Biogeosciences Discuss., 6, C2198–C2207, 2009
www.biogeosciences-discuss.net/6/C2198/2009/

Biogeosciences Discussions

Interactive comment on "The Arctic Ocean marine carbon cycle: evaluation of air-sea CO_2 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 We thank Dr. Miller for her 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 LM to denote referee comment and NRB/JTM response.

We have added a new caveat section (4.5.1) that discusses the potential for wintertime outgassing in the Arctic. We have added two new figures that we hope will aid the

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reader in following the discussion 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.

LM. General comments: This review paper on the marine carbon system in the Arctic by Bates and Mathis is a very nice collection of information and references and should be a useful resource to the community. The authors do need to take more care in the weight they place on ideas that are still only assumptions. They very correctly note that the data are sparse and seasonally biased, but then go on to draw elephants based on those data with a confidence that isn't warranted.

NRB/JTM response. In section 4, we draw upon all published results to conclude that, at present, the Arctic Ocean is a CO2 sink. We are very careful to state that this appears to be the present and near-term situation with the future trajectory highly uncertain. We have added a new caveat section (section 4.5.1) to discuss the seasonal biases of sampling, potential outgassing and ingassing. The nearshore, river dominated areas of the Laptev Sea and East Siberian Sea certainly has high pCO2 conditions but, Semiletov suggested, in balance, a potential for a minor source of CO2 (a few Tg at most) and Nitishinsky et al., a near neutral status of these shelves. We also suspect that the high-sea-ice melt surface waters locally observed in the Beaufort Gyre (Yamamoto-Kawai et al., submitted) are likely degassing CO2. But in summary, drawing objectively from published work, the weight of evidence is strongly tilted to the Arctic Ocean as a CO2 sink, at present. We stress the caveats to this assessment and the timescale (present and near-term). Given rapid change in the Arctic, we anticipate that there will be significant changes in the balance of Arctic CO2 sinks and sources. It may even reverse over the next few centuries but we don't yet have enough evidence to make more than a speculation.

LM. In general, the caveat needs to be more clearly emphasized that the current estimate that the Arctic Ocean is a net CO2 sink is based on very limited spring and summer data. Even aside from the new evidence that sea ice does not block air-sea gas exchange (which admittedly has not yet been widely published), leads and polynyas would not have to be open for long under stormy winter-time conditions to allow substantial outgassing that could largely counteract the estimated summer-time drawdowns. We just do not know what happens in the winter, or really even in the fall, and it's not appropriate to so confidently state annual fluxes that are based on such severely seasonally-biased data.

NRB/JTM response In section 3.1 we reiterate that there is very limited spring and summer data, and that there is virtually no winter data. In section 4.5, we have added a caveat section to the discussion of the estimates of Arctic Ocean CO2 sinks. Firstly, we repeat that these estimates are based on limited spring/summer data and virtually no wintertime data. Second, we have reviewed all the published papers and simply present these results as they stand. The carbon mass balance studies suggest that the Arctic is a CO2 sink in the sea-ice free regions on the shelves, while the observational data suggest that the Arctic is a CO2 sink during the seasonally sea-ice free period. In addition, under-sea ice observations across the central basin AOS, Beringia) also indicate that ΔpCO2 values are negative with the potential to absorb CO2 (although blocked by sea-ice). Given the residence times of central basin surface waters, we expect that the under-ice trans-Arctic sections are likely to reflect wintertime conditions as well. In addition, the Revelle factors and pre-conditions of Atlantic and Pacific surface waters entering the Arctic and cooling during transit into the Arctic are likely to favor negative ΔpCO2 values and thereby the potential for Arctic surface waters to absorb CO2. Thus, synthesis of available data and papers indicates that in the 2000's, the Arctic is a CO2 sink, at least for the near term.(i.e., less than a decade). However, the Arctic seems to be in transition and this situation could easily reverse. Thirdly, as pointed out by referee Lisa Miller, we discuss the issue of wintertime outgassing from polynyas and flaw-leads. For example, in the nearshore regions of the shelves, summer ΔpCO2 values are positive (highly positive in places like Tiksi Bay and in the river outflows onto the Siberian and Beaufort shelves; e.g., Semiletov, 1999; Semiletov et

