I find the report up to the high standard I expect from these authors. The objectives and approaches are very clearly laid out and well-addressed. I think some alternative hypotheses were not adequately addressed as I detail below, but in general I admire the effort to incorporate all viewpoints and the rationales for reaching various conclusions.

My comments are divided into Major Issues, Smaller Issues, and editorial nit-picking.

 Major Issues fall into two categories:

1. The treatment of weather, flow and hydrology effects on smelt
2. The confusing conflation of the 4 test-years with various parts of the entire data record.

Weather and flow

In the CM figures (8-12), weather is connected only to temperature and turbidity, with some lateral influence on hydrology. It is not clear in the CM figures, but the text refers to some impacts of temperature on predation rates, behavior of predators and prey, and bioenergetics of both.

The differential effects of temperature on different predators, and therefore different smelt life stages is not discussed – silversides and striped bass are predatory for a much broader temperature window and therefore portion of the year than are centrarchids. This is reflected in the predator species in each of the separate figures, but is worth calling out explicitly.

Not addressed in the figures or the report, weather importantly varies in terms of local insolation, precipitation, wind aas well as in the degree, duration, timing and frequency of peaks and valleys in delta inflows.

1. Insolation varies with time of day, season, cloud cover and occasionally air quality. For important processes, particularly feeding by smelt and their predators, it can interact with turbidity. Lehman attributed the failure of HABs in 2010 to a high volume of smoke in the Central Valley that reduced Microcystis growth rates. While the reduction in HAB may have one effect, if similar inter-annual variation in air quality affects plant growth generally, it would presumably have effects further up the food chain. 2013 may offer a chance to look for this process again.
2. Wind effects on turbidity are nicely described in lines 950-970, but the processes described are not included in any of the discussions on smelt processes. As described, wind resuspension of springtime-delivered sediments is a plausible mechanism for increasing turbidity in the LSZ, but it is not described. Nor is its possible significance to smelt discussed. Did 2006 differ from 2011 in this regard? Have there been decadal shifts in wind patterns that might affect local turbidity? I know exact data is limited, but broad-scale patterns should be readily available and discussed.
3. Precipitation within the delta has very different effects than can be captured in ‘year-type’ or delta outflow. Local precipitation, of course, is associated with local decreases in insolation but intensifies the effect on predators by interfering with lateral line and silhouettes identification by predators and prey. Local precipitation mobilizes seasonal pesticides as shown by Kuivila and doubtless has substantial local effects on turbidity. Therefore, when, where, and how much it rains may have substantial impacts on the health and survival of smelt.
4. Characteristics of Inflow variability have undergone intensive analysis in this estuary, at least in regard to first-flush and its effects on smelt migration, but little of that thinking is reflected in the report (lines 1775-1789). First flush studies have uncovered a great deal of complexity in the relationships amongst flow, temperature, and turbidity while documenting some serious interannual variability in smelt response, none of which is discussed here. There is also, for future work, a lot of scope for particle tracking modeling and 3D modeling of salinity distributions to help better characterize how different years are different for the various life stages of smelt. At the other extreme, line 2422 cites Lehman as saying that high flows discourage HAB blooms, which seems a very loose use of the idea of flow – blooms occur in the central and south delta where ‘flows’ are controlled mostly by tides and operations; I cannot understand how ‘high flows’ might affect Microcystis in that region.

Hydrology

Hydrology is generally addressed through year-types, which is probably adequate for the 4 test years because 2 are dry years, and dry years are generally alike, vs 2 wet years that were quite different but both were substantially different than the dry years.

In discussing year-types (line 1703), the claim is made that it reflects “unimpaired hydrological conditions.” This is wrong; wrong in a way that generates unnecessary noise in the analysis. For the two basins year-type is determined (differently for the two basins) from carryover storage, reservoir inflows in the snow season and in the rainy season. Then, since the Sac basin dominates supply, its classification is used for the entire Central Valley. This system of year-types was developed for water-supply use; as an example in 1992, the year-type is wet, but reservoirs were all empty and demand after a 6 year drought was exceptionally high. So reservoirs were recharged and deliveries were made but outflow stayed at the same level it had for the preceding drought years. “Unimpaired flow” on the other hand, is a calculation of what river flows would be in the absence of any control structures and bears no relationship to anything that either fish or suppliers would ever see. Thus, there is the most tenuous of connections between year-type and unimpaired flow and an even more tenuous connection between year-type, unimpaired flow, and anything likely to affect fish in the delta.

I have recently been using quartiles of outflow in the season of interest to characterize the ‘wetness’ of a given year/season. One could also use seasonally averaged X2, or some manipulation of total inflow. But in any case if we wish to parse the effects of ‘wetness’ on the different life stages of delta smelt, some measure of actual freshwater entering or leaving the delta during the season of interest is more likely to show a measurable response than ‘year-type’ as developed to facilitate annual water delivery forecasts.

Use of the 4 test-years vs other parts of the data record.

California climate, delta smelt biology and human impacts are so intertwined and confusing that I find the use of 4 ‘test-years’ to be an admirable way to focus attention. However, in this report the 4 years are included at the end and the reader must slog though some very confusing and dense methods of analysis to get to the simple view at the end. I think it should be the other way around. Set up the issue as you do, use the 4 years to show how the hypotheses can be examined in that confined dataset, and then look at the broader picture to see how well those 4 years can be extrapolated.

