



Interactive comment on “Hydro-climatological non-stationarity shifts patterns of nutrient delivery to an estuarine system” by A. L. Ruibal-Conti et al.

Anonymous Referee #1

Received and published: 17 October 2013

I found the paper to be poorly organized and as such it was very difficult to follow. There are many kinds of trends and relationships that are discussed (loads versus runoff, loads versus time, concentrations versus time, flow-adjusted concentrations versus time, water quality variables versus land use or population variables, ratios of dissolved to total concentrations versus flow). These topics seem to come and go throughout the paper and the overall approach never became clear.

The authors seem to be unaware of some approaches to water quality data analysis that could have been very useful in gaining understanding of the factors influencing the changing nutrient conditions in the watershed.

The end of section 1 states two objectives. The first is the consideration of the idea

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that the changes in nutrient conditions may be a result of changes in streamflow or land use or both. This is something that could be addressed in a very straightforward manner through the use of some form of regression analysis where water quality variables (such as concentrations or ratios of dissolved to total concentrations) are modeled as functions of streamflow and one or more land use variables. But, there seems to be no such joint analysis of this question. The second objective is to “test the hypothesis that dry years significantly differ from wet years in terms of nutrient export, nutrient partitioning. . .” It would come as a great shock if these things were not related to hydrologic conditions. The questions of interest are the nature of the relationship of concentration to streamflow conditions (current and recent flow conditions) and the nature of the relationship of nutrient partitioning to streamflow. I know of no system in the world where nutrient export is not positively correlated with streamflow. Exploring this is not new research.

The title of the paper and some sub-sections make note of the idea of hydrologic non-stationarity, but in fact it doesn't seem to be truly addressed. Indeed there have been large changes in average flow conditions over this period of record, but they never really address evidence that suggests that this is truly non-stationary behavior or an example of long-term persistence (looking at paleo records may shed some light on that). The question of interest is what might be the impact of a very protracted period of very low or very high flow versus a pattern that might be described more as random variations between high and low conditions. In other words, do long dry periods reset the behavior of the system or do they just influence the water quality of the moment.

I was disturbed to see (page 11044 lines 1-3) that low values of TP were simply deleted from the data set. Perhaps if there were only a few such values this might not be problematic but in general the idea of deleting censored values is well known to cause bias in statistical studies (see the text by Dennis Helsel, Statistics for Censored Environmental Data Using Minitab and R, Wiley Publishers, 2012).

The basic approach to computing nutrient loads is a very simplistic one (page 11044

section 2.3.2 lines 1-10). There are regression techniques that are widely recognized as being much more accurate because they account for the fundamental relationship between flow, season, and concentration. These include the LOADEST method (Runkel, Crawford, and Cohn, USGS Techniques and Methods 4, chapter A5) or WRTDS (Hirsch, Moyer and Archfield, 2010, Weighted Regressions on Time Discharge and Seasons (WRTDS) with an application to Chesapeake Bay River Inputs, Journal of the American Water Resources Association).

Figure 4 and related figures on mass fluxes would greatly benefit from being expressed in terms of yields (for example kg yr⁻¹ km⁻² and related to runoff in units like mm yr⁻¹ . The reader is left to ponder how much of the differences between loads are just a result of differences in watershed size and how much is about fundamental differences in watersheds.

On page 11051 lines 3 – 5 there is a statement that the proportion of DIP in wet years was about twice as high as in dry years. This is very counter-intuitive. Typically with greater discharge there is a greater ability to carry sediment and with that the associated suspended fraction of phosphorus. This odd result deserves some serious discussion.

The land-use changes described in 3.4 lines 13-29 are not very informative. For example, there is reference to a 100-fold increase in mining. It is impossible for the reader to understand what this might mean in practical terms. Did the watershed go from 0.0001% mining to 0.01% mining? Or, did it go from 0.2% mining to 20% mining? The former is a trivial change in terms of what it might mean for water quality. The latter is potentially a highly important change. The reader has no basis to evaluate because the baseline is never established.

Finally, it is difficult to understand the idea that a water quality variable may be related to the yearly population growth rate (page 11060 line 6). It is understandable to consider how DIN might be related to population, but why would it be related to population growth

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rate?

I would encourage the authors to consider a major rewrite and simplification of the paper to look explicitly at how concentrations of nutrients are related to climate (or flow) conditions and land use conditions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11035, 2013.

HESSD

10, C5657–C5660, 2013

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