Memorandum

To: Ben Nelson, Bureau of Reclamation, Bay-Delta Office, Sacramento, CA

From: Marin Greenwood and Lenny Grimaldo, ICF

Date: 9/27/2017

Re: Response to Delta Smelt Scoping Team and Natural Resources Defense Council Comments Received During or After Preparation of the *2017 Fall X2 Adaptive Management Plan Proposal*

This memorandum provides a summary of comments provided by the Collaborative Adaptive Management Team's (CAMT) Delta Smelt Scoping Team (DSST) on the *Public Water Agency 2017 Fall X2 Adaptive Management Plan Proposal* (Proposal), with a focus on the draft effects analysis. The comment summary is based on the summary provided by Bruce DiGennaro (Program Manager, Collaborative Science and Adaptive Management Program [CSAMP]) in a memorandum provided to the CSAMP Policy Group on 8/26/2017. Herein we summarize our responses to the comments received on the Proposal's draft effects analysis, as well as additional comments from the DSST and NRDC that were subsequently received in response to the revised Proposal's effects analysis. Note that we have limited our summary to include only comments and responses pertaining to our effects analysis, as opposed to the Proposal itself. We hope that this information is useful context for the October 2017 Delta Smelt Outflow action Environmental Assessment for which Reclamation is seeking comment.

Timeline of Process

The main events in the process for providing comments were as follows by date:

- 8/15/2017: The DSST met with us. CAMT directed DSST to review our draft effects analysis for the proposed 2017 Fall X2 Action. In addition to discussing the effects analysis, the DSST also discussed the proposed action itself, particularly with regard to the experimental design and the ability to learn from the action as proposed. Verbal comments were provided to us, with written comments following. We began to revise the effects analysis in response to the comments.
- 8/21/2017: We had a conference call with the DSST to discuss our initial responses to the verbal comments and written comments received prior to the call. We continued to revise the effects analysis in response to the comments.

- 8/28/2017: A summary of the process was provided to the CSAMP Policy Group, and we summarized our main responses to comments at a meeting with the Group. Subsequent to this, we completed our revisions to the effects analysis contained within the Proposal.
- 9/15/2017: Additional comments were received from Sam Luoma of the DSST on the revised effects analysis included in the Proposal.
- 9/25/2017: We were forwarded comments from NRDC that had been provided to USFWS, Reclamation, and the California Department of Fish and Wildlife (DFW).

Comments and Responses

In the following text, comments are taken directly from the summary provided by Bruce diGennaro or as provided by Sam Luoma and NRDC; these are shown in bold, with our responses in italics.

Initial Comments from 8/15/2017 DSST Meeting

Comment: 2. The analysis does not cite (or try to replicate) the right Feyrer study – check to be sure the right study is being referenced. *ICF Response: The effects analysis in fact was updating an analysis from the USFWS (2008) SWP/CVP Biological Opinion (BiOp) that was based on Feyrer et al. (2007). The necessary references were corrected for the final Proposal document.*

Comment: 3. The FLoAT Workplan updated the FLaSH look-up tables – use updated tables. *ICF Response: We found no evidence that the FLoAT Workplan included any such update of the FLaSH look-up tables (assuming that these were the area of low salinity zone and the abiotic habitat index tables).*

Comment: 4. Provide updated estimates of the area of the LSZ -see Table 2 from Feyrer 2011. *ICF Response: To our knowledge, the estimates of the area of the LSZ that we included were the best available, as used in the IEP MAST report (2015). Feyrer et al. (2011) does not have a Table 2. Our Figure 2 includes the same information as presented in the IEP MAST report..*

Comment: 6. Provide plots of projected flows under the proposed alternative – will allow for an analysis of potential impacts in other parts of the system, not just in the Delta. *ICF Response: Hydrological forecast information (i.e. projected flows) was provided in the final effects analysis; see Figure 13 and Table 1. Consideration of effects in other parts of the system were provided in the discussion of* Upstream Effects (Reservoir Storage) *on p.128 of our final analysis.*

Comment: 7. Compare operations under the BiOp RPA with operations under the proposed action. Provide 50% and 90% exceedance forecasts, similar to what has been done in the past. *ICF Response: The previously described hydrological forecast information considered an 80% confidence interval of the range in fall hydrology, bracketed within 'wet' and 'dry' bounds. See e.g., Figure 13.*

Comment: 8. Consider regime changes. Do analyses on different time series to reflect current conditions (post POD). Mixing regimes distorts the results. Is X2 location more important under the current regime? *ICF Response: Additional analyses were conducted to focus on the post-POD era (taken to be 2003 onwards), but did not change the conclusions.*

Comment: 9. Examine availability versus use of habitat. Look at relationship between fish presence and salinity. Distribution is partly geographic, not just driven by salinity. Is salinity determining distribution/position? Look at spatial distribution. *ICF Response: The analysis focused on the low-salinity zone (salinity = 1 to 6) as this has been the focus of recent investigations in addition, this area would be most affected by changes in Delta outflow. The final effects analysis acknowledged (p.43): "Although Delta Smelt fall occurrence is generally greatest in the low salinity zone and the centroid of distribution generally moves upstream as the salinity field moves upstream (Sommer et al. 2011), the overall distribution occurs over a broader range of salinity than solely the low salinity zone (Sommer and Mejia 2013; Moyle et al. 2016)."*

Comment: 10. The analyses should address key uncertainties and alternative hypotheses, not just repeat previous analyses (e.g. are fish habitat limited in the fall?) *ICF Response: It was beyond the scope of the present analysis to address key uncertainties and alternative hypotheses; the focus was on assessing potential effects of the Proposal, given available information. We support the agencies' adaptive management program and would support investigating these alternative hypotheses.*

Comment: 11. Add analyses of 2 model runs from John Leahigh and compare them. *ICF Response: As previously noted, hydrological forecast information was included which considered an 80% confidence interval of the range in fall hydrology, bracketed within 'wet' and 'dry' bounds.*

First Round of Written Comments

Bill Bennett (The Bay Institute)

Comment: 1. Although overall the proposal is well-written, I find the use of citations uneven; the results of some studies being accepted outright, or overemphasized, without a more critical view. Some processes are glossed over without a better explanation of how, or the scales, at which they work, e.g. turbidity and zooplankton. ***I****CF Response: The Proposal's effects analysis focused on a comparative assessment of the potential effects of different X2 values. The purpose of the analysis was to explore the extent to which components of Delta Smelt habitat change when X2 is at 74km as compared to 81km. The final draft included additional citation. This analysis was not intended to be a critical review of the published literature. In general, citations were included only to briefly introduce the topics being explored.*

Comment: 2. Statistically the analyses are computed and described well, however, the use of the Ricker stock-recruitment model for delta smelt is simply not warranted. The primary assumption being the existence of strong density dependence, which is highly unlikely for delta smelt.Several alternative models are readily available, e.g. Beverton-Holt, etc. ICF *Response: A preliminary Beverton-Holt stock-recruitment analysis was included in the revised effects analysis (p.41-42; Figure 20). The use of the Beverton-Holt model did not change the results.*

Comment: 3. While we have had to focus on the MWT and TNS indices, they are used here without some regard to their limitations. In simple terms, the TNS is only about 50% effective at retaining juvenile smelt, and the MWT especially in Sept-Oct is barely 40% effective. This alone could explain a fair bit of the variability shown in the modeling results. I've worked with K. Newman and Wim to develop gear-selection curves to understand this better. In recent years, population abundance has been hovering at limits of these indices to detect interannual change except when it is dramatic, e.g. 2011. *ICF Response: This analysis used the available data consistent with the USFWS (2008) BiOp. However, this comment was acknowledged in the final effects analysis (p.40-41). I It was noted that many studies have used the same data (e.g., MWT and TNS), including the USFWS (2008) BiOp, while also noting that this is certainly an issue to be revisited for future investigations, possibly with gear collection adjustments.*

Comment: 4. Likely, I've missed something, but with regard to X2 position and habitat availability, it isn't surprising X2=74 moves LSZ into Suisun Bay... we knew that. However, the San Joaquin/south Suisun Bay region, especially in Sept-Oct, is often not

suitable habitat anymore... i.e., high water temperatures and Egeria, etc... can preclude use and survival of smelt in that region. Given that, does X2 @ 74 actually provide more suitable habitat than X2 @ 81 in early fall? *ICF Response: The effects analysis used well established methods to demonstrate that the extent of suitable habitat would be greater with X2 at 74 km compared to 81 km, and acknowledged that the Proposal would result in reduced extent of the low salinity zone and related effects.*

Pat Coulston (California Department of Fish and Wildlife)

Comment: 1. I strongly agree with Sam's comment "1" about focusing the analysis on the POD era. I will add that the suggested focus is particularly compelling given that we are addressing a flow-related proposal, and a change in Delta Smelt response to flow is evident in the present regime. *ICF Response: The draft analyses and additional preliminary stock-recruitment analyses were revised to include consideration of the POD era, taken to be 2003-2015/2016, although this did not change the main conclusions from the draft analysis.*

