



DEPARTMENT OF ZOOLOGY

University of British Columbia, 6270 University Blvd., Vancouver, BC, Canada, V6T 1Z4
 Fax: 604-822-2416; email: etaylor@zoology.ubc.ca

September 16th, 2013

Subject: Independent Scientific Review of the 2013 IEP Management, Analysis, and Synthesis Team (MAST) report titled “An updated conceptual model for delta smelt: our evolving understanding of an estuarine fish.”

Independent reviewer:

Dr. Eric B. Taylor
 Professor of Zoology
 and Director, Beaty Biodiversity Museum
 University of British Columbia
 Vancouver, BC, Canada, V6T 1Z4
 etaylor@zoology.ubc.ca
 604-822-9152

General comments:

I reviewed this report as if I was the granting agency and thus looking to see how effective the time and money expended has been. I was less interested in “publishable results” at this stage (i.e., some analyses would be preliminary), but more looking for some clear accomplishments in terms of informative data and synthesis. Overall, I acknowledge the vast amount of work that has been done, the complexity of the issues at hand, and the difficulties of writing by committee. In summary, however, I was a bit disappointed because: (i) this is not a complete draft report, (ii) the hypotheses tend to be extremely simplistic and vague, (iii) some of the analyses are rudimentary at best, and (iv) the organization seems a bit confused in places. Perhaps I was expecting too much, but it reads mostly like a review and data compilation that might have been able to have been written several years ago rather than a clear articulation of testable hypotheses (based on existing data and conceptual models), a testing of predictions of these hypotheses, and a synthesis of the results (even if rudimentary) of new data which is then used to refine an existing conceptual model into an updated conceptual model.

Overall, the MAST review represents an excellent start on a summary of the “state of the SF estuary and its fishes”, particularly the delta smelt, the potential causal relationships between a number of perturbations to the estuary and responses of the fish synthesized in a revised conceptual model, and proposes hypotheses based on the updated conceptual model to help try and guide responsible adaptive management of the estuary and its biodiversity. Even disregarding the obvious management focus of the document, the text represents a valuable summation of the biology of delta smelt and the estuary in general. I would also add that when one considers: (i) the intricate biology of a hitherto little studied fish, (ii) the multitude of remaining mysteries of the species (they have never been observed spawning in nature for instance, nobody has generated a plausible population size estimate), (iii) the vastness of the

estuary (which themselves are a complex mix of fresh and marine waters), (iv) myriad stressors on the environment, and (v) the multitude of interests and agencies involved in the issue of delta smelt as a whole, it is impressive indeed that such a document can be pulled together as a reasonable roadmap to further our understanding of how the system may be managed to sustain biodiversity under such challenging environmental conditions.

All that being said and after reading about the scope and purpose of the report in the “Scope of Work to Independent Reviewers”, it is slightly disappointing that the report is not better developed from a hypothesis testing perspective (see comments below on the quality of the hypotheses) and that a few important elements of the draft report were missing. First, there was no executive summary (ES) which would have been a valuable addition to help bring out the essential messages of the review in a succinct matter. The strengths of the existing draft are, in some ways, also its greatest weaknesses. It is vast and complex and demands a succinct summary of the issues and potential ways forward. Without a succinct summary it is very easy to get lost in the details of the full report. An ES will obviously be in the final version, but an incomplete draft report is, well, incomplete. Similarly and more seriously is the lack of chapter 6 which would contain more information on year 2012 results and how they would impact adaptive management options. In addition, this chapter is supposed to describe approaches to quantitative population modelling of smelt demography and population responses and provide ideas on key indicators of smelt demography and measures of success of management actions. These missing elements of the report are critical to the central issue that the report is concerned with, or at least the “meat” of how the issue can be managed and monitoring success of actions. To not have them in the report makes much of the existing report seem rather preliminary and descriptive. In sum, what information is in the report (and there is lots!) is great, but the lack of certain critical elements makes it seem a tad preliminary. Finally, I did not fully understand the “Notice to Reviewers” about the lack of chapter 6 (or the ES earlier), i.e., the rationale for its absence was not really clear. If chapter 6 is still under development, why was the draft report issued? [same goes for “rationale” for lack of ES, seems weak – “waiting for review comments”]

The following are responses to the “questions provided to help reviewers formulate their comments”.

Q1: Are objectives of the report clearly described? Fully addressed? Do the authors go beyond these objectives/questions?

