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Interactive Comment

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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Bates and Mathis have done an excellent job in synthesizing the current status of CO2 air-sea exchange rates and biogeochemistry in the Arctic Ocean shelves and basins. They also went through the processes that determine the fluxes, and the effects of possible current/future changes including the ocean acidification will do to these processes. This paper will have a good impact on the future of the Arctic C cycling research. Overall the paper is well written and very informative, and I have enjoyed reading it. The Introduction section (Sect. 1) is straightforward and tells readers the

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structure of the paper (I like this style). Sect. 2 provides some useful physical and biogeochemical background for understanding various issues to be discussed later. In particular the three categories (inflow, interior, and outflow shelf) are very helpful in understanding the differences in CO_2 uptake flux in various shelves that will be discussed in Sect. 4. Sect. 3 discusses historical data (which is necessary) and CO_2 chemistry in general. While it is helpful, I find the textbook content not all that useful. See further comment in Specific comments 1. Sect. 4 provides a complete and excellent synthesis of the state of knowledge. I learned a lot by reading this part twice. I also enjoyed reading Sec. 5 very much as it provides an excellent synthesis on how the current climate changes in the Arctic may affect air-sea CO_2 flux. The authors have made some visionary statements. However, I do feel they, occasionally, speculated too much (see Specific comments 2). Ocean acidification is all new in the Arctic Ocean research, and the authors have laid a good foundation for this field in the paper. But again, I feel not all the words are needed (see Specific comments 1). In several places, I wish the discussion can be more quantitative (see Specific comments 3) and more balanced (see Specific comments 4 and 5). I feel the authors may have put a little bit too much confidence on the factors that would increase CO_2 uptake. But there are many factors that could go the other way and deserve the attention as well (see Specific comments 4). Overall, an excellent review paper!

Specific comments 1. The authors are gifted in introducing and explaining questions to readers (which are often very helpful), but, occasionally, they seem to have the tendency to be excessive in providing background and textbook information. None of the equations (R1-R5) is used in the paper, thus not all are necessary. Also, for example, p.6703, lines 6 to 11, repeat what has been already said in lines 3 to 5. I feel that, at least, lines 6 to 11 can be shorten to half (negative delta(pCO_2), CO_2 undersaturation, CO_2 uptake, and CO_2 sink really mean the same thing but they occurred in the same sentence). I appreciate the authors' intention to make the reading smooth and easy, but feel they could trust the readers a bit better. Another example is on the ocean acidification, I feel the leading paragraph before 6.1 is all correct but not have much to do with the Arctic situation. It can easily be shortened to half.

Specific comments 2. The authors have made some visionary statements, but occasionally, they speculated too much. One clear example is in p.6722, lines 10-14. The release of alkalinity could be important in shallow water environment (in the context of influence surface water pCO2). But no evidence so far has suggested its importance in any deep water environment (again, in the context of influence surface water pCO2). Speculation on this possibility in the Arctic Ocean Basin, I feel, is appropriate at community discussion but not in a published paper (although it is also part of the community discussion).

Specific comments 3. Discussion can be more quantitative. For example, in p. 6707, CO2 input from the air is cited as one reason for pCO2 increase in fall and winter (probably not winter as ice will block the inflow of CO2). I agree. I suggest make this a bit more quantitative by estimating how much the air-sea flux would change pCO2 in the surface (or mixed-layer) water (probably in one high flux and shallow water area, Chukchi, and in one low flux/deep water area). pCO2air-sea can be estimated by estimating delta(DIC)air-sea (i.e., time integrated CO2 flux) during the ice open period and assuming a constant TA. Or the Revelle equation can be used for this purpose.

Specific comments 4. I feel the authors put more confidence on the factors that would increase CO2 uptake and thus more discussion on them. But there are many factors that could go the other way. For example, stronger upwelling can bring more nutrient, but also high DIC and pCO2 subsurface water. Same is true with the increased inflow of Pacific water after warming. The mixing with high pCO2 water should be taken into account. Other factors such as the amount of nutrient input vs OC input from river will also provide a more balanced view on river influence.

Specific comments 5. Sect. 3.4 and p. 6704. I feel the references of Cai and Dai (2004) and Cai et al. (2006) together with those of Borges et al. (2005) and Chen and Borges (2009) should be used for two reasons. First, Cai and Dai (2004) was the first to point out the latitudinal distribution pattern of uptake CO2 in the high-mid latitude shelves and release (or neutral) in lower latitudes. Second, the Cai et al. (2006) paper provides a difference approach, province-based method, to synthesis shelf CO2 air-sea flux. 1.Cai, W.-J. and Dai, M. 2004. A Comment on “Enhanced open ocean storage of CO2 from shelf sea pumping.” Science, 306, 1477c. 2.Cai, W.-J., M. Dai, and Y. Wang. 2006. Air-sea exchange of carbon dioxide in ocean margins: A province-based synthesis, Geophysical Research Letters, 33, L12603, doi:10.1029/2006GL026219.

Other minor issues: Abstract: Well written but concentrated too much on the CaCO3 saturation issue. The summary is actually a better one (more balanced)

p.6700, L13, Tanhua et al., 2009 is not the refs.

p.6701. R1, “-2” should “2-“

p.6701. R3, there is no need to –[minor species] (or even use []), it is sufficient to say +minor species, which could be positive or negative.

p.6701. L18, please do not use italic p in pH or pK (to be consistent with what is used in chemistry and to differential it from <italic-p>CO2). While there is no rule that p or italic p must be used, I think it is good to recognize the difference between them (i.e., in pH and pCO2).

p. 6702, line 10, delete + in R4.

p.6705, line 14, add (Fig.1) to the end (after freshwater inputs). Do the same for all subsections/shelves.

p.6712, line 21, delete one “principally”?

p.6715, line 25 and 27. use of “-“ or “+” for flux is a bit confusion. Strictly to say, when a direction is given (such as influx or degassing), then it should be positive.

Lots of formatting issues and errors in the references.

Please also note the Supplement to this comment.

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

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