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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.

Anonymous Referee #2

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The paper is well written and is a useful description of the changes in the upper ocean CO2 system during the spring retreat of sea-ice in the east Weddell Gyre. 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. 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. They may have historical

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data that can help them understand the trends in alkalinity across the two sections that are the focus of the paper. Some references are made to previous studies and data in the region, but the interpretations largely ignore these.

The figures are difficult to read in paper form and improvements would make the paper more readable. Figure 3 (larger color coded dots); Figure 4 Dark Blue and Black lines are difficult to distinguish; Fig 5 the black dots cannot been seen against a dark blue background; Fig 6 same as figure 5; fig 7 the stars referred to in the caption are difficult to see; fig 8 "rest" in the caption should be "residual" or "remainder".

Introduction Page 1208, line 6. An "intrusion" from the east is mentioned to explain the retreat of the ice seen on satellited images. What intrusion? The rest of the text seems to talk about ice melting, which seems more realistic. If there was an intrusion of water from the east the discussion and results indicate it might have a different CO2 signature, but this intrusion is not discussed.

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. 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.

Results:

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

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figure of the mixed layer depth along the transects should be included.

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 CTD's 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.

Discussion.

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?

Page 1215 lines 10-15. I really doubt the authors can say much about CaCO3 in sea ice. The sampling was in a period when sea ice was unlikely to be forming. Brine associated with the ice would most likely have drained in the winter and after during ice formation. CaCO3 may be retained in the ice, but they are only sampling at 20m and 50m and the ice is often only 1m thick (ie about only 1m of water frozen). There would need to be a large amount of ice formed to see any influence. Thus, their conclusion they have not seen any signal related to CaCO3 precipitation in the ice is not surprising. They should state that the sampling time, the coarse resolution sampling, and the lack of alkalinity data from below the mixed is not optimal for seeing any signal.

Page 1215 lines 23-28. The slope of the estimated TA vs TCO2 for surface waters is discussed here. The lack of subsurface TA data hampers this discussion. One likely explanation for the slope of 2:1 is that the subsurface waters have this slope (ie this is a mixing line) and carbon uptake represented by the dashed line is shifting these data to the left line. The authors do mention CaCO3 production may be significant because of the 2:1 slope, although this is not possible to determine with these data. Is there any evidence for large scale calcification in the literature and do the historical TA vs DIC data show the same trends in the subsurface? Given the lack of alkalinity data and the attempt to interpret estimated alkalinity in surface waters, the authors need to consider the existing data, if that is available.

Interactive comment on Biogeosciences Discuss., 5, 1205, 2008.