

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.

J.-É. Tremblay et al.

jean-eric.tremblay@bio.ulaval.ca

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We thank the two anonymous referees for constructive comments that helped improve the manuscript. A point-by-point reply to their comments and a description of the changes made to the manuscript is included below.

C = reviewer comment; R = reply

Anonymous Referee #1

C. The authors present a detailed data set of the particulate and dissolved constituents of the Mackenzie River plume in the shallow region of Mackenzie Bay and Kugmallit Bay

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and the adjacent southeast Beaufort Sea during summer. This region is interesting but also complicated due the various source waters as the riverine discharge, the Pacific water inflow, etc., all with very different compositions of nutrients and organic matter. This makes a general interpretation difficult. The authors make a lot of calculations some based on assumptions. These calculations are complex and should be presented more clearly. R. See detailed answers below

C. They describe conservative and non-conservative behavior of individual compounds but the explanations are sometimes vague and not really supported by their data. For example, silicate, DON and DOC declined in a conservative manner from the river towards the open sea but there must be remineralization processes of DOM and utilization of the huge amount of silicate. R. Two explanations account for this – the constituents that show near-conservative behavior are those for which concentrations are very high relative to those showing non-conservative behavior. There must be some biological use of silicate, but since waters in the region are either P or N limited this silicate use must be small and is presumably lost in the “noise”. We also indicate that the phytoplankton communities dominating in the estuarine transition zone are comprised mostly of non-diatom groups with no requirement for silicate, which also explains why this nutrient seems to behave conservatively. For DON and DOC the concentrations are very high relative to the impact of the different processes affecting them (same argument as for silicate). In addition, Conservative behavior for the bulk does not imply conservative behavior of the different components – along the estuarine gradient, river DOC and DON may be partly replaced by fresh molecules produced by organisms even though the total quantities seem to behave conservatively.

C. The authors should avoid publishing their data in many small publications. They have already published quite a lot based on the MALINA project and will publish another one on 18O although these data are very important for the interpretation of the influence of sea ice meltwater and riverine water for this manuscript. R. Agreed, but there is no other MALINA manuscript focused on the distribution of inorganic and or-

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organic nutrient pools in relation to primary production and nitrogen uptake. So this manuscript does not repeat what has been published otherwise. Also the present paper and its appendix is already quite long and figure-intensive and we felt that providing a detailed treatment of the ^{18}O data would unduly lengthen the paper.

C. The manuscript is generally well-written but is sometimes not carefully worded (typos, missing references) and suffers from very long and complicated sentences. I recommend publication with minor to moderate revision. R. See answers to detailed comments below.

C. Minor comments: At the end of the introductions objectives or hypotheses are missing. The authors present just an outlook of the content of their manuscript. R. This information has now been added to the paper, with one preparatory sentence and explicit statements of the central working hypotheses and objectives. A section of the introduction now reads: "A comparison between riverine and oceanic nutrient sources (e.g. upwelling, vertical mixing and advection) considering quantity, stoichiometry and their importance for primary production relative to local recycling processes remains to be achieved at different spatial scales, which is the main objective of the present study. Our working hypotheses were that nutrient supply by the Mackenzie River makes a highly localized and small overall contribution to new primary production on the Canadian Beaufort Shelf and that this production is contingent on the mixing of P-deficient freshwater with N-deficient seawater."

Methods C. The determination of the organic matter is quite inaccurate. There are much better methods than the wet oxidation. CN analyzers for particulate and dissolved organic C and N are much more precise. R. We could have opted for the high-temperature catalytic combustion method had we been interested only in the TOC and TN pools. The main benefit of our method with regards to the specific objectives of the current paper is to allow the determination of the particulate or dissolved pools of all three elements (C, N and P) on the same sampling bottle – which makes for internally coherent estimations of pools and ratios. It should also be noted that a comparison

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of combustion and wet incubation methods for total dissolved nitrogen concluded that "none of the routinely used methods appears to be grossly inaccurate, thus, most routine TDN analyses being reported in the literature are apparently accurate" (Sharp et al. 2002; Mar Chem 78). Since the main emphasis of our paper is on N and P we find that our approach is robust and amply justified.

C. Calculation of dissolved organic matter by subtracting the particulate part from the total is also rather rough. This has to be considered for the interpretation of the data. R. Actually, this procedure is rather common and avoids filtration artifacts and biases leading to the leakage of dissolved organic matter into the filtrate.

C. How were the acidified samples stored? R. In the dark and refrigerated at 5°C . The information has been added to the main text.

C. Page 16680, line 24: are filters pre-combusted? R. All glass fiber filters are pre-combusted at 500°C during 4 hours. The information has been added to the main text.