For example, figures 41 and 42 are the heart of how monitoring data are going to be used to address survival across life stages and for stock-recruitment discussions. Those figures are impenetrable. But if you graph just 2005 and 2006 vs 2010 and 2011 (and dump the baffling axis labels) you can tell a much more comprehensible story. In fact it invites expanding the story to 2007 and 2012 (i.e. dry conditions negate the influence of adult population size on larval recruitment. Then you could discuss each of the hypotheses in terms of the test years and expand it into the larger POD dataset. Finally, you could put the POD years in the context of what data we have from pre-clam and from pre-decline periods to tell a comprehensive concluding story – maybe density dependence before 1983, impacts of drought and clam, late 90s uptick due to wetness and regulatory changes and then POD.

Also, using just the 4 years would allow you to use 4 suites of side-by-side bars rather than stacked bars and so the differences in each ratio would stand out; as it is the differences in orange segments makes the purple segments seem more different because they are at different elevations.

This use of ratios to get at survival is critical and needs to be very clearly explained. A progressive approach would be more likely to communicate.

Smaller Issues (in order of occurrence)

Line 341 – so correlation is causation? This invites a simple blaming of non-natives and ignores the human impacts that facilitates native dominance

711-713 This sentence is silly – its like saying that all revenue streams are equal because they all contribute to me paying the rent. No, some are more important than others. Your job should be to d=identify which aspects are most important to the growth and survival of smelt.

832-835 I don’t buy this issue. If they are food limited, as you say repeatedly, than the hungrier ones can hardly be expected to spend more time foraging then their still hungry brethren. And I think most of their anti-predatory behavior is simply being in turbid water, which does not prevent them from feeding – they aren’t bugs hiding from daylight predators under rocks. Or for that matter salmon hiding in the littoral weeds. They are little transparent fish living in a turbid environment and that should be enough anti-predator activity for all of them.

868 Quantity and quality of the LSZ. I understand the quantity measure, but I think you have missed a bet on the ‘quality’ aspect of it. As I say above, not all aspects of habitat are equal, and so your job could be to define what makes the habitat in one year, in one season, or in one place quantitatively better than in another. This approach allows you to integrate all the habitat aspects into an N-dimensional space without getting lost in the details – what are the important dimensions? Salinity, turbidity, and ?? and which ones are essential and which ones add value?

1192. everything gets eaten, but ‘Predation is likely the dominant source of mortality grossly simplifies the factors at work.

2034 I need more description to believe that – and I don’t think I would really ever believe it – they are either buoyant or they aren’t – river flows are irrelevant in the delta and tidal flows seem unlikely to keep anything suspended very long.

2186. I don’t buy the correlation of abundance as a measure of predation impact or likelyhood. Silversides occur in large numbers and smelt occur in small numbers; silversides are unlikely to congregate in order to feast on smelt larvae, but their usual abundance is more than enough to have a major impact on whatever smelt are in the area.

2513 Microcystis in the LSZ is outside of its usual ‘habitat’ and is all low, so comparisons of such low numbers is not comparable to comparisons within its usual freshwater habitats where the numbers are more credible.

Editorial nit-picking

335. awkward

348 more than one species = fishes, one species are fish.

353 awkward ‘by as early as’

373 – what ‘other natural resources’?

380 “in return for regulatory agency approval’ awkward

428 ‘corresponding years’ unclear to any but the initiates

477 probability of transitioning is one – they really can’t do anything else. But the proportion making that transition could be any number up to 100%

586 ‘evalue’

773 et seq. I find myself annoyed by the numerous times this report refers to itself – ‘our intent is,’ ‘later in this chapter,’ the remainder of this paper,’ ‘as discussed below.’ Just say it and stop describing what you are saying.

777 extra word

788 missing word

789 misspelled word

825 “starting life after hatching’ – you know what I’m saying as I read that, yes?

840 ‘erffects’

861 – X2 is only for SFE.

862 extraneous word

906 “in the in the”

925 extra word

926 You ‘clearly’ making a joke here about turbidity!

933 Not ‘this approach,’ OUR approach, own it proudly.

999 missing word

1069 ‘Entrainment’ is the wrong word. Reducing pumping reduces ‘entrainment’ the screens do something else.

1152 – this happens a couple of places – ‘low salinity habitat becomes fresh’ that’s logically impossible.

1235. A figure or other backup for this statement might be good.

1240 not’ invaded’ it only did that once, but it has expanded and come to dominate parts of the delta.

1276 &1277 (e.g. ammonium) yes, we got that already.

1379 wrong tense

1561 missing word

1728 Bennnet

1781 – drop “It should be noted”

1786 – there’s that illogic again.

1803 drop ‘now’

1805 drop ‘being able’

1942 missing word

1947 why are pikeminnow (a littoral, lie-in-wait predator) a likely predator on pelagic fish?

1974 Drop ‘it is recognized that’

2017 unspecified pronoun, unless you mean life history theory

2037 “resorbed’ seems wrong for something that was never in them in the first place. Absorbed is more in keeping with my dictionary.

2067 and 2099 ‘width’ of the spawning window. I like metaphors as much as the next guy, but this is a duration not a width.

2077-2078 . awkward

2251 climate is not weather

2255-2257. I need more convincing description, or more clarity.

2290. “historically’ needs some years associated with it. Again, I think explicitly identifying some smelt relevant time periods would greatly clarify this discussion – pre-decline, post decline, post clam(?), late 1990s recovery, and POD are what I suggest. 2290 is clearly talking about pre-decline and needs to say so.

2534 typo