

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

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The authors would like to thank the reviewer for the constructive comments and suggestions made regarding our manuscript. In response to the suggestions the authors will re-organise the manuscript and enrich the discussion of results to more clearly relate findings with the overall aim and to clarify the issues raised by the reviewers.

A more detailed response to each comment within the review is given below.

1.1_ 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

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dissolved to total concentrations versus flow). These topics seem to come and go throughout the paper and the overall approach never became clear.

We acknowledge the amount of information presented in the paper requires a stronger organization of the ideas to convey a clearer message to the reader. This concern will be addressed by restructuring the paper to better frame the problem and to highlight new insights gained from the analysis. In particular these changes aim to: 1) simplify the paper by placing less emphasis on the information that, though important to describe the context, is not strictly linked to the objectives of the paper. 2) Improvement of relationships to better describe the effect of climate and land use conditions on nutrient delivery. 3) provide more refined explanations to clearly link the results with the overall objective of the work ; and 4) Elimination of figures that present redundant information (e.g. land-use maps)

1.2_ 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.

The authors believe that the non-stationarity in climate has been well described by other authors cited in the manuscript, and further analyzing the long-term record would be beyond the paper's scope and compound the issues raised above. Non-stationarity is itself difficult to specifically define, however, this region of Australia has experienced a substantial drying trend over the past decades relative to the previous century.

We acknowledge that the way the term non-stationarity is used in the title may bring confusion to the reader as the comparison of nutrient delivery between periods of non-

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stationarity and stationarity is not the focus of the paper. We were merely trying to build the case that the drying trend experienced over the past decades leads to shift in the amount of nutrients exported, the type of nutrient exported and the N:P stoichiometry. The authors will alter the title of the paper to better reflect the specific focus of the study: "Effect of a drying climate trend on the export rate and ratio of total and dissolved nutrients".

1.3_The idea 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 reviewer has raised an interesting point regarding the use of regression analysis to express a joint analysis of climate and land use. In fact, this approach was considered; however, the main problem faced was to find information on land use that has changed over the time period considered since only 'snap-shots' were available. The only information we found over the whole period of analysis related to population. We also explored the use of the runoff coefficient as a proxy for land use change. Neither of these displayed any association with nutrient delivery. Without such a mechanistic connection we moved, then, into a descriptive and exploratory analysis of the effects of land-use. However, we agree with the importance of a quantitative analysis. With the current land use data available (1993 & 2006), we propose to instead investigate the change in the export coefficient of different types of land use. Assuming a linear change for the period 93-06, will allow us to interpolate the values for the rest of the years and analyse these new values in a new regression analysis.

1.4_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

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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 introduction of the paper acknowledges a vast number of studies that have indicated the positive correlation between streamflow and nutrient export and it was not our intention to focus on flow vs load, but to identify if changes in the export rate per unit area occur under variable hydrological conditions. This will be clarified in our revision. Further, while the positive correlation of nutrient export and streamflow is expected, to our knowledge, the effect of flow conditions on the ratio of nutrients (stoichiometry and partitioning) is an area to be explored. In fact, the data shows that there is no positive correlation with nutrient stoichiometry. The authors acknowledge that the objective should be restated in order to better highlight the significance of the research.

1.5_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).

TP values recorded as <0.4mg/l were deleted for the following reason: The measurable range of the analytical technique used to measure TP is 0.5-0.010mg/l. The value 0.4 falls within the top limit of the measurable range, as such, a value reported as <0.4mg/l could be any value between 0.399 and 0.010mg/l and could have been measured with analytical precision and accuracy (unless it falls below 0.010). This indicates that there was a problem in the laboratory analysis and consequently the value was considered to be faulty and was therefore eliminated.

1.6_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

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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).

We acknowledge that the paper could be greatly improved by a more accurate computing methodology and the authors will evaluate the results obtained by both techniques and the paper will be updated. Nonetheless, we highlight that whilst the accuracy of the load is very important, the paper is oriented to identify relative trends rather than in the quantification of load. For this reason, the authors focused more on minimising the errors generated from different nutrient sampling techniques over the years and they carefully paid attention to the fact that loads could be comparable among years.

1.7 Figure 4 and related figures on mass fluxes would greatly benefit from being expressed in terms of yields (for example $\text{kg yr}^{-1} \text{ km}^{-2}$ 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.

The authors acknowledge this concern and the paper will be updated accordingly.

1.8 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.

Some possible reasons behind this result were stated in the discussion (page 11057 lines 10-15, pag 11058 line 26-28, pag. 11059 lines 23-27). The authors acknowledge this should be further explored and covered. However, while the increase of DIP with

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flow is counter-intuitive, the generally understanding that is pointed out here is based on catchments where there is substantial sediment from the catchment and where the landscape has a much greater slope. The landscape in the Peel is very flat and the soils are very sandy. When the soils of the flat landscape become saturated, there is a tendency for sheet flow across the inundated sandy paddocks to dominate the flow path. Flooding occurs and flow in the drainage system is restricted and the flow channels become much wider rather than much deeper. The erosive power is not greatly increased and there is not much sediment to move even if it did. The saturation of the landscape results in greater wetting of usually water-repellent soils, more anaerobic conditions and greater contact times all contributing to greater solubilisation of P.

1.9 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.

Section 3.4 starts by mentioning that between 10 and 15% of each sub-catchment experienced land use changes. Figure 16 (pag 11089) shows the changes as a percentage of the total sub-catchment area.

1.10 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 rate?

The mechanisms behind this relationship will be further explored and discussed. However a possible explanation is that The Peel has previously had a relatively small population and the increase in population has more recently been in “green-fields” de-

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velopments where farming land and some remnant vegetation has been converted to residential use. Small developments are generally characterised by limited land disturbance but as the demand for more housing has increased, very large scale development has taken place with land disturbance, altered drainage and reforming of land being economically viable. The proportionally large increase in soil disturbance has resulted in mineralisation of nutrients from residues of predominantly permanent pastures that have built up nutrients and have not been disturbed for decades. Rather than the activities of people coming into the catchment being the dominant reason for increased inputs of nutrients (eg sewage or urban fertiliser use), it is our view that the relatively higher impact of soil disturbance has much greater effect. The soil reserves of nutrients are many times greater than the annual application rates of nutrients by the population.

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