

## **Response to anonymous Referee #1**

**Received and published: 18 December 2014**

I would, on the behalf of all co-authors on this manuscript, like to thank the reviewer for a very thorough and thoughtful review. Section 3.4 has significantly improved after this revision, and overall the readability of the manuscript has improved due to the helpful comments from the reviewer. Below each comment from the reviewer is restated, with my response below in bold font. All technical comments not specifically answered here have all been revised according to the reviewer's suggestions. Please also refer to the tracked changes version of the resubmitted manuscript for details on how the revisions were implemented.

### General comments

I found the paper very thorough but sometimes lacking clarity. I needed to repeatedly re-read several sentences and sections. This is partly due to the complexity of the methods but I still think the authors should consider simplifying/clarifying some parts of the manuscript for greater accessibility. **We have revised where appropriate and where the text was clearly ambiguous or confusing. This has helped the readability.**

As an example, I found section 3.4 (Recent changes in the Southern Ocean biomes) challenging: Page 15564 line 8: 'This study generally does not have statistically significant results in the SO...' clarification on timescale would be useful. Line 14: 'no change in what drives the pH trend for this decade compared to the longer period' How is this evidenced? The numbers for SOSTSS do not increase from table 2 to table 3 by the same scaling. Line 15: 'the change in the observed pH trend (table 3) appears to be dominated by the change in DIC as this is approximately four times larger (more negative)..'. The change due to DIC is indeed 4 times greater in table 3 than table 2 but the other drivers also increase or decrease by similar amounts. Please clarify. Also, the DIC component isn't negative. **We have revised section 3.4 for clarity and removed some errors. In particular, we have re-written the above-mentioned sentences and have added references to the correct figures and tables on which our discussion is based. We have removed the statement about the negative DIC component as this was related to the fCO<sub>2</sub> trend and merely a residual of a longer discussion of these in a previous version of the manuscript. The corresponding author takes full responsibility of not catching this in the proof-reading.**

There are quite a lot of figures with lots of subplots. I don't think it would be too detrimental to the message of the paper if only pH were shown in tables and figures. fCO<sub>2</sub> tables and figures could perhaps go in appendix. This would also have the advantage of making the pH plots easier to read. **We agree with the reviewer on this point and Figs. 2-4 and 8-10 have all been revised and now only shows the pH data. The results from the analysis of fCO<sub>2</sub> are all retained in all the tables, which renders an appendix or supplement unnecessary.**

The authors write 'carbon chemistry' throughout the paper. In most instances I am confident that they are referring to inorganic carbon parameters. Personally I interpret 'carbon chemistry' to include organic components and would prefer 'carbonate chemistry' when only inorganic components are being discussed. This is just my personal opinion though and I don't feel it significantly affects the clarity of the paper. **Reviewer #2 had the exact same concern regarding this terminology. The entire text has been revised and we now consistently use "CO<sub>2</sub> chemistry". We have also added a sentence in the introduction stating that we are hereafter using the term CO<sub>2</sub> chemistry when discussing the inorganic carbon cycle/chemistry in the ocean.**

Specific comments:

Page 15552 Line 26: You mention that a spatially uniform and constant Revelle factor may be a good assumption for the global average but then state that Bates et al (2014) found the Revelle factor to be increasing at all time-series stations. Isn't this a contradiction? **We agree with the reviewer that this section was poorly worded and there did appear to be a contradiction here. With uptake of anthropogenic carbon the Revelle factor will change globally and when looking for global averages it is safe to assume spatially uniform changes. However, local and regional variations are likely and have been observed at the different time series stations. This entire section has been rewritten for clarity.**

Page 15555 Lines 1-2: I really like the use of biomes to make best use of the available data. Perhaps readers would find it useful to know how the Fay and McKinley biomes are defined? Perhaps: 'The dataset was divided into the 17 biomes defined (using mixed layer depth, sea surface temperature and chlorophyll a) by Fay and McKinley (2014) as shown in Fig 1' **The sentence has been revised according to the reviewer's suggestion.**

Page 15556 Line 10: The sentence starting ‘The spatial bias’ would scan better if it read ‘The spatial bias correction has no statistically significant impact on the long term trend in most biomes, but...’ Also, I’m a little unclear on the logic here. The ANOVAs were performed to see if the corrections corrupted the analysis. However, in some biomes the spatial bias correction has an effect on the trend and you use this as a reason to retain the correction rather than evidence of it corrupting the analysis. **The sentence in question has been revised according to the reviewer’s suggestion. We did not do this analysis to see if the corrections corrupted the analysis, we did this analysis to see if the corrections have any statistically significant impact on the resulting trend and standard error. It is assumed that they are both necessary and must always be performed and we wanted to, in a simplistic manner, test this assumption. The section has been revised, and slightly expanded to show the actual results of the ANOVA test.**

Page 15558 Line 16: If I have understood correctly, you are excluding statistically insignificant trends from the area-weighted global average pH decrease because they have high RMSE and high decadal variability which is likely masking the long term trends. The global average is therefore, strictly, not a global average but an average of the areas you have found to have a significant trend. I think this should be made clearer in the text. **We agree with the reviewer that the overall global results were somewhat overstated and the text (both section 3.1 and the conclusions) have been revised to reflect that the global average is not really global as it excludes the Arctic and high-latitude Southern Ocean.**

Page 15559 Lines 8-9: Any ideas why there is greater variability in the SA-STPS than the NA-STPS? From the map of the biomes it looks like the SA-STPS includes more coastal regions than the NA-STPS so perhaps the greater variability comes from up-welling or riverine influences? **We have not undertaken any specific analyses to look into why the SA-STPS biome has greater RMSE and interannual variability compared to the NA-STPS biome. That being said we find it likely that *e.g.* the upwelling region off western Africa (which is included in the SA-STPS biome) influences the data in this region. A sentence has been added to the text reflecting this.**

Lines 10-11: You find a significantly lower trend in the NA-STPS than observed at BATS. This is interesting. Do you know why? **I agree this is interesting but it is beyond the scope of this paper to look in detail at the differences between the time series results and the**

**larger scale biome results. That being said, note that the pre-2000 data in the NA-STPS biome are highly variable. Such variability is not seen in the BATS data as presented in Bates et al. (2014) for example.**

Technical corrections

Page 15550 Line 8: I see a negative rate of decrease as an increase. I'd prefer 'decrease of 0.0018...' **This has been changed accordingly throughout the paper.**

Page 15553 Lines 14-17: This sentence needs shortening/clarifying. **The sentence in question has been rewritten for clarity and now reads: "This also shows that analyses of global pH trends, including the regional distribution of changes and the dynamics of the changing ocean CO<sub>2</sub> system, are required for a comprehensive understanding."**

Sincerely,

Siv K. Lauvset, on the behalf of all authors.