The MAST review contains a series of specific objectives listed on lines 432-435 and more general ones on lines 439-451. These are clear and easily understood. I do believe, however, that the various questions and goals require a slightly broader framework which I know the authors have, but they do not articulate well here. Obviously the broadest goal is to develop a better understanding of delta smelt biology and response to water conditions (and all the factors involved therein) and how best to “manage the system” to optimize the competing demands of smelt and humans for water – their shared resource. The conceptual model and all the data and analyses that go into it have little meaning in the absence of this overarching objective. I believe that the authors should be more explicit in stating this as the overarching objective and one that FLaSH and the MAST components and their interactions contribute to. After this, then state the specific objectives of this report. This will make the significance and context of the MAST report more obvious and give it greater impact. In addition, a more general statement(s) on goals will make a better match between the title of the report (nice and general) and the objectives on lines 432-435 in particular (which are very specific, suggesting understanding responses in single years which have, by definition, no generality).

That being said, the rationale (as an “organizing framework”) for the updated conceptual model made perfect sense to me and pointed out the clear advantages and critical nature of generating a biological process-based and realistic conceptual model. It is central to a rational plan for gaining a better understanding of delta smelt demography and its responses to water conditions and management actions and adaptive adjustment of such actions (in particular FLaSH-related ones).

Given points under “General Comments” above about the lack of an ES and Chapter 6, the objectives of the report are clearly not “fully addressed” (see above for significance of these absences).

I feel that the authors stuck closely to the objectives of the report and did not include material that went beyond the specific questions asked. The report is quite focussed and, beyond making some broader objectives a bit more explicit, I feel that this focus is appropriate.

I do feel, however, that many of the hypotheses are extremely simplistic and vague and no alternative hypotheses are offered (other than the obvious and unstated null hypotheses). For instance, for hypothesis one (“Hydrology and water exports...”) is very vague. What specific aspects/directions of these factors increase/decrease entrainment risk? What viable, if any, alternative hypotheses might be associated with increased/decreased entrainment risk? Water temperature? Interaction between any of these factors and fish behaviour? The general results are expressed as high/low water and high/low entrainment so why are the hypotheses not expressed as such so as to be more directly evaluated by the results as stated? Directional hypotheses would allow for more specific predictions to be made.

From the text following Hypothesis 1, it would be much better to evaluate explicit predictions from directional hypotheses using the subsequent narrative e.g., “If X is true, then we would expect Y, and Z in the data that we do have”. Right now the hypothesis 1 is so vague that it is impossible to see how the text presented is testing/supporting/rejecting any plausible processes despite what the authors “conclude” on line 1936. It is all just too descriptive.

The same goes for hypothesis 2 (“Hydrology interacting with turbidity...”). Too vague and no directionality. Enough is stated in the rationale section to state directional hypotheses and alternatives.

H1: increased flow and turbidity decreases predator effectiveness (cite plausible studies from other systems) and decreases predation-related mortality of delta smelt. These would also help in planning possible experimental studies on turbidity and behaviour done in other systems (i.e., one could test for fewer smelt in diet and perhaps reduced condition factor of predators under such conditions).

The point here is that the data to test these hypothesis may not (as appears) be available, but more specific hypotheses will lead to a better understanding of what data are needed to test more CM-relevant and meaningful, process-based hypotheses.

Hypothesis 3: *Predator distribution affects predation risk of adult delta smelt.* As another example of so general a hypothesis that it is hard to imagine when it would never be supported. For instance, under what conditions would predator distribution NOT influence predation risk???

I could go on to each and every hypothesis, but in general, the hypotheses are too vague and should be made directional where possible (based on existing assumptions, info) so as to generate actual testable predictions to help guide the research and data collections needs. For example, a testable prediction stemming from H1 above is:

Prediction: stomach contents of major predators show reduced smelt in diet under high flow, high turbid conditions.

Lines 625-628 need a re-think given the comments above. The hypotheses stated may be clear, but they are not specific enough to lead to testable predictions or provide a meaningful way forward.

Q2: Are conclusions and recommendations supported by evidence and analyses?

While the report is indeed an impressive compilation of facts and results from various studies, this is a difficult question to evaluate because the report does not really provide succinct conclusions nor recommendations (see comment above about lack of executive summary which would have forced the authors to offer succinct points). As many of the hypotheses are so vague, it is hard to support, at least as written in the text, many of the “conclusions” stated at the end of each “Hypothesis” section (see above section about making explicit predictions). The “Key Points” of the proto-Chapter 6 could be viewed as summarizing some level of conclusions, but again, they are so general as to be of limited informative value. In addition, they are so general that I think a literature survey of other systems (i.e., collecting no new data for Delta Smelt) would have generated the same conclusions. Anyone even remotely familiar with biological systems as complex as the SF estuary would have likely concluded “Key Points” 1 -3, and 5 without having to collect any data. Key point 4 is the only one that points to a specific relationship involving data collected to assess some relationship.

