

Interactive comment on “Impact of river discharge, upwelling and vertical mixing on the nutrient loading and productivity of the Canadian Beaufort Shelf” by J.-É. Tremblay et al.

Anonymous Referee #2

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This is very extensive analysis of a number of oceanographic variables and parameters collected as part of a major field program in the summer of 2009 on the continental shelf of the southeast Bering Sea, with particular attention given to the role of the Mackenzie River outflow. (This detail, when and where the study was done, is not in the Abstract, but should be).

Overall I have no problem with the approach and the results, nor with the authors' interpretation of those results; however, I am not familiar with this area of the world ocean, or with prior work done in this area, and therefore I cannot place their results into a proper context for critical assessment of their overall significance. I am assuming that this manuscript has been reviewed by others more familiar with the region.

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The papers reads a little like a data report, in that the main ideas to be presented do not stand out; instead the authors state that they have "assessed and compared" all these variables and parameters, and made "comparisons...to elucidate some of the processes taking place in the estuarine transition zone". e.g., Was this was a shotgun approach, or, an "expedition of discovery", in a region about which very little is known? If so, then it is fine. They just have to recast their opening paragraphs to reflect this. The way the paper is written, it is not made clear what the underlying scientific basis was. At least it is not made clear initially; it does become clearer when one reads deeper into the text, and I would strongly suggest the authors move into the front end of the paper their motivation, justification, hypotheses, and overall rationale for having done all this work. I understand that, as they point out in their introduction, knowledge of the importance (they use the word "impact") of rivers on the chemical and biological oceanography of the shelf "is rudimentary", but more than that statement is needed.

I did not really understand the significance of their N^* and P^* parameters. I can see how they computed it, of course, but where did the parameter "r" come from? How do they determine the fraction (?) of remineralization? And how is this parameter (N^* , P^*) useful? This section of their methods needs some text to explain it a little better.

I was unable to follow much of their results and discussion for the simple reason that I (and 10-20% of male population) cannot distinguish the colors they used in their tiny figures (the contour plots). These need to be re-done with red-green colorblindness considerations in mind (there are several websites that can help them (one of the better sites is that for the journal *Limnology and Oceanography*). Also, I found it difficult to follow their results and discussion as they kept referring to figures in the Supplement (Supplement A?). If these results are important to the paper (and, indeed, their being cited in paper itself would mean that they are), then just include them.

They discuss the surface distributions of nitrate, silicate, DON and DOP (on p. 16684), but do not show those data (contour plots are needed here).

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There were several missing references; four on page 16680 alone: Kirkwood 1992; Raimbault et al., 1990; Aminots and Kerouel 2007; Bergeron and Tremblay 2013). A cope editor, I assume, will check these for others that may be missing?

In summary, this is a very exhaustive study, the results of which should be published. I would suggest publication only after the authors attempt to reorganize it to explain to the reader the reason for having done all this work to begin with, what problems were being attacked by making all these measurements, and what scientific issues, problems or hypotheses are specifically addresses. These points are in there, they just have to be recast.

Interactive comment on Biogeosciences Discuss., 10, 16675, 2013.

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