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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.

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We would like to thank Review #1 for taking the time to critique this manuscript and provide valuable comments that have greatly improved our work.

Reviewer 1 commented, "This is a relatively long paper, but I think the amount of information based on measured data is not that large to justify this. In particular the background section is much too long. It contains a lot of info which is not relevant for the paper. I get the impression that it is part of a thesis. For a thesis, this level of detail is of course acceptable, but a scientific paper should be more concise. Also the number of references seems a bit too high."

Response: It was the goal of the authors in this paper to use the background and introductory sections to review past measurements of net community production in the Bering Sea. However, we recognize Reviewer One's comment on brevity and have significantly trimmed sections 2.1 and 2.2 in order to convey the same review in a more concise manner.

Reviewer 1 commented, "I am surprised that NCP is only calculated using DIC. As shown by the authors in Section 5.3, using DIC has some serious drawbacks. NCP can also be estimated using nitrate. It also opens up the possibility to compute Redfield ratios of drawdown. Moreover, comparison with previous estimates may be more useful with nitrate."

Response: The authors have updated section 4.2 to include a discussion of seasonal distribution of phosphate and silicate (nitrate was already discussed), and section 5.1 to include NCP estimates based on net utilization ratios of nitrate, phosphate, and silicate. Section 5.1 also features an inter-comparison and discussion of the strengths and weaknesses of each approach. Overall, we found that NCP measurements based on nitrate were much lower than those based on DIC, as is expected of highly productive, high-export systems (Sambrotto et al., 1993). Phosphate measurements tended to overestimate NCP with respect to NCP (DIC), except in the middle domain. NCP calculated from silicate drawdown was low, but this is also expected in iron-limited systems (Aguilar-Islas et al., 2007).

Reviewer 1 commented, "The authors should provide evidence on the importance of CaCO3 in the CO2 budget between the two cruises. This can be done with alkalinity, or biological data."

Response: The authors did measure total alkalinity in conjunction with DIC and have applied the suggested correction to the NCP estimates based on DIC. We found that seasonal changes in alkalinity were highly variable across the shelf; in some cases, alkalinity decreased from spring to summer, but increased in others. The authors have

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added a brief discussion of this correction factor to section 3.3 and included a fourth subsection in Section 4 to describe the seasonal distributions of alkalinity across the shelf. Section 5, Tables 3, 4, and 5, and Figures 7, 8, 9, and 10 now all reflect this correction factor. However, in response to these comments, we would like to note that pH changes due to the remineralization of DIC in bottom waters are excluded from the upper 30m, and do not affect our NCP measurements. Any remineralization of DIC occurring in the upper 30m is likely very small.

Reviewer 1 commented, “Similarly (section 3.3) the authors should justify the normalization procedure. If part of the salinity decrease is due to terrestrial runoff as they write, a simple normalization to a fixed salinity of 35 is not correct. The runoff has a non-zero concentration in DIC, which should be accounted for. I presume that the DIC concentration of the runoff is significant.”

Response: Ideally, the freshwater end members for ice melt and river water and their resultant volume fractions in seawater should be calculated in order to perform the most correct normalization procedures. These end members are typically determined through the analysis of δO18 samples, which are unfortunately unavailable with this dataset. As a secondary estimation, however, the salinity-variable normalization equation provided by Reviewer 2 is also not correct. Much of the salinity change between spring and summer across the shelf in the upper 30m was due to ice melt. Ice melt has a DIC concentration of near zero, and a much greater shelfwide influence than riverine discharge. Assuming that all salinity change is due to river water drastically overestimates the amount of DIC contributed to the shelf over 100 days.

In order to estimate the greatest possible effect of rivers on our NCP calculations, we performed the following estimation. While the Kuskokwim River, which discharges over the southern shelf, discharges less water and has a lower DIC value than the Yukon River, we assumed that influence of each river would be equal, and that all areas of the upper 30m of the inner domain would be affected equally. While stream flow data was unavailable for 2008, we referenced the average stream flow and DIC concentrations in the Yukon River from 2001 - 2005 as reported by Striegl et al., 2007. Assuming that all this DIC was confined to the upper 30m in the coastal domain, the authors estimated that ~12 μmol kg-1 of DIC should be subtracted from the DIC defecit in the coastal domain. Rivers also contribute a minimal amount of alkalinity, and this riverine alkalinity must also be included in the correction factor suggested by the reviewers as above. Using the stream flow and alkalinity data for the Yukon River reported for the period of 2003-2006 by Cooper et al., 2008, we determined (again assuming that the Yukon and Kuskokwim Rivers were equal, and that all alkalinity was confined to the upper 30m in the coastal domain), that ~12 μmol kg-1kg alkalinity were contributed over a 100 day period. Applying these alkalinity and DIC corrections to the inner domain NCP results in a <1 mmol C m-2 d-1 change in the rate of NCP. Because this is likely a dramatic overestimation of the influence of river water, we are reasonably certain that this value is much lower and that the river contribution of DIC and alkalinity is therefore negligible.

Because the estimation of the actual influence of river waters over the shelf is inaccurate without δO18 data, the authors have decided to briefly mention the influence of river waters in section 5.3, but retain the fixed-salinity normalization procedure originally used in their calculations.

Reviewer 1 commented, "As to the methods, in section 3.1 it is mentioned that a suite of measurements was carried out. Most of these data are discussed elsewhere in the paper. However, only DIC measurements are described in this section. Please add the other measurements including their precision and accuracy. . . . In section 4, not a single figure is presented on the hydrography, nutrients, and DO of the region. I think we definitely need those."

Response: The authors have updated section 3.1 to reflect the precision and accuracy of the hydrographic measurements and expanded the discussion of the seasonal distributions of nitrate, phosphate, and silicate in the study area. However, we feel that adding an additional eight figures to describe the seasonal hydrography seems excessive and unnecessary to the core objectives of the paper and overburdens the

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reader.

Reviewer 1 commented, “NCP is obtained just from the difference of dissolved inorganic carbon (nDIC) concentrions 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 domains 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.”

Response: We note that the residence time of waters over the shelf can have a significant impact on the estimation of NCP. Coachman (1986) notes that the residence time of the outer domain is approximately three months, and that the residence time of the middle and coastal domains is likely much longer due to reduced flow fields. Although the seasonal delay in their sampling was approximately 100 days, we note that because the spring distribution of DIC was nearly uniform the residence time does not introduce a large error in the NCP estimate. Our ultimate goal was to report a domain-integrated NCP, rather than specific NCP at any one specific latitude / longitude. The authors have added discussion to the text to better illustrate that the specific values of NCP over the shelf are integrated values and the water is moving generally northward. However, we would like to point out that our estimates of NCP are very consistent with other estimates based on varying methods, further illustrating that water movement did not cause a significant error in our estimates.

Reviewer 1 provided comments on minor errors (typographically, formatting, etc.)

Response: We have addressed all typographic, grammatical and other small errors in the text identified by Reviewer

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