I would have preferred some conclusive statements in this section that provide answers to the specific questions posed on lines 432-435. Specific answers (as conclusions) do not seem to have been provided or at least they are not obvious to me.

Q3: Are the data and analyses handled competently and appropriately?

The general approach outlined on line 678 as a comparative one is reasonable and appropriate given the lack of an ability to (yet) perform manipulative, controlled experiments. Most of the “analyses”, however, do not permit statistically supported inferences, but are quite descriptive instead. Some of this is owing to a low number of samples (years) and thus replication. This is fine for describing trends in habitat features within the delta (temperature, low salinity zone, turbidity) and that part is done well. When opportunities arise to test actual relationships, however, there seems to be a reticence to engage in analyses. For instance, when discussing Fig 25, it is stated that striped bass abundance does not appear to be related to smelt survival (line 1225). Why was even a simple correlation analysis not performed? Even if it does not “appear” to be significant, this is open to visual interpretation, especially when both scales are logged so why not report a correlation coefficient and significance level? The caption states that a “correlation” is shown, but in fact no correlation coefficient is reported. Why are similar relationships between invasive fishes (bass and bluegill) not assessed for significance here also (using data in figs 22, 25). Citations are listed, but it would seem the current data could also be used to test these relationships?

Another example is the analysis of diet changes in smelt over years and salinities. These are represented at stacked bar charts but it is exceptionally difficult to see if there are any trends or patterns that emerge. Calculating things like pairwise diet overlap (Schoener’s index for example) across salinity conditions or years would provide a succinct way to see if diet shifts have occurred. Isn’t that what the authors are looking for??

Figure 39. How were these lines drawn? They are described as lowess splines as a method of curve fitting. Why was this very general approach taken versus more rigorous relationship testing?

Figure 43. Here are some actual statistical analyses. Stage specific relationships are stated as non-significant, but I think at least some example r^2 (not R^2) values should be reported and P values to show how far they might be from significance especially given the different scales for the different life stages (y-axis).

Figure 45. Please report the correlation coefficient. “ $P > 0.05$ ” is vague. Was it $P = 0.051$ or $P = 0.51$?? To me, it looks like there might be a relationship, but you have high variability and low power (only 8 degrees of freedom).

Again, I could go on. In summary, the various analyses (e.g., LOWESS) or lack of analyses in places need to be better justified. Also, more statistical rigour, consistent reporting of even non-significant statistics needs to be incorporated into this report. Right now is too vague and descriptive, too open to interpretation (i.e., uninformative)

Q4: Is the report’s organization effective?

Yes and no. I think the separation between figures and text is very distracting. I also think that there are far too many, usually not too informative, figures. Most should be in an appendix and only key figures that support a key result or conclusion or that are used to support a key recommendation should be included, and included in the body of the report.

The organization of the report starts out fine (chapter 1), but then I must admit that I found the rest of the organization not necessarily poor, just not what I would have expected. I would have expected a brief section on conceptual models (CM) in general, the pre-existing general model and then the data summary/analyses of the various years. After this summation of data and key findings, I would have thought would have followed a description of the updated CM based on what has been learned. Then what would naturally follow would be a description of what the key next steps are to test/refine the updated CM. This, I think, would be more consistent with what is stated as the overall goal of the report on lines 439-441. Here it seems to me that the CM would be updated after describing results from the data synthesis/analysis, not before as is in the current draft report. Then again, at line 443 it does seem that the updated CM is used to help organize the new data which seems inconsistent with lines 439-441. I think this section just needs some clarification and a clear rationale for the steps proposed.

Q5: Is the report objective? Is the tone impartial?

Yes, to a large degree. I think the tone is objective and impartial, but the lack of statistical rigour (even in just generating testable hypotheses and predictions) makes many of the “conclusions” subjective (lots of use of vague words like “appear to be” or “do not appear to be” owing to lack of statistical tests, unreported statistics, etc).

Q6: What other significant improvements, if any, might be made to the report?

