

## Response to Reviewers

We would like to thank the reviewers for their further comments and suggestions, which we hope has further improved the manuscript.

### Reviewer #1

In general I think that the authors have responded in a thoughtful way to the comments from me and the 2nd reviewer, and so made the manuscript significantly better. There are a few minor comments (editorial) that I mention below that should be dealt with. More importantly though is the response to two major remarks by both reviewers that is not really dealt with in a satisfactory way. In light of this my review is that the manuscript should be published, but that additional revision is necessary.

Major issues:

The author's response to my request for a secondary quality control of the data used in this study is not satisfactory. The authors of the paper are well familiar with 2nd QC and cross over analysis. No comparisons need to be shown in the paper, but the analysis needs to be done, it is almost a standard procedure these days, particularly in any work dealing with temporal trends and changes of properties. The authors should compare the accuracy/consistency of their data to publically available data collections such as CARINA or GLODAP. This is valid for both the carbonate system variables as well as supporting variables for the MLR analysis. An attempt is done for a comparison with the 194 A17 data, although the biases that the authors report on for, for instance, silicate is not convincing. I suggest using the consistent data products that exists to deal with this.

We have now included the results of a crossover analysis performed with the CARINA database, which we hope leads to a more robust indication of the consistency of our dataset with others.

The second major issue is the neglect of including an analysis of other repeats of the A17 line. The focus of the paper now seems to have shifted (?) towards discussing buffer factors and thus the sensitivity of various water masses to the CO<sub>2</sub> perturbation, rather than focuses on temporal changes. In this sense the neglect of the additional A17 lines might be justified. However, the authors mention in the introduction how the southwest Atlantic might have become more important in recent times for storage of anthropogenic carbon, including more A17 would give the authors some leverage to verify, or not, that suggestion.

We agree that the potential increasing importance of the South Atlantic CO<sub>2</sub> sink could be well assessed through performing a similar study on a number of reoccupations of this line. Such an approach would also give insight into the rate of change and how this varies on different time scales. However, we have avoided doing that here so that we could focus on the total anthropogenically-driven change to pH and the buffering factors, particularly in the sensitive SubAntarctic Zone. As such, we really aimed to highlight the impact that anthropogenic DIC has had in this region since pre-industrial times.

Sections in Figures 3 and 4: I think it would be more useful to state (for instance) "Revelle factor" rather than "(a)" in Figure 4a as a text field over the bottom contour.

This has been done.

Line 475: I assume the authors mean “The highest buffer factors”, not “Revelle factors”.

This has been changed accordingly.

## Reviewer #2

The authors have properly assessed the questions raised in the first review. And I approve the new version of the ms which is more organized and clear.

However I have two additional questions:

a) Why AABW has no CANT in 2011 if you are applying the phi-C method?? Please check Fig3 in Ríos et al (JMS 2010) or Fig1 in Vazquez\_rodriguez et al (BG 2009). There should be some CANT signal in water below 2°C.

The  $C_{ant}$  in 2011, determined from the phi-C method is shown in Figure 4(a) – top panel. We do see concentrations of  $C_{ant}$  in AABW up to  $\sim 12 \mu\text{mol kg}^{-1}$ , which is consistent with the manuscripts you mention.

b) With regard to point 2.3 Deepwater consistency...: the differences are so high that the if the 2011 is included in the 2QC for GLODAPV2 it should be corrected compared to CITHER 1994 or viceversa. Please comment on this because it questions the results presented afterwards.

We have now also included the results from a crossover analysis performed with cruises in the CARINA database to further assess the differences between datasets. The differences are much more significant when just looking at lower Circumpolar Deep Water, which can partly be attributed to fewer data points in the area, however, when crossover analysis was performed, there is good coherence with data in the CARINA database.

thanks