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***Interactive comment on “The Arctic Ocean marine carbon cycle: evaluation of air-sea CO<sub>2</sub> exchanges, ocean acidification impacts and potential feedbacks” by N. R. Bates and J. T. Mathis***

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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

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confidence that isn't warranted.

In general, the caveat needs to be more clearly emphasized that the current estimate that the Arctic Ocean is a net CO<sub>2</sub> 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.

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 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.

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.

Specific comments:

pg. 6701 – The definition of pCO<sub>2</sub> is conspicuous in its absence. It's worth a bit of

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Discussion Paper



effort, because after alkalinity, it causes the most confusion.

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.

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

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.

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.

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.

Section 5.2 – A contradictory effect of increased exposure to storms would be an increase in deep mixing, which could more efficiently bring high-CO<sub>2</sub> waters to the surface, eroding the air-sea CO<sub>2</sub> gradient. This is touched on in later sections, but I think an additional mention or cross-reference is warranted here.

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

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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.

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.

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.

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.

Technical corrections:

pg. 6706, line 2 – either 'with a seasonal minimum' or 'with seasonal minima'

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

Page 6715, line 13 – Comma needed after parentheses

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.

Page 6717, line 9 – comma needed after parentheses

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

References:

There are a lot of errors in the references. Some are minor (such as incorrect author

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6, C1509–C1517, 2009

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Comment

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Interactive Discussion

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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:

Aagaard et al., 1981

Anderson, 1995

Anderson et al., 1990

Anderson, Olsson, & Skoog, 1994

Anderson, Bjork, et al., 1994

Arrigo et al., 2008

Bacastow & Keeling, 1973

Bates et al., 1996

Bates et al., 2005b

Bates et al., 2006

Carmack & Chapman, 2003

Cavalieri & Martin, 1994

Chapman & Walsh, 2003

Chen & Borges, 2009 (needs to be updated)

Cooper et al., 2005

Cooper et al., 2008

Dickson, 1990

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6, C1509–C1517, 2009

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Discussion Paper



'Dickson & Goyet,' 1994 (should be listed w/DOE as primary author – see correct citation on the frontspiece of the manual)

Dmitrenko et al., 2008

Doney, 2006 (more information is needed on the specific issue – the page numbers aren't sufficient in Scientific American)

Ducklow & McAllister, 2004 (not 2005)

Falkner et al., 1994

Fransson et al., 2001

Gammelsrød et al., 2009 (authors' names and date)

Goyet & Poisson, 1989

Guay & Falkner, 1997

Häkkinen, 1993

Hameedi, 1978

Hill & Cota, 2005

Jones & Anderson, 1986

Jones et al., 2008

Kattner et al., 1999

Kelley, 1970

Macdonald, Paton, et al., 1995

Macdonald, Carmack, & Paton, 1999 (not 1995)

Macdonald, Anderson, et al. is still in press

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6, C1509–C1517, 2009

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Discussion Paper



Macklin et al., 2002

Makkaveev, 1994 (be consistent – either give the title in Russian, or give the journal info and date for the English translation)

Mathis et al., 2007b

Mathis et al., 2009

Mucci et al., 2008

Mysak et al., 1990

Nagurnyi, 2008

Nakaoka et al., 2006

Nikolopoulos et al., 2009 (needs to be updated)

Nitishinsky et al., 2007

Omar et al., 2003

Omar et al., 2005 (editors are missing)

Parkinson, 2000

Pickart et al., 2005

Pickart et al., 2006

Rigor & Wallace, 2004

Roach et al., 1995

Roy et al., 1993

Salisbury et al., 2008

Sambrotto et al., 1994

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6, C1509–C1517, 2009

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Discussion Paper



Santana-Casiano et al., 2007

Schlosser et al., 1995

Semiletov et al., 1999

Semiletov et al., 2004

Semiletov et al., 2007

Skjelvan et al., 2005

Smith & Hollibaugh, 1993 (not 1985)

Solomons et al., 2007 (should be referenced according to the guidelines at the front of the specific document desired; what's given is a combination of the reference for the technical summary and the whole WG1 report)

Stabeno et al., 2001

Subba Rao et al., 1984

Takahashi et al., 2002

Takahashi et al., 2009

Walsh & Chapman, 1990

Wang et al., 2003

Wanninkhof & McGillis, 1999 (date missing)

Weingartner et al., 1998

Weingartner et al., 1999

Wijffels et al., 1992

Wilson & Wallace, 1990

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6, C1509–C1517, 2009

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Winn et al., 1994

Woodgate et al., 2005 (GRL)

Yamamoto-Kawai et al., 2005

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

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