Marcus and James: I have reviewed your manuscript “Adaptation of a weighted regression approach to evaluating water quality trends in an estuary.” I think this is a great contribution to description of water quality change. I’m so pleased that you were able to see a good way of extending my approach from the river environment to the estuary environment. I’m anxious to see your work get published and to see this type of work get wide-spread notice and use. I wonder if the Journal of the American Water Resources Association is the right place to publish it, because I don’t think the estuarine research community will likely see it there. Something more in the mainstream of estuarine research publication may have a higher impact.

I had a few concerns. I had particularly difficulty with Table 4 (I really couldn’t understand what the numbers in the table actually represent) and hence I couldn’t really understand the interpretations you made. I also have concerns that Figure 8 may not be a stable and reliable method of understanding trends in relationships. I’ve made a suggestion of another way to achieve that end.

Below are a list of more detailed comments. Please feel free to ask me if you have any concerns or if you found my suggestions obsure or unworkable.. Thank you for asking me to review this excellent paper.

Sincerely yours, Robert M. Hirsch, Research Hydrologist, USGS (rhirsch@usgs.gov)

Line 256, you seem to be focused on a particular year-to-year change of 48.5% but couldn’t some of that be an effect of salinity or some other factor. It seems to me that any comparison of particular years is really contrary to what you are trying to do in this paper and you should leave it to the WRTDS results to show where the steep change takes place and that it isn’t right at the same time as the treatment took place.

Fig 3. I don’t understand the dashed line in the lower left panel of the figure. Also, I think most of the dots in the upper panel are ones with zero weight. As such, you might want to reduce them to just a tiny point. It is hard to grasp the transition from zero weight up to modest weight to a high weight.

Figures 4 and 7, there is so much jaggedness due to the seasonality that the main patterns don’t show up very well. What would it be like to pick out the most critical month of the year and just show these figures for that month? You could put the other months into figures in supplemental material. I just think that these are rather hard to interpret because of the strong seasonal variation.

Sentence on lines 304-306. The wording seems odd. Do you mean that the mean shows little trend but the range (or variability) appears to be decreasing (90th percentile decreases and 10th percentile increases).

Table 4. I don’t think I understand it. Are these numbers chl-a levels or are they a coefficient of variation (in %). Whatever they are, how were they computed? The text (on lines 318 – 320) and the table are obscure to me. I don’t understand the idea of a CV of a model. Is it the standard error divided by the predicted value? This is usually not considered a CV, but maybe something like a standard error in percent. Lines 325 through 330 make it appear that the table includes both chl-a levels and CVs. Again, I’m confused here.

Paragraph starting at line 400. I’m having some trouble with the idea that since the inputs were dominantly point source in the early years, that salinity would have little influence on chl-a concentration. Indeed, it would have little influence on the nutrient inputs, but high water inflows would tend to dilute the nutrients and that could result in lower chl-a. You state: “For example, the relationship of salinity with chlorophyll for Hillsborough Bay during earlier periods indicated no trend as expected, whereas the opposite was true for later periods.” Where is this shown in the results?

Lines 509 – 511. I don’t understand this sentence. It sounds almost circular.

The development of Figure 8 and the explanation of this information is troubling to me. I don’t use the slope coefficients themselves because I think there is too much of a risk of colinearity with the other coefficients. I am not at all sure that the results shown on figure 8 are really meaningful. Why would the curves for Hillsboro Bay go from flat, to downward, to upward, to downward? I think that there is a more reliable way to describe the changing dynamics of the system. I would suggest the following: Pick a particular time of year (perhaps a date that is usually around the chlorophyll peak) and make predictions of chlorophyll-a for different salinity levels. You can produce several such curves on one graph: say one for 1977, one for 1985, one for 1995, and one for 2006. This will give you a more meaningful idea of how chlorophyll changes as a function of salinity and how that relationship changes over time. In my implementation of WRTDS in the EGRET software that is the plotConcQSmooth graphic. You would just be using salinity in place of Q in such graphs.

An overall comment: I think the paper could benefit from some tables of results. For example, for each estuary segment, and for a selected critical time of year, you could show the expected chl-a level changes from 1980 to 2010 – once for high salinity, moderate salinity, and low salinity. In addition, you could provide an overall summary of changes in the salinity normalized chl-a levels, by season, as the most straightforward way to describe the overall change in the system. I think this would provide a nice way of summarizing the extent of the improvement.