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# ***Interactive comment on “Biogeochