August 30, 2013

via email: interagencyecologicalprogram@gmail.com

Interagency Ecological Program Directors
980 9th Street, 14th Floor
Sacramento, CA 95814

Re: SWC, 2013 MAST Report Review

Dear IEP Directors:

The State Water Contractors ("SWC") appreciate this opportunity to comment on the Draft IEP Mast Report (herein "MAST Report" or "Report"). The SWC recognize the significant effort put forth by your staffs to assemble the information contained in the MAST Report and understand the difficulty of such a significant undertaking. Acknowledging the importance of the MAST Report, the SWC have thoroughly reviewed the Report and have provided detailed and specific comments in an effort to describe where and how the Report could be strengthened. In order to thoroughly explain our comments, we have attached exhibits to this letter that include supporting graphs and citations. Since the Report is over 100 pages plus exhibits, we would request some flexibility regarding page limits, as without some flexibility the opportunity for a meaningful dialog is unnecessarily foreclosed.

While there is a lot of good information in the MAST Report, we have identified several areas where the report should be augmented, as follows.

- Several of the conclusions and recommendations are inadequately supported by the evidence presented.
- There are alternative hypotheses and conceptual models that should be included.
- The report should more explicitly acknowledge the uncertainties and limitations in the evidence presented.
- While the three stated objectives on page 20 are interesting questions, the use of data from only two dry-wet year combinations undermines the technical rigor of the analysis and evaluation of the conceptual model hypotheses.

Addressing these shortcomings will greatly improve the MAST Report, making it a more objective and impartial description of our evolving understanding of delta smelt.

The SWC have organized their comments according to the six questions posed to reviewers.

<u>MAST Report Questions 1 and 4: Are the objectives and/or questions the report seeks to address clearly described in the report? Are they fully addressed? Do the authors go beyond the objectives/questions? Is the report's organization effective? Is the title appropriate?</u>

The stated goal of the MAST Report is to update previously developed conceptual models for delta smelt to organize our current understanding of the factors affecting delta smelt abundance and of delta smelt responses to these factors and then to use the updated conceptual model as a framework to a) organize and synthesize existing knowledge and b) formulate and evaluate hypotheses. (MAST Report, pp.20 and 27.) However, the MAST Report's narrow focus on four recent years artificially limits the strength of its analyses and conclusions. The agencies have collected decades of data. Looking at a very small subset of years reduces the chances that the causes of the apparent declines in abundance can be parsed out, and the result is an increased chance of spurious findings. At a minimum, the MAST Report could have examined why abundance in 2011 was apparently higher than the entire set of POD years from 2002-2010, as well as the years leading up to the POD.

Further information supporting these responses to questions 1 and 4 is provided in Appendix 1, attached.

<u>Questions 2 and 5: Is the report objective? Is the tone impartial? Are uncertainties, alternative hypotheses and conceptual models, or incompleteness in the evidence explicitly recognized?</u>

There are alternative hypotheses and observations that should be acknowledged. The following are specific examples of where the Report could be strengthened.

The conceptual model described in Glibert (2010) and Glibert *et al.* (2011) was not described in the MAST Report, even though it is particularly relevant to the development of the MAST conceptual model because food, predation, contaminants, and harmful algal blooms are listed as stressors for multiple delta smelt life stages. The findings in Glibert (2010) and Glibert *et al.* (2011) are also relevant to the discussion of regime shift, as they specifically discuss break points in the historic data where changes in nutrient ratios and changes in phytoplankton speciation co-occur. Finally, Glibert (2010) and Glibert *et al.* (2011) do not suggest that the POD decline was caused by a single variable (MAST Report at p. 18) rather their model links changes in nutrient ratios to multiple changes in the physical environment, many of which are likely effecting delta smelt and other POD species. Glibert at al.'s work could be viewed as an alternative to the hypothesis that changes in flow have been the primary driver of the multiple changes in the environment.

The MAST Report also does not adequately acknowledge that delta smelt are distributed across a range that is broader than just the LSZ. (MAST Report, p. 16.) The Dege and Brown 2004 paper is discussed but other literature suggesting the species distribution is quite broad is not discussed. (MAST Report, p. 16.) For example Sommer 2013 explains,

> …the overall distribution of delta smelt habitat is much broader. The surveys do not necessarily capture the extremes of distribution and habitat shifts among years. Our analysis showed that delta smelt habitat is often located well downstream of the Delta, commonly Suisun Bay…."

Similarly, the MAST Report states that delta smelt use the upper estuary for spawning and rearing, but it does not acknowledge that spawning distribution varies and is not necessarily limited to the upper estuary. (MAST Report, p. 16.) The MAST Report should also acknowledge Bennett 2005 which states, "In years of high freshwater discharge spawning distribution is broader encompassing most of the Delta, Suisun Marsh channels, and the Napa River…."

Further information supporting these comments, as well as other examples of where alternative hypotheses and observations should be acknowledged, is provided in Appendix 2, attached.

Questions 2 and 3: Are the data and analyses handled competently and applied appropriately? Are conclusions and recommendations adequately supported by evidence and analysis? If the report's content is based on unpublished results, are findings and conclusions properly attributed to an individual or a specific program or project.

There are a number of improvements in the statistical analyses that we would recommend. For example:

- Figures 41 and 42: A larger data set should have been used, representing a greater number of years. The problem with using fewer years is not just ignoring decades of data, but it is also that catch numbers in recent years have been so small that the index ratios are increasingly uncertain. A change in catch of just a few fish can cause significant changes in the index ratios, making interpretation of the ratios too uncertain to be meaningful.

  The use of the 20mm survey is a further complicating factor. The 20mm survey is only able to sample larger larvae, which were necessarily spawned early in the season. Therefore, if most delta smelt are spawned either at the beginning or at the end of the season, half of the ratios in Figures 41 and 42 will be impacted.

- Figure 43: The Sacramento River plus San Joaquin River index on the x-axis represents the entire water year, and this occurs well before and well after the two surveys used in each abundance ratio on the y-axis. This is an inappropriate comparison.

The other major concern with Figure 43 is that it only uses data from 2002-2011, which means that 2012 and all of the data from the preceding decades are missing. The use of such a small subset of years greatly magnifies the chances of incorrect inferences. In Appendix 2 to these comments we attempt to recreate Figure 43 using a larger number of years; the result is an increasingly weaker statistical relationship as more years are added.

- Figure 44: The linear correlation between the SKT index and the previous FMWT index is problematic. The analysis should look at log SKT versus log FMWT so that large values do not dominate the results and so that we can see whether SKT is directly proportional to FMWT or not.

  We performed this analysis and found that the SKT varies with the FMWT as FMWT$^{\wedge 0.62}$ or fairly close to its square root. (See Appendix 3, attached.) This suggests that the FMWT varies much more than the SKT and is likely biased downward, particularly at low index values.

- Table 4: The MAST Report largely deferred to FLaSH on the topic of fall X2. However the Report does contain a calculated area of habitat based on McWilliams (not Feyrer 2010) to represent simple open water acres within certain salinity ranges for 2005, 2006, 2010 and 2011. The use of so few years of data is a violation of generally accepted statistical principles. We recreated the analysis considering an increasing number of years. The more years that are considered, the weaker the statistical relationship. (See Appendix 3, Attached.) As a result, the conclusion in the MAST Report that data generally support the fall X2 conceptual model is unsupported.

Further information supporting these comments, as well as other examples of where the analysis could be improved, is provided in Appendix 3, attached.

Question 6: What other significant improvements, if any, might be made in the report?

There is recent evidence that the existing surveys may not be representative of delta smelt abundance and distribution due to several factors including sampling time of day, vertical and lateral position of gear, turbidity, and tidal stage at time of sampling (Feyrer et al 2013; Bennett and Burau 2011; Fullerton unpublished data). The MAST Report should acknowledge the limitations of existing surveys and incorporate into the conceptual model the potential role of survey bias or inefficiencies on abundance indices. The MAST Report should also identify an investigation of survey efficiencies and biases as a critical next step. Identifying and trying to quantify survey bias is a critical precursor to determining likely factors affecting species abundance.

Specific evidence of survey bias in the existing surveys is described in detail in Appendix 4.

The SWC look forward to discussing the MAST Report with the authors, and would like to be involved in the development or future refinement of the MAST conceptual model. If the MAST Report authors have questions about the SWC comments, please feel free to contact our primary reviewers, as follows:

David Fullerton. Email: dfullerton@mwdh2o.com
Dr. Paul Hutton. Email: phutton@mwdh2o.com
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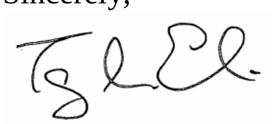
Sincerely,



Terry Erlewine
General Manager

Appendix 1

Questions 1 and 4: Are the objectives and/or questions the report seeks to address clearly described in the report? Are they fully addressed? Do the authors go beyond the objectives/questions? Is the report's organization effective? Is the title appropriate?

The stated goal of the MAST Report is to update previously developed conceptual models for delta smelt to include our current understanding of the factors affecting delta smelt abundance and of delta smelt responses to these factors and then to use the updated conceptual model as a framework to a) organize and synthesize existing knowledge and b) formulate and evaluate hypotheses. (MAST Report, p.20 and 27.) The updates to the conceptual models are an improvement over prior versions as we support the use of the Miller hierarchy approach as an organizing principle. However, we prefer Miller's original format since the MAST Report's version of the effects hierarchy obscures primary and secondary effects and omits several factors.

The report does use the updated conceptual models to organize existing knowledge in that the discussion is organized by environmental driver, habitat attribute and life stage, although the report deviates sharply from the conceptual models in its use of hydrology as the organizing principle for the analysis of new data by focusing only on two dry-wet year combinations. Why the two wet years of 2006 and 2011 were selected as being particularly informative for determining what is driving species abundance is unclear. While it is certainly appropriate to discuss flows as they relate to each life stage, it is inappropriate to highlight them over all other environmental drivers.

The MAST Report's narrow focus on four recent years also artificially limits the strength of its analyses and conclusions. As a result, the MAST Report results were largely inconclusive as to which factors are likely affecting delta smelt abundance. The agencies have collected decades of data. Looking at a very small subset of years reduces the chances that causes of the apparent declines in abundance can be parsed out, and the result is an increased chance of spurious findings. At a minimum, the MAST Report could have examined why abundance in 2011 was apparently higher than the entire set of POD years from 2002-2010, and in the years leading up to the POD. Assuming that the authors choose to retain the use of flows as the organizing principle, an examination of the historical water year types indicates that 1975-1976, 1981-1982, 1985-1986, and 1994-1995 were all wet years preceded by drier years (based on the Sacramento Valley Index). These years span both the pre- and post-*Potamocurbula* period. While still not constituting a strong statistical data set, addition of these years of data would strengthen the understanding of delta smelt population dynamics under this combination of flow conditions.

While the conceptual models contained in the MAST Report is an improvement over previous models, they are still too poorly defined to use as the basis for developing testable hypotheses. The models need to be more explicit about how and which driver and habitat attribute affects each process (e.g. survival, maturation, growth, fecundity). It would also be helpful to indicate our current understanding of the relative importance of each factor and the interactions between variables as well as the certainty of our knowledge and the potential magnitude of effects. MAST Report, p. 32, lines 711-712, states that "we consider all habitat attributes discussed here as equally important…" While this may be true, not all habitat attributes are equally limiting. The report should include some indication of which attributes may be limiting survival, growth, and reproduction.

There remain many foundational questions that should be captured in the conceptual models and translated into testable hypotheses. We cannot list all of the foundational questions in this comment letter, but would be pleased to discuss the types of questions that need to be addressed in a follow up conversation.

Finally, the MAST Report should also acknowledge the potential for survey bias as well as the existence of random error- particularly in years with low catch. The MAST Report appears to assume that adult and larval survey data can be used without any consideration of survey bias or uncertainty. As there is evidence of bias and random error in the survey data, see Appendix 4, the MAST conclusions based on consideration of index ratios is problematic.

The following are specific recommendations for further improving the conceptual model diagrams:

<u>Comments on MAST Report Figures 8-11</u>

- The variable "food availability/visibility" is appropriate, but visibility should be directly, not indirectly, linked to turbidity.
- The MAST Report's "food production/retention" variable is directly linked to turbidity and hydrology, but it should also be indirectly linked to ammonium levels and/or N:P ratios.
- There should also be a variable that includes food quality, rather than just quantity. Food quality could be heavily influenced by N and P, as well as past clam invasions. Proximity to wetlands may also affect food quality and quantity (Murphy *et al*., in press).
- Predation risk is properly linked to predator abundance and turbidity, but it may also be indirectly linked to N:P ratios (Glibert *et al*. 2011). To the extent predator populations could be impacted by stoichiometric shifts, more predators means more risk.
- The migration variable for adults assumes that delta smelt migrate. This assumption may not be valid given the finding that a sometimes significant portion of the population are year-round residents in the Cache Slough/Sacramento Deep Water Ship Channel region

(Jim Hobbs presentation at EET 8/22/2013). A more appropriate habitat attribute might be spawning cue which should also include a temperature factor.

- Entrainment risk at the adult life stage is not just related to hydrology and exports, but also turbidity.
- Turbidity is not just a function of hydrology, but also of past suspended sediment loading patterns and wind speed. Suspended sediment loading is in turn partially determined by the weather, but also by historical land use patterns (*e.g.*, gold rush sediment, changes in upstream vegetation).
- Model should include considerations of geography or physical habitat or bathymetry of spawning substrates. The current model assumes that geography is fixed; but it's not fixed and the BDCP envisions making major changes to physical habitat. Based on Murphy et al. (in press), physical habitat variables should include "availability of tidal wetlands" and "availability of high quality spawning substrates" and perhaps availability of "bathymetric up-wellings."
- The temperature variable should explicitly recognize that favorable temperatures may allow for additional clutches of eggs.

Appendix 2

Questions 2 and 5: Is the report objective? Is the tone impartial? Are uncertainties, alternative hypotheses and conceptual models, or incompleteness in the evidence explicitly recognized?

The report makes a good effort at summarizing the information and conceptual models objectively and impartially; however, there are several places where the impartiality could be improved, for example:

- At its foundation, the basic structure and the objectives of the report place undue importance on hydrology as the key driver of delta smelt abundance. The fact that the report focused specifically on the comparison between the wet years of 2006 and 2011 implies that the authors assume wet hydrology is a key driver of abundance. In fact, the second report objective on page 20 asks, "why did delta smelt fail to respond to wet conditions in 2006?" This question pre-determines that wet conditions should increase delta smelt abundance.

- Several statements do not objectively describe the influence of CVP/SWP operations compared to other anthropogenic influences on the estuary. For example:

  1. Statement in MAST: "These alterations include diking and draining of the historical wetlands, large scale water diversions from the southwestern Delta into the California State Water Project (SWP) and the Federal Central Valley Project (CVP), inputs of contaminants, and species introductions." (MAST Report at p. 15, lines 337-339.)

     This list is incomplete and inappropriately focused on the SWP/CVP diversions when up-stream and in-Delta diversions have also greatly altered the estuary. Besides the changes identified above, the list should include: deepening and straightening of channels including the Sacramento River, and the Sacramento and San Joaquin Deep Water Ship Channels, significant increases in agricultural development (and associated water use) throughout the Sacramento Valley and in the Delta, and the construction of the extensive network of rip-rapped levees throughout the Delta. While many species are introduced; only the ones that are able to proliferate have altered the estuary.

  2. Statement in MAST: "Moyle and Bennett (2008) and Baxter et al. (2010) suggest that the SFE, particularly the Sacramento-San Joaquin Delta has undergone an ecological regime shift. Specifically, the Delta has changed from a pelagic-based estuarine system with variable salinity on seasonal and annual scales to a system reminiscent of U.S. southeastern reservoirs. In the present system an invasive aquatic macrophyte (*Egeria densa*) dominates the littoral areas of many areas of the Delta and provides ideal habitat for many invasive fishes…invasive clams...and [a] current management of water for agricultural, industrial and urban purposes is focused on stabilizing flow and salinity regimes to optimize water exports by the federal Central Valley Project (CVP) and State Water Project (SWP)." (MAST Report at p. 18, lines 390-401.)

The MAST Report states that this theory of a system reminiscent of a southeastern reservoir was "suggested" by the cited references, however the document is written as though it is a scientific fact. It should be noted that the cited references did not establish that the flow regime had been stabilized by water project operations, nor do the references establish that changes in water project operations resulted in the laundry list of identified changes in the environment.

The SWC have completed an analysis of flow and salinity trends. The preliminary analysis was presented during the SWRCB Phase II workshops last fall. That analysis indicates that flows from the Sacramento River continue to exhibit significant variability. Comparatively speaking, the San Joaquin River exhibits significantly less variability, but that change in the San Joaquin River system cannot be solely attributed to the CVP-SWP, as upstream water use is a significant contributor.[1]

In addition, optimizing exports by CVP/SWP is not the sole intent of water management actions. In-Delta water uses also dictate water management actions to maintain fresher water conditions.

3. The MAST Report describes flows from north Delta to OMR via the artificial delta cross-channel. (MAST Report, p. 48, lines 1060-1063.) Report should recognize that flows also pass through the natural Georgiana Slough.

4. The MAST Report needs to clarify that pumping by SWP and CVP are sufficient to cause the loss of ebb tide flows only in some areas and at some times. (MAST Report, p. 48, lines 1063-1066.)

<u>There are several places where a more balanced presentation is needed, including:</u>

- Statement in MAST: "The other native osmerid fishes commonly found in the upper SFE is longfin smelt (*Spirinchus thaleichthys*) which regularly spawns in the Delta." (MAST Report at p. 16, lines 348-349, see also, p. 17, lines 383-385.)

  First, the relevance of the reference to longfin smelt in a paper about delta smelt is unclear. Longfin smelt have very different biology than delta smelt, primarily being a marine species. Second, it is true to say that some longfin smelt spawn in the Delta, but it isn't accurate to imply that all, or even most, longfin smelt spawn in the Delta. There is evidence that many longfin smelt spawn in the Napa River and farther downstream. (*See e.g.*, COE trawling program data for Napa River in 2001 and 2003.)

- Statement in MAST: "Most delta smelt complete the majority of their life cycle in the low salinity zone (LSZ) of the upper estuary and use the freshwater portions of the

[1] The SWP has no facilities on the San Joaquin River system.

upper estuary primarily for spawning and rearing of larval and early post-larval fish." (MAST Report at p. 16, lines 356-359.)

The statement that delta smelt complete the majority of their life cycle in the LSZ should be further qualified. Dege and Brown 2004 describe the "centroid" of the delta smelt population as occurring in the LSZ. However, as Sommer 2013 explains:

> "…the overall distribution of delta smelt habitat is much broader. The surveys do not necessarily capture the extremes of distribution and habitat shifts among years. Our analysis showed that delta smelt habitat is often located well downstream of the Delta, commonly Suisun Bay…one of the most surprising discoveries was their presence in the Napa River…Hobbs et al. (2007) found that use of habitat in this region results in a unique chemical signature in the otoliths of delta smelt and revealed that the portion of fish that use the Napa River can be substantial (e.g., 16% to 18% of the population in 1999).

There is also some question regarding the extent that delta smelt spawning and rearing is limited to the freshwater portions of the upper estuary. Even Bennett (2005)[2] indicated that spawning distribution changed from year to year, stating, "In years of high freshwater discharge spawning distribution is broader encompassing most of the Delta, Suisun Marsh channels, and the Napa River [cite omit]." Bennett's description is consistent with that articulated by Moyle 2002[3] and 1992[4], reflecting previous observations reported by Radtke (1996), Wang (1986, 1991) and Wang and Brown (1993).

This migration hypothesis is further questioned by Murphy and Hamilton (in press),[5] where the authors suggest that the delta smelt population expands in all directions seeking fresher water for spawning and rearing rather than limiting their search for fresher water only to upstream locations.

- Statement in the MAST: "…leading to concerns that the population might now be subject to "Allee" effects (Baxter et al. 2010) and have lost its resilience, meaning its ability to recover to higher population abundances when conditions are suitable…Unfortunately, the increase in delta smelt abundance was short-lived and did not carry over into the following year-class in 2012, a drier year." (MAST at 19, lines 410-412.)

  The MAST report needs to provide a more balanced presentation of this issue. Baxter *et al.* 2010 presented the potential Allee effect as an untested hypothesis so the Mast

[2] Bennett WA. 2005. Critical assessment of the delta smelt population in the San Francisco estuary, California. San Francisco Estuary and Watershed Science 3(2).
[3] Moyle PB. 2002. Inland fishes of California. University of California Press. Berkeley, CA.
[4] Moyle, P.B, Herbold B, Stevens D.E, Miller, L.W. 1992. Life history of delta smelt in the Sacramento-San Joaquin Estuary, California. Transactions of the American Fisheries Society 121:67-77.
[5] The paper is titled, "Eastward migration or marsh-ward dispersal: understanding seasonal movements by delta smelt."

report needs to be cautious about presenting this concept without appropriate qualifying statements. We are unaware of any published analysis that tests the Allee hypothesis so significantly more work would need to be done before it could be put forth as a potential concern. The MAST Report does properly point out that the increase in abundance in 2011 does not support the Allee hypothesis.

The MAST Report also seems to assume that since 2012 was drier than 2011, the comparative dryness of 2012 is the reason the apparent abundance increase in 2011 did not carry over to 2012. However, there is no evidentiary support provided for the expectation that the apparent 2011 abundance increase should have carried over to 2012. Conversely, if the MAST expectation regarding 2012 abundance is based on Feyrer *et al.* 2007, and an increase in abundance was expected in the Summer Townet Survey, based on high fall 2011 outflows, that should have been explicitly stated. If that is the case, then the Feyrer *et al.* 2007 analysis should have been discussed, along with its limitations.

<u>There are several places where uncertainties and the incompleteness of the evidence should be explicitly recognized. For example:</u>

- Statement in the MAST: "Longfin smelt, age-0 striped bass (*Monrone saxatilis*), and threadfin shad (*Dorosoma petenense*) decline simultaneously with delta smelt…." (MAST Report at p. 17, lines 383-385.)

  The MAST Report should acknowledge that the various surveys, or population indices, suggest different abundance trends. For example, the Otter Trawl data suggests that longfin smelt abundance has not declined since the 1980s, while the FMWT data suggests a significant decline in longfin smelt abundance during the same time period. The fact that different surveys suggest different abundance trends indicates that some surveys are be more effective at sampling longfin smelt than others, which is something that needs to be investigated before one survey can be relied on more heavily than another. It is also an uncertainty that needs to be acknowledged in the MAST Report.

  One possible explanation for differences in the surveys is a change in species distribution, either within the water column or between areas that are sampled and those not sampled. The surveys are limited in their ability to identify changes in species distribution because the surveys monitor the same locations each year. There are examples of where this has occurred. For example, striped bass age-0 fish have likely changed their distribution away from areas sampled by the FMWT, moving from channels to shoal areas (Sommer *et al*. 2011)[6]. This observation is further substantiated by the survey data for age-1 fish, which did not show the same decline (Sommer *et al.* 2011). This change in age-0 striped bass distribution should be discussed in the MAST Report as an uncertainty about the extent to which the age-0 striped bass have declined.

[6] Sommer, T., Mejia, F., Hieb, K., Baxter, R., Loboschefsky, E., Loge, E. 2011. Long-term shifts in the lateral distribution of age-0 striped bass in the San Francisco Estuary. Transactions of the American Fisheries Society, v. 140, pp. 1451-1459.

The MAST Report should acknowledge the limitations of the surveys and indicate that part of the testing of the MAST Report's conceptual model should include evaluating the surveys (*i.e.*, testing efficiencies, changes in species distribution, etc.)

- Statement in the MAST: "Since the beginning of the POD in 2002, the delta smelt population indices have often been at record lows…." (MAST Report at p. 19, lines 409-410.)

  The MAST Report should acknowledge the limitations of the surveys and the evidence of survey inefficiencies. For example, Jon Burau and Bill Bennett have observed that delta smelt move to the sides of the channel during the ebb tide and to the middle of the channel during the flood tide. Feyrer *et al*. 2013[7] confirmed this behavior. What this suggests is that surveys on the flood tide are going to catch significantly more fish where delta smelt are present, and that surveys on the ebb tide are going to fail to successfully sample delta smelt even when they are present.

  There is evidence of other survey errors and inefficiencies that may have been particularly acute during the POD years. Please see Appendices 3 and 4.

<u>Alternative conceptual models are not accurately described or appropriately recognized.</u>

- Statement in MAST: "…although some researchers have suggested that single variables may have particular or even primary importance (*e.g.,* Glibert *et al.* 2011)." (MAST at p. 18, lines 389-390.)

  Glibert et al. 2011[8] described a regime change in nutrient ratios and explained how that change could cause a wide range of biological changes in the Bay-Delta, like those already being observed (*e.g.*, changes in dominant species of zooplankton and fishes (rise in centrarchids), increased blue-green algae and SAV, and increases in clam abundance). Glibert *et al.* did not suggest that the observed declines in delta smelt abundance indices were caused by a single factor rather Glibert *et al.* described a model of how changes in nutrient ratios could have led to multiple changes in the environment.

  The model described in Glibert *et al.* is actually an alternative model to the single-variable model described by Moyle and Bennett (2008) and the POD Synthesis Report, referenced immediately below, which suggests that all of the aforementioned changes were caused by a change in salinity and flow patterns rather than changes in nutrient ratios.

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[7] Feyrer, F., Portz, D., Newman, K.B., Sommer, T., Contreras, D., Baxter, R., Slater, S.B., Sereno, D., Van Nieuwenhuyse. 2013. SmeltCam: Underwater Video Codend for Trawled Nets with an Application to the Distribution of the Imperiled Delta Smelt. PLoS ONE 8(7):e67829. Doi:10.1371/journal.pone.0067829.
[8] Glibert, P.M., Fullerton, D., Burkholder, J.M. Cornwell, J.C., Kana, T.M. 2011. Ecological stoichiometry, biogeochemical cycling, invasive species, and aquatic food webs: San Francisco Estuary and Comparative Systems. Reviews in Fisheries Science, 19(4): 1-60.

The entire nutrient topic should be further developed in the report and we are happy to provide assistance in this area. There is a tremendous amount of published research and available data in SFE as well as elsewhere in the world that could be included and evaluated in this report.

- Statement in the MAST: "One hypothesis to explain these changes in fish population dynamics is that lower prey abundance reduced the system carrying capacity…." (MAST Report, p. 66, lines 1477-1479.)

  This is only one hypothesis, and it has not been shown to be any more possible that any other hypothesis. Another hypothesis is that abundance of these species was never responding to outflow, but rather to a factor related to outflow such as ammonium concentration or the ratio of nitrogen to phosphorous (Glibert *et al*. 2011).

- Statement in the MAST: "…the decline in *P. forbesi* in the Suisun region may be related to increasing recruitment failure and mortality…in this region due to…entrainment of source population in the Delta…."

  While this hypothesis has been frequently cited, we are unaware of any evidence that *P. forbesi* populations in the Delta would make it to the Suisun region, even if the CVP/SWP pumps were not operating.

- Statement in the MAST: "Currently, *E. affinis* abundance peaks in spring [cite omit] coincident with hatching delta smelt. *E. affinis* abundance has been negatively related to X2 since the clam invasion [cite omit]. When X2 is "high" outflow is low and *E. affinis* densities are low. These lines of evidence suggest that the first feeding conditions may improve in spring with higher outflow."

