

## Manuscript bg-2011-478

Author response on Anonymous Referee #1

Biogeosciences Discuss., 9, C373–C375, 2012 www.biogeosciences-discuss.net/9/C373/2012/.

### Author Response:

We thank Anonymous Referee #1 for posting a very helpful review of the paper. The referee comments were very minor in nature. In the revised paper we have addressed all of the comments brought forward by the reviewer and this has improved the paper.

Our responses are interspersed with the comments by the referee (in black), and we have used indented blue Arial font for ease of review.

Review of "Detecting anthropogenic carbon dioxide uptake and ocean acidification in the North Atlantic Ocean" by N.R. Bates, M.H.P. Best, K. Neely, R. Garley, A.G. Dickson, and R.J. Johnson.

This manuscript by Bates et al., discusses observations of dissolved inorganic carbon and alkalinity at the hydrostation S and BATS, just off Bermuda. These measurements are sufficient (together with the hydrographic properties) to define the carbonate system at this location. This time-series provides one of the longest (perhaps the longest) records of carbonate system observations at one location in the ocean, and the data are therefore very valuable for the understanding of the trends in carbon uptake during the, roughly, last 3 decades. The manuscript not only discusses the anthropogenic carbon uptake, but also changes in ocean pH, buffer capacity and calcium carbonate saturation state. The data record is impressive, both with regard to the overall quality of the data, as well as the length of the record. These facts by itself provide some justification for the publication of this manuscript. This paper could be viewed as "just one in a series" of papers discussing the carbonate system record at the BATS site, but this paper provides an important extension of the data series, both forward and backward in time (i.e. the inclusion of historic data from locations close by), and can therefore be justified as an important contribution to the science regarding the ocean uptake of anthropogenic carbon. The paper deserves to be published in Biogeosciences, once the authors have considered the (minor) comments I have made below.

The authors refer to Supplementary information. However this information is not available to me from the BGD site.

The reference to supplementary information was an error. All information about the paper was contained in the original submission of the paper.

The authors argue for a linear trend in sea-surface  $p\text{CO}_2$  values from the mid-1970's to 2011, and show this with data (although the error bars on the earlier data points are significant). Why should one find a linear increase in  $p\text{CO}_2$  in the sea surface when the atmospheric  $\text{CO}_2$  concentrations have increased at an exponential rate during this time period?

The referee makes a good point. The linear trend hindcasted back from 1983 to 1969 was an extension of the 1983-2011 trend. Alternatively we could also show a trend pre-1983 that fits the atmospheric  $p\text{CO}_2$  trend (1969-1983).

Also, by carefully viewing Figure 6, there seems to be a deviation from the linear increase trend of  $p\text{CO}_2$ , DIC, etc during the last 2-3 years. The authors, probably rightfully so, argue that sufficiently long time series is required in order to be able to assess a trend with some confidence (as opposed to shorter time variability). However, it would be interesting to read a discussion on the deviation from the long time trend during the last few years, even though this would turn out to temporal variability.

The referee is right that there has been stronger deviation from the trend during the last few years. This reflects short-term variability in winter mixing, and as shown in the companion paper, likely reflects changes associated with variability of the NAO. We will add a brief statement indicating that undertaking trend analysis using shorter time-periods can alias trends to short-term variability. Similar the trends, shown for  $p\text{CO}_2$  measurements by McKinley et al., decadal records are required to establish confidence in long-term trends.

Minor comments: It would be useful to have a map of the BATS site in relation to the stations occupied during TTO and GEOSECS.

This will be added to the revised paper.

For the TTO data, there are two different versions of the carbonate variables available at CDIAC; the original data, and data that have been recalculated and adjusted to better match manometric measurements and more recent measurements in deep water (Tanhua and Wallace, 2005). Which data did the authors use for this study, and why? Several different adjustments have been proposed to the DIC data from GEOSECS. Did the authors use any of these adjustments? The GEOSECS data has been shown to have large biases, (Peng and Wanninkhof, 2010).

We agree with the reviewer. Using GEOSECS data has caveats and we will clarify that the adjustments were made according to Tanhua and Wallace (2005)

Page 992, lline 16: Additiional

This is corrected.

Figures 5 and 6: These are busy, and central, figures. They should be printed larger than in this ms. so that the details can be more easily seen. I don't understand why symbols with almost identical colors are used for the same panels. This makes them very difficult to distinguish from each other (e.g.  $p\text{CO}_2$  and Revelle factor in the 4th panel, but this is true for almost all panels).

The figures will be modified to make them more readable.

References: Peng, T.-H., and Wanninkhof, R.: Increase in anthropogenic CO<sub>2</sub> in the Atlantic Ocean in the last two decades, *Deep Sea Research Part I: Oceanographic Research Papers*, 57, 755-770, 2010. Tanhua, T., and Wallace, D. W. R.: Consistency of TTO-NAS Inorganic Carbon Data with modern measurements, *Geophys. Res. Letters*, 32, L14618, 2005.