Comment: 2. I am struggling to make sense out of the EA's choice of the stock-recruitment relationship over the FMWT through STN life history span as the basis for the EA analysis. As I understand it, the analysis examines influences on the stock-recruitment, with the FMWT index being the measure of stock and the subsequent STN index being the measure of recruitment. The problem, as I see it is that the STN index is heavily influenced by spring conditions which are very much independent of the fall conditions we are trying to learn something about. It seems like it would be less messy to look at survival from the STN index through the SKT index. *ICF Response: The draft analysis used the same framework as that adopted by Feyrer et al. (2007) and the related analysis from the USFWS (2008) BiOp. In response to the comment, an additional preliminary analysis considering the SKT/STN ratio was provided, which showed only a weak effect of fall X2 (see Figure 19 in revised effects analysis).*

Comment: 3. I think the present (dire) status of the Delta Smelt population is important context for the EA, and should be briefly described. The species' status needs to be considered when interpreting the risk of the uncertainties in the EA analysis results. Another aspect of the current context is the recent flow requirement relaxations associated with the drought. Given that (as always) the next drought may have already started, losing this year's opportunity to test 74/74/November, and retain its expected species benefits, seems risky. *ICF Response: A discussion of the status of the species was added (beginning p.20 in revised effects analysis).*

Comment: 4. Is it correct to assume that the 74/81/November proposal will result in greater-than-otherwise export pumping? If so, this collateral effect should be addressed in the EA. *ICF Response: The potential effect of additional south Delta export pumping was addressed in the* Entrainment Effects *section on p.126-127 of the final effects analysis.*

Comment: 5. As I studied Figure 1 of the draft EA [draft effects anlaysis] I became concerned about how the analytical technique employed would use/interpret the 2011 data point. 2011 had the lowest "R" per "S" ratio, which might be seen as evidence of a density-dependent survival effect. I believe the offspring of the strong 2011 cohort encountered poor (drought-related) conditions in the spring of 2012 which may have applied strong density-independent pressures on the recruits. Any suggestion of density-dependent survival in the analysis needs special scrutiny given the low abundance of Delta Smelt. *ICF Response: In response to other comments, a Beverton-Holt relationship was explored to address an alternative form of density-dependence (or lack thereof).*

Comment: 6. The EA should include a comparison among years of the progression of indices (SKT to 20mm to STN to FMWT to SKT) as the annual cohorts age. Of particular interest for assessing the effects of the proposed fall action would be an examination of indices progression in the recent (post-POD) wetter years 2003(AN), 2005 (AN), 2006 (W), and 2011(W). The patterns of flow (X2) and related survival in these years could inform a[n] assessment of expected effects in 2017 from application of the extant RPA (74/74/November) or the proposed alternative action (74/81/November). *ICF Response: The effects analysis documented that similar conditions as could occur in 2017 (i.e., ~74 km in September followed by up to 81 km in October) did not occur during 1960 to 2015 (see discussion on p.69-70 of the revised effects analysis); therefore it is unclear how such an exploration could inform the situation that might occur in 2017.*

Comment: 7. During the 8/21 call, Bill [Bennett] did a much better job of articulating the stock-recruitment approach concern I attempted to express in comment "5" above. As I recall, Bill made the points that the S/R formulation incorporated into the modelling can have large effects on the analytical outcomes, that the EA's use of the Ricker formulation unrealistic, and that other formulations (e.g. Beverton-Holt) should be considered. *ICF Response: As previously noted, a preliminary Beverton-Holt exploration was conducted and included in the revised effects analysis (p.41, Figure 20).*

Comment: 8. The second paragraph on page 21 notes that the EA's environment-recruitment (FMWT-STN) analysis does not reveal a population-level response. This is not surprising given the impact spring conditions have on the STN index, and the mixture of pre- and post-POD years. The strong positive STN to SKT response in 2006-7 and 2011-12 does suggest a within cohort population response to late summer-fall flows in the POD era. *ICF Response: As previously noted, preliminary analyses focusing on the relationship between recruitment (SKT) and stock (STN) in relation to fall X2 were included in the final effects analysis (see Figures 19 and 20). The results were consistent with the prior analysis.*

Scott Hamilton (Coalition for a Sustainable Delta)

Study Structure

Comment: Regarding the structure of the draft Effects Analysis, we suggest adding a clear statement of the study's purpose. We assume this to be an analysis of the effects on delta smelt performance in 2017 of X2 positioned at 81 km versus 74 km in October. Given the extensive consideration of available of data in the analysis, we suggest that the draft Effects Analysis consider other X2 locations, including positions greater than 81 km. *ICF Response: The analysis focused on the proposed 2017 action, as described in the Project Description—this had an upper bound of 81 km, and additional consideration of intermediate values of X2 between 74 km and 81 km was added on the basis of the operational forecast provided by DWR. For those interested in additional values of X2, the full range of available data are provided in the plots relating X2 to the various outcomes that were analyzed.*

Comment: Considering the elements essential to a successful effects analysis, a conceptual ecological model describing the environmental factors and system pathways that affect the performance of delta smelt in the fall is necessary. However, the conceptual model from the MAST report referenced in the current draft is not consistent with that that is implicit in the Biological Opinion. Accordingly, management-relevant hypotheses that might be drawn from the implicit conceptual model are neither identified nor tested in the draft Effects Analysis. *ICF Response: It is unclear why the commenter suggests that management-relevant hypotheses were not tested or examined, specific examples include the relationship of X2 to zooplankton density and X2 to invasive clam density.*

Method and Approach

Comment: The Service identifies potential adverse modification of habitat resulting from water-export operations in the Biological Opinion and references Feyrer et al. (2007) as

describing the source of that impact. Feyrer et al's assertion that a decrease in "abiotic habitat" (an inappropriate term referencing the extent of the low-salinity zone) in the fall is relevant to delta smelt success depends on the validation of at least two hypotheses -- (H1) that habitat attributes essential to delta smelt performance have been identified correctly and(H2) that delta smelt are habitat limited in the fall. Given the recent finding by Service scientists that salinity in the winter and spring explains "very little of the variation in adult catch when a regional spatial adjustment to density is included" (Polansky et al. 2017), and that Feyrer et al. (2007) did not consider food in their assessment of the contribution of habitat factors to delta smelt abundance, we encourage evaluation of both of these hypotheses in the document using available data. The concern is that although the salinity field may change position and extent in response to fall outflow, there are no data that establish that salinity is a determinant of position and extent of habitat, and, moreover, no evidence indicates that delta smelt are habitat limited in the fall. Combined, these facts suggest that modifying the monthly average location of X2 in the fall is unlikely to benefit delta smelt. The evaluation of the two standing hypotheses from the Biological Opinion is acritical element of the effects analysis.** *ICF Response: The effects analysis focuses on the potential for fall X2 to affect Delta Smelt as expressed by a population-level outcome (summer abundance) following the framework applied in the USFWS (2008) BiOp, with some modifications. As previously noted, the revised effects analysis acknowledged (p.43): "Although Delta Smelt fall occurrence is generally greatest in the low salinity zone and the centroid of distribution generally moves upstream as the salinity field moves upstream (Sommer et al. 2011), the overall distribution occurs over a broader range of salinity than solely the low salinity zone (Sommer and Mejia 2013; Moyle et al. 2016)." The Polansky et al. (2017) finding was in regard to species distribution in the winter, not fall, and therefore its relevance to this study is unclear. Critical habitat as defined by USFWS was a necessary focus for the analysis and therefore was included.*

Comment: We would encourage the authors of the document to use a life-stage-explicit approach in the analyses, and recognize that delta smelt abundance is regulated by one or more limiting factors during the course of a year (see a discussion of limiting factors in Cade and Noon 2003). If the factors that limit the performance of delta smelt rarely act on the population in the fall, management actions undertaken in the fall will be ineffective. For that reason, the document authors might explicitly examine the relevance of the four quantitative studies that considered the position of X2 in the estuary in the fall, but found no statistical support for X2 as a determinant of delta smelt abundance (See MacNally et al. 2010, Maunder and Deriso 2011, Miller et al. 2012, Thomson et al.