  The negative relationship between *E. affinis* and X2 is described, suggesting that higher outflow increases abundance of this prey item for delta smelt. However, *E. affinis* is also related to nutrient forms and ratios (Glibert *et al.* 2011).

Appendix 3

Questions 2 and 3: Are the data and analyses handled competently and applied appropriately? Are conclusions and recommendations adequately supported by evidence and analysis? If the report's content is based on unpublished results, are findings and conclusions properly attributed to an individual or a specific program or project?

While the report includes an impressive compilation of references to published literature, it still makes numerous statements that are unsupported, many of which could be supported. For example, page 35, lines 782-784; page 784, line 784; page 35, lines 787-790; page 35, line 790.

Specific comments regarding the use of data is provided, below:

- **Comments on MAST Report Figures 41 and 42**

There is inadequate scientific rational for limiting the years in the analysis to the years since 2002. The MAST Report justified the use of the post 2002 years because that is when the SKT survey started. However, we have data from decades earlier; that data is relevant and should be utilized. The analysis could go back to 1995 and use the 20 mm survey. The analysis can go back to the 1960s and use the FMWT and STN. The problem with using fewer years is not just ignoring decades of data, it is also that catch numbers in recent years have been so small that the index ratios in the Figures 41 and 42 are increasingly uncertain. A change in catch of just a few fish can cause significant changes in the index ratios in Figures 41 and 42, which makes interpretation of the ratios too uncertain to be meaningful.

The way the 20mm survey (larval) is calculated is also a concern for purposes of this analysis because larvae are generally not detected in the survey until they are 20mm. Since the smaller larval delta smelt will not be detected, [1] the survey is only measuring the larger larvae, which were likely spawned earlier in the season.[2] Therefore, if delta smelt spawn earlier or later or if the spawning window is short or long in a particular year, the ratios in Figures 41 and 42 will be greatly impacted. This problem may well be distorting the data in Figures 41 and 42. It is worth noting that the height of the orange bar (larvae/previous adults) is inversely related to the height of the green bar

[1] The MAST Report observes that the 20mm survey begins before the delta smelt egg clutches have even hatched based on temperature. (MAST Report at 94, lines 2098.) The laying window based on temperature does not close until June or July. Given that eggs may not hatch for 35 more days and then are not large enough to be detected in the survey for weeks after that, the 20mm survey may not be an accurate measure of larval abundance? For example, the 2013 20mm survey index used only data from April and May. This would represent delta smelt that were laid as early as February through perhaps early April. But according to the MAST Report, the delta smelt spawning window in 2013 extended until June and these smelt would not have been detectable until July or August, a full two to three months beyond the coverage of the 20mm survey index.

[2] MAST Report, p. 81, line 1805, observes the delta smelt's ability to spawn twice. The practical effect is that the 20 mm survey is subject to enormous bias as with double spawning there will be many delta smelt too small to be captured in the 20mm survey. The MAST report should acknowledge this limitation and the uncertainty it creates in the use of the data.

(juveniles/larvae). This means that in Figures 41 and 42 when it appears the larvae phase has had terrible survival, the subsequent survival from the larval to juvenile phase is typically great. This could be density dependence, but another explanation is that the 20mm index is not representative of actual abundance,[3] and giving inaccurate measurements. The STN may be more accurate (or at least flawed in different ways) so that errors in the 20mm survey are partially corrected by the time of the STN. However, that there is strong evidence of size selection bias in the STN Index caused by inconsistency in the start date of the STN each year. Figures A-B, below, illustrate this point.

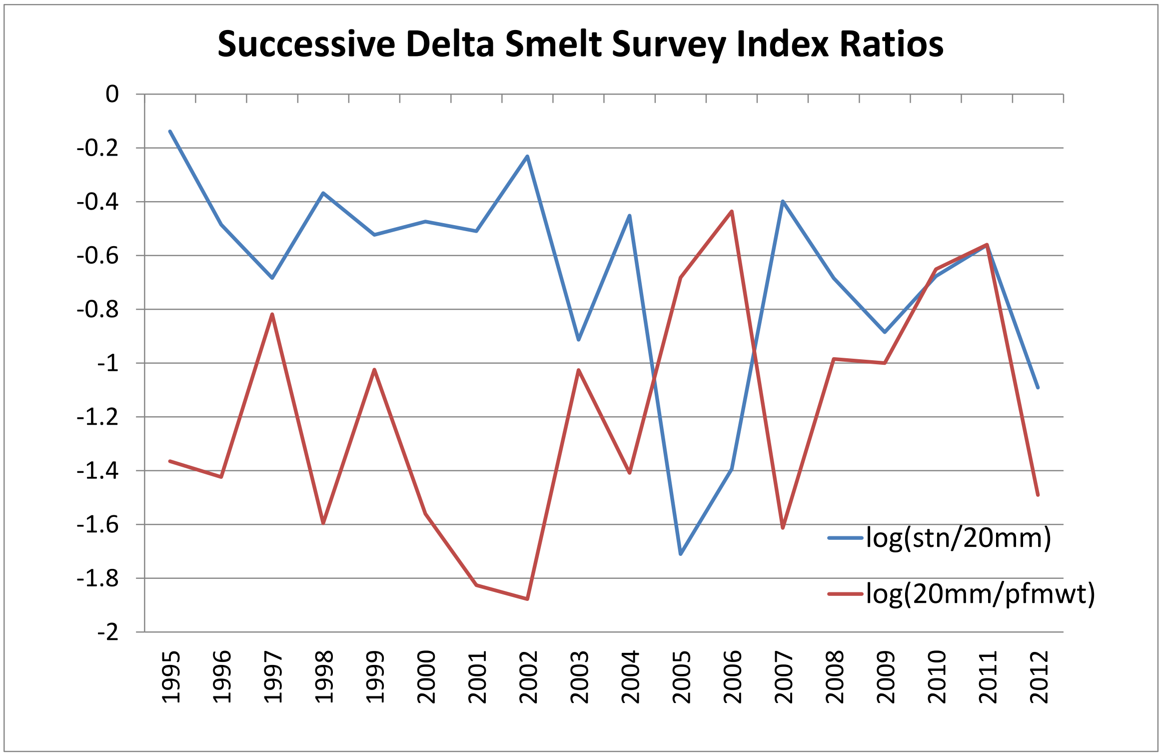

Figure A. Log STN/20mm survey compared to log 20mm/previous FMWT for years 1995 through 2012.

[3] MAST Report at p. 92, lines 2044-2064, does not acknowledge that the 20mm survey may not be representative of larval abundance.

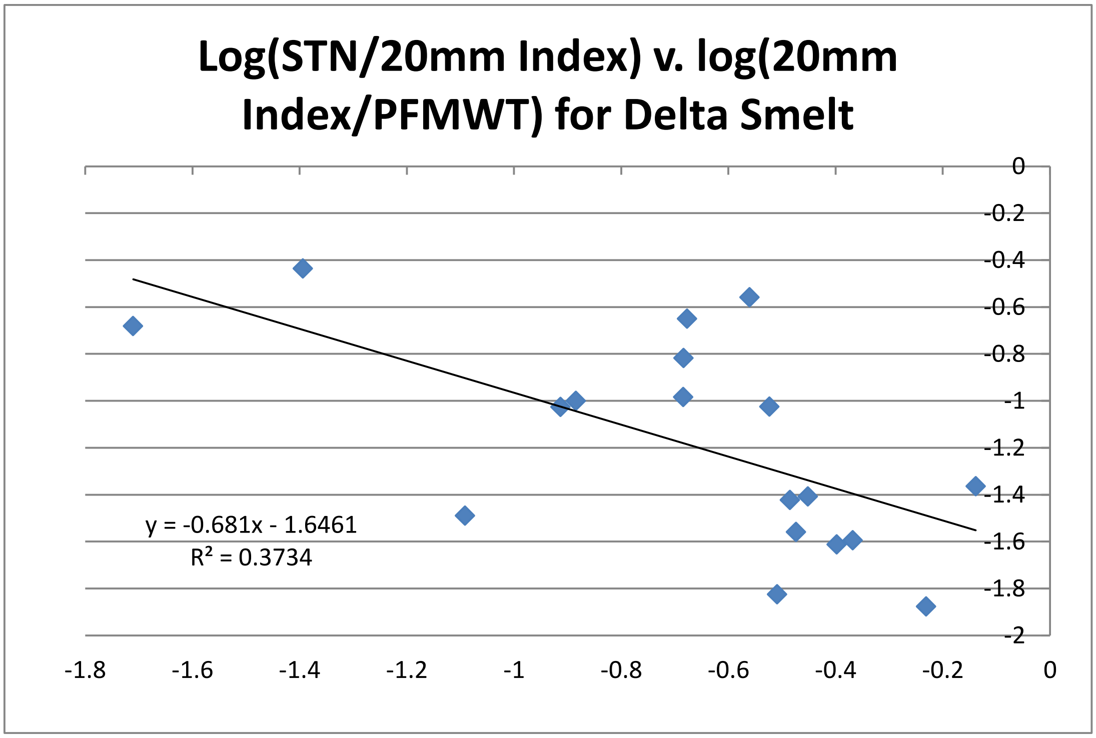

Figure B. Log (STN/20mm index) v. log (20mm index/previous MWT) for delta smelt.

Figure A shows successive index ratios for delta smelt in individual years. Figure B shows the same data plotted as a dot plot. The pattern is very clear. When survival from FMWT to 20mm is poor, survival from 20mm to STN is good and vice versa. This is either density dependence (and this is very unlikely at current abundance levels) or it is survey errors. If it is survey error, then the 20mm index may not be useful as an index of delta smelt larval abundance and should either be corrected or abandoned.

Again, the fact that we can use FMWT and STN to detect errors in the 20mm survey does not necessarily mean that FMWT and STN are without problems, but does suggest that the errors in these surveys are probably not fully correlated with the errors in the 20mm survey.

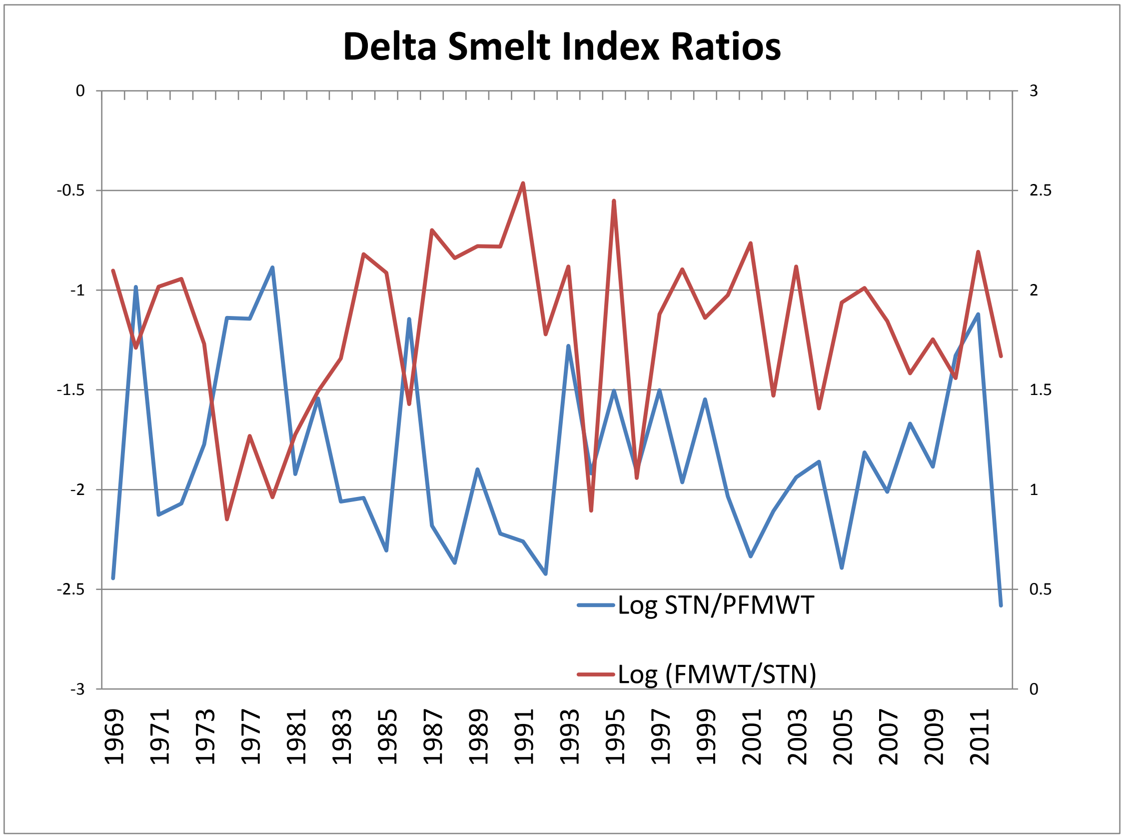

Figure C. Delta Smelt Index Ratios. Log STN/previous FMWT comparison to long (FMWT/STN).

By a similar process we can look for error in the STN survey. Figure C shows successive log(STN/PFMWT) and log (FMWT/STN) values since 1969. Clearly, the two ratios tend to move in opposite directions. Either this is some form of density dependence (though it is hard to see how density dependence could have applied during the low abundances of the 1980s) or it is an indication of a bias/error relationship. That is, bias or error in one survey (either FMWT or STN) tends to get corrected in the succeeding survey because the errors in the two surveys are not well correlated with each other.

In fact, the 20mm Index/PFWMT index provides fairly strong evidence that FMWT survey error jumped during the POD years, potentially exaggerating the estimated decline in delta smelt abundance. Figure F shows these ratios from 1995 – 2013. The ratios took a significant upward jump almost exactly when the POD occurred, with the 2004 FMWT and 2005 20mm survey. In other words, supposed survival from adults to larvae took a major leap upward during the POD years (years supposedly very bad for smelt) or the FMWT Index has been biased downward during the POD years or the 20 mm survey has been biased upward during the POD years. Given that the SKT also suggests that the FMWT Index has been biased downward during the POD years, FMWT bias may be the most likely explanation. In turn, if the FMWT index is suffering from significant bias or error, then unless that bias remains constant from year to year it will be difficult to parse out biological conclusions simply by looking at index ratios – the values are simply too uncertain.

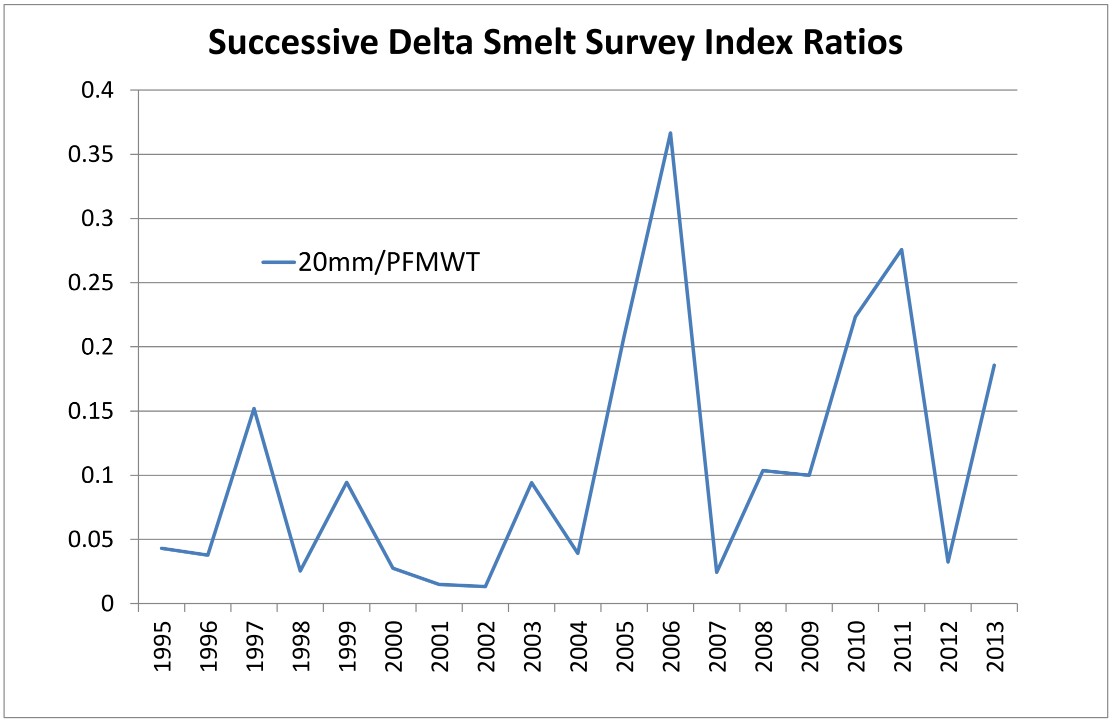

Figure F. Successive Delta Smelt Survey Index Ratios, 1995-2013.

The MAST Report's search for biological meaning by looking at successive survey ratios is fraught with problems unless and until survey errors are examined and corrected in the data.

- **<u>Comments on MAST Report Figure 43</u>**[4]

There are multiple technical errors underlying Figure 43 that undermine the utility of the comparison. First, the Sacramento plus San Joaquin River index on the x-axis represents the entire water year, and much of this data occurs before or after the two surveys used in each abundance ratio on the y-axis. This is not an appropriate use of data in a statistical analysis.

Second, only data from the years 2002 – 2011 are used in the analysis, which of course means that data from the year 2012 is missing, as is all data from the preceding decades. The use of such a small subset of years greatly magnifies the chances of incorrect inferences. As an example, we have attempted (without complete success) to reproduce the larvae/prior adults data points shown in Figure 43 for the years 2002 – 2011. We used FMWT instead of SKT.[5] We do not get the same fit using the relationship with the Sacramento + San Joaquin River flow index (Figure D) (although there still is a good relationship). However, once we add in additional years of data (1995 – 2001, 2012 – 2013), the relationship virtually disappears (Figure E). What appeared to be a strong

[4] This MAST Report figure is unpublished and authorship is not attributed to any individual or entity.
[5] We used the FMWT so we could recreate the analysis considering a greater number of years.

relationship now becomes (at best) a very weak relationship. This is a good example of how limiting the number of years can lead to incorrect inferences.

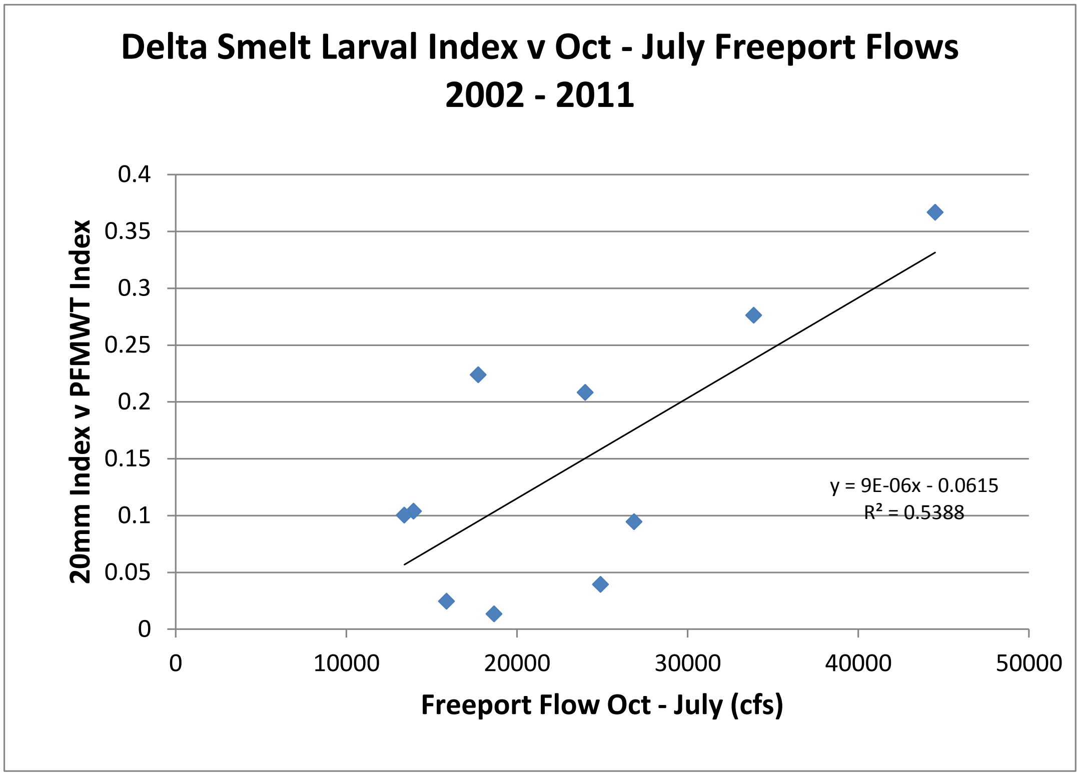

Figure D. Delta Smelt Larval Index (20 mm index v. previous FMWT index) v. October-July Freeport Flows (2002-2011).

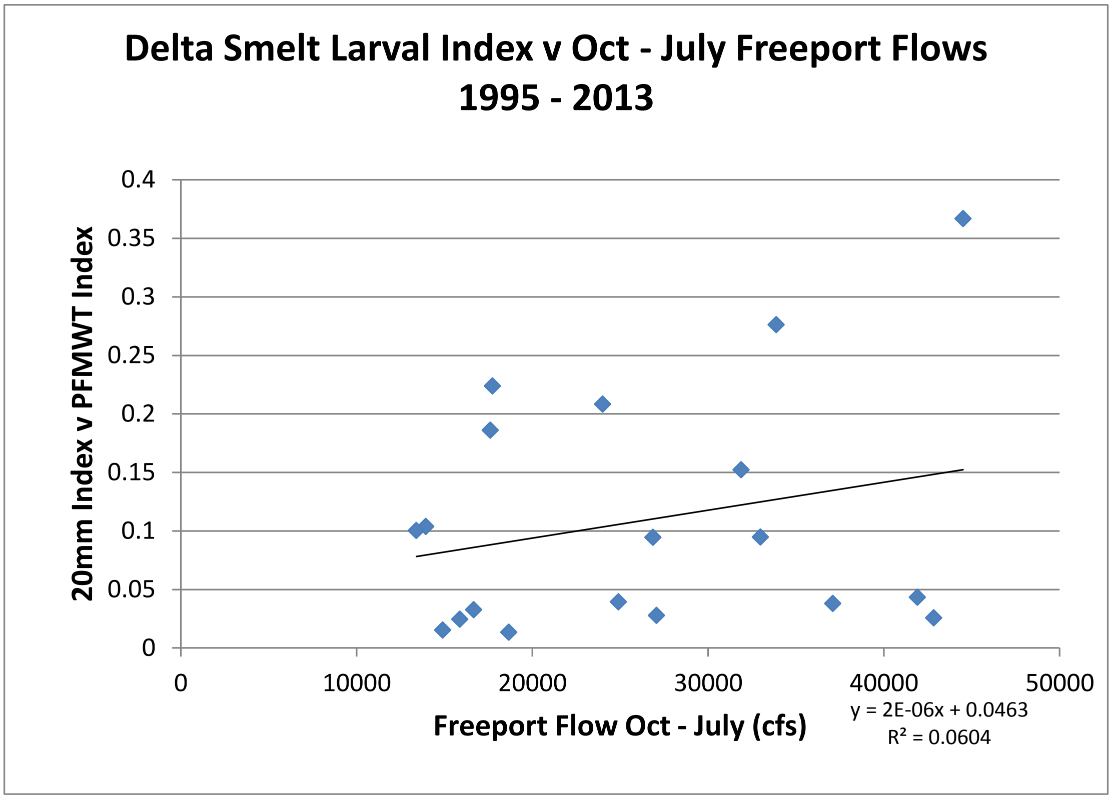

Figure E. Delta smelt larval index v. October-July Freeport flows, 1995-2013.

- **<u>Comments on MAST Report p. 106, lines 2315-2335</u>**

The analysis only considers temperature data in four specific years. As a result of using only a few years of temperature data, the MAST Report was unable to reach a conclusion. This illustrates the problem with ignoring decades of temperature data which could have been used to analyze the impact of temperature on survival.

- **<u>Comments on MAST Report p. 107, lines 2340-2342</u>**

It is unclear why striped bass are assumed to be a major predator. The more interesting analysis would be testing whether the centrarchids and/or inland silversides, which have increased significantly in abundance during the last decade, are causing changes in species abundance. The MAST Report just describes what happened in individual years but provides no insight into whether predation is or may be causing changes in abundance.

- **<u>Comments on MAST Report p.77, lines 1725-1727 (see also, p. 69, lines 1539-1540)</u>**

Sweetnam (1999) is outdated and not relevant to a discussion of delta smelt length during the POD years. FMWT delta smelt lengths have nearly returned to levels that existed prior to the drop in lengths recorded around 1992. See Figure H. It should also be acknowledged that prior to about 1992, not all delta smelt were routinely measured for length. As there were no standard procedures for measuring delta smelt, there is the possibility of selection bias (*e.g.*, the personnel measuring the fish might have tended to grab larger than average fish). The Summer Townet dataset also has length data. The STN length data from July does not support the pattern identified in the MAST (a collapse in smelt length after the early 1990s). Average STN length is shown in Figure I. Figure I suggests that lengths may have been slightly enhanced during the 1980s, but that lengths from the 1990s to the present are similar to lengths seen during the 1970s. Therefore, there is no evidence of a collapse in length and the so-called Big Mama hypothesis first proposed by Bennett should be rejected.

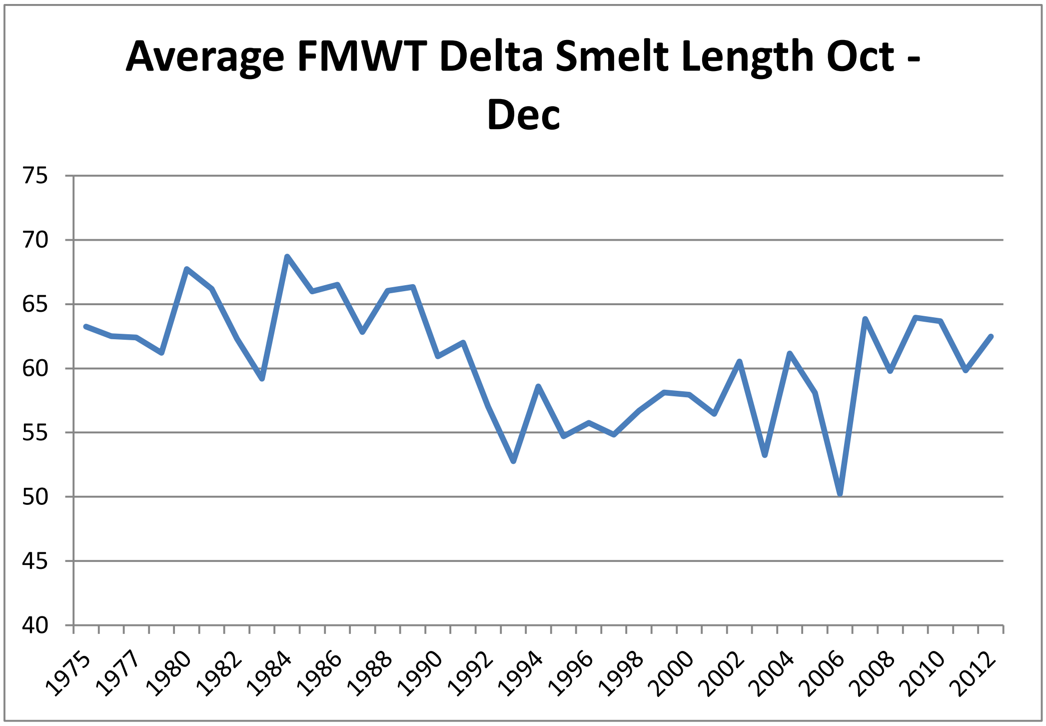

Figure H. Average FMWT delta smelt length, October- December for the years 1975-2012.

