Response to reviewer comments "Four decades of water quality change in the upper San Francisco Estuary" Beck MW, Jabusch, TW, Trowbridge PR, Senn DB.

We thank the editor and reviewer for providing helpful comments on our manuscript. Responses to these comments are shown in italics.

Editor's comments:

Thank you for submitting your manuscript to Estuarine, Coastal and Shelf Science. We have completed the review of your manuscript. One reviewers recommended rejection, while the other was more lenient. If you can address the comments of Reviewer 1 adequately I will consider the MS for another round of reviews by the same reviewers. A summary is appended below. Particularly, the paper is not suitable for publication because ".....in its present form unless there is a substantial revision to better explain/identify how this research advances our knowledge on nutrient / phytoplankton dynamics in estuarine ecosystems". While revising the paper please consider the reviewers' comments carefully. We look forward to receiving your detailed response and your revised manuscript.

We appreciate the concern of reviewer 1 regarding significance of this work beyond the study system. However, we argue that this work will have good readership in ECSS for a number of reasons. San Francisco estuary, although a single system, is one of the largest and most studied estuaries in the world. Although we hate to play the 'large-prominent system' card as our burden of proof, the reality is that research conducted here has widespread implications for other systems and methods that are proven to work here are often adopted elsewhere. See for example, ... We also disagree that the application of WRTDS to tidal systems has been extensive in the literature, as compared to the original method developed for streams (Hirsch et al. 2010). In fact, the only other applications in tidal waters were conducted by myself and colleagues. Extension of WRTDS to SFE should be considered as a novel application given that the approach has only been applied to two other locations. We emphasis in the text some of the unique challenges and finding in applying WRTDS to SFE. Finally, we also want to mention that previous work specific to SFE has already been published in ECSS. Sutula et al. (2017) described an approach for estimating biostimulatory nutrient targets for SFE. The relevance of this work was of immediate regional importance and the broader importance was implicitly assumed.

That being said, we have revised the manuscript to help emphasize the relevance of this work outside of the region. Please see my specific responses to reviewer 1.

Reviewer 1:

The authors conducted a detailed data/trend analysis of nutrient variability in the upper San Francisco Estuary (SFE) based on observations collected during the last four decades. Overall, this is a very nicely written, organized and presented manuscript and I don't have major comments related to the content or analysis presented in the manuscript. However, I am very concerned that the paper seems to be a study of only local significance as it is

difficult to identify new methodologies or findings of widespread impact. The method implemented for the trend analyses, namely WRTDS, has been previously used in other investigations to describe decadal trends in rivers and tidal systems (see references in MS-Lines 92-96). So, unfortunately, it doesn't seems that the implementation of WRTDS for the SFE can be considered as a novel approach. The conclusions and analyses presented from the use of WRTDS seem to be supported by the data (e.g. DIN changes at P8 following the implementation of the WWTP), but again, it is difficult to identify new causal relationships or new information that can be used to better understand nutrient dynamics in other systems around the world.

Because of the limitations identified above, and having in mind that the journal discourages the submission of research of mainly local significance, I believe the paper should be declined in its present form unless there is a substantial revision to better explain/identify how this research advances our knowledge on nutrient / phytoplankton dynamics in estuarine ecosystems.

Minor comments

Check figure 1. Seems disorganized. Map of California in wrong place and not at scale.

Figure 1 was simplified and the California map was given its own inset.

Line 270 - 271. The figures don't seem to support this statement. If the colors are correct, then I see NH4 as the predominant form of Nitrogen in most stations.

Reviewer 2:

This is a very well written article on an important subject. It makes a significant contribution to the literature with regard to analysis of nutrient loading to an estuary. This is a very well studied estuary already, and the results of the study are not at all unexpected, but it is an excellent application of new analysis methodology for estuaries subject to temporal, seasonal, and hydrologically influenced changes. There is not much that I could find in the way of suggested improvements. I would recommend it for publication with minor revisions as follows.

p. 7, line 130 Seasonally, inflow from the watershed ...

This a long sentence with three clauses that don't quite follow from one to the next, in my opinion. I think the manuscript would read better by splitting the sentence into two.

p. 10, line 186 - I think readers would appreciate another sentence or two on how the weighting within the time window is accomplished. I assume it is with some sort of exponentially decaying time function, but there isn't any description provided. A short explanation is all that's needed.

p.10, line 195 - This is a follow-up to the previous comment. Adding a mention of the time constant in the description of the method will help setup the presentation of the times used for averaging. I think you should add some information here on how these time windows were chosen.

Figure 2. More contrast in colors (perhaps blue and gray) would make it easier to distinguish ammonia and nitrate in the time history. As is, it is somewhat difficult to see

p.18, line In the text, I believe in the introduction, you mention one invading asian clam (potamocorbula), but the second clam genera (corbicula) isn't mentioned until the results and in figure and figure caption. Is corbicula a native clam that was displaced? It is not clear from the text what we are to make of the decrease in abundance of corbicula. A few explanatory sentences in the introduction and/or the results sections are needed.