2010). *ICF Response: These studies were cited on p.37 of the effects analysis, with the exception of Maunder and Deriso (2011), which did not examine the effect of fall X2.*

Analytical Approach

Comment: The effects analysis might be enhanced by considering the distribution of the delta smelt at a sub-regional level in the Sacramento-San Joaquin Delta and adjacent San Francisco Estuary. If delta smelt are predominantly located in Cache Slough, as evidenced by EDSM returns over the past several months, the effects analysis might explicitly ask what exactly the benefits of altering outflow through the Delta could be. *ICF Response: The most recent distribution information for Delta Smelt was included in a new section entitled* Current Delta Smelt Spatial Distribution *(p.23-26 of final effects analysis), which used both the summer townet survey data and EDSM data to illustrate that most Delta Smelt appeared to be in or near the low salinity zone (Figures 8-12).*

Comment: We laud the graphical presentations that relate the availability of delta smelt prey to theposition of the low-salinity zone (in figures 7 through 46), noting that densities of calanoid copepod adults appear greater when X2 is positioned at more eastern locations. The document might speculate on the underlying mechanism. All other environmental factors being equal, the question might be asked do increased flows in the fall distribute food away from the center of the distribution of the delta smelt population? *ICF Response: We see little evidence of calanoid copepod responses, either positive or negative, to fall X2 in the data that we analyzed (Figures 61 to 69 for POD-era calanoid copepod analyses in the revised effects analysis).*

Comment: We suggest that the recruitment analysis (Tables 1 and 2) consider both food and a regional spatial adjustment (Polansky et al. 2017). Because the position of X2 is correlated with a number of other factors, including food production earlier in the year, without consideration of a more comprehensive set of covariates, it is likely that cause-and-effect relationships have been omitted, which can lead to incorrect conclusions. The conclusion on page 21 that the "finding does not invalidate work by others hypothesizing fall X2 predicts the quantity and quality of delta smelt habitat" appears to be premature without a more comprehensive analysis, and likely incorrect. *ICF Response: As previously noted, the analysis focused on applying the framework underlying one of analyses from the USFWS (2008) BiOp. Inclusion of other covariates, while certainly of potential importance, would have taken the focus off X2 and in any case would have been challenging to use in a predictive manner given the lack of information for the magnitude of other covariates in fall 2017, particularly food.*

Comment: The effects analysis would be enhanced by including a discussion of the distinction between necessary conditions and sufficient conditions as they pertain to the effects of the position of X2 in the estuary in the fall X2 on delta smelt. That the FMWT Index increased nearly sevenfold from 1992 to 1993 when the averaged X2 location in September and October of 1992 was positioned at 89.9 km and was located at 82.9 km in 1993, seems highly relevant to the effects analysis and raises the question as to what fall X2 position is sufficient for realizing high levels of delta smelt performance. If X2 positioned at 83 km is sufficient to accommodate a high rate of increase in delta smelt abundance, it would suggest that X2 positions west of that location are not per se limiting the species. *ICF Response: It is unclear how 'necessary' and 'sufficient' might be defined; regardless, the analysis sought to bring in all years of data as opposed to focusing on individual-year examples such as the one provided by the commenter.*

Comment: Investigation of the specific mechanisms that may affect delta smelt performance in the hydrologic year-types pertinent to the Fall X2 Action seems appropriate. While there have not been many wet and above-normal years since the Biological Opinion was released, the red dots in Figure 1(b), which suggest that recruitment decreases in wet and above-normal years as outflow increases, combined with diminishment prey numbers during high outflow circumstances (Figures 7-9), indicate that the Fall X2 Action may have unintended deleterious consequences for delta smelt that are worthy of consideration and discussion in the effects analysis. *ICF Response: We would hesitate to ascribe too much significance to trends evident in three datapoints (Figure 15b, the revised version of Figure 1b from the draft effects analysis). As previously noted, we see little evidence of calanoid copepod responses, either positive or negative, to fall X2 in the data that we analyzed (Figures 61 to 69 for POD-era calanoid copepod analyses in the revised effects analysis).*

Comment: The authors of the draft Effects Analysis fit trend lines to many of the graphs from Page 32 extending through the end of the document. In many cases a kinked curve seems more representative, and a number of adverse factors may not act on delta smelt at X2 positions below 84 km (for example invasive clams (Fig 49-51), *Microsystis* (Fig 53), and Secchi depth (turbidity) (Fig 54)). Given the cursory consideration of these and certain other factors considered above, one might wonder if a position for Fall X2 in the range of 83-84 km should be considered as a more appropriate target for both wet and above-normal year-types (particularly given that a "scientific" basis for the 81 km location is lacking). *ICF Response: With respect to nonlinear relationships, this is a fair* *comment for invasive clams and* Microcystis*, but would not change the overall conclusions of there being little effect. The relationships for Secchi disk depth and X2 (original Figure 54, revised Figure 105) seem quite linear to us.*

Comment: It appears that some consequences of increasing outflow in the fall are beneficial and some detrimental, leading to the question of how should a net effect of a predetermined position of X2 be assessed? Without a formal statement of conclusions in the current document, it is difficult to deduce aggregated net effects from the draft Effects Analysis. *ICF Response: The revised Proposal provided a* Conclusions *section (p.128-129)..*

Chuck Hanson (State Water Contractors)

Comment: The relevance of biotic habitat in 2018 (pg 12) on the conditions experienced by delta smelt under the two alternative operations plans for fall 2017 is not clear. How are the conditions and potential management actions proposed to be implemented in 2018 linked to the decision regarding 2017 fall operations? This also comes up later in the effects analysis (pg 71). *ICF Response: Actions subsequent to fall 2017 were considered qualitatively as part of the overall proposal for adaptive management to benefit Delta Smelt food and low salinity zone habitat outside the main range.*

Comment: The stock-recruitment analysis (pg 16) assumes that the change in average X2 location over the two months is a good representation of effects. Is there any basis for this assumption? Is the effect of 74 km in September and October comparable to 74 km in September and 81 km in October (average 77.5 km)? This would benefit from further discussion in the text. *ICF Response: The analysis investigates the correlation of recruitment with X2 averaged over two months, as opposed to the change in X2 over two months. As described on p.32 of the revised effects analysis, September-October was used as the averaging period because the X2 values in these two months could be provided based on the proposed values (i.e., 74 km in September, up to 81 km in October). Use of an averaging period has as its basis many other similar analyses, for which X2 is averaged over several consecutive months. With respect to the comparability of average X2 at 74 km versus 77.5 km in terms of effects, this is the focus of the analysis presented in the* Application to Proposed 2017 Fall X2 Action *of the revised effects analysis (p.38-40).*

Comment: Given the high variance in model results (Figure 1b) can results of this analysis quantify a difference among the two alternatives for use as a basis for assessing potential differences to delta smelt? Does this need to rely on a weight of

evidence type assessment? *ICF Response: The high variance demonstrated that there is low certainty in what a Delta Smelt abundance would be, in so far as the effect of fall X2 on the species. The additional analyses for critical habitat provide a weight-of-evidence type assessment.*

Comment: Many of the analyses use different time periods with little to no explanation. For example, the analyses presented in Figure 1 (pg 17) represent two different periods in the estuary (1987-2004 and 2005-2014) while other analyses seem to combine time periods such as in Figure 2 (1987-2014). The analyses would be stronger if the time periods used in the analyses were segregated into biologically meaningful groups (pre- and post-POD? Etc.) to capture temporal differences in estuarine dynamics. Analyses that are based on only data from 2002 to present would be stronger in reflecting current trends and conditions. Why are some analyses only through 2014 rather than using data through 2015 or 2016? *ICF Response: The revised effects analysis aimed to provide justification for time periods used to the extent possible. In most cases the discrepancies arose because of data availability. For example, X2 data were only available up to water year 2016 (ending in September 2016), so analyses for the month of October could not be done for the fall following water year 2016. In response to other comments, analyses were provided for the POD era (2003-2015/2016) in addition to the original analyses covering broader periods of data availability.*

Comment: The model results presented in Figure 3 are interesting but may be made stronger by only presenting and discussing a comparison of X2 to 74 and 81 km rather than including conditions that are not under consideration in 2017. *ICF Response: We felt that inclusion of a broad range of conditions provides useful context for the magnitude of change necessary to elicit predictable outcomes.*

Comment: Many of the sections presented in Effects on Delta Smelt Critical Habitat present results of various analyses but do not present either an interpretation or significance of the findings to delta smelt. For example, page 27 reports that the extent of the low salinity zone would be 37% smaller at 81 km vs. 74 km. Given the low level of delta smelt abundance in 2017 is availability of habitat space likely to be a limiting factor for the fall population? What is the relevance of this result to the assessment of conditions in 2017? This occurs in several sections that would benefit from interpretation of the results of these analyses. *ICF Response: Regardless of whether Delta Smelt are habitat-limited or not, such analyses are an important component of the effects to critical habitat, as defined by USFWS. A* Conclusions *section was added at the end of the Proposal.*

Comment: The discussion of conceptual models for other actions (pg 71), although interesting, did not appear to be linked to the assessment or decision regarding the 2017 fall action? Further discussion of the application and relevance of this discussion to the 2017 action would be helpful. *ICF Response: As previously noted, actions subsequent to fall 2017 were considered qualitatively as part of the overall proposal for adaptive management to benefit Delta Smelt food and low salinity zone habitat outside the main range.*

Comment: The discussion of Upstream Effects (pg 78) starts to present results of an analysis but no results are presented. I expected to see a discussion of the effects of the action and alternative on instream flows effecting spring-run, fall-run, and steelhead on the Feather River as well as potential effects on spring water temperatures next year. Are there potential effects on Shasta operations? Winter-run egg incubation and hatching? Other upstream effects that need to be considered in the overall effects analysis? I recognize this may be added as time allows. *ICF Response: Subsequent clarification was received from DWR that there would be no upstream operational differences as a result of the 2017 Fall X2 action, which has been reflected in the discussion of* Upstream Effects (Reservoir Storage) *presented on p.128 of the revised effects analysis*.