1. More explicit comparison to FLaSH results. For instance, what are the specific implications of the results of the MAST report to management actions such as the fall flow alterations (if any)? Do the current results of the MAST report suggest, or not, that actions such as described in the FLaSH studies are warranted? Full answers are obviously likely beyond the scope of the MAST report, but some commentary would be appropriate. How, explicitly, do the current results extend the reach of FLaSH if at all? The FLaSH aspect of the whole issue is rarely, if at all, mentioned after about line 900. This seems a tad odd given the text around line 460-463.

2. The “Key Points” section, as mentioned above, is very weak and most people would have acknowledged these points at the very beginning of the whole Delta endeavour. This part seems very hastily-written and seems like the authors were up against an inflexible deadline. It should contain:

2.1 Key results on smelt biology, state of the environment, and their interactions, even if supported only by non-statistically supported associations (just make that clear).

2.2 How these results have supported, or overturned, our thinking on how the system works.

2.3 How, specifically, the previous CM has been informed by these results (even if only a guess at this point) and how, specifically, is the updated CM, new in this regard?

2.4 What the MAST data and synthesis indicate are the key remaining data gaps/analysis gaps that can be used to refine a presumably still imperfect CM.

3. The “Overall Next Steps” section is also weak/vague and this is a result of the weak “Key Results” section. I do not think I need to elaborate much other than it should be much more specific based on an updated section on “Key Results”. For instance, by now surely one could suggest (i.e., name) possible “indicator variables” of smelt and habitat status. I know the authors must have some ideas, why not be specific about them?? Same for quantitative modelling of smelt demography. That has been talked about for years, what progress has been made (others may be working on that, but surely the MAST results will provide some key inputs to that effort??)?

4. The report needs a “Overall conclusions” section. It just ends too abruptly with no clear idea of what the vision of the group is. Don’t just state the “Key Results” again. Rather, tell us in general what has been learned from the MAST process. What general progress has been made? What is the authors “gut feeling” on the amount and pace of progress that has been made? How will immediate efforts (e.g., completion of chapter 6) contribute to the over all goals? Some of this may be “opinion”, but as a reader, I am dying to know what the authors, experts and accomplished individuals all, actually think about this and I feel that this would be appropriate.

Line-specific comments:

Line 484-485. I do not understand this statement. The report outlines how the various POD fishes may interact. There are broad regime shift issues that affect the POD and its CM helps to elucidate them. If that is true, how can the two CMs not be relevant to one another? If the scale of one CM makes processes not functional at the other scale then this should be made explicit and perhaps an example cited. Interactions between processes at different scales seem to be being dismissed without any evidence.

Line 525. I do not think “complementary” is the right word as it implies to me that the two approaches are independent. Rather, as the authors state in the next line, the CM is integral to developing a QM.

Line 580. I think somewhere the authors should remind us that the delta smelt is essentially an annual fish. Year to year variation in environmental conditions can cause wild fluctuations in abundance.

Line 778. Perhaps explicit reference and definition of “aerobic scope” should be added in here. This is very topical with fishes, e.g., salmon and migration physiology under warming waters.

Line 795. Use “fecundity” or “number” instead of “abundance” of eggs.

Line 838. How about indirect effects on other organisms like predators or competitors??

Line 1027. Presumably there is some level of turbidity, however, when smelt visual acuity is impaired by turbidity or interferes with respiration?? There must be an optimal level of turbidity (the authors imply this at the beginning of the narrative, but at the end the impression is that any turbidity is good turbidity).

Line 1456: what about average size (better in terms of total calories), handling time, etc?
It is more complicated than just calories/gm

Line 1547 Add "(HAB)" to the section title.

Line 1574. Typo

Line 1602 Typo

Line 1671. How much of this is an artefact of the different sampling efficiencies of different life stages?

Line 1732. Should, briefly, say why loss of variation is a concern.

Line 1810. Change wording to "...spawn multiple times (per year?)."

Line 1992. Typo (Cache)

Line 2005. What about lack of control for density effects on growth?

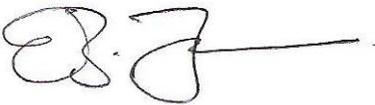
Line 2120, 2128. larval

Line 2126 Typo (..), line 2143. Lack of period between "data" and "The"

Line 2199. Typo, Line 2209. Typo

I am sure these are not the only typos, just ones I happened to see.

Yours truly,

A handwritten signature in black ink, appearing to be 'E. Taylor', with a long horizontal line extending to the right.

Eric Taylor
Professor of Zoology
Interim Director, Beaty Biodiversity Museum
Curator of Fishes, Beaty Biodiversity Museum