

Manuscript bg-2011-448

Biogeosciences Discuss., 8, C5844–C5847, 2012 www.biogeosciences-discuss.net/8/C5844/2012/

Author Response:

I 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, I have addressed all of the comments brought forward by the reviewer and this has improved the paper.

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

Review of the manuscript Submitted to Biogeosciences

Title: Multi-decadal uptake of carbon dioxide into subtropical mode water of the North Atlantic Ocean. Author: N.R. Bates MS No.: bg-2011-448 MS Type: Research Article

Decision: This manuscript is acceptable for publication after revisions

General comment:

In this study, N Bates evaluates and analyzes multi-decadal changes of the oceanic CO₂ system in the North Atlantic, more specifically at BATS station near Bermuda and within the subtropical mode waters, STMW. This manuscript follows previous analysis focused on seasonal to interannual variability at the same location. Although the observed changes of DIC in STMW has decreased in very recent years (Fig 2A), the analysis seems to confirm previous results, the increase of CO₂ in STMW is faster than in surface waters. In addition to CO₂ uptake variability in relation with NAO, the study also presents results that concerns ocean acidification (Figure 1), but this is not discussed. Regarding the discussion and conclusion (and Abstract), I suggest to focus the paper on the CO₂ fluxes, add a plot of alkalinity observations (in Figure 1), delete the results and description associated to acidification (Figure 1d,e and section 3.2). This specific topic, acidification, should be presented in another manuscript.

The referee makes an interesting point that the OA parts of the study should be elsewhere. However, I feel that the case for OA stands better alongside the discussion on inorganic carbon changes in the STMW. Rather than lose this small section, the revised paper will have added statements on the OA trends (given that they are higher than changes in surface waters at BATS).

After corrections and clarifications (see specific comments below), I recommend this paper for publication.

Specific comments:

C1: Abstract: In the results section, the author evaluate not only the DIC changes but also pH and other properties related to ocean acidification. These results should be include (one sentence) in the abstract if (and only if) author wants to include acidification in the discussion section. However, I think this could be a topic for another manuscript.

I have added a few more statements on the OA trends and comparisons to the surface at BATS.

C2: Almost all oceanographers know the location of BATS. However I think it is important to show a map and locate BATS station (in relation to large scale circulation ?).

This is a good idea. I will add a figure.

C3: Introduction: Author refers to Atlantic Multidecadal Oscillation. However, I would prefer to call this index Atlantic Meridional Variability (AMV, Metzl et al., GBC 2010); the terminology -Oscillation- should refer to an identified periodical variation. But for this index, there is not yet a clear period identified as far as I know (including back to historical data of 19th century).

This is a good point. AMV is used in the revised paper with reference to the Metzl paper.

C4: Page 12453, line 5. Thomas et al (2009) is not in reference list. Maybe Thomas et al 2007 or Thomas et al (2008, GBC) ?

The correct reference is added in the revised paper.

C5: Page 12455 line 19 (and other place): Reference to Bates et al., 2011. Is this manuscript has been revised, accepted ?

The reference to the companion paper in Biogeosciences is corrected in the revised paper.

C6: Page 12455 line 22: HgCL2 or Hg2Cl ???

This is corrected.

C7: Page 12458, line 5. Author indicates that Alkalinity did not change over time. However, I remember a story, from the same author, where large anomalies in ALK have been observed for short period. It would be interesting to add ALK data in Figure 1 to complete the full oceanic CO2 system (and if author agrees delete the pH, Omega plots that are not discussed in the paper).

It is correct that TA anomalies (due to calcification) have been observed in surface and mixed layer water. However, such non-conservative changes in TA have not been observed in the STMW. I will add TA to the figure.

C8: Page 12458, line 15. Thomas et al (2009) is not in reference list. Maybe Thomas et al 2007 or Thomas et al (2008, GBC) ?

This is corrected in the paper.

C9: Page 12459, line 14. What is number 30, a reference ?

This is corrected in the revised paper.

C10: Page 12461, line 1. For anthropogenic CO2, author lists a number of 0.692 Pg with a reference to Figure 3b. This is not clear to follow this discussion (when looking at numbers listed in figure 3).

The statements are clarified in the revised paper for readability.

C11: Page 12463, line 2; I apologize for the delay of this review, but I my feeling is that it is not to late... please correct reference Bates et al 2022 (2002 ?).

Yes, I'm 10 years ahead! Will correct of course.

C12: Page 12464, line28. Again check reference Thomas et al 2007 in this discussion.. Figure 1: the legend indicates results from 1983 to 2011, but this is 1988 to 2011.

This is corrected in the revised paper.

Figure 1b: legend indicates both DIC and salinity normalized DIC but nDIC is not plotted on the figure.

This is corrected in the revised figure.

Figure 2: again, legend indicates results from 1983 to 2011 instead 1988 to 2001

This is corrected in the revised figure caption.

Figure 3: for Figure 3e, the DIC difference is positive (should be refered as the differ-ence between STMW and surface).

This is corrected in the revised figure/figure caption.

Figure 3: correct axis references: DIC diif, DIC diffFigure 3: I don't see the red symbols (NAO) on figure 3fFigure 3: I don't understand the plots in Figure 3e and 3f. For example, in Figure3e.

The figure is clarified in the revised paper.

DIC-diff of about 22 umol is related to NAO around -1.8. In Fig 3f the same DIC-diff is associated to NAO around -1.3.

The text is clarified in the revised paper.

Final comment: as usual in such impressive analysis of ocean CO2 observations, I recommand author to refer a location where these data are archived (e.g. CDIAC ?)

The reader will be directed to the BATS website. I agree that the data should also be at CDIAC and will be!