Shaara Ainsley (Friant Water)

Comment: With regard to Sam's [Luoma] comment of focusing more on the comparison (RPA of 74-74 vs. Proposal 74-81), the paragraph describing relevant conclusions of the Effects on Delta Smelt section (page 23) gets a bit lost after the discussion of the comparison between the two stock-recruitment-X2 models and the simulations. It may help to separate out the discussion of the simulation results from that of the model selection, to focus on the key point of the comparison. *ICF Response: The paragraph has been retained at essentially the same location, but additional reference to the main result was provided in the* Conclusions *section of the revised Proposal.*

Sam Luoma (NGO Representative)

Comment: 1. I think it is important that the effects analysis consider the period 2000 – 2016 for each of the variables you are statistically assessing. This period represents the latest "regime" change that drove the POD. *ICF Response: As previously noted, additional analyses were conducted to focus on the post-POD era. The POD is defined as 2003 onwards, per the rationale provided on p.29 of the revised effects analysis: "2003 was chosen to represent the start of the POD because it represented an intermediate year between a common* *regime change point for multiple species (2002) and a Delta Smelt-specific regime change point (2004) (Thomson et al. 2010).")*

Comment: 2. The proposal and the effects analysis should more explicitly make this a comparison between two proposals (74,74 and Nov, as stated in the RPA vs 74, 81 and Nov as stated in RPA), not just a discussion of the alternative proposal. *ICF Response: Our analysis aimed to assess potential effects of the 2017 proposal versus the situation that otherwise would occur (implementation of the RPA as written in the USFWS [2008] BiOp). This analysis is a comparison of two scenarios in the manner we understand the commenter to be meaning.*

Comment: 3. An explicit statement of the scientific justification for the alternative proposal would be useful (it is not clear to me what that is?). *ICF Response: The justification for X2 of 81 km in October as opposed to 74 km is that X2 of 81km is considered protective of Delta Smelt. In above normal years, per the USFWS (2008) BiOp, X2 is to be located at 81km in October and therefore was considered appropriate for consideration in this wet year.*

Comment: 5. The effects analysis does include on calculation of reduction of habitat as defined by abiotic parameters (e.g. Feyrer et al 2011) but it is buried in a small paragraph. Should not the proposal and effects analysis mention that avoiding degradation of habitat itself is in the RPA and include an explicit detailed analysis of the differences. There is less uncertainty in this calculation than there is in most of the analysis. Is not a clearer differentiation of this "effect" warranted? *ICF Response: The analysis based on Feyrer et al. (2011), plus an additional more detailed analysis based on UnTRIM modeling which was included in the revised effects analysis. Consistent with the draft effects analysis, the final effects analysis states that the proposal would shift the location and volume of the low salinity zone.*

Comment: 6. Ted [Sommer] made an excellent point in our discussions that calculation of habitat area as defined above should consider several scenarios using different flows as shown by DWR's (John's) analysis. Ted makes the point that the difference between the alternatives will be affected by changes in flows. Again, please be direct with regard to this. *ICF Response: As previously noted, consideration of the operational forecast from DWR/John Leahigh was provided in the revised effects analysis for the Proposal, and is accounted for in the* Conclusions *section presented in the revised effects analysis.*

Comment: 7. The discussion of food does not mention or take into account the uncertainty that comes from studies that show fuller guts and better health of DS in Suisun Bay despite more zooplankton occurring in the rivers. Doesn't this mean you need a statement that zooplankton abundance may be an ambiguous indicator of food supply? *ICF Response: We are familiar with the study of Hammock et al. (2015that showed the opposite of what the commenter suggests, i.e., lowest stomach fullness and relatively low condition factor in Suisun Bay compared to other areas. Hammock et al. (2017, their Figure 5) showed that in both freshwater and brackish habitat, Delta Smelt stomach fullness had a type II functional response to zooplankton abundance, i.e., increasing to an asymptote as prey density in the environment increased. . Therefore zooplankton density provides a reasonable indicator of food that may be used by Delta Smelt and to us seems an appropriate measure to examine for X2 effects, as postulated by the FLaSH investigations.*

Comment: 8. X2 represents the best surrogate we have to date of all the knowns and unknowns that define habitat? Maybe we need a statement to that effect or presentation of an alternative if the case is being made (implicitly) X2 is problematic. *ICF Response: The proposed 2017 Fall X2 action differs from the fall X2 action as prescribed in the USFWS (2008) BiOp's RPA only in so much that X2 would be different; therefore, we felt it was appropriate to focus our analyses on empirical evidence for potential effects of differences in X2.*

Ted Sommer (DWR)

Comment: I think this issue is already covered in the summary comments from Bruce and Sam, but I really think that this analysis should focus on the projected operations as described in the latest simulations from John Leahigh's group. In other words, focus on comparing the base condition (RPA 4) vs. DWR O&M's estimate of X2 for this fall under the contractor proposal. As Kaylee requested, the analysis for this fall should include wet and dry scenarios. *ICF Response: As previously noted, consideration of the operational forecast was provided in the revised effects analysis for the Proposal.*

Comment: As written, the document reads more like an analysis of the relative value of the Fall X2 standard than an evaluation of proposed operations. There are some valid points in the document, but this fall 2017 "mini-BA" doesn't need to tackle the broader issue of X2—it just needs to evaluate the expected operations. As a crude example, if my kid makes $10.80/hour for 8 months then gets a raise to $12.00/hr for the rest of the year, at the end of the year we don't do two separate sets of taxes for each income level. Instead, we would do the evaluation based on what he earned over the course of the

year. You should therefore be evaluating the cumulative effects of the changing X2 over the course of this entire fall. *ICF Response: The effects analysis explicitly aimed to examine potential effects of different values of X2 on biological and critical habitat outcomes, on a monthly basis where possible, and with months combined as appropriate (e.g., stock-recruitment-X2 analysis considering mean September-October X2). The initial focus of the analysis was on comparing X2 at 74 km vs. 81 km because these were the worst-case bookends that were provided; the intent was not to analyze the relative value of the fall X2 standard.*

Erwin Van Nieuwenhuyse (Reclamation)

Comment: Attached are my initial comments in Track Changes…mostly asides and a couple of typo corrections. The main text change I recommend is removing all but the first mention of the Sacramento deep water ship channel initiative because the earliest it could be implemented is 2019. Including it in an analysis of actions to be taken in 2017-2018 is an unnecessary distraction. Also, I agree with Ted that focusing the analysis specifically on projected operations under wet and dry scenarios would be desirable. I think Sam's request to take a separate look at the post-POD period is reasonable, but I think the consensus on the POD's start date is 2002 not 2000. *ICF Response: We appreciated receiving the marked-up text and incorporated edits as we found them to be appropriate. Rather than omitting the various references to the Sacramento Deep Water Ship Channel action, we revised the text to indicate that the earliest that this could occur is 2019 (revised Proposal, footnote 4 on p.19). As previously noted, consideration of the operational forecast from DWR was provided in the revised effects analysis (p.27-28). Also as previously noted, additional analyses were conducted to focus on the post-POD era.*

Subsequent Written Comments on the Final Proposal's Effects Analysis

Sam Luoma (NGO Representative)

Comment: The revised EA shows much useful data, it considers the POD period alone in additional analysis and does a more explicit job of directly comparing 74km and 81km. However, the report avoids the question of the scientific justification for not holding X2 at 74km in October; or any scientific benefits to DS from moving X2 to 81km in October. Apparently there are none. *ICF Response: The justification for X2 of 81 km in October as opposed to 74 km is that X2 of 81km is considered protective of Delta Smelt. In above normal years, per the USFWS (2008) BiOp, X2 is to be located at 81km in October and therefore was considered appropriate for consideration in this wet year.*

Comment: The EA concludes that the alternative X2 proposal would not harm DS. It is difficult to see how such a black and white conclusions can be justified (see below). The authors have put a great deal together in a very short time. Because it does not fully consider the existing literature and because it relies on a limited approach it does not allow this strong conclusion. The alternative proposal should be burdened to prove it offers no risk or at least it should show it offers some benefits to balance obvious risks. *ICF Response: The analysis assess the potential risk and does consider existing literature. In addition to our analysis following the stock-recruitment-X2 framework of one of the analyses providing the basis for the USFWS (2008) Fall X2 action, we also cited several published studies that did not find a correlation between fall X2 and survival of Delta Smelt.*

Comment: The EA remains a preliminary analysis because it relies heavily on statistical analysis and does not fully consider the available literature. Lenny was explicit about this. It is legitimate scientifically to do a report that considers a narrow approach to a problem, but it is not justifiable from the point of view of risk analysis. The more conceptual information that recent reports have employed is not fully exploited in the EA. For example Moyle et al and Hobbs et al's excellent recent reviews are not even cited; the MAST report is not adequately acknowledged. The EA does show the table comparing wet and dry years used in the FLaSH report; but largely ignores those in favor of its heavy reliance on linear regression, best fit central tendencies of relationships, and means. Inadequate consideration of the more conceptual work of Hobbs et al, Moyle et al, MAST etc. makes the EA incomplete. *ICF Response: As the commenter notes, we have favored the empirical examination of the evidence for relationships with fall X2 as a means to give predictions for the situation that could arise in 2017. We focused on the analyses and conceptual models underlying the USFWS (2008) BiOp. We anticipate that additional consideration to the conceptual foundation will be provided by the agencies ultimately making the decision on whether or not to accept the 2017 Fall X2 proposal.*