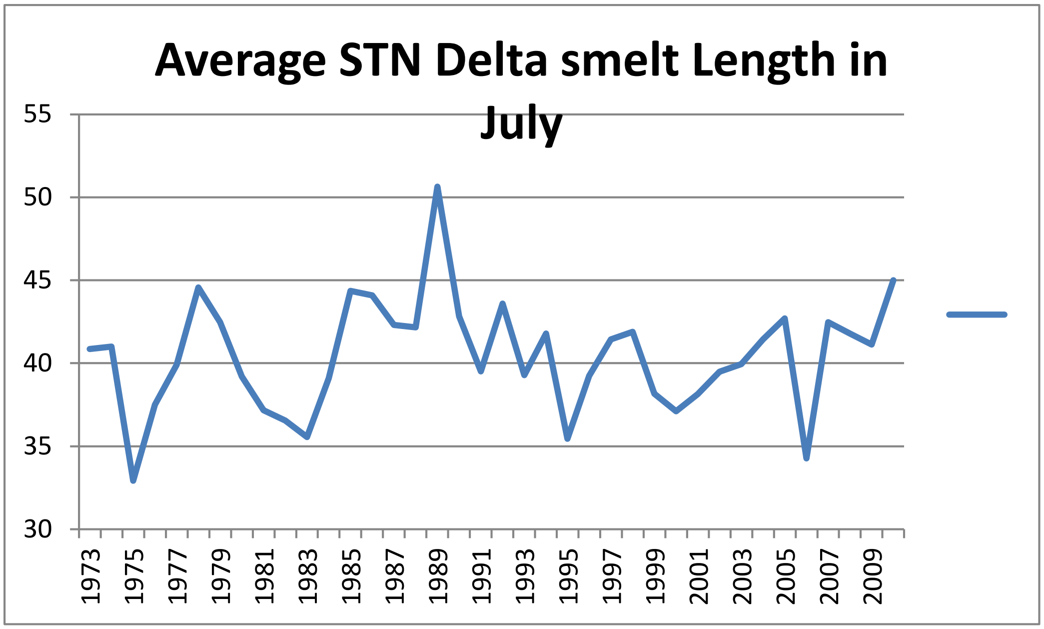

Figure I. Average STN delta smelt length in July, 1973-2009.

- **<u>Comments on Mast Report Figure 24, p. 101, lines 2244-2254</u>**

We are unable to find Figure 24. However we are concerned about the conclusions contained in the MAST Report that appears to be based on a correlation with four data points. A correlation using four data points is meaningless, suggesting a misapplication of standard statistical practices. In addition, many things are correlated with OMR flows; so even if the correlation described here existed, it would not be particularly informative and interpreting the results would be difficult.

The referenced discussion again refers to Figure 43, which was discussed above.

In light of the misapplication of standard statistical principles, the strong conclusions at lines 2250-2254 are **<u>not supported</u>** <u>by the analysis in the MAST Report</u>. (MAST Report, p. 101, 2250-2254, ["This suggests that overall hydrology (and perhaps overall climate) and its interactions with other environmental drivers has a very strong effect on habitat available to delta smelt spawning and larval rearing. This includes the effect of hydrology on OMR flows and entrainment, but likely also on many of the other habitat attributes shown in the conceptual models presented here (figs. 9-12)."]

- **<u>Comments on MAST Report Figure 44</u>**

Figure 44 is a linear correlation between the SKT index and the previous FMWT Index. This linear correlation is problematic.

First, large abundance values are given undue weight. We are interested in the index ratios between values in all years, not just the big abundance years.

Figure 44 uses a linear correlation between data measured on two different metrics, which can produce misleading results. We are interested in whether SKT Index is directly proportional to the FMWT index (e.g., if FMWT doubles, does SKT double?). The way to learn this answer is to correlate Log SKT versus Log FMWT. We have done so and the result is below. See Figure J. There is still a good correlation. But now you can see that the SKT varies as FMWT^$^{0.62}$ or fairly close to the square root of FMWT. This indicates that the FMWT (or less likely the SKT) may be inaccurate and that the true population of delta smelt may have dropped much less than suggested by the FMWT Index. One way to see this effect is the look at the range of the trend line. Log SKT varies from about 1.3 to 2.1 or SKTmax/SKTmin=6.3. But over the same period log FMWT Index goes from 1.2 to 2.5 or FMWTmax/FMWTmin = 20. Both show declines, but the fractional decline is quite different. Thus, if the FMWT were to be linearly related to the SKT, then the lowest values of FMWT during the POD years would need to be approximately tripled.

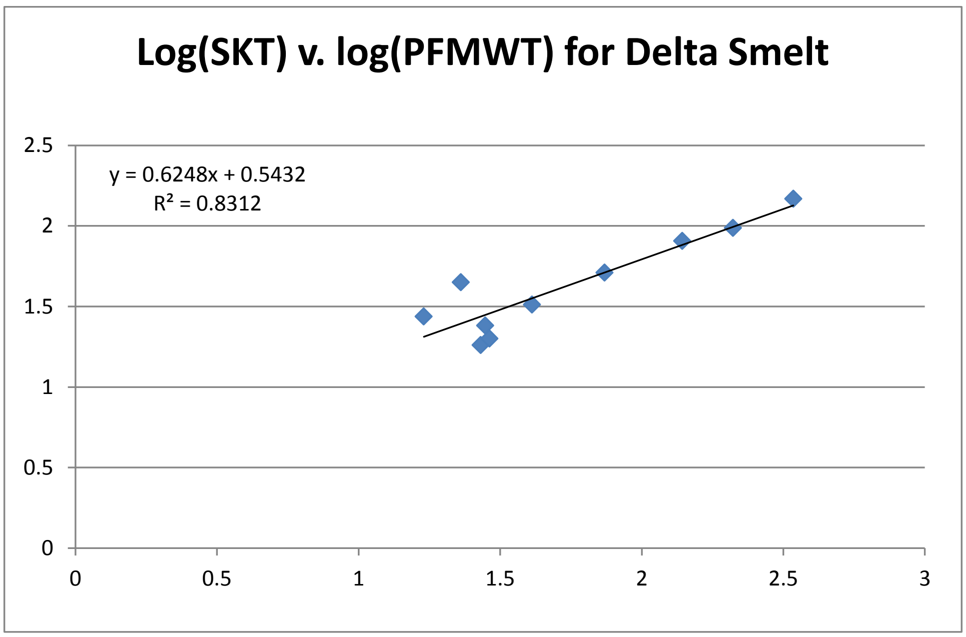

Figure J. Log (SKT) v. log (previous FMWT) for delta smelt.

- **<u>Comment on MAST Report p. 115-116, Table 4</u>**

The MAST Report did not even address the Fall X2 issue, largely deferring to FLaSH. The analysis that was included calculated the volume of habitat based on McWilliams (not Feyrer 2010) to represent simple open water acres within certain salinity ranges for 2005, 2006, 2010 and 2011. The MAST Report conclusion that the data "generally" support the fall X2 theory contained in the BiOp is not sufficiently suppported. The use of so few years of data is a violation of generally accepted statistical principles. The problem with this approach can be illustrated by considering an increasing number of years in the analysis. The more years that are considered, the weaker the statistical relationship. See Figures K through M.

Looking at all the years since 1975, there is no relationship between FMWT and Fall X2.

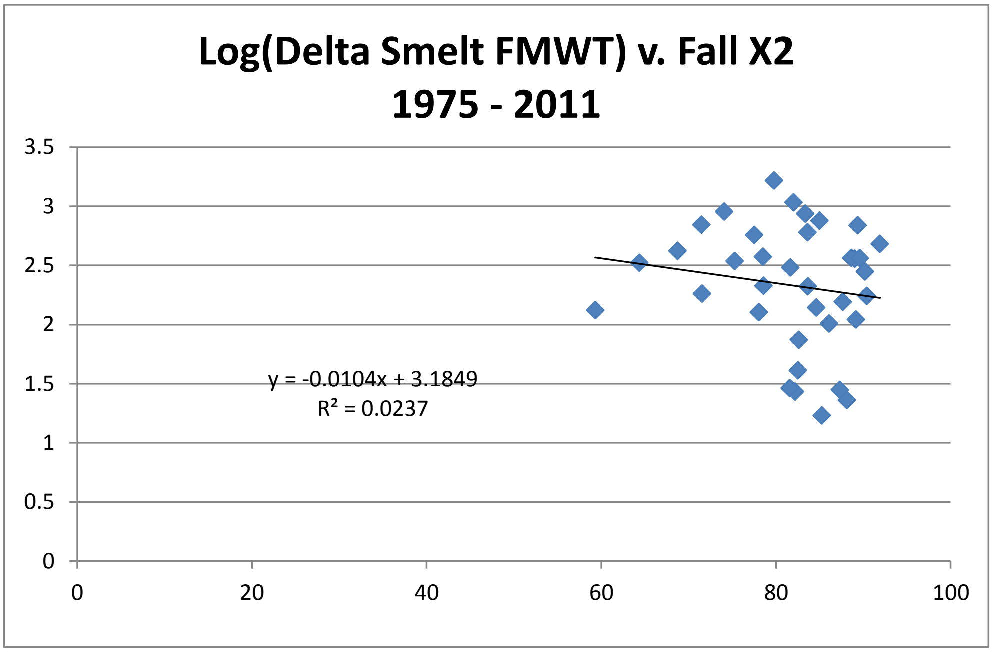

Figure K. Log (Delta smelt FMWT) v. FMWT X2 (1975-2011)

Looking at all the years since 1987, there is no relationship between FMWT and X2

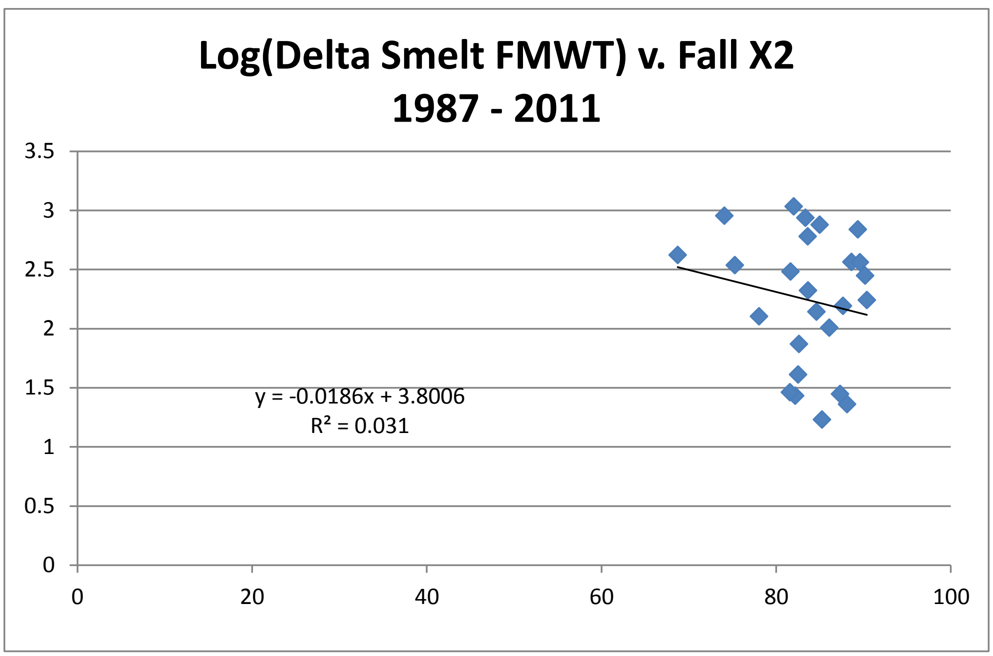

Figure L. Log (Delta smelt FMWT) v. Fall X2 for years (1987-2011)

There is a moderate correlation during the POD years between FMWT and Fall X2 driven entirely by a single datapoint (2011). The only way to generate a strong relationship is to exclude all years except 2005-2011 (making the influence of the single outlier in 2011 more dominant), and such an exclusion of data is not justifiable.