C. Page 16682, line 23: It is ultra-pure water from a Milli-Q ion exchange unit and not distilled water. Distillation is a totally different method. R. Modified.

C. Page 16682, line 27: How were real-time measurements performed? R. Tracer additions were adjusted according to nutrient measurements made on a previous cast at the same location or at a nearby station. This has been clarified in the main text.

C. There is no need to introduce the following discussion at the end of the first paragraph (page 16688). R. We removed the problematic sentence.

C. The calculations or better estimates of the new production are extremely difficult to follow. The authors are asked to improve this perhaps by adding a table. R. We added a new table (Table 2) and modified the text to make the reasoning and progression easier to follow. However the calculations of new production need to be explained step-by-step, which is hard to express in a Table.

C. Page 16690, line 3ff: It is hard to believe that sea ice meltwater has such a strong

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dilution effect although it is well known that sea ice has very low nutrient concentrations. This is a surface sample so the dilution effect would be maximum. R. A prior study of sea ice in the general area (Pineault et al. 2013, JGR 118) shows that silicate and nitrate are nearly exhausted in bottom sea-ice in non-upwelling areas. This implies that the ice would release little nutrients and dilute those present in underlying waters. A diatom bloom developing in the melt-water lens could therefore produce the signature we observed.

C. Sometimes silicic acid is used but also silicate. I propose to use generally silicate. R. We have modified the text and now use silicate throughout.

C. There is a mixture of the unit for nutrients and organic compounds using M or mole L⁻¹, etc. Both are correct but it is better to be consistent. R. For concision, we have modified the text to include only M where reporting concentrations. However vertically or spatially integrated values resulting from calculations are provided in mol units (i.e. not mol L⁻¹) for clarity.

C. Missing in References: R. The following references have been added.

Bergeron and Tremblay, 2013 Granger et al., 2011 Raimbault et al. (1990): Aminot and Kerouel (2007):

C. There are several typos (some are listed here): C. Page 16677, line 16: Correct Le Fouest R. Modified C. Page 16681, line 25: study not with capital R. Modified C. Page 16684, line 16: delete psu and wherever it is used as unit in the text (it occurs several times) R. Modified C. Page 16684, line 8: nitrate not with capital R. Modified C. Page 16687, line 7: ..in stark contrast the shark drop of TPP R. Modified C. Page 16690, line 11: and N recycling R. Modified

Figures C. Remove psu from the salinity figures and figure legends. As you write yourself it is no unit for salinity. R. Modified

C. Fig. 1: Change phosphorous to phosphorus in the legend R. Modified

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Anonymous Referee #2 C. 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). R. This information has been added to the abstract.

C. 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.

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. R. We have modified the introduction to answer this comment and be more explicit on the motivation and hypotheses driving the work. The discussion and conclusion now better relate to the ideas presented in the introduction.

C. I did not really understand the significance of their N* and P* parameters. I can see

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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. R. We modified the text to explain this better.

C. 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*). R. I sympathize with this request but after experimenting with the software we used to create the figures, we find that modifications would result in the loss of detail for 96% of readers (according to the statistics I find, 8% of men and 0.5% of women have one form of color blindness; assuming a sex ratio of 1 means less than 4% of the population). Although we would consider using another software in the future, it still is not clear to us how to color code a figure without discriminating against one form of color "challenge" or another — for example my father sees reds and greens, but does not perceive intensity or nuances within a given tone... In the mean time I have seen at least 2 pieces of online software that colorblind people can use to recode images to their liking. Is this compromise acceptable to Biogeoscience for the time being?

C. 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. R. We understand the inconvenience of switching between files to look at the supplemental data, but those data serve mostly as input for various calculations and estimations (e.g. discharge and nutrient data to estimate nutrient transport, relationship between PON and primary production to fill in gaps in sampling etc.). Incorporating those to the main manuscript would use up a lot of space and the manuscript is already long.

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C. 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). R. These are included in Figure 2.

C. There were several missing references; four on page 16680 alone: Kirkwood 1992; R. References have been added

Raimbault et al., 1990; Aminots and Kerouel 2007; Bergeron and Tremblay 2013).

C. 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. R. A portion of the introduction now reads "A comparison between riverine and oceanic nutrient sources (e.g. upwelling, vertical mixing and advection) considering quantity, stoichiometry and their importance for primary production relative to local recycling processes remains to be achieved at different spatial scales, which is the main objective of the present study. Our working hypotheses were that nutrient supply by the Mackenzie River makes a highly localized and small overall contribution to new primary production on the Canadian Beaufort Shelf and that this production is contingent on the mixing of P-deficient freshwater with N-deficient seawater."

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

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