Comment: One example of the weakness of heavily relying on best fit regressions and means is the argument that the mean September-October X2 is likely to be 77.5km. That mean is irrelevant to the potential risk from the proposal. The typical centroid of the population will be at 81km for a period of time (weeks most likely); even though the mean is somewhere in between. The EA itself shows that models project a sharp increase in water clarity at 81km. There is also a sharp gradient in air temperatures as one progresses upstream from Suisun. How can we conclude there is no risk to DS from being in an environment with reduced protection from predation and reduced feeding capability (Moyle et al discuss the importance of turbidity to feeding) without thinking

about the characteristics of 2017. What will be the effect of low turbidity at 81km (is 2017 a high or low turbidity year)? Is this a warm or cool year and will this be a year when temperature at 81km in October slides into the 20 – 22 degree stress zone that it has in other years based upon the EA's temperature graphs? A DS only has to die once and it is gone; the mean condition is irrelevant if, even for a short time, it is exposed to sufficiently stressful conditions or increased predation. Maybe we cannot quantify these effects but that does not mean a conclusion of no adverse effect or no risk is justified. These data-supported concepts are just as important as statistical analysis in considering the key question: is there a risk that we will subvert a potentially excellent year for DS by cutting short their exposure to Suisun.
The X2 provision of the Biop implicitly acknowledges that a Fall X2 of 74km may not be very useful if DS do not survive through spring and summer. But if spring and summer conditions are good (e.g. high flow years; cool, turbid years), the Biop provides an opportunity to protect the survivors through fall and produce a bump in the overall population. The year 2011 is quite instructive as an example of this. The EA does point out the higher flow years in some of the regressions; but it reverts to regression rather than lessons from the high and low flow years in drawing conclusions about the potential for effects. *ICF Response: Where possible, the analysis considers September and October separately. In the case of the stock-recruitment-X2 analysis, the use of an averaging period is consistent with the framework of the USFWS (2008) BiOp analysis. We have also provided additional consideration of temperature; see analysis below.*

Comment: In a number of cases the EA points out the percentage change of things like turbidity and habitat area. The tone is that of "these are small changes". Frankly, since we cannot relate percentage change to DS, I think that is mis-leading. For example, if such changes cross a threshold (e.g. temperature; turbidity) the percent change is irrelevant. *ICF Response: The percentages were included to provide context to the relative potential effect on low salinity zone critical habitat. The examples for which percentage values were provided are based on continuous responses of area, habitat index, or probability of occurrence in relation to individual covariates such as Secchi depth or temperature. None of these represent threshold responses, per the foundational relationships as shown in Feyrer et al. (2007), for example.*

Comment: Building from the concepts in Hobbs et al, if DS are confined to freshwater they are subject to stress and death if temperatures get into the 20 – 22 degree range. But there are examples of success in years that are cool (the FW subpopulation does ok) even when X2 does not facilitate access to Suisun. One could argue that in a warm year

access to Suisun is critical for the overall population because the FW subpop will suffer and the migrants need access to cooler waters in Suisun. An X2 of 74km could allow access to Suisun and reasonable survival for those that get there;, but then if the population is pushed or constrained toward FW by an X2 of 81 in October, even for a little while, they are gone if temperatures are sufficiently difficult. Therefore isn't a critical factor temperature in October 2017 in the zone around X2. The best fit lines in the EA temperature plots are meaningless in this regard; it is the high residual years that are the bad ones. The EA plots do show that September is often a little warmer than October. We cannot predict October, but we can get a rough idea from September temperatures what October might be like on the warm to cool scale? Is this a warm water or a cool water September? If this is a warm water, high flow year then even a short hiatus in an X2 of 74km could be seriously problematic. *ICF Response: We agree that the final portion of this comment merits further investigation and attempt to do so herein. First, we compare mean water temperature in September to mean water temperature in October for the CDEC stations that we included in the effects analysis (Figure 1). This suggests little relationship between the two for the most relevant stations, i.e., Antioch and Mallard. Therefore we proceeded by examining water temperatures for 2017. Before doing so, we note that the study cited by Hobbs (2016) suggested that temperatures 4-6°C below the critical thermal maximum resulted in the onset of thermal stress, based on the study of Komoroske et al. (2015: Molecular Ecology 24(19):4960-4981); per the study by Komoroske et al. (2014: Conservation Physiology 2(1):cou008), the juvenile Delta Smelt critical thermal maximum temperature ranges from around 27°C to 29°C, depending on acclimation temperature of test fish. Thus, with thermal stress onset at 4-6°C below critical thermal maximum temperature, this in fact would result in stress onset at 21-23°C, as opposed to the 20-22°C suggested by Hobbs (2016). For the proposed 2017 Fall X2 action, X2 could be as far upstream as 81 km in October; the salinity distribution on average would be similar to that shown below in Figure 2. Defining the low salinity zone to be salinity of 1 to 6, as per the Proposal, the low salinity zone therefore would extend from approximately Honker Bay to the Sacramento and San Joaquin Rivers near Sherman Lake, as well as much of Montezuma Slough. Relevant CDEC stations and their temperature data for September 2017 are shown in Figure 3. The red boxes overlaid on each temperature plot show temperature of 21ºC (69.8ºF), i.e., the onset of thermal stress as described above. It is apparent from these plots that temperature at almost all locations is already well below this threshold (by as much as 2-3ºF depending on location), with the exception of Antioch, for which temperature was briefly above 21ºC. Barring a considerable heat wave, we would expect water temperature to continue to be below the threshold at which thermal stress could begin.*

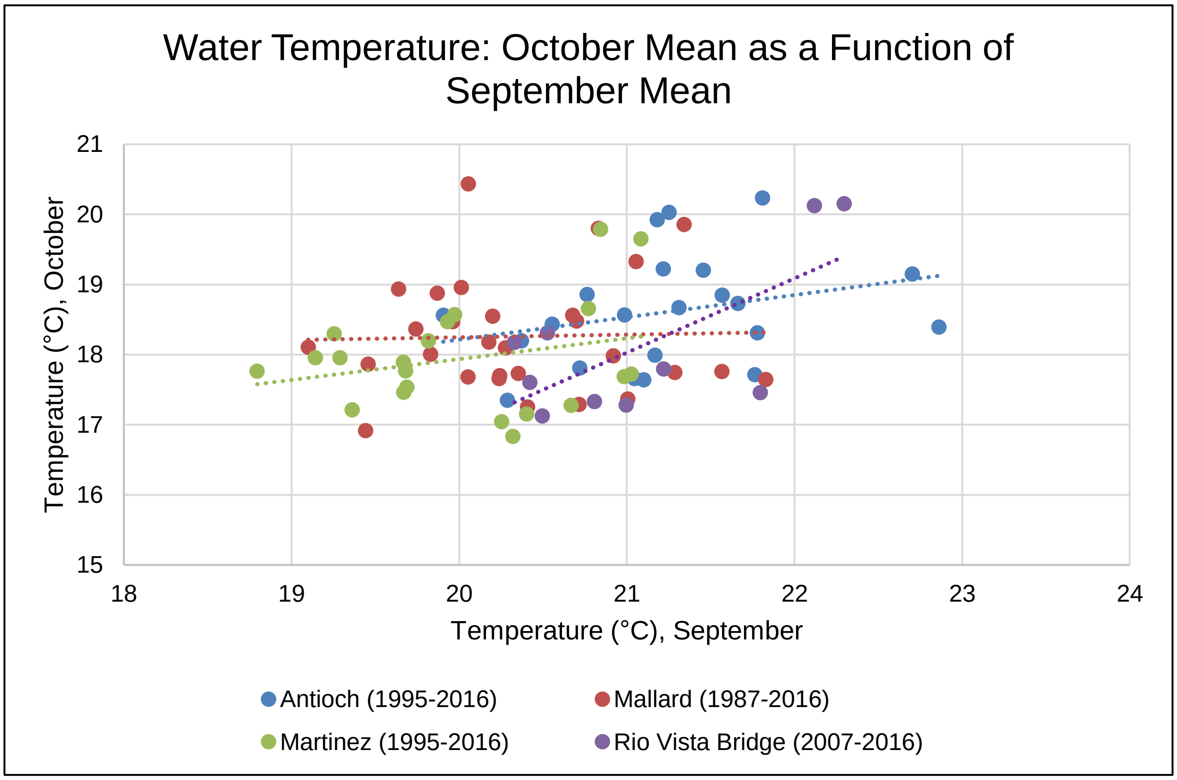
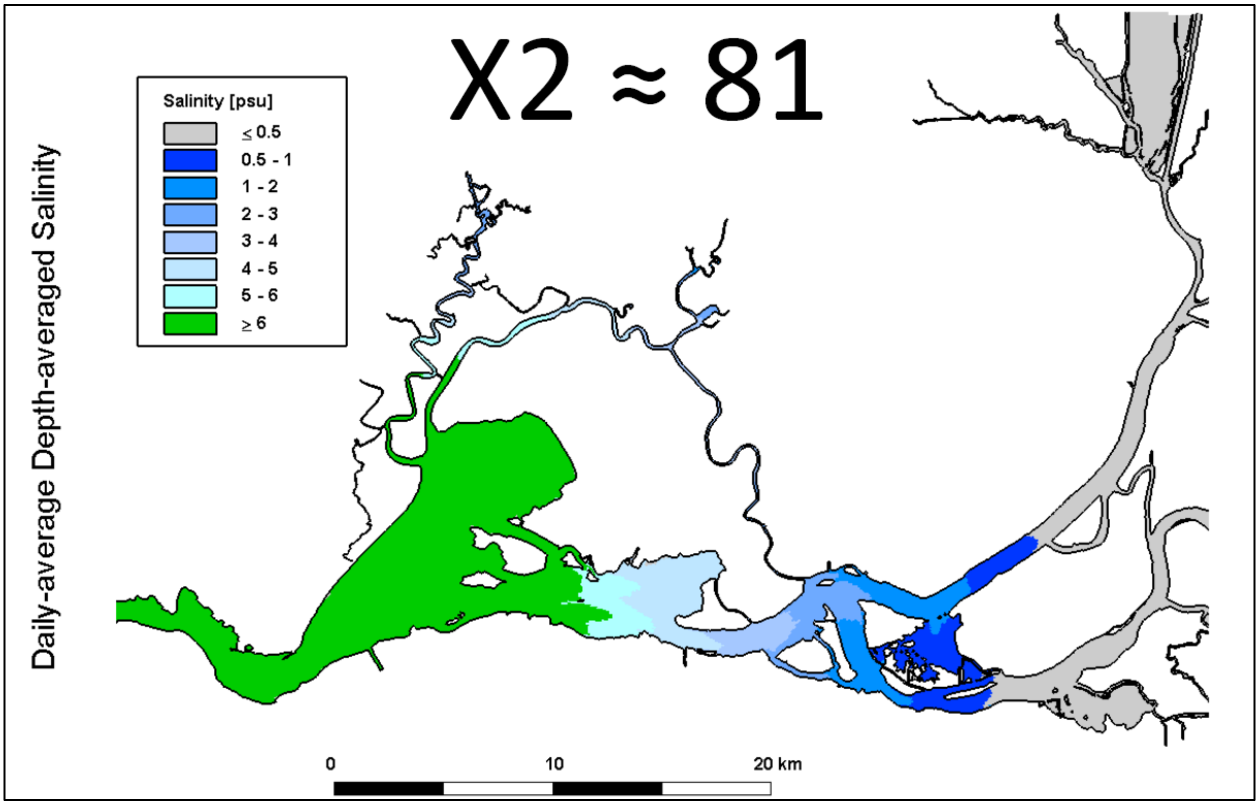


