Biogeosciences Discuss., 12, C2553–C2556, 2015
www.biogeosciences-discuss.net/12/C2553/2015/

Biogeosciences
Discussions
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Interactive comment on "Two decades of inorganic carbon dynamics along the Western Antarctic Peninsula" *by* C. Hauri et al.

Anonymous Referee #1

Received and published: 3 June 2015

This paper summarizes a large and long data set. The paper is descriptive and concludes that DIC drawdown is greatest inshore, redfield ratios are not always followed with nutrient depletion, and the balance of freshwater flux and biological production is a strong determinant of change in aragonite saturation state. The paper does not increase understanding of what was driving change in the region, but it is a useful descriptor of the variability in DIC and other carbon parameters.

I did find at times the relationship between the various parameters or why various some changes were selected to describe difficult to follow (see below). This caused me to have to stop reading and search through the paper for other information. For example, for most of the paper DIC, TA and nutrient data were available and why a TA vs salinity relationship was calculated in an early section was not apparent until later when salinity

and year-round pCO2 measurements were used to calculate aragonite saturation state while the other measurements were Summertime only.

The change in sea ice in the region is also mentioned a number of times in the paper, but there is no description of how sea ice is changing for the region (seasonally, interannual and spatial change over 20 years) apart from one sentence in the introduction. If there is more information on sea-ice change, it seems important to include and discuss the relevance in influencing the biogeochemical properties. Also, I was also not able to find a good description of the physical oceanography of the region data are presented from. Again, there are mentions here and there in the text, but it is scattered and difficult to follow.

Other comments:

p 6931, lines 9-11: It is not clear how la Nina years influence the carbon cycle dynamics. Do more intense storms and a poleward displacement of the polar jet have an influence. There is a description of possible changes in carbon cycling for SAM. What does the literature indicate is happening to sea ice extent over the period?

p 6933, section 2.1:Nutrient data are used in the paper, but I cannot find information on how these data were measured and where to access these.

p 6935, line 7: how many outlier (per cent) were excluded out of the total number of samples? The text suggests there may be an analytical problem. I suspect this isn't the case, but the sentence beginning "These outliers included..." indicates there were many more than described in the section.

p 6935, lines 10-19: I am not sure what the point of this regression analysis broken down into different years is. I first thought the intercept might be meaningful, but it seems more like the authors are trying to check the internal consistency of their measurements. Why not consider the residuals? Is the need to split the years used to compare pCO2 measured and pCO2 calculated an indication that the quality of the measurements has issues some years? If so, please state what years and why?

P 6936, section 2.4: Why are nutrient concentrations ignored in the TA vs salinity relationship given what appears to be a large range in pCO2 and presumably nutrient concentrations? Nutrient data are used with TA on page 6939. I am also unclear on the relevance of this salinity vs TA relationship. Most of the following sections in the paper do not seem to use the relationship as there are TA, DIC and nutrient data used to calculate the carbonate system parameters, or is this incorrect? Section 3.4 does use the relationship and it would be helpful to state in section 2.4 that it is used later with data pCO2 data to calculate the saturation state in fall, winter and spring seasons when bottle data are not available.

p 6937, section 3.1: This is OK, but it averages data from the Summer, when there is large variability. The point that there are large and persistent decreases inshore relative to offshore is well defined. However, the section does not indicate the range of values used in the averaging. For example, what range of sDIC and salinity values occurs inshore compared to offshore for the averaged data points. It would be good to get some idea of the variability.

p 6939, line 16: A reference to Wolf-Gladrow et al (2007) Total alkalinity: The explicit conservative expression and its application to biogeochemical processes, Marine Chemistry, 106 (1-2), 287-300 is approprate here.

p 6943, line 6: I could not find any mention in the Anderson et al 2000 paper on how glacial meltwater influences aragonite saturation state. It is in the Yamamote-Kawai paper.

p 6944, lines 11-12: These refer to DIC drawdown in the WW layer as biological, which seems reasonable as an ultimate cause of drawdown. I suppose this drawdown will occur in the summer season? Is this correct and why can't the DIC decrease in Figure 5 be due to mixing ofsurface water into the WW layer or mixing of lower DIC WW water from other regions.

p 6944, lines 14-18: Is the text here referring to Figure 5? This is the only figure I could locate that shows anything that might relate to the text.

p 6945, lines 10-25: Why would not accounting for the drivers of TA influence the TA vs salinity relationship? If TA+nutrients are used, it may help the relationship with salinity, but the authers have not done this. Invoking ikaite is unlikely to explain the differences. The occurence of ikaite in sea ice is limited and it is not clear how changes in a 1-2 m sea ice layer spread over a 50m mixed layer could have much effect (ie any effect would be diluted in the 50m thick mixed layer). This section is not much more than a statement that TA variability could be explained by just about any process. One other possible explanation is the TA measurements have a large amount of error although the methods section states the measurements are high accuracy.

p 6946 line 16-20: Why have two high values been singled out to consider the decadal rates of change in the central sub-region? The fall and spring are when rapid change might occur and it is not clear from Table 3 or the text if this is a persistent pattern each year or due to limited data. The more interesting data may be for winter when biological effects are small compared to Spring. Here, the decadal trend is small in the central region and similar to the atmospheric increase in the north region. Do these changes agree with Takahashi's previous estimates and why the differences? The same applies to the fall and spring rates of change (ie why the regional differences?).

Interactive comment on Biogeosciences Discuss., 12, 6929, 2015.