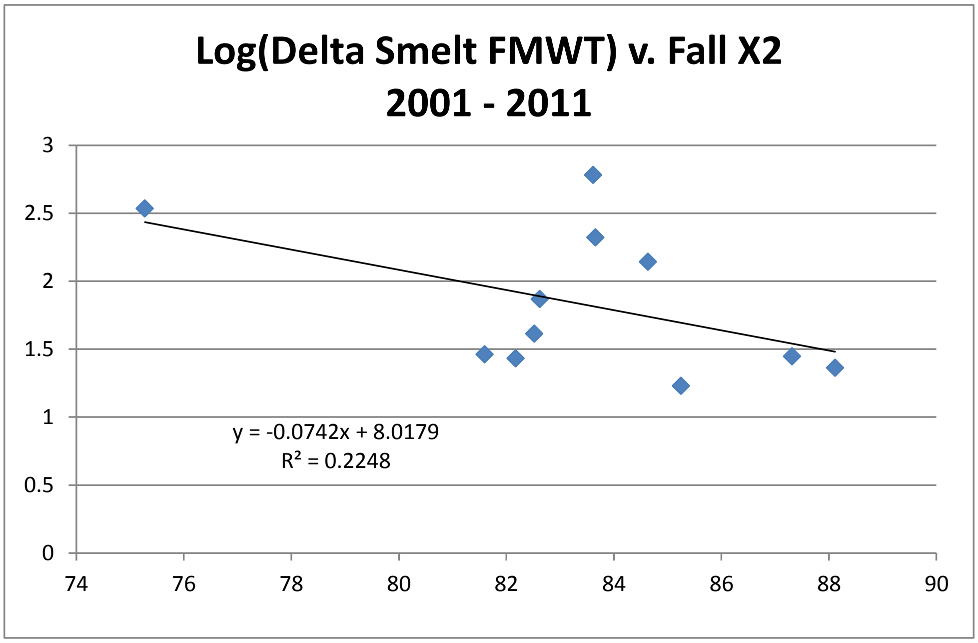

Figure M. Log (Delta smelt FMWT) v. Fall X2 for years (2001-2011)

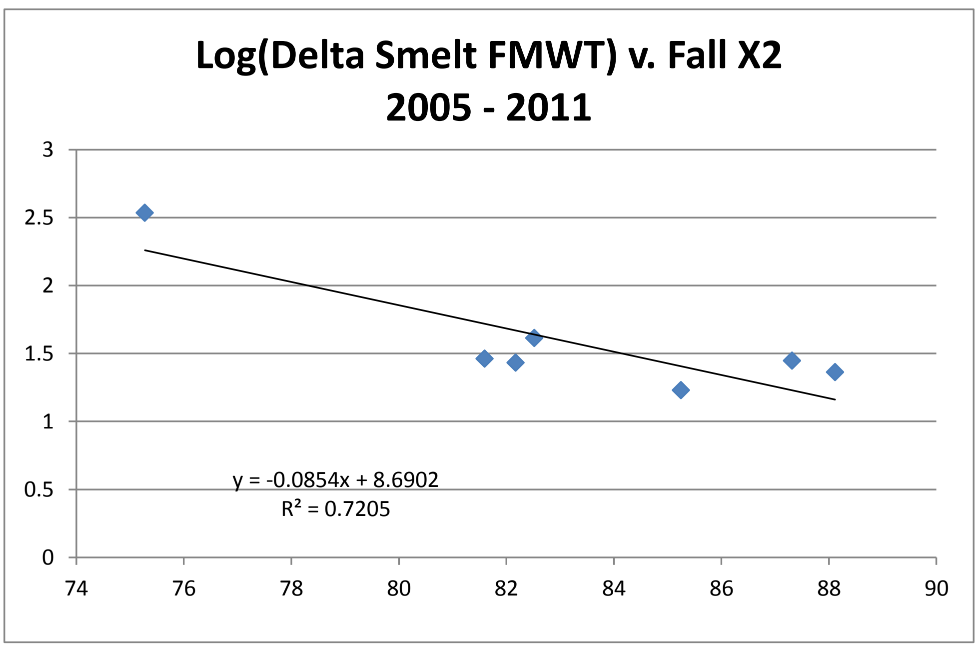

Figure N. Log (Delta smelt FMWT) v. Fall X2 for years 2005-2011.

- **<u>Comment on MAST Report, p. 82, lines 1829-1843</u>**

The MAST Report suggests that the FMWT might be a good surrogate for estimating long-term trends. However, as explained above, while the SKT and the FMWT indices track, the MAST Report fails to acknowledge that the SKT is roughly proportional to the square root of the FMWT index, meaning that if FMWT changes by a factor of 4, then

SKT changes by a factor of 2. If the FMWT changes by a factor of 9, then SKT changes by a factor of 3. As discussed above, there is good reason to think that the FMWT should not be relied upon during low abundance years and thus abundance ratios which use the FMWT Index during the POD years should not be relied upon.

- **<u>Comments on MAST Report, p. 112, lines 2453</u>**

The statement in the MAST Report is that the apparent carrying capacity from STN to FMWT has declined. This statement is partially contradicted elsewhere in the MAST Report, where it states:

> Despite this low level, the 2011 adults produced the highest adult abundance observed to date in 2012 [meaning SKT]. This suggests that within the range of adult variability observed in the SKT, adult stock size has not been a limiting factor in subsequent adult recruitment." MAST Report at p. 93, line 2077-2080.

This statement on page 93 is limited to the SKT years since 2002. The statement is more fully contradicted by looking at 2011 FMWT. The bounce in FMWT from 2010 to 2011 was enormous – a factor of ten – and that was the largest percentage bounce since 1975. Moreover, looking at absolute terms rather than just as a ratio, the value of the FMWT in 2011 was in the same range as FMWT values during earlier periods when conditions were supposedly better. This was impressive considering that the STN value of 2011 was not particularly high. So the idea that carrying capacity has declined is questionable, even if we were to assume that the abundance indices are representative. If potential survey error considerations are included, then the observed shift in the FMWT/STN relationship may be significantly overstated.

- **<u>Comments on MAST Report Figure 18 and p. 39, lines 878-881</u>**

The MAST Report states in reference to the historical X2 position that "The seasonal and interannual variations have become muted, especially in the summer and fall (fig. 18)". Although this statement has been made elsewhere in the literature, to our knowledge it has not been supported in a rigorous quantitative manner.

Figure 18 is a fails to confirm the statement for the following reasons:

1. The 2001-10 decade is the third driest decade since the beginning of the 20th century – wetter only than the extremely dry decades of the 1920s and 1930s (reported by Hutton to the SWRCB in the 2012 Analytical Tools Workshop, Section 3, pp. 16-34, attached).

2. Unimpaired X2 estimates do not represent reality, as the unimpaired Delta outflow calculation is significantly different than natural Delta outflow conditions (as reported by Hutton to the SWRCB in the 2012 Analytical Tools Workshop, Section 3, pp. 34-57, attached).

3. Even assuming for the sake of argument that unimpaired X2 estimates had analytical value, the comparison should have been made for the same hydrologic period, *i.e*. show unimpaired X2 calculations for the years 2000-2011.

- **<u>Comments on MAST Report p. 49, line 1083; p. 61, line 1362; p. 65, line 1462; p. 65, line 1462; p. 68, line 1534-1535; p. 70, line 1561</u>**

The MAST Report consistently ignores the significant amount of published research by Drs. Glibert, Dugdale, Wilkerson, Parker and Jassby on nutrients, primary productivity and food web structure and function. There is a passing reference but no in-depth discussion of their work. This oversight results in a Report that is incomplete and unbalanced.

- **<u>Comments on MAST Report p. 50, lines 1107-1117</u>**

The MAST Report cites Kimmerer 2008 but fails to also mention the significant error bars acknowledged by Kimmerer, improperly citing the 0-50% range as if these differences occur in different years. The MAST Report goes on to cite Kimmerer 2008 as supporting a finding that entrainment has a population level effect, while Kimmerer specifically stated that he did not find a population level effect.

The MAST Report cites Maunder and Deriso as having found that high entrainment can affect subsequent generations. The Maunder and Deriso best fit model did not find that entrainment was significant. There was a lesser model that identified entrainment as having a marginal effect; but when the data in the model was updated to 2010 (from 2006), the model no longer identified entrainment as even having a marginal effect.

Thomson et al. (2010) is also referenced as supporting the notion that high entrainment losses can adversely affect subsequent populations. In fact, entrainment was not one of the covariates tested by Thomson et al. (2010) and the word "entrainment" does not even appear in the body of the manuscript.

- **<u>Comment on MAST Report p. 84, lines 1880-1884</u>**

The MAST Report argues that delta smelt are density independent due to low abundance. The Report cites Kimmerer 2011 as evidence that any source of mortality will accumulate year-by-year. Kimmerer did not show that such an impact is accumulating, he merely made the theoretical argument that such accumulation is possible.

Dr. Richard Deriso analyzed this statement regarding accumulating impact, and it is his position that within standard fish stock-recruitment models a new source of mortality will merely lead to a new steady-state population that is slightly lower than before. Specifically, Dr. Deriso's[6] view is that:

[6] Dr. Richard Deriso, Personal communication.

If the population is at a low level of abundance then with conventional stock production models, such as the Ricker recruitment model, then it is true that substantive compensatory density-dependence is unlikely to be occurring. However it is also true that natural survival is maximized at a low level of abundance. Therefore the population would not increase only if the impact mortality is roughly greater than the species maximum intrinsic rate of growth. Furthermore in impact analysis the long-term equilibrium reduction in a population due to a constant annual mortality (such as through entrainment) is dependent on the maximum intrinsic rate of growth. For example, in a Ricker model, $B(t+1) = B(t)(1-F)\exp(a-b*B(t))$, the percent reduction in equilibrium abundance due to a given constant annual mortality "F" is equal to $-\ln(1-F)/a$ (Lawson and Hilborn 1985).. The parameter "a" is the maximum intrinsic rate of growth. Note that the long-term equilibrium abundance does not depend on initial population size. (Lawson, T.A. and R. Hilborn. 1985. Equilibrium yields and yield isopleths from a general age-structured model of harvested populations. Can. J. Fish. Aquat. Sci. 42: 1766-1771.)

It is not clear at present whether or not delta smelt abundance is low, at least based on the high FMWT index for 2011. Needless to say some caution should be exercised in basing a strong conclusion on a single year's index.

- **Comments on MAST Report, p. 41, lines 912-916**

The MAST Report describes the hypothesis by Feyrer et al. (2007 and 2011) that reductions in habitat area may be related to reductions in delta smelt abundance. To balance this discussion, the report should also describe the finding by Kimmerer *et al.* (2009) that delta smelt abundance does not appear to be related to habitat volume

- **Comments on MAST Report, p. 41, lines 921**

The position of the LSZ also affects ammonium concentrations, which may in turn affect phytoplankton and zooplankton biomass and species composition (Dugdale *et al.* 2007[7]; Glibert *et al.* 2011[8].)

- **Comments on MAST Report, p. 42, lines 935-938**

[7] Dugdale, R.C., F. P. Wilkerson, V. E. Hogue and A. Marchi. 2007. The role of ammonium and nitrate in spring bloom development in San Francisco Bay. Estuarine, Coastal and Shelf Science 73: 17-29

[8] Glibert, P.M., Fullerton, D., Burkholder, J.M. Cornwell, J.C., Kana, T.M. 2011. Ecological stoichiometry, biogeochemical cycling, invasive species, and aquatic food webs: San Francisco Estuary and Comparative Systems. Reviews in Fisheries Science, 19(4): 1-60.

The Report states that there is no evidence to support the effect of low turbidity on survival, growth, and reproduction. However, studies by Linberg and Baskerville-Bridges have found low turbidity effects feeding success of larval delta smelt.

- **<u>Comments on MAST Report, p. 43, line 950</u>**

The Report says there are two main sources of turbidity in the upper estuary. A third source of turbidity is plankton concentration. A discussion of this third source should be included.

- **<u>Comments on MAST Report, p. 49, lnes 1101-1103</u>**

Salvage is described as occurring nearly year-round in the beginning of the time series and now only from December to June. This observation seems to merit additional inquiry. For example, does this observation suggest that delta smelt may have occupied freshwater regions year-round in the past, as is now being observed in Cache Slough region? When did this occurrence change? Were delta smelt salvaged at approximately the same quantities year-round, or was there a peak that corresponds to the period of time when we observe salvage now?

- **<u>Comments on MAST Report, p. 50, lines 1124-1127</u>**

Castillo *et al.* (2012) is described without also describing the limitations of that study's design, such as water temperatures, location of releases, and pumping rates at the time of the study.

- **<u>Comments on MAST Report, p. 84, lines 1869-1871</u>**

The Report describes years with bigger females and higher spawning stock size as having better reproductive potential. Years with suitable spawning temperatures over longer periods of time should also be considered as having greater reproductive potential.

- **<u>Comments on MAST Report, p. 87, lines 1936-1939</u>**

The Report concludes that hydrology and exports interact to influence entrainment risk for adult delta smelt (Hypothesis 1). While there is evidence to support this, it is not presented in the discussion for this hypothesis beginning on p. 85. The information presented in pages 85-87 under Hypothesis 1 does not support his conclusion.

- **<u>Comments on MAST Report, Figure 52, p. 90, lines 2005-2007</u>**

The Report concludes that Hypothesis 4 is partially supported seemingly based on an observation of growth in 2011 being higher than in the comparison of years. However, Figure 52 does not show any difference in growth between 2011 and 2005 (a wet year and a dry year), and based on the variability, it is not apparent that there is a significant difference between any of the years.

- **<u>Comments on MAST Report, p. 105, lines 2311-2313</u>**

It should be noted that high water temperatures can also increase susceptibility to disease and to some contaminants.

Appendix 4

Question 6: What other significant improvements, if any, might be made in the report?

Three additional areas of discussion within the Report would significantly improve the report: 1.) survey error, 2.) the role of nutrients, and 3.) the role of contaminants.

Survey Error:
The MAST Report should acknowledge that the existing surveys are imperfect and include a hypothesis to the conceptual model that investigates the role of survey error. At the very least, the MAST Report should acknowledge that before extensive data analysis can be undertaken to determine likely factors affecting species abundance, there needs to be an investigation into the nature and extent of survey error, and that error needs to be corrected in the data (to the extent possible) before extensive data analysis is undertaken. We understand that the existing data is the best that we have and that we have all used that data for decades in various analyses in attempts to tease out potential factors affecting species abundance, but it has become increasingly clear that the surveys may not be reliable, particularly for teasing out the effects of specific variables on species responses, but also for assessing trends over time to the extent that the influence of these survey errors may have changed over time. The unreliable nature of the existing data makes results of data analyses difficult to interpret and the resulting confidences on the results are low.

There is good reason to believe our existing delta smelt surveys are not representative of smelt abundance or distribution. For example, Feyrer et al (2013) observed in their "smelt-cam" research that delta smelt change their distribution according to the tidal cycles in apparent attempts to control their position. On the ebb tide delta smelt were observed moving to the sides of the channel. On the flood tide delta smelt were caught in the middle of the channel. These findings are consistent with previous observations by Bill Bennett and Jon Burau. This is significant as the surveys only sample in the middle of the channels and there is no established protocol for only sampling on the same tidal cycle each survey. Other evidence of tidal cycle sampling error can be observed in Figures U through BB.