Figure 1. Mean Water Temperature in October as a Function of Mean Water Temperature in September.



Source: Delta Modeling Associates (2014). Low Salinity Zone Flip Book.

Figure 2. Daily-Average, Depth-Averaged Salinity at X2 = 81 km.

ICF

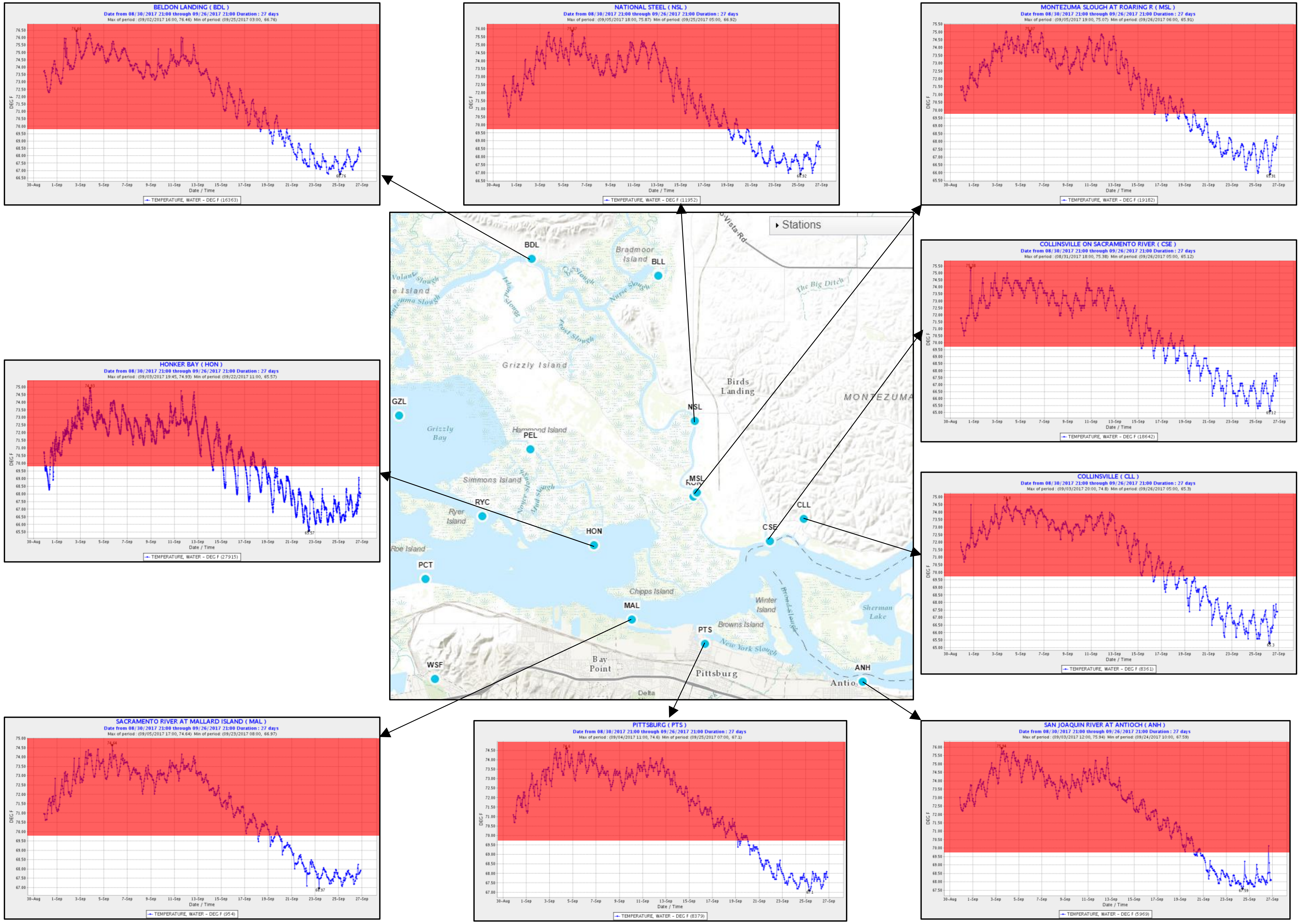


Figure 3. Hourly Temperature at Various Stations in the San Francisco Estuary, September 2017, With Red Indicating Temperature Above 21ºC (69.8ºF) Threshold for Onset of Thermal Stress in Juvenile Delta Smelt.

Comment: Volume 4 of Hobbs et al focuses on comparing life history of Delta Smelt from 2005-2006 and 2010-2011 year classes based on otolith analysis.

a. "growth of the 2011 Year class was higher than other year classes examined" (page 107) particularly in the fall months (page 111).

b. "Data from this study suggests, the significant increase in abundance for the 2011 year class was likely the result of both increased freshwater flows to the estuary and cool temperatures in 2010 and 2011." (page 110)

What are temperatures in September 2017 and can they tell us anything about possibilities in October? *ICF Response: See previous response.*

Comment: Hobbs et al also discuss the importance of subpopulations to the life history of DS. One could interpret this to conclude that high survival of each subpopulations at least in some years could be crucial to maintaining the genetic diversity of the species necessary for recovery. 2017 is a year when high survival of the dominant migrant sub-population should be maximized. Risks to this subpopulation should be minimized. Because of its reliance on overall statistics, the EA does not consider the benefits of maximizing survival in good years for the migrating sub-populations and therefore the population overall. *ICF Response: Comment noted. The analysis did attempt to assess the potential effects on Delta Smelt population outcomes (summer townet abundance index) from the proposed fall X2 action; the evidence was that there would be little effect from X2 as proposed relative to X2 that otherwise would have occurred.*

Comment: The graphics on page 48 (Fig 22, X2 at 74 km) and page 52 (Fig 29, X2 at 81 km) show a dramatic decrease in the amount of time that salinity is less than 6 psu in Suisun Marsh and Bay. Grizzly Bay and habitat west of Ryer Island goes from fresh to salty. Also, by using 6 psu, they likely understate the effects, as the vast majority of Delta Smelt are found in water less saline than 6 psu (Hobbs et al page 137: "Greater than 90% of all Delta Smelt collected in the SKT survey from 2012-2015 reared in habitats with salinity less 2.5psu". With sufficient time a thorough EA could do similar graphs with a cutoff of 2.5 psu. *ICF Response: The salinity cutoff of 6 was based on the analysis by Bever et al. (2016). It does not follow that the effects are understated because Hobbs (2016) found most rearing in salinity less than 2.5; large portions reared in freshwater in some years (e.g., Cache Slough complex), which could be upstream of the area considered in the analysis (i.e., the confluence and Suisun Bay/Suisun Marsh; Figure 21 of revised effects analysis).*

