

Interactive
Comment

Interactive comment on “A rapid transition from ice covered CO₂-rich watersto a biologically mediated CO₂ sink in the eastern WeddellGyre” by D. C. E. Bakker et al.

Anonymous Referee #1

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This paper investigates the processes driving the decrease of fugacity of CO₂ during sea ice breakdown in the Weddell Sea. This is a very interesting and original paper that investigates some new issues regarding CO₂ dynamics in polar seas. In my opinion, it deserves publication in Biogeosciences. In addition, the overall clarity of the text and the figures, up to date methods and bibliography comply with the high standards of Biogeosciences. The paper is robust - the authors have a strong knowledge of CO₂ dynamics and the area investigated -, well written and what delighted me is the concern of the authors to bridge the gap between some new insights in sea ice biogeochemistry and CO₂ dynamics in the water column. However, from my point of view, I felt that the authors were somewhat "timid" in further digging these relationships, and especially

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the fate of CaCO_3 formed within sea ice and its effect on CO_2 dynamics within the water column. Four recent papers underline that CaCO_3 precipitation occurs within sea ice (Papadimitriou et al., 2004; Langer et al., 2006; Delille et al., 2007; Dieckmann et al., 2008) while Rysgaard et al. suggest that this might be a significant process to sequester atmospheric CO_2 in polar seas. The authors could, for the very first time, consistently examine this conclusion, with an approach based on water column data. So, I would recommend to the authors to pay more attention to this issue by performing a simple computation described below. Depending of the results of the computation, the authors might further discuss the significance of CaCO_3 dissolution and might have to slightly adapt their conclusion.

Major comments:

Discussion page 1215. In my opinion, the figure 10 exhibits conspicuously that processes involving CaCO_3 precipitation/dissolution are taking place. I do not understand why the authors state that they have failed to find any direct evidence for the CaCO_3 ice hypothesis. Furthermore, the discussion from line 8 to line 14 is not very clear from my point of view, and may need further explanation or rephrasing. For instance, the sentence "if seasonal CaCO_3 -ice processes..." is not very intuitive for me. Indeed, processes behind CaCO_3 precipitation within sea ice and transport to the underlying water are complex (Papadimitriou et al., 2007) and still poorly understood. From my point of view, the right line with a slope of 2:1 delineating the data is a clear evidence of CaCO_3 dissolution is taking place. The left line, is even more clear, however the authors assumes that this tail reflect organic matter production. However, to my best knowledge, the observation of calcifying phytoplankton in the Weddell sea are rather sparse (Thomsen et al., 1988; Winter et al., 1999) while in contrast sea ice- CaCO_3 precipitation processes currently receives more attention. The present work is a very nice opportunity to assess the significance of CaCO_3 processes on the CO_2 dynamics in the water column. I would suggest that the author try to decipher, the effect of CaCO_3 dissolution and air-sea CO_2 exchanges in the figure 8, by assuming

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that CaCO_3 dissolution corresponds to the difference in TA from 50 m to 20 m and compute the related effect on DIC. This may help to assess if CaCO_3 dissolution occurs in surface waters during sea ice melting, and to quantify this dissolution. If CaCO_3 dissolution occurs, this would likely originate from sea ice fall and winter CaCO_3 precipitation, while other processes, including dissolution of forams could not be excluded. In the light of these computations, the authors may discuss more consistently the significance of CaCO_3 precipitation within sea-ice on CO_2 dynamics within surface waters.

Minor comments

Results P 1212, line 25. "The DIC maximum at 300 m depth between 59.5°S and 61.5°S along 0°W (fig. 6a) is associated with the Central Intermediate Water. From the Fig 6.a, such narrow maximum does not appear very obviously. I rather see a maximum extending from -64° to 59° up to 250 m. While I do trust the author's observation of a narrower DIC minimum, the fig 6.a is not very clear in this respect and the color scale might be adapted to exhibit this feature.

Discussion p1214, line 20 and 1216, line 29. "as a result of compensating effects by lower DIC and alkalinity". This explanation of two concurrent processes does not make sense to me. For me, the process is a simple dilution with freshwater coming from the ice matrix or precipitation, and I do not see any reason to invoke such concurrent processes.