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al., 2007). There are also indications that the highly river-dominated surface waters on the Laptev and East Siberian Sea shelves have positive ΔpCO2 values (inferred from the very low Ω values for river end-members shown by Salisbury et al., 2008). Yamamoto-Kawaii et al., (2009; in review) also show low Ω values for localized areas of surface waters (upper 3 m in the Beaufort gyre) that are highly influenced by sea-ice melt. This evidence, combined with winter homogenization with CO2 rich subsurface waters are highly likely to create conditions favoring the potential for surface waters to outgas CO2 (during winter through polynyas). In the Laptev Sea, for example, the flaw-lead polynya occurs over the outer shelf away from the nearshore high summertime ΔpCO2 values. The area of polynyas and flaw-leads on the Arctic shelves has been reported at 3.5 x1010 m2 (e.g., Winsor and Bjork, 2000). Assuming a 100 d period for gas exchange from polynyas/leads, an efflux of 10 mmoles CO2 m-2 d-1 would result in an efflux of 0.4 Tg C. Compared to influxes in the Chukchi and Barents Seas, ΔpCO2 values would have to be in the order of +150 μatm for an efflux of 0.4 Tg, and +1500 μatm, for an for a wintertime polynya/lead efflux of 4 Tg, In addition, wintertime efflux from polynyas/leads appears to be counteracted by net influx of carbon due to brine rejection during deep water formation. Omar et al., (2005), scaling results from Storfjorden, Svalbard, reported a wintertime influx of 2.3, 6.8 and 33 Tg for coastal shelf ploynyas, central basin polynyas, and brine-rejection during ice formation in the seasonally sea-ice free areas of the Arctic, respectively. The caveat for this study is that the results are extrapolated from Storfjorden to the entire Arctic. In summary, we simply do not know how the balance of wintertime polynya/lead outgassing and ingassing contributes to the overall annual air-sea CO2 exchange.

LM. The assumption that a decrease in sea ice will lead to an increase in primary productivity is also shakier than implied. Primary productivity could also be decreased by sea ice loss. If extensive sea ice continues to form in the winter, but with increased summer melt-back, the increase in stratification could decrease primary production by limiting nutrient fluxes into the surface waters. On the shelves, increasing river flow could increase shading by suspended particulate matter. Also, as is noted, there is

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already evidence that the river-influenced shelves can be net heterotrophic, at least during some times and in some places, and that may also increase as temperatures rise.

NRB/JTM response. This caveat is added to section 5.3 discussing the impact of primary production.

LM. More figures would be nice – maps, cartoons, etc. In particular, it could be useful to have more detailed maps of each of the regions as they're discussed – I suspect that some of the circulation descriptions could be hard to follow for people who haven't already spent a lot of time thinking about these areas. While I realize that not everyone has the facility with cartooning that Eddy Carmack and Wally Broecker have, it is a skill worth cultivating.

NRB/JTM response. This is a good idea and we have added two new schematic figures that show the processes operating on "inflow" and "interior" shelves during the sea-ice free period (i.e., summer) and during sea-ice cover (i.e„ winter). We use these figures as a means of illustrating the vulnerabilities of the Arctic carbon cycle to change.

LM. Specific comments: pg. 6701 – The definition of pCO2 is conspicuous in its absence. It's worth a bit of effort, because after alkalinity, it causes the most confusion.

NRB/JTM response. The definition of pCO2 (i.e., partial pressure of CO2) was introduced earlier in the first sentence of section 3.2 (page 6700). In the revised paper, we added a reference to the Dickson et al., 2007 manual at that point. On page 6701, in the statement that pCO2 is expressed as μatm, we added an additional comment about fugacity (fCO2) and referenced Dickson et al., 2007. As reviewer Wei-Jun Cai pointed out, the equations are not directly referenced again in the paper. But we felt that it was useful to the reader to retain the equations since we discuss DIC, TA, pCO2 and Ω throughout the paper and use DIC and TA data to calculate pCO2 (as many others have done).

LM. Section 3.4 – The discussion of net auto- vs. heterotrophy of the arctic shelves should include some of the other studies that have specifically looked at that question on the Arctic shelves – e.g. Alonso-Saez et al., 2008, Environ. Microbiol. 10: 2444; Garneau et al., 2008, JGR 10.1029/2007JC004281.

NRB/JTM response. We have made reference to these studies (in section 3.5) about the microbial component to the net metabolism of the Arctic.

LM. pg. 6706, paragraph 2 – It's specified that the Fransson and Kaltin papers used a mass balance approach to estimate the air-sea CO2 flux, but the approaches used by the Nakaoka and Omar papers are not specified.

NRB/JTM response. We have made reference to these studies being observational rather than mass balance.

LM. Table 1 – Include a footnote clarifying how the Canadian Archipelago estimate was derived. The datasets used in the CARINA compilation shown in figure 2 and discussed in section 4.3.1 need to be properly referenced, in the reference list, giving credit to the scientists who produced them. CDIAC is also beginning to assign doi numbers to their data sets, and if they've done that with those data sets, that should be included, as well.

NRB/JTM response. We have added the CDIAC metadata descriptors and CARINA ID's to the original Figure 2 caption. This metadata information includes CDIAC cruise number, Cruise Name, CARINA Table ID, ship, cruise dates, Chief Scientist and scientist responsible for inorganic carbon measurements. We checked to see if doi's had been assigned but they had not. This would be a great idea!

LM. Section 4.3.1, paragraph 2 – The CASES data presented in Mucci et al., 2008, are more properly considered to be from the Beaufort Shelf, not the Archipelago.

NRB/JTM response. We have corrected this in the revised draft. We have added statements about the Fransson et al., 2009 paper that observed strong undersaturation

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in the CAA, but close to equilibrium values in the Banks Island polynya.

