of Appendix J, Attachment J.3) - "*Provide biological, ecological, and operational explanation for the observation*". That has not happened yet in the table explanation of the results but the Panel concurs that these comments should be included.	More text was added after the transmission of this document. Additional text added: "*Kimmerer (2002) found a significant relationship between adult E. affinis and outflow. However, this relationship was only present after 1987, the post Potamocorbula amurensis invasion period, which also coincided with a seven-fold decline in E. affinis. This decline is likely due to predation by P. amurensis and replacement by another introduced calanoid copepod, Psuedodiaptomus forbesi which is able to overcome predation pressure due to subsidies from more freshwater regions where P. amurensis isn't present (Durand 2010). Peak abundance of E. affinis has shifted several months to the spring season from the summer season (Merz et al. 2016). While there is a significant relationship between outflow and CPUE of adult E. affinis in the fall, the effect is likely negligible for fish species that prey on calanoid copepods.*"

Comment	Response
The document (Appendix J, Attachment J.3) discusses regression equations from Hennessy and Burris 2017, which predict E. affinis and N. mercedis, and P. forbesi to mean June-September Delta outflow. Reclamation decided these regressions were geographically too simplistic and too temporally broad for applying to the effects of operations (Appendix J, Section 5.3.2.2, p. 36). However, the Panel did not see how the approach used by Reclamation was a major advancement for being more geographically complex. And further, with the approach that was used, P. forbesi was dropped from the equations because there was not a statistically significant relationship with the Delta outflow.	The Zooplankton-Delta Outflow analysis used a narrower range of data (2000 onwards) to better reflect more recent conditions compared to Hennessy and Burris 2017 which analyzed data from 1988 - 2015. Additionally, Hennessy and Burris 2017 used data from spatially fixed stations for *P. forbesi* whereas the Zooplankton-Delta Outflow analysis uses salinity zones rather than a geographic region since the low salinity zone can move in and out of Suisun Bay depend on many factors including outflow. While *P. forbesi* was dropped as not statistically significant, the p-value was lowest during the fall season for adults (p = 0.08, table J.3-4). The analysis by Hennessy and Burris 2017 combines both juvenile and adult copepods whereas this analysis separates out the life stages. Different life stages may be more capable of different behaviors such as demersal migration (see Kimmerer and Slaughter 2016 which found evidence of behavioral plasticity in adult *P. forbesi* depending on local environmental conditions).
The Panel suggests that Reclamation consider adding a spatial component to the analysis. The results, as presented, provide an analysis of potential changes to the food web for the Proposed Action and each of the alternatives binned by water year. Further dividing responses spatially might be helpful, and that could complement if a seasonal reporting of Spring, Summer, Fall, and Winter is also done. (This is because Delta smelt use different areas of the estuary in different percentages, and it would be important to know if the areas where zooplankton are most likely to change (+ or -) overlap with the areas with the most Delta smelt.) What spatial areas might become more limited? What areas might become less limited?	N/A

Comment	Response
The following statement is made in Appendix J: "In the spring CPUE is LESS under alternatives compared to NAA for all but critical water years. Thus, alternatives appear to provide benefits over NAA to smelt in the spring of critical water years only." This is an important result. However, the Panel does not see how the figures in Appendix J support this conclusion. Spring CPUE for the NAA vs Alternatives is variable by the Alternative, water year, and species. Mostly there is strong overlap, and it would be difficult to statistically and ecologically distinguish differences. The Panel does note that Alt2woTUCPAllVA appears better than the NAA for spring E. Affinis across multiple water-year types.	This summary statement has been removed from the current version of Appendix J. Reclamation recognize that there is strong overlap between the different alternatives and that there is high variablity in CPUE and has added a statement recognizing the fact in the new uncertainties section for the Zooplankton-Delta outflow summary.
(Z) Attachment M.1 American River Redd Dewatering Mortality	*N/A*
Additional material reviewed: a) Appendix M Folsom Reservoir Flow and Temperature Management, and b) Appendix O Tributary Habitat Restoration	No response identified.
Modeling dewatering risk applies to Steelhead and fall-run Chinook salmon below Nimbus Dam on the American River. Historic Steelhead redd dewatering is mentioned (Appendix M, section 6.7), along with a comment that it has not been reported in recent years. The modeling approach described for the American River is reported in Appendix O Tributary Habitat Restoration as part of the Redd Dewatering Analysis (section 4.7.2). The analysis for the Stanislaus River is missing (a placeholder) in Appendix O and there does not seem to be a corresponding description for the Upper Sacramento below Keswick Dam, although dewatering is referred to in the LCMs.	Redd dewatering analyses were conducted for the American and Sacramento rivers (not Clear Creek or the Stanislaus). The Sacramento River redd dewatering analysis will be provided in the Final Biological Assessment

Comment	Response
The approach described in Appendix M for Steelhead trout assumes that dewatering mortality occurs when monthly flows decrease from those at the time (month) of spawning. The approach is very coarse in some respects. For example, to represent a 2-month period of potential risk, only the maximum flow reduction based on monthly flow is used to estimate dewatering mortality. If the Panel understands this correctly, this would give only two numbers to compare, the value in the first month and the value in the second month. In addition, temperature, which influences the rate of egg and larval development (but is also moderated by groundwater), is neglected.	The analysis for steelhead computes stage reductions using the month of spawning and the minimum of the next two months. For fall-run Chinook salmon, the analysis compares stage in the month of spawning with the minimum of the next three months. Modeling daily flows of the lower American River would be needed for a more a fine-grained analysis of redd dewatering and for temperature effects on the duration of the period of incubating eggs and alevins for dewatering risk.
In other respects, the analysis is detailed and site-specific, considering the distribution of spawning times based on redd counts and estimates of redd depths for each reach. It is unclear whether this spawning time “distribution” is also across just two months. The distribution of redd depths also influences the estimate of mortality (Table M.1-1). A minor suggestion is that Table M.1-1 would be more concisely presented as a graph of cumulative curves. In addition, an equation is needed to communicate exactly how the estimate (and its variance) is produced. The approach estimated that the proportion of redds dewatered was fairly high, especially in wetter years.	Page 2 of the attachment gives the spawning months as: "... January through March for steelhead and October through December for fall-run Chinook." The variance of the redd dewatering estimates was not determined as there is insufficient information available to do so. As noted in Item #2 of Section M.1.2.2, Assumptions / Uncertainty of the Attachment, the analysis assumes 100% mortality of eggs and alevins in dewatered redds, which is likely high. Regarding use of cumulative curves in place of Table M.1-1, if this approach is used in the future, these recommendations may be adopted through future iteration in LTO adaptive management program related to tributary habitat restoration effectiveness monitoring for salmonid fishes.
The Panel recommends: a) tracking daily changes and the duration of exposure of redds, giving credit for rewetting, b) consider using HEC-5Q daily output to allow simulation of development and differences between eggs and alevins, c) providing information about model validation, and d) presenting an equation for mortality.	If this approach is used in the future, these recommendations can improve the model through the LTO adaptive management program related to Shasta coldwater pool management and tributary habitat restoration effectiveness monitoring for salmonid fishes.