Natural Resources Defense Council

Comment: First, the analysis compares the effects of implementing Fall X2 on Delta Smelt survival from the fall (as measured by the Fall Midwater Trawl, or FMWT) to the subsequent summer (as measured by the Summer Townet Survey, or TNS). However, as FWS and other agencies have recognized, the effects of implementing Fall X2 may not show up in the STN the following summer if there are significant environmental effects in the winter and spring months. A better approach is to analyze the effects of Fall X2 on survival from the FMWT to the 20 mm survey or the Spring Kodiak Trawl (SKT). The MAST Report (2015) included similar modeling that found that implementation of Fall X2 has a statistically significant relationship between survival from the fall to early in the subsequent year. The EA and proposal fail to consider any of this information. At a minimum, the analysis should also look at the relationship between FMWT and 20mm survey, and between FMWT and SKT. This would be similar to analyses performed in the MAST Report, and recognizes that there are significantly more years of data available than when the original Feyrer et al 2007 paper was published. *ICF Response: As noted in the Proposal's effects analysis (p.40), "the stock-recruitment-X2 relationship presented in this effects analysis aimed to revisit and advance the basic analysis presented in the USFWS (2008) BiOp." Given that the BiOp's analysis formed part of the basis for the fall X2 action, the emphasis was on updating the analysis in a more appropriate analytical framework with additional years of data. The comments received on the draft effects analysis suggested examination of survival from the summer juvenile stage to the winter/spring adult stage because this avoids potential spring outflow. The preliminary results of this analysis were presented in the effects analysis as stock-recruitment relationships (both Ricker and Beverton-Holt forms) and found only a weak correlation between X2 and the residuals of the stock-recruitment relationships (p.41-42). To follow up on the commenter's suggestion to examine 20-mm and SKT data, we performed analyses similar to those contained in the MAST Report[1], specifically focusing on predictions of 20-mm and SKT indices as a function of previous FMWT index and previous mean September-October X2; consistent with the Proposal's effects analysis, the mean of September and October X2 was used because potential values for these months in 2017 were known. Consistent with comments received on earlier drafts of the effects analysis, the analysis focused on the period after the start of the POD, taken to be 2003-2016. The data used in the analysis are shown in Table 1 below. General linear models (PROC GLM; PROC GLMSELECT) were undertaken in SAS v.9.4 software. The relationship of the 20-mm and SKT survey indices to prior FMWT and X2 was examined with three models for each response: FMWT alone, X2 alone, and both variables together, and the relative evidence for each model*

[1] Specifically, the regressions shown in Table 8 of the MAST Report; we did not explore the influence of spring X2, however. Index data were log-transformed.

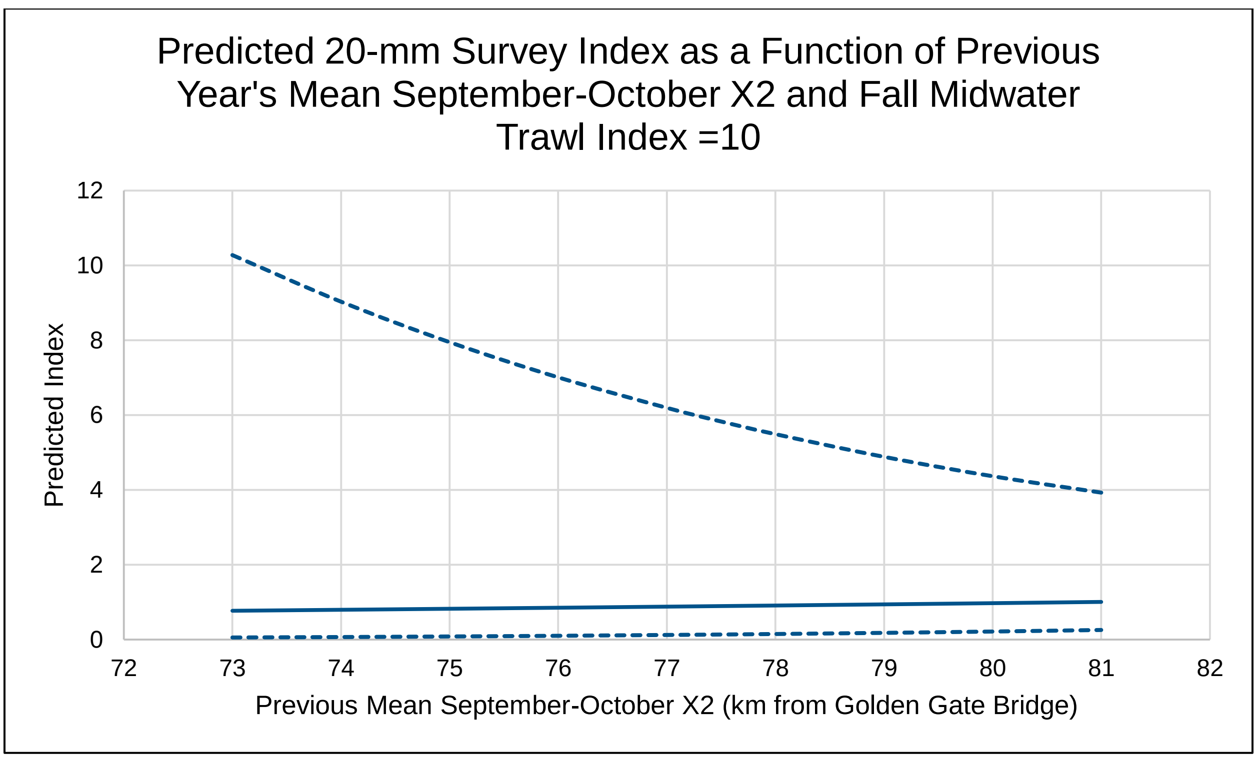
providing the best explanation of the data was compared with AIC_C (Akaike's Information Criteron corrected for small sample sizes). The results of these analyses found good support for previous FMWT as an important predictor of 20-mm and SKT, but essentially no support for mean September-October X2 as a predictor of these indices (Table 2). Thus, under the assumption that the 2017 FMWT index is 10, there would not be expected to be a meaningful difference in 20-mm or SKT indices as a function of mean September-October X2 being at 77.5 km (assuming 74 km in September and 81 km in October) compared to 74 km (following the prescription from the USFWS 2008 BiOp); this is illustrated in Figures 4 and 5 (produced with SAS PROC PLM).

Table 1. Data Used To Investigate Relationship of 20-mm and Spring Kodiak Trawl Survey Indices as a Function of Previous Fall Midwater Trawl Index and Mean September-October X2.

year	SKT	20mm	Prev. FWMT	Prev. Sep-Oct mean X2	Log SKT	Log 20mm	Log FMWT
2003	.	13	139	87.20134	.	1.113943	2.143015
2004	99.7	8.2	210	86.9195	1.998695	0.913814	2.322219
2005	52.9	15.4	74	84.3329	1.723456	1.187521	1.869232
2006	18.2	9.8	26	82.56434	1.260071	0.991226	1.414973
2007	32.5	1	41	81.29475	1.511883	0	1.612784
2008	24.1	2.9	28	87.32326	1.382017	0.462398	1.447158
2009	43.8	2.3	23	89.06864	1.641474	0.361728	1.361728
2010	27.4	3.8	17	86.50265	1.437751	0.579784	1.230449
2011	18.8	8	29	85.37014	1.274158	0.90309	1.462398
2012	130.2	11.1	343	74.65829	2.114611	1.045323	2.535294
2013	20.4	7.8	42	86.46844	1.30963	0.892095	1.623249
2014	30.1	1.1	18	84.95666	1.478566	0.041393	1.255273
2015	13.8	0.3	9	88.84984	1.139879	-0.52288	0.954243
2016	1.8	0.7	7	85.28454	0.255273	-0.1549	0.845098

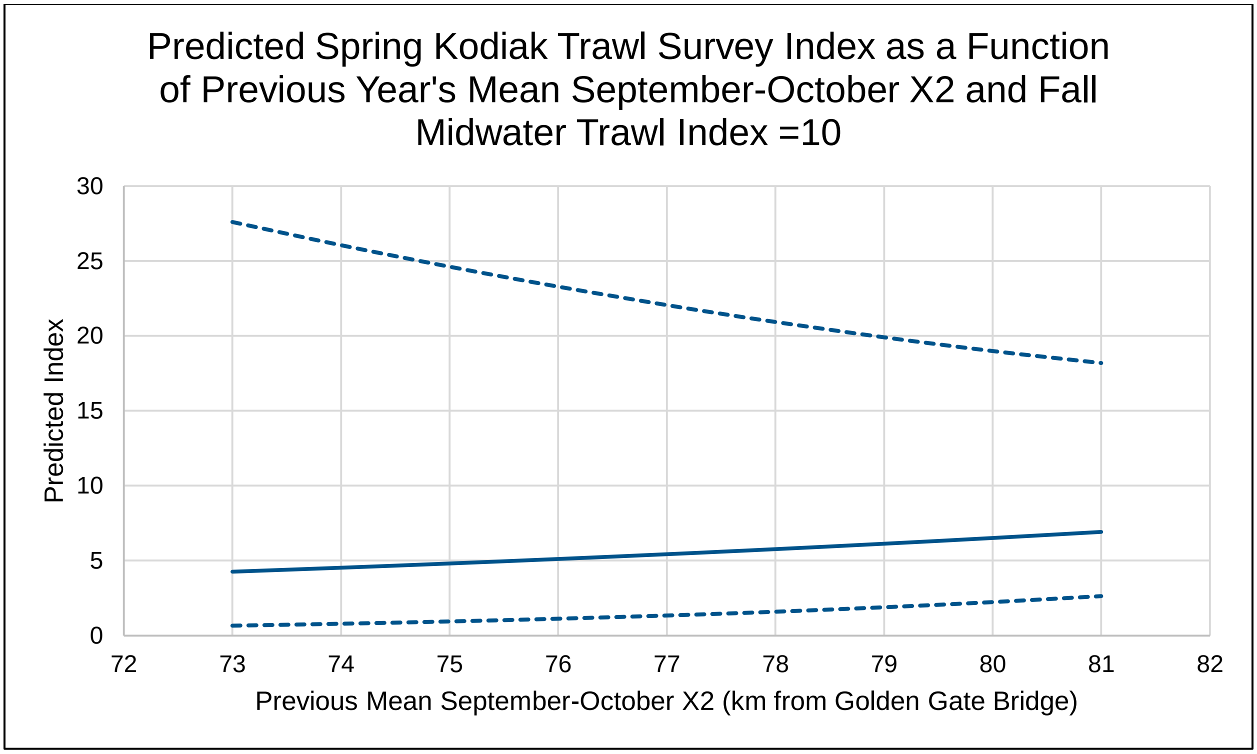
Table 2. Summary of Relationships Between Log-Transformed 20-mm and Spring Kodiak Trawl Survey Indices and Previous Fall Midwater Trawl Index and Mean September-October X2.