LM. Page 6715, line 15 – Saying that you estimated that value for the exchange in the presence of '100%' ice cover is misleading, since you'd just rather arbitrarily chosen a correction factor.

NRB/JTM response. We have reworded this is the revised paper. The correction factor of 1% (i.e., an effective sea-ice cover of 99% when 100% is reported) was taken from Bates, 2006 and used to account for gas exchange through leads and polynyas. The 1% correction factor was not arbitrarily chosen as it was based on the 1% area for polynyas and leads in the central basin estimated by Gow and Tucker, 1990, Omar et al., 2005 also used this value for their paper.

LM. Section 5.2 – A contradictory effect of increased exposure to storms would be an increase in deep mixing, which could more efficiently bring high-CO2 waters to the surface, eroding the air-sea CO2 gradient. This is touched on in later sections, but I think an additional mention or cross-reference is warranted here.

NRB/JTM response. We agree and have reworded this into the revised paper. If mixing also bring up nutrients, and new production is enhanced, there may be no net impact on gas exchange. Mixing would have to bring up excess carbon relative to nitrogen (i.e., higher C;N stoichiometries compared to Redfield for example) to leave residual CO2 in the mixed layer.

LM. Section 5.3 – Be wary of placing too much emphasis on the Arrigo et al., 2008 analysis of primary production changes, because at many times and places in the Arctic, the bulk of the primary production is sub-surface (below the low-nutrient ice-melt lense), and therefore invisible in the satellite data that Arrigo et al. used. For an example from the Beaufort Shelf, see Tremblay et al., 2008, JGR, doi: 10.1029/2007JC004547. A longer melt season may simply be giving the fresh water lense time to dissipate, thereby allowing the primary production to approach the surface.

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NRB/JTM response. We have added the paper by Tremblay to the paper and briefly discussed it in section 5.3. If increasing vertical stability reduces the upward flux of nutrients, new production should decrease reducing the decrease in seawater pCOç. At the same time, however, there would probably be reduced upward flux of carbon through vertical entrainment and diffusion, reducing the increase in DIC and pCO2 due to these processes. The net effect might be no net impact or very minor impact on gas exchange.

LM. Section 5.5 – Clarify that you're talking about summer ice loss. At other times of the year, the surface waters aren't necessarily undersaturated.

NRB/JTM response. Yes, we have added "summertime" to the text to clarify.

LM. Page 6724, line 10 –Orr et al., 2005 is missing from the reference list. If it's the Nature paper (doi: 10.1038/nature04095), that one says that it's the Southern Ocean where acidification is likely to become a problem first – they don't really talk much about the Arctic.

NRB/JTM response. The Orr et al., 2005 reference is added as well as Orr et al., 2006.

LM. Section 7, first paragraph – As in section 5.5, clarify that you're talking about the summertime being when surface waters are undersaturated. The fact that you're talking about surface waters should also be explicitly stated.

NRB/JTM response. Yes, we have added "summertime" to the text to clarify.

LM. Technical corrections: pg. 6706, line 2 – either 'with a seasonal minimum' or 'with seasonal minima'

NRB/JTM response. This is corrected.

LM. Page 6714 – The subtitle 4.4.1 is inappropriately labelled, as well as unnecessary.

NRB/JTM response. The incorrectly labeled and unnecessary header was deleted

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LM. Page 6715, line 13 – Comma needed after parentheses

NRB/JTM response. Comma added

LM. Page 6717, line 8 – The length of the ice-melt season has actually increased, not declined. That is, the ice is melting earlier in the spring and freezing later in the fall.

NRB/JTM response. Yes, you are right. The wording is corrected.

LM. Page 6717, line 9 – comma needed after parentheses

NRB/JTM response. Comma added

LM. Page 6718, line 4 – '...a seasonal minimum...'

NRB/JTM response. This is corrected

LM. References: There are a lot of errors in the references. Some are minor (such as incorrect author initials, special characters and symbols aren't right, doi is missing, or the section of the journal isn't specified – i.e. JGR C, Tellus B, or DSR I), but many are quite substantial, like misspelled family names, authors in the wrong order or missing altogether, titles substantially different from the final published versions, wrong journal, wrong dates or page numbers, etc. The specific references in which I've notice problems are:

NRB/JTM response. We have to apologize for the errors in the citation list and the irritation it will have caused. In response, we have carefully revised the references, including misspelling, wrong author order, issue and doi numbers etc. We have also added a few citations in response to comments from this and other reviewers. We have added the following papers by: Alonso-Saez et al., 2008; Cai and Dai, 2004; Cai et al., 2006; Dieckmann et al., 2008; Dmitrenko et al., 2005; Fransson et al, 2009; Garneau et al., 2008, Gow and Tucker, 1990; Tanhua et al., 2009; Rysgaard et al., 2008; Signorini and McClain, 2009; Trembley et al., 2009; Winsor and Bjork, 2000, Yamamoto-Kawai et al., 2009.

Please also note the Supplement to this comment.

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

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