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Comment

Interactive comment on “The Arctic Ocean marine carbon cycle: evaluation of air-sea CO₂ exchanges, ocean acidification impacts and potential feedbacks” by N. R. Bates and J. T. Mathis

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Received and published: 25 September 2009

response to Biogeosciences Discuss., 6, C1789–C1791, 2009

We thank Dr. Murata for his very helpful comments that have improved the paper. We have addressed all the comments below (as blue, Arial 11 font in the supplemental file) and revised the paper accordingly. In the online version of our response, we have added the AM to denote referee comment and NRB/JT Response.

We have added two new figures that we hope will aid the reader in following the discus-

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sion in Section 5. The confidence levels for the impact of many processes on the Arctic CO₂ sink or source are uncertain in the near-term and are highly uncertain over the next century (Table 2) due to the limitations of data that exist in the Arctic at present.

AM. General comments This paper is a well-written paper, which reviews carbon cycles in the Arctic Ocean, focusing mainly on air-sea exchanges of CO₂ and secondarily on ocean acidification. This paper would become one of the best papers reviewing carbon dynamics in the Arctic Ocean. However, I would like to ask the authors to re-consider some parts of the manuscript, although they are minor. They are listed as specific comments in the followings.

AM. Specific comments Abstract: line 20, “negatively” seems to be ambiguous. What is negative, and what is positive for the benthic ecosystem? NRB/JT Response: The term has been deleted from the abstract to avoid ambiguity.

AM. Page 6701, line 18, [H+], the parenthesis is usually used to express concentration of a species. So “[H+] activity” is not appropriate. NRB/JT Response: Corrected in the revised text. Activity deleted.

AM. Page 6702, lines 2-7, Here, relationships between dissociation constants and water temperature are described, but how about relationships between the constants and salinity? We often observe very low salinity like <10.0 in the Arctic Ocean shelves. NRB/JT Response: We agree with the reviewer and have added a statement to this effect. On the Arctic shelves, salinity can be less than 10 in surface waters locally influenced by sea-ice melt with additional uncertainty in the calculation of pCO₂ and Ω for example.

AM. Page 6704, lines 5-10, there are two sentences, which repeat almost the same thing. NRB/JT Response: We have revised this paragraph to avoid repetition.

AM. Page 6710, lines 12-20, very high pCO₂ values up to 844 uatm are introduced. However, were the values found in shelves? not in estuaries? Through the manuscript,

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distinction between shelves and estuaries seems to be not made. It is better to do the distinction, if possible. NRB/JT Response: We agree with the reviewer and have made the distinction in the revised paper. High values for surface seawater pCO₂ were observed nearshore on the shelf within the river plume of the Lena River (~850 μatm) that drains into the Laptev Sea, and the Kolyma River (~500 μatm) that drains into the East Siberian Sea shelf (Semiletov et al. 1999, 2007). In addition, very high values (~500 to +1500 μatm) have been observed in bottom waters of the inner shelf and also in the nearshore bays (e.g., Tiksi Bay) and estuaries of the Laptev and East Siberian Seas (Semiletov et al. 1999, 2007).

AM. Page 6711, lines 3-, CO₂ fluxes based on the eddy correlation method are introduced here. This method often presents results considerably different from those obtained by the bulk method. As stated in the text, they are often contradictory. The differences are possibly related to methodology. This point should be described in the text. NRB/JT Response: The eddy correlation technique was introduced earlier in the text in section 3.3 on original page 6703. We have added a statement that this methodology, as an “instantaneous” measurement, often yields different results to other bulk methods.

AM. Page 6701, lines 10-11, “The four directly: : : pH.” Almost the same thing is already described at lines 22-24 on page 6700. NRB/JT Response: We have revised the second sentence (Page 6701, lines 10-11) to avoid repetition.

AM. Page 6717, lines 7-8, “The length of the : : : has declined”, only from this phrase, because of shortened ice melting period, cooling seems to occur. Is that what you means? NRB/JT Response: This sentence has been corrected.

AM. Page 6719, lines 14, atmospheric (pressure) gradients? NRB/JT Response: Pressure has been added to the sentence.

AM. Page 6721, lines 12-15, There are lots of possible carbon cycle changes as a result of ecosystem shift. Why do you raise this change? NRB/JT Response: We have

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introduced this concept as the Bering Sea ecosystem has shifted and the Chukchi Sea may follow.

AM. Page 6722, lines 10-13, the release of alkalinity from sediments is a result in the North Sea. Can this be applied to the Arctic shelves? Any observed results suggesting this process? NRB/JT Response: We have clarified this statement to highlight the potential of this process to alter the inorganic carbon system of “shelf waters”. We have added the caveat that no evidence has yet been forthcoming about the potential influence of this process for Arctic shelves. It may not be significant!

AM. Page 6723, line 17 and others, “CO₂ content”, the word “content” implies quantity. So in this context, “pCO₂” is better, because this part states an increase of CO₂ with increasing temperature without changing concentrations (content in a specific volume). NRB/JT Response: The sentence has been revised to include pCO₂ rather than CO₂ content.

AM. Page 6725, line 6, “high TA”, do you mean high TA causes decrease of omega values? NRB/JT Response: The sentence has been revised to indicate that the rivers have high DIC:TA ratios that are favorable conditions for high pCO₂ and low Ω .

AM. Page 6726, line 27, why does the presence of a deeper shelf cause the attenuation? NRB/JT Response: The biogeochemical modification of shelf waters is diluted by homogenization through a deeper mixed layer on the Barents Sea shelf compared to the Chukchi Sea.

AM. Technical corrections Page 6700, line 12, Tanhua et al., 2009 is not listed in the reference. NRB/JT Response: We have added the Tanhua et al., 2009 reference to the revised paper.

Please also note the [Supplement](#) to this comment.

Interactive comment on Biogeosciences Discuss., 6, 6695, 2009.

C2197

BGD

6, C2194–C2197, 2009

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