Response	Predictors	n	P (by predictor)	R^2	Adjusted R^2	AIC_c	$\Delta(AIC_c)$
Log 20-mm	Log FMWT	14	0.0029	0.54	0.50	-6.67	0.00
Log 20-mm	Log FMWT, Sep-Oct X2	14	0.0064, 0.6786	0.54	0.46	-2.85	3.82
Log 20-mm	Sep-Oct X2	14	0.3322	0.08	0.00	2.97	9.64
Log SKT	Log FMWT	13	0.0004	0.70	0.67	-15.46	0.00
Log SKT	Log FMWT, Sep-Oct X2	13	0.0008, 0.3033	0.73	0.67	-12.573	2.89
Log SKT	Sep-Oct X2	13	0.2553	0.12	0.04	-1.54	13.92



Note: Broken lines show 95% confidence intervals.

Figure 4. Predicted Delta Smelt 20-mm Index As a Function of Previous Year's Mean September-October X2 and Fall Midwater Trawl Index = 10.



Note: Broken lines show 95% confidence intervals.

Figure 5. Predicted Delta Smelt Spring Kodiak Trawl Index As a Function of Previous Year's Mean September-October X2 and Fall Midwater Trawl Index = 10.

Comment: Exhibit A admits that survival and recruitment of Delta Smelt is predicted to increase more when Fall X2 is at 74 km compared to when Fall X2 is located at 81 km ("For example, moving mean September-October X2 from 95 km to the RPA-required location following an above normal water year (81 km) is predicted to increase recruitment to the STN by a factor of 1.24, and a factor of 1.39 if fall X2 is moved to the RPA-required location following a wet year (74 km)."). *ICF Response: This selection of text omits the subsequent sentence noting "However, the objective of increasing recruitment to the STN is met in only 58% and 61% of simulations when the statistical uncertainty of the model is accounted for." In other words, X2 of 74 km and 81 km have little difference in terms of the uncertainty with which there might be an increase in the STN when compared to a very high fall X2 level (95 km).*

Comment: FWS or USBR performed analyses like those in the 2015 MAST Report, we likewise expect it would show that a more westerly Fall X2 location is likely to result in higher survival and subsequent abundance. For instance, the graphic on page 157 of the MAST Report shows that a more westerly location of Fall X2 is likely to result in a higher

20mm abundance index the following year. See also MAST Report at 155 ("In summary, low values of prior fall X2, high prior FMWT abundance, and intermediate values of spring X2 have positive associations with the abundance of larval/postlarval Delta Smelt, but the effects of individual variables are mediated by the presence of the other variables."). *ICF Response: The preliminary analyses presented in the MAST report include a variety of combinations of flow variables, with some of the better models including fall X2 and some not. For example, the MAST report provided more easily reproducible models using linear regression and quadratic predictors, which found that a quadratic relationship for spring X2 and prior fall abundance index provided a model that was best supported for predicting Delta Smelt abundance in the 20-mm survey (Table 8, p.160). See the analysis we conducted above, similar to that contained in the MAST Report, which showed little support for fall X2 being statistically related to 20-mm or SKT indices.*

Comment: We also note that FWS cannot rely on speculation that X2 would be more westerly than 81 km under the proposed action, as this would not reasonably be certain to occur under the proposed action. *ICF Response: We consider hydrological forecasts by the California Department of Water Resources to be the best available information, as opposed to speculation.*

Comment: Second, the analysis uses a Ricker stock-recruit model to assess the effects of Fall X2 on Delta Smelt abundance and survival. However, Exhibit A admit that Delta Smelt likely are not limited by density dependence at current, near record low levels of abundance. As a result, use of the Ricker model is inconsistent with the best available science and will not accurately assess potential impacts of this action. A linear model, consistent with the analyses in the MAST Report (2015) and earlier studies is equally appropriate given the density independence for these life stages. As noted above, we expect that such analyses would likewise show that maintaining X2 at 81 km would result in lower survival and abundance of Delta Smelt than maintaining X2 at 74 km. *ICF Response: As noted above, additional exploration was undertaken with a Beverton-Holt model, at the suggestion of the DSST, and likewise found only a weak correlation with fall X2. In addition, the analyses shown above with linear models did not show evidence of a fall X2 effect.*

Comment: Third, Exhibit A presents modeling information using data from 1987-2004 and from 1987-2014, but it fails to analyze data from the post-POD period. In contrast, they provide post-POD data for certain food web analyses, but not for the stock-recruit model. This is inappropriate, as noted by CAMT scientists, and the analysis should use post-POD data and consistent data sets, rather than cherry picking data. *ICF Response: The* *additional exploration of stock-recruitment relationships and fall X2 in response to DSST comments focused on the POD era and, as previously stated, found only a weak correlation with fall X2.*

Comment: Fourth, the analysis ignores existing scientific information which demonstrates that Delta Smelt have higher fecundity and higher growth rates when X2 is as 74 km. The EA and Exhibit A wholly ignores the attached report by James Hobbs for the Interagency Ecological Program, which concluded that delta smelt growth rates and fecundity were higher in the fall of 2011 than in the other years that were analyzed. There is no reason to exclude this important data and analysis, which undermines the conclusions in Exhibit A. *ICF Response: Although growth and fecundity was highest in 2011, the cited report provides somewhat limited insight as to the potential effects of having X2 at 81 km compared to 74 km, for the analysis compares 2011 to several years (2005-2006, 2010, 2012-2014) with greater October X2 (83.5-88.2 km) than is proposed for October 2017.*

Comment: In 2011, the agencies largely implemented the Fall X2 action at 74 km, and the agencies observed a nearly 10 fold increase in the FMWT survey of the abundance of Delta Smelt from the prior year. Several analyses conclude that fully implementing Fall X2 is likely to significantly increase the abundance and survival of Delta Smelt, including MAST 2015, the 2013 analysis by Wim Kimmerer, Feyrer et al 2007 and 2011, and the WaterFix biological opinions and ITP. *ICF Response: See prior response regarding MAST (2015) analyses. The analysis included in the Proposal specifically aimed to update the analysis of Feyrer et al. (2007) and suggested little effect of X2 at 74 km vs. 81 km in October; the analysis by Kimmerer (presumably in Mount et al. 2013, BDCP review) is conceptually similar. We are not aware that the WaterFix biological opinions and ITP conducted a quantitative analysis of Delta Smelt response to X2 at 74 km vs. 81 km.*

Comment: This proposal ignores those analyses and relies on a new, significantly flawed analysis that has not been peer reviewed and which has obvious flaws. As noted above, the analysis by ICF concludes that Delta Smelt recruitment and survival is likely to be higher if X2 is maintained at 74 km than if X2 is located at 81 km. *ICF Response: See earlier response related to uncertainty in recruitment and survival outcomes from fall X2 at 74 km vs. 81 km.*

References

Note: The list below only includes new references in response to comments; other references provided in comments generally are in the Proposal.

Hammock, B. G., J. A. Hobbs, S. B. Slater, S. Acuña, and S. J. Teh. 2015. Contaminant and food limitation stress in an endangered estuarine fish. Science of the Total Environment 532:316-326.

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Komoroske, L. M., R. E. Connon, J. Lindberg, B. S. Cheng, G. Castillo, M. Hasenbein, and N. A. Fangue. 2014. Ontogeny influences sensitivity to climate change stressors in an endangered fish. Conservation Physiology 2(1):cou008.

Komoroske, L. M., R. E. Connon, K. M. Jeffries, and N. A. Fangue. 2015. Linking transcriptional responses to organismal tolerance reveals mechanisms of thermal sensitivity in a mesothermal endangered fish. Molecular Ecology 24(19):4960-4981.