

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.

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Reply to referee #1

We thank referee #1 for his/her very constructive review. Below we reply to every comment of the review by first copying the comment by the reviewer (#).

The reviewer found that we are timid in further digging these relationships, and especially the fate of CaCO₃ formed within sea ice and its effect on CO₂ dynamics within the water column. He/she proposed to do that by assuming that CaCO₃ dissolution corresponds to the difference in TA from 50 m to 20 m and compute the related effect on DIC.

Unfortunately, we do not have alkalinity from 50 m depth for ANT XX/2. Instead we have studied vertical profiles of alkalinity and DIC from cruise ANT X/6 between 47°S and 60°S along 6°W in spring 1992. On the basis of these data we conclude that north-south contrasts in alkalinity in the upwelled water and vertical gradients in DIC relating to the production and breakdown of organic matter contribute to patterns in surface water TA and DIC. However, these observations do not fully explain the 2:1 ratio between TA and DIC in surface water. We cannot exclude that CaCO₃ dissolution during and upon ice melt contributed to the 2:1 ratio.

Discussion page 1215. In my opinion, the figure 10 exhibits conspicuously that processes involving CaCO₃ precipitation/dissolution are taking place. I do not understand why the authors state that they have failed to find any direct evidence for the CaCO₃ & 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 CaCO₃-ice processes..." is not very intuitive for me.

We agree with the reviewer that our data may suggest the occurrence of CaCO₃ processes, but we have been unable to fully identify where and when these processes occurred, hence our caution. This was done as we do indeed have only indirect evidence - because we have no data about actual CaCO₃ in the ice, and neither do we have alkalinity data in the entire surface layer, which could account for changes at station scale. In the revised manuscript, we rephrased the text on page 1215 and also included a discussion on findings by other investigators as referenced by the reviewer.

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.

The reviewer is right and this has been changed in the text.

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.

We agree with the reviewer and have changed the text accordingly. Recent findings by Friis et al. concerning this issue have been added to the discussion.

P1214, line 21, "This relationship implies that processes affecting both parameters play a major role in the CO₂ chemistry of Weddell Sea surface waters". I am not sure of what the authors mean.

This text has been removed.

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.

What we meant here is that Anderson et al. (1991) reported alkalinity to be conservative, while they showed data with a similar spread as our data. We have rephrased the text to clarify this.

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

Text was changed accordingly.

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 through the ice, but that more complex processes taking place within or at the surface of sea ice are responsible of such fluxes.

We are aware of the fact that processes at the water-ice-atmosphere interfaces are complex. However, factually ice cover impedes gas exchange, which has been evidenced in many investigations of strong under- or supersaturation of gases under the ice cover (cited in the manuscript). We have changed the text to clarify this.

Figure 8 caption. I would write:

Text has been changed accordingly.

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With kind regards,

Dorothee Bakker and Mario Hoppema on behalf of the authors.

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