P1214, line 21, "This relationship implies that processes affecting both parameters play a major role in the CO_2 chemistry of Weddell Sea surface waters". I am not sure of what the authors mean. In my opinion, any process that affect DIC or $f\text{CO}_2$ act to affect CO_2 chemistry whatever the relationship between both parameters. This may require more explanation or rephrasing.

Line 25. "The narrow range of normalised alkalinity ... reflects the conservative behaviour of alkalinity in the Weddell Sea. " In my point of view, this sentence somewhat contradicts what it is nicely exhibited in the figure 10, i.e. some obvious none con-

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servative (regarding to the salinity) processes affecting TA. In addition, a study have also showed that some deviation for a conservative behaviour can occur (Weiss et al., 1979). The authors ascribed these deviations to calcification/ CaCO₃ dissolution.

P1215, line 3 "by calcification". I would add "or precipitation of CaCO₃ "

P1216, line 13. "Assuming that only insignificant gas exchange occurs through the ice itself". Zemmeling et al. suggest that CO₂ fluxes over sea ice are not driven by a simple transport of CO₂ from seawater to the atmosphere trough the ice, but that more complex processes taking place within or at the surface of sea ice are responsible of such fluxes. In my opinion, the idea of a simple transport of CO₂ from seawater to the atmosphere through the ice is somewhat too simplistic. Sea ice peculiar processes control the fluxes of CO₂ at the sea-ice and ice-atmosphere interface. Anyway, I agree there is no winter ventilation of ocean surface trough the ice. But the reality is more complex and exchanges of CO₂ between the ice and surface waters - mediated by brines transport - can occurs as suggested by Rysgaard et al.. I would replace "gas exchange through the ice itself" by perhaps a more general "CO₂ ventilation through the ice".

Figure 8 caption. I would write: The changes are ascribed to freshwater inputs (denoted as "Salinity"), biological carbon uptake for organic matter production (denoted as "Org. Matter"), and further processes (denoted as "Rest").

References

Delille, B., Jourdain, B., Borges, A.V., Tison, J.L., Delille, D., 2007. Biogas (CO₂, O₂, dimethylsulfide) dynamics in spring Antarctic fast ice. *Limnology and Oceanography* 52(4), 1367-1379.

Dieckmann, G.S., Nehrke, G., Papadimitriou, S., Gottlicher, J., Steininger, R., Kennedy, H., Wolf-Gladrow, D., Thomas, D.N., 2008. Calcium carbonate as ikaite crystals in Antarctic sea ice. *Geophysical Research Letters* 35(8).

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Papadimitriou, S., Kennedy, H., Kattner, G., Dieckmann, G.S., Thomas, D.N., 2004. Experimental evidence for carbonate precipitation and CO₂ degassing during sea ice formation. *Geochimica et Cosmochimica Acta* 68(8), 1749-1761.

Papadimitriou, S., Thomas, D.N., Kennedy, H., Haas, C., Kuosa, H., Krell, A., Dieckmann, G.S., 2007. Biogeochemical composition of natural sea ice brines from the Weddell Sea during early austral summer. *Limnology and Oceanography* 52(5), 1809-1823.

Rysgaard, S., Glud, R., Sejr, M.K., Bendtsen, J., Christensen, P.B., 2006a. Inorganic carbon transport during sea ice growth and decay: A carbon pump in polar seas. *Journal of Geophysical Research* 112(C03016), doi:10.1029/2006JC003572.

Thomsen, H.A., Buck, K.R., Coale, S.L., Garrison, D.L., Gowing, M.M., 1988. Nanoplanktonic coccolithophorids (Prymnesiophyceae, Haptophyceae) from the Weddell Sea, Antarctica. *Nordic Journal of Botany* 8(4), 419-436.

Weiss, R.F., Ostlund, H.G., Craig, H., 1979. Geochemical studies of the Weddell sea. *Deep-Sea Research Part I* 26, 1093-1120.

Winter, A., Elbrachter, M., Krause, G., 1999. Subtropical coccolithophores in the Weddell Sea. *Deep-Sea Research Part I* 46(3), 439-449.

Zemmelink, H.J., Delille, B., Tison, J.-L., Hintsä, E.J., Houghton, L., Dacey, J.W.H., 2006b. CO₂ deposition over the multiyear ice of the western Weddell Sea. *Geophysical Research Letters* 33(L13606), doi:10.1029/2006GL026320.

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