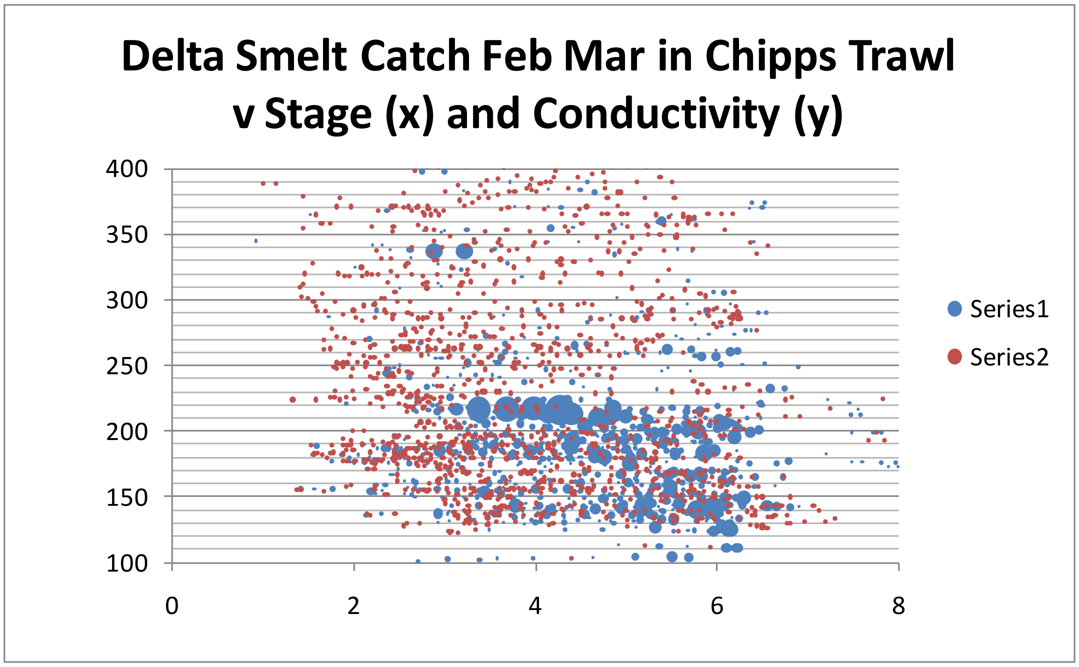

Figure U. Delta smelt catch (February-March) in Chipps Island v. Stage (x) and Conductivity (y)

Figure U is from the Chipps Island Trawl where stage is drawn from the Mallard Slough dataset rather than from DSM (data compiled by Dr. Ken Newman). Only the conductivity range 100 – 400 EC is shown. There are a few interesting things to observe in Figure U. There are frequent catches of smelt when conductivity is below about 250. Catches above 250 EC are rare. Why would this be? Are delta smelt absent from the Chipps area when flows are somewhat reduced such that salinity is in the 250 – 400 EC range? Or, are delta smelt invisible to the nets? Second, note that the stage at which delta smelt are caught becomes increasingly limited to the highest stages as conductivity falls (i.e., flow increases). Thus, as flows increase, the fraction of time that delta smelt are visible (caught in the surveys) decreases. Given that FMWT and other surveys are not always taken on the same phase of tide at different stations and in different years, the expected catch probably shifts dramatically and could impact estimated distributions and abundance. Unless this survey error is accounted for, it will be difficult to have confidence in the distribution and abundance data.

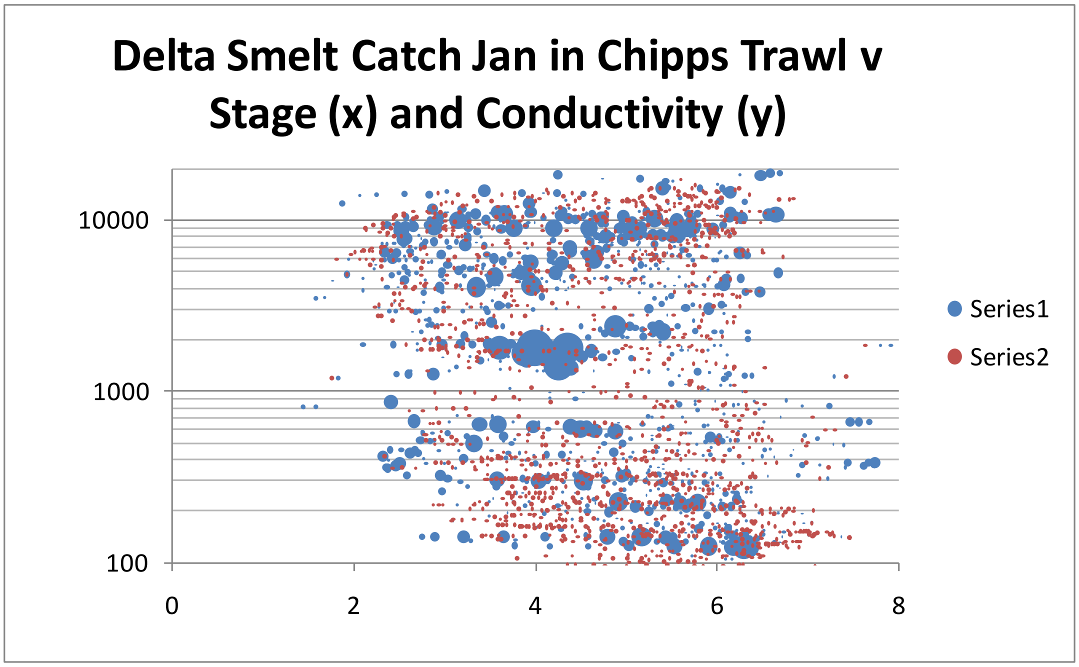

Figure V. Delta smelt catch in January in the Chipps Trawl v. Stage (x) and Conductivity (y)

Figure V is the same graph as Figure U but for January. Note the restriction in stage at the lowest salinities as in the previous figure. Note that from EC of about 2000 to 10000 delta smelt appear to be present at all stages of the tide. Could this phenomenon be responsible for the peak in delta smelt presence/absence around X2 identified by Feyrer? That is, could the supposed peak in smelt presence really be an artifact of surfing behavior?

There are other potential survey biases as well. There is evidence of a wind bias in Figure W. During months when turbidity could be quite variable due to changing winds, FMWT catch is heavily influenced by wind. The reason for the change in catch is likely due to wind generated turbidity which increases catch efficiency. Figure W.

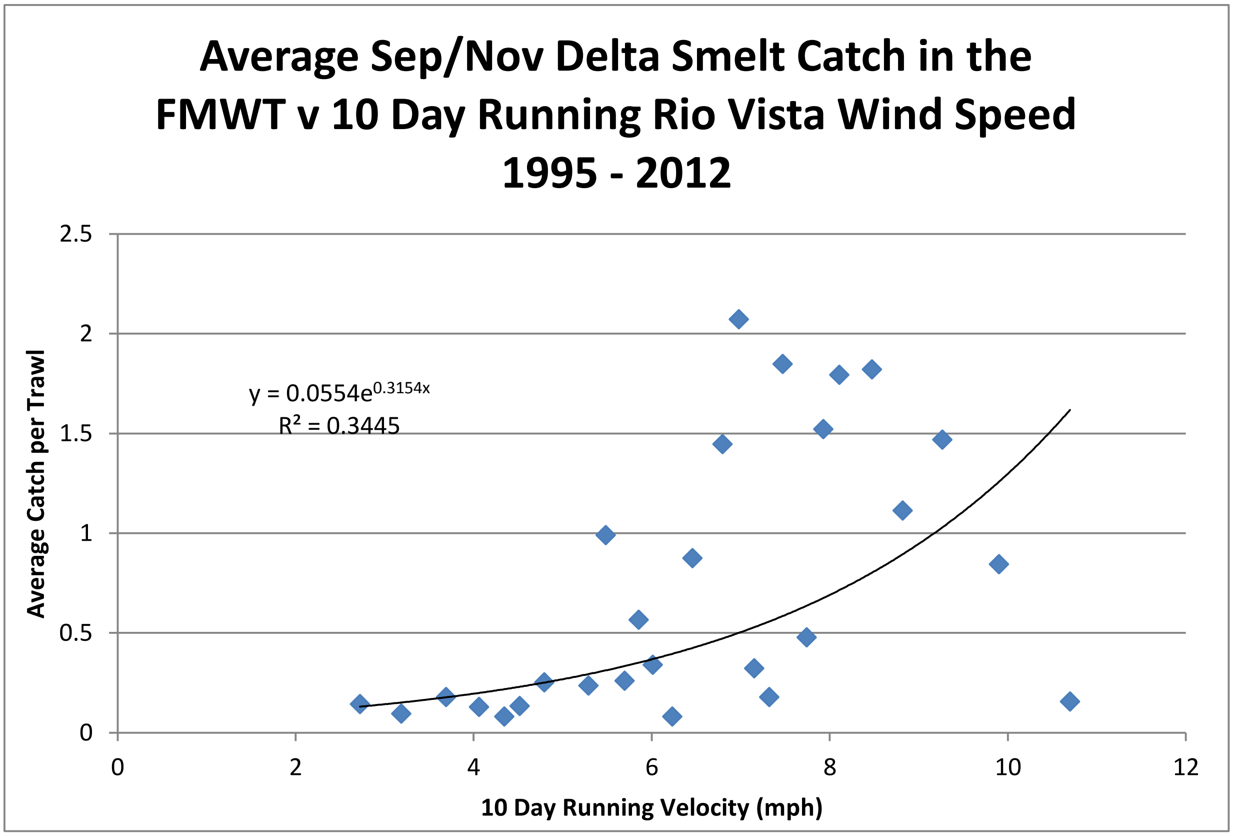

Figure W. Average September/November delta smelt catch CPUE in the FMWT v. 10 day running average Rio Vista wind speed.

There is evidence of a time of day bias in Figure X. A strong time of day signal is observable in this dataset. The majority of the normalized catch occurs before 9:00 am in the morning between the conductivity range of about 300-9000 EC. Figure X.

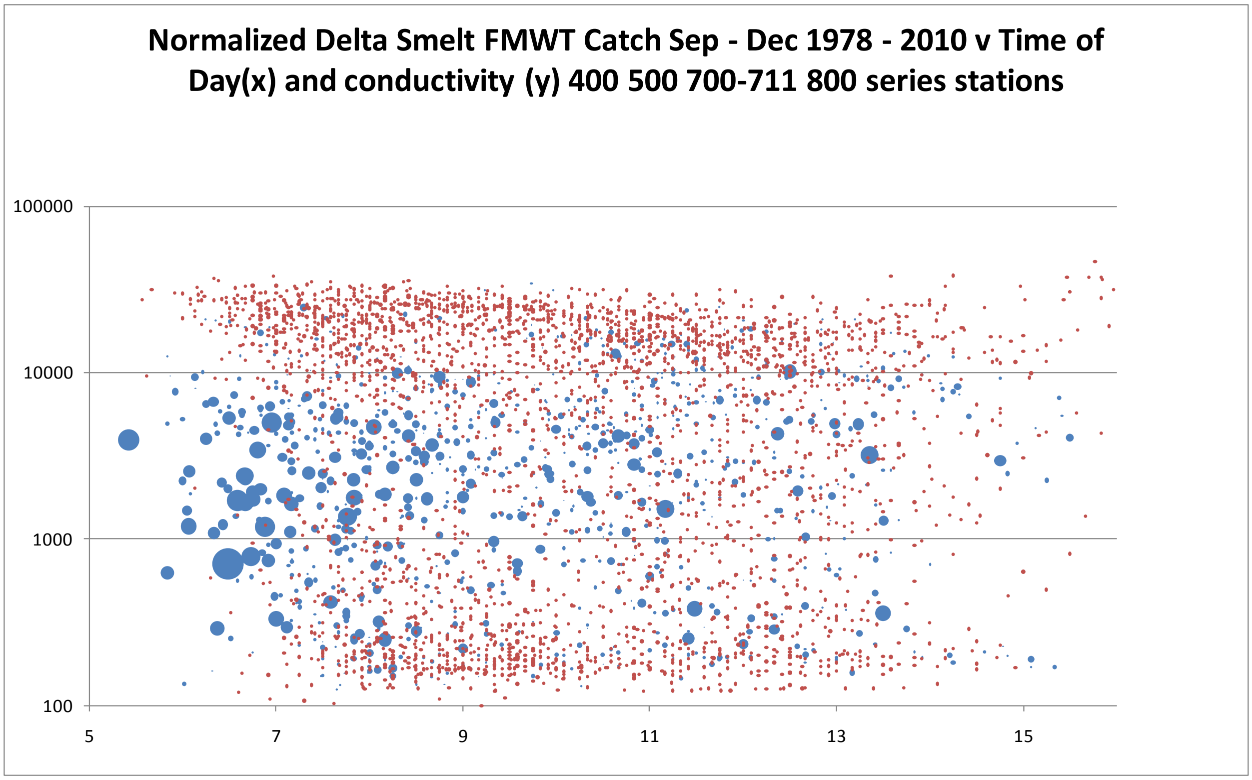

Figure X. Normalized delta smelt FMWT catch September-December 1978-2010 v. time of day(x) and conductivity (y), stations series 400, 500, 700-711, and 800.

