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Biogeosciences
Discussions

Interactive comment on “A rapid transition from ice covered CO_2-rich watersto a biologically mediated CO_2 sink in the eastern WeddellGyre” *by* D. C. E. Bakker et al.

D. C. E. Bakker et al.

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We thank referee #2 for his/her very constructive review. Below we reply to every comment of the review by first copying the comment by the reviewer (#).

However, I think the authors need to address the fact they did not measure alkalinity, but calculated it from DIC and fCO2, and they need to justify that their collection and storage of samples used for DIC analyses has not caused artefacts in data presented. The data from below the mixed layer may help justify the sample storage chosen.

In the manuscript we reported the fact that alkalinity was calculated from DIC and fCO2 (page1211, line 1-3). In the revised manuscript, we stressed more clearly and repeatedly that alkalinity was calculated and only available at the surface. The DIC samples

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were stored cool and in the dark. The majority of the DIC measurements have been done within a few hours of sampling. Only some few samples were analysed within in 24 hours. The error from non-poisoning is less than 0.7-2.4 umol/kg, as suggested by fCO2 changes uptake in a productive phytoplankton bloom that is described in the manuscript (and similar to Bakker et al., 2001), but this error will not have occurred due to optimal sample storage.

The lack of alkalinity measurements, the limited resolution of TCO2 (ie 20, 50 and 100m) samples, and the only surface fCO2 from about 10m depth, does detract from the paper. However, provided they address these issues and some others mentioned below I believe the paper will be a good contribution, suitable for publication.

In the revised manuscript we now discuss the implications of the limited resolution of DIC samples and the absence of alkalinity samples in the Methods section.

They may have historical data that can help them understand the trends in alkalinity across the two sections that are the focus of the paper.

We have added a description of vertical profiles of alkalinity and DIC from 47°-60°S at 6°W in austral spring 1992 (ANT X/6) for a more informed discussion of our results (Fig. 10).

The figures are difficult to read in paper form and improvements would make the paper more readable.

We expect that this has to do with the A5 format of Biogeosciences Discussions. After consultation of the editorial office, we don´t expect problems for the final version of the manuscript in Biogeosciences, which will be in A4. We have made improvements to the figures.

Introduction Page 1208, line 6. An "intrusion" from the east is mentioned to explain the retreat of the ice seen on satellite images. What intrusion?

We did not mean an intrusion with water, but rather the ice-free area in the east. The wording has been changed.

Methods: The lack of poison for samples is a concern that needs to be addressed. I am sure the authors are quite careful, but it sets a precedent that others might follow and they may not be so careful. Many of the waters sampled were undergoing rapid change in CO2, as indicated in the discussion section. What evidence do the authors have that there were no significant changes in TCO2 over the 24hr period? This would lead to changes in the calculated alkalinity.

The referee is right. However, as explained above, we think that not poisoning has a relatively small impact on our data. The calculated alkalinity is thus thought to be reliable.

They also calculate the alkalinity from DIC and fCO2 and a reference or calculation to show this works well is needed, particularly at the low temperatures they are working at. It is likely to be an internally consistent calculation, even if the estimated alkalinity is not accurate. The text describing the DIC analyses should provide some information on the precision and accuracy compared to the CRM analyses and duplicates.

The relationships of the dissociation constants of carbonic acid are valid for the temperature range of sea water (including freezing point temperatures). We added information on the precision and accuracy of the DIC measurements.

Is 20m representative of the mixed layer? Did the authors only take one sample in the mixed layer at 20m? It looks like that based on the plots in figure 4. A description or figure of the mixed layer depth along the transects should be included.

Samples for DIC were systematically taken at 20, 50 and 100 m depth, with occasional samples at 10 and 75 m. We now discuss the implications of using 20 m depth by comparing DIC at 10 and 20 m depth.

The 50m DIC value is taken as representative of the winter mixed-layer DIC value. The results section page 1213, line 11, do indicate the T minimum layer was shallower

than 50m in parts of the Southern Gyre and the CTDs where this was found are identified. Figure 6 indicates some large gradients in DIC between 50m and 100m. For figure 8 the authors should describe how the need to take the 50m sample as representative of the winter mixed layer might influence the result. The assumption is also that the 50m Winter mixed layer water is directly related to the overlying water at 20m, but there are clear gradients of DIC and salinity through the region that mean any advection of water will cause some uncertainty in the calculation. A brief mention and discussion of this should be included. The interpretations also rely on calculations based on salinity and in the format of journal, it seems reasonable to expect the salinity data will be presented in a figure like figure 5.

The implication of using DIC from 50 m as a proxy for the winter mixed layer is now discussed, e.g. by comparison to DIC at 75 m, in section 3.2. A short discussion on the possible role of horizontal advection has been added. Salinity has been added to several figures in order to facilitate interpretation of the results. Discussion with a colleague on CO2 chemistry further highlighted the need for more information on salinity.

Page 1214 line 18 should say "estimated alkalinity" or "calculated alkalinity" rather than alkalinity. On line 25 the estimated alkalinity, normalised to salinity, for surface waters shows a 25 umol/kg range and is claimed as a justification of conservative behaviour for alkalinity. It seems like quite a large change to me. How does this compare with the Anderson paper referenced here?

A similar comment was made by referee #1. 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. Additional mention is made of the fact that alkalinity is calculated (estimated).

With kind regards,

Dorothee Bakker and Mario Hoppema on behalf of the authors.

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