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Biogeosciences Discussions

Interactive comment on "Seasonal distribution of dissolved inorganic carbon and net community production on the Bering Sea shelf" *by* J. T. Mathis et al.

Anonymous Referee #2

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Review of "seasonal distribution of dissolved inorganic carbon and net community production on the Bering Sea shelf" by J. T. Mathis et al.

This manuscript looks like a sequel of a previous one by Bates et al. (Deep-Sea Research II, 52, 3303-3323, 2005) in the adjacent Chukchi and Beaufort Seas. Mathis et al. have used the same approach to compute the net community production (NCP) of the Bering Sea shelf from April-May to June-July 2008. I have a major concern with this calculation. NCP is obtained just from the difference of dissolved inorganic carbon (nDIC) concentrations on June-July minus April-May. To my understanding, this calculation would be valid only if: i) the residence time of water in each of the six do-

mains is longer than the period elapsed between the two cruises; and/or ii) there are no changes in the water masses that circulate through the Bering Sea apart from those caused by the mixing with freshwater between the two cruises. Although I am not an expert on the oceanography of the Bering Sea, given the intricate surface circulation of the study area (Figure 1 of the manuscript), the authors should demonstrate that conditions i) and/or ii) are fulfilled. If this is not the case, they should estimate the error that their assumption introduces in the estimation of the NCP.

My second concern refers to the use of nDIC to correct the effect of freshwater mixing. This correction would be suitable if the DIC concentration in the freshwater end member(s) is nil. According to the web page of the USGS, Alaskan rivers are "moderately hard". Therefore, the assumption is not correct. For each domain, NCP should be calculated as:

NCP = 35* [DICj/Sj – DICr*(1-Sj/Sa) – DICa/Sa]/time

Where DICj and Sj are the DIC and salinity of the domain in July; DICa and Sa are the DIC and salinity of the domain in April; and DICr as the DIC of the incoming freshwater.

Assuming that DICr = 0 implies an overestimation of NCP. Note that for DICr = 500 micromol/kg, Sj =30 and Sa = 32, the overestimation would be of the order of 10 mmol C/m2/d.

My third concern refers to the meaning of NCP in this manuscript. In page 263, lines 12-13, you wrote: "NCP is calculated from the observed seasonal drawdown of DIC attributed to primary production over time". NCP is the result of the balance between the utilisation of DIC during phytoplankton photosynthesis minus the release of DIC due to the whole community respiration. This definition implies that changes of DIC over time due to CaCO3 synthesis and dissolution should be corrected by -0.5*(TA+NO3) to estimate NCP. To do that, total alkalinity (TA) measurements should have been preformed. If you cannot apply this correction, then you are not really measuring NCP but just "net carbon utilisation" unless you can demonstrate that the contribution of CaCO3 is neg-

ligible. However, in page 268 you say “similarly, this remineralized DIC lowers the pH of these bottom waters suppressing the carbonate mineral saturation states (Mathis et al., 2010)”. Therefore, the carbonate chemistry seems relevant.

Re. also the meaning NCP, on page 272, lines 3-4, you wrote: “underestimation of NCP may also occur due the remineralization of organic matter in the upper 30m between spring and summer”. This is conceptually incorrect because the NCP includes by definition the mineralization processes. Note that NCP is not the same than primary production (PP) or net primary production (NPP).

My forth concern refers to the objectives of the paper. The article by Bates et al. (2005) was one of more than twenty contributions to a special issue on the Western Arctic Shelf-Basin Interactions (SBI) Project. In that context, focusing just on the DIC based NCP looks satisfactory. However, it is a poor objective for an independent research paper in which oxygen, phosphorus, nitrogen and silicon based NCP could have also been calculated to present and discuss the stoichiometry of the net utilisation of these key elements.

I would like to see a revised version of the manuscript that takes these fourth points into consideration before recommending that it would be publishable in Biogeosciences.

Interactive comment on Biogeosciences Discuss., 7, 251, 2010.