

Author Response:

We thank Anonymous Referee #2 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.

General comments

The BATS time series observations are one of the foundations for evaluating the response of the oceans to increasing atmospheric carbon dioxide. Time series observations in key ocean environments are yardsticks for model developments. This manuscript describes the carefully executed and quality controlled work at BATS from the beginning in 1983 to 2011. Intermittent evaluations of the time series are to be welcomed and the publication of this one is recommended after minor revisions.

The BATS time series is leagues apart from the GEOSECS data when one considers confidence in the numbers. This reviewer has serious reservations about presenting them together as is done in Fig. 8. Does it throw light on ocean processes or improve our understanding, by observing (page 1003), that the GEOSECS (adjusted for bias and seasonality??) data fall not far away from the back extrapolation of the regression line? The later TTO data is of better quality, but still with adjustments (?). My doubts about Figure 8 are in fact also the argument for the necessity of sustained observation work that the BATS series represents.

We agree with the reviewer that using GEOSECS data has caveats and has to be undertaken with caution. In the revised paper, we will clarify that the adjustments were made according to Tanhua and Wallace (2005). We will also add statements that the GEOTRACES/TTO data is used with caution, the issue of seasonality, and the very good point that sustained, very high quality measurements of the inorganic carbon system are needed for the longterm.

In section 4.2 the authors observe that the CO₂ sink in the N-Atlantic subtropical gyre has not changed significantly over the last 30 years. They challenge the results from shorter term observations (<10 yr) which suggest differently (e.g. Schuster and Watson, 2007; Schuster et al., 2009, Watson et al., 2009). This brings up the question: What did BATS show for the short periods examined in these other works?

The referee is right that undertaking trend analysis using shorter time-periods can alias trends to short-term variability and be quite different to long-term trends. There has been stronger deviation from the trend during the last few years that reflects short-term variability in winter mixing, and changes associated with variability of the NAO. For example, a higher rate of increase in DIC is evident for the period ~2001-2007, but the trend is much flatter for the period 2007-present, and 1996-2001. It's difficult to choose which timeframe to compare to other papers, but we will add brief statements of how the time-series compares to the results of Ute, Andy, etc.

Specific comments

P990 Line 4 Where does the 30% figure come from? I could not find it in the main text.

This is corrected in the revised paper.

P996 Line 3 This section on a comparison of measured and calculated $p\text{CO}_2$ seems irrelevant in the context of the time series.

We felt it important to show that the calculated $p\text{CO}_2$ (from DIC and TA) were very similar to measured $p\text{CO}_2$, and gives some confidence that seawater carbonate chemistry parameters calculated from DIC and TA are reasonably accurate.

P1002 Line 18 The reference Olafsson et al. 2010 is not listed. P1015 Line 2 Add Revelle factor
P1015 Line 3 Replace E with F

This is corrected in the revised paper.