Longitude and time of day (Figure Y) arguably give an even tighter fit to the FMWT data than does salinity and time of day. Most catch occurs before 9:00 between -121.7 and 121.8 longitude (*i.e*., near the confluence). Catch at this longitude is sparse after 9:00. There are few, if any, samples in the early morning for the longitude range -121.9 to -122.1, meaning that the survey might be missing the opportunity for large catches in much of Suisun Bay. Additional support for the hypothesis that delta smelt survey catch is subject to time of day survey error comes from the salvage dataset. Salvage is recorded day and night and so differences in smelt vulnerability to catch might be revealed by salvage patterns. Figure Z shows average salvage densities for juvenile delta smelt, May-July 1993-2013 versus time of day. Expected salvage densities vary by a factor of 5 from day to night. FMWT sampling is not standardized by time of day. Sampling began before dawn during the 1990s. The earliest sampling times shifted to several hours later in the day at the same time the POD occurred. This could account for at least some of the declines in the delta smelt FMWT index. Thus, it is no surprise that average time of day of the FMWT trawl is one of the most powerful correlates to the FMWT index since time began to be recorded in 1978. See Figure AA.

Many stations rarely, if ever, are sampled early in the day because those stations occur late in the sampling order for the boats. If sampling occurred earlier in the day at these stations, catch might be higher. Thus, the protocol for which stations are sampled first in the day and which are sampled later in the day could be influencing apparent distributions of delta smelt. For example, the stations near the bottom of Sherman Island are generally sampled early in the day because they are near to where the trawling boat is docked.

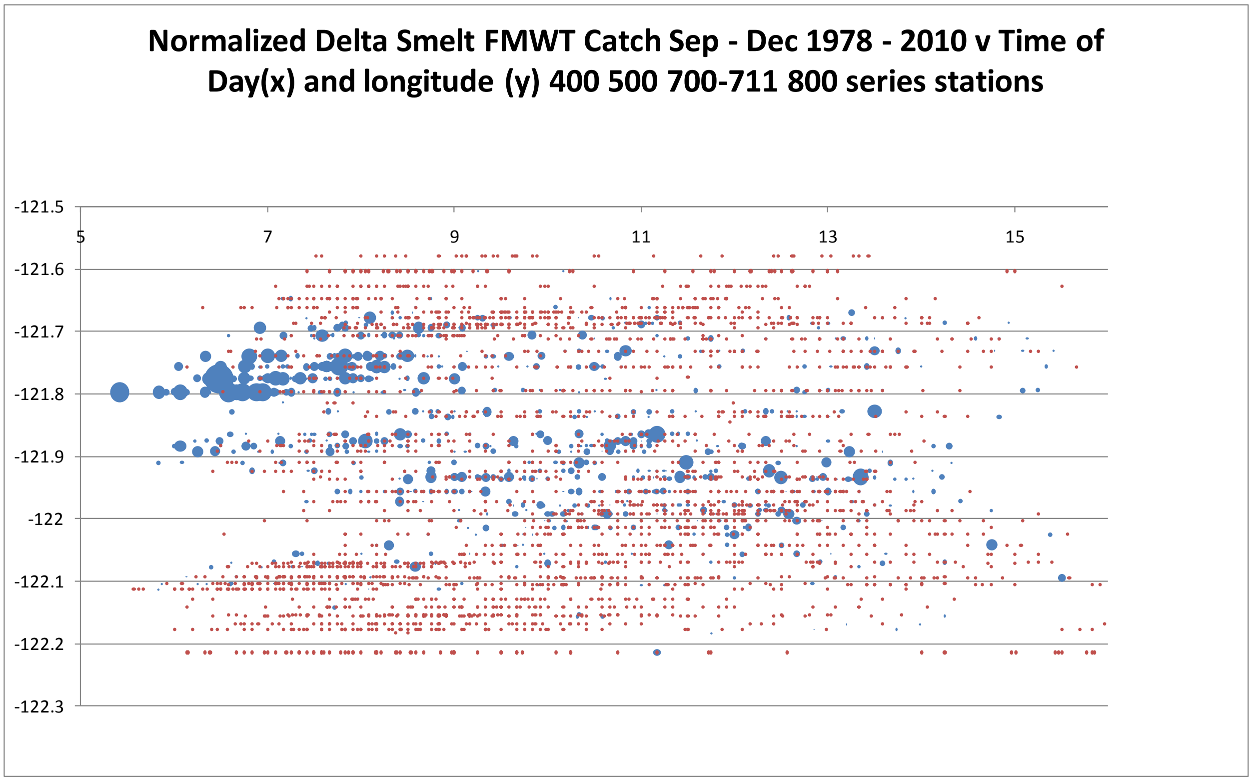

Figure Y. Normalized delta smelt FMWT catch, September through December (1978-2010) v. Time of day(x) and longitude (y), stations series 400, 500, 700-711 and 800.

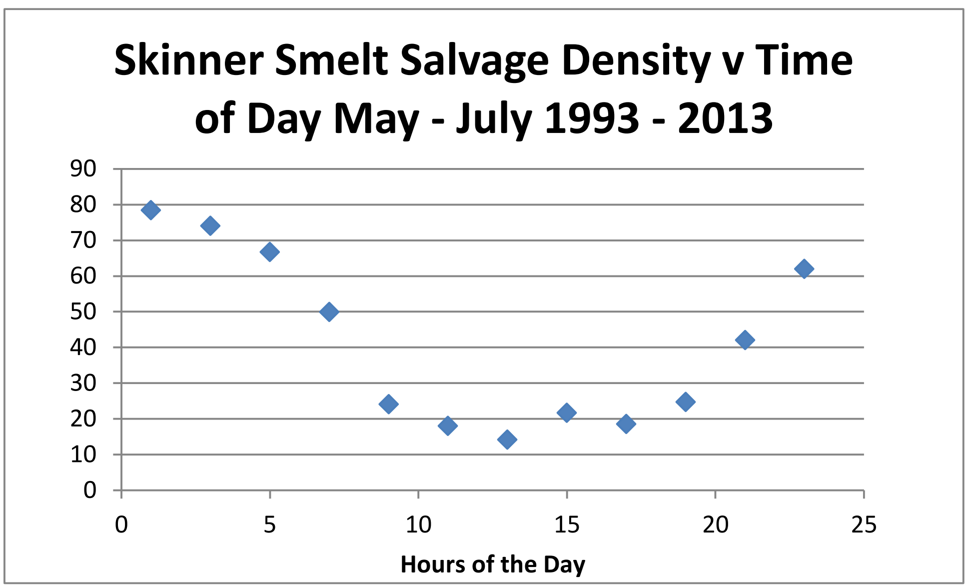

Figure Z. Average delta smelt salvage density at Skinner computed as 1,000,000*salvage/test/pumping rate May-July 1993-2013.

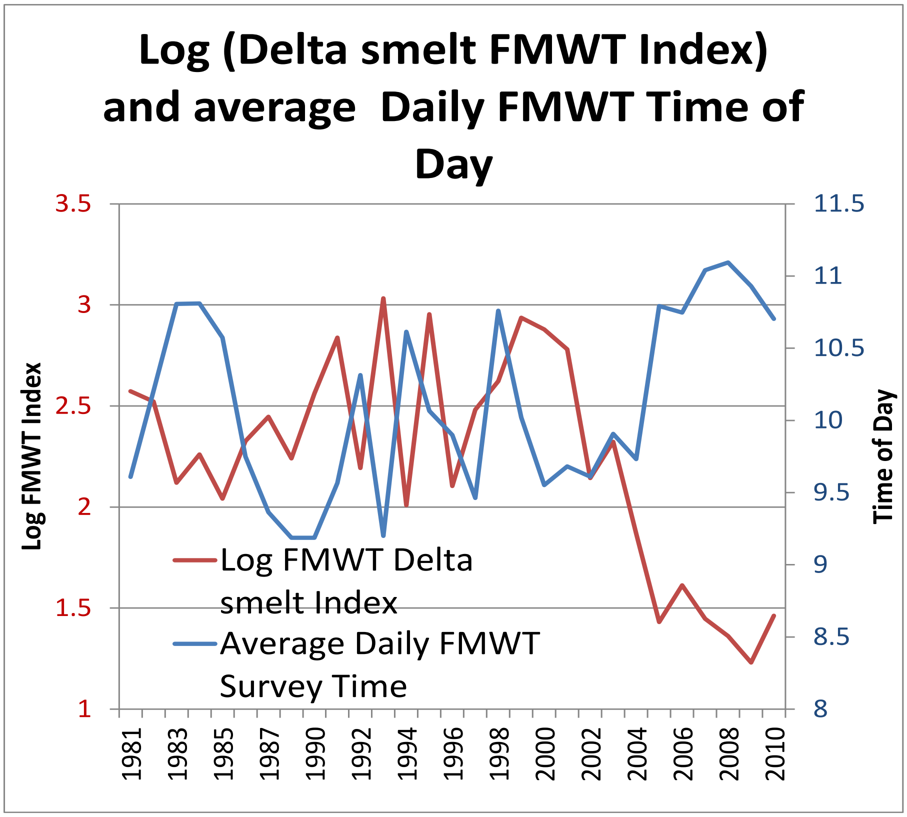

Figure AA. Log delta smelt FMWT Index v. average daily time of day in the FMWT.

There is also evidence of a geographic bias related to water depth in the FMWT. See Figure T. The FMWT surveys sample heavily in the deep water channels between 25 and 40 feet deep. There are very few measurements in water shallower than 25 feet deep. However, it is apparent from the data that there could be substantial catch at shallower depths. Therefore, the FMWT surveys give us little information about what is happening in water below 25 feet deep, which is an area that covers the majority of Suisun Bay, Grizzly Bay and Honker Bay.

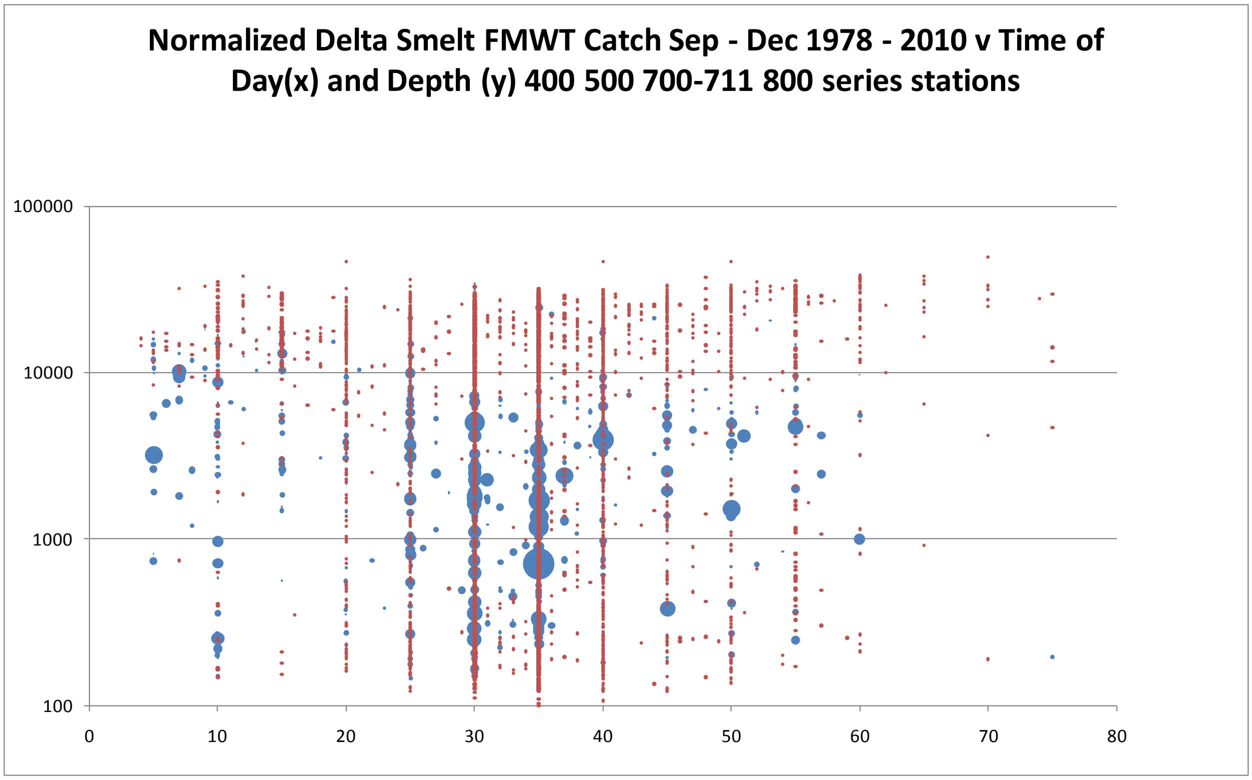

Figure BB. Normalized delta smelt FMWT catch, September-December (1978-2010) v. Time of Day (x) and depth (y), stations series 400, 500, 700-711, and 800.

<u>Role of Nutrients</u>:

The Report would be significantly improved by additional discussion and analysis of the role of nutrients in SFE structure and function as well as the differences in nutrients during the four years analyzed in this report. The SWC would be pleased to provide additional information to inform this discussion and attach a technical memorandum, "Nutrient Science Summary" as a start.

<u>Role of Contaminants:</u>

The discussion of contaminants could also be improved with additional discussion and analysis. For example, on MAST Report, p. 38, line 840, it should also state that higher water temperatures can also affect fish vulnerability to disease and contaminants. On MAST Report, p. 57, lines 1265-1266, it should acknowledge that while the concentrations of individual pesticides were lower than would be expected to cause acute mortality, the effect of pesticide mixtures is unknown. The studies cited all detected multiple pesticides in every sample analyzed. The interaction between pesticides should be acknowledged. It should also be acknowledged that contaminants can also affect predator-prey interactions by altering prey behavior (Brooks *et al*. 2009).[1] Finally, there is additional, newer information on pesticide occurrence and the effect of pesticide mixtures on the food web that can and should be included.

[1] Brooks, A.C., Gaskell, P.N., Maltby, L.L. 2009. Sublethal effects and predator-prey interactions: implications for ecological risk assessment. Environmental Toxicology and Chemistry, Vol. 28, No. 11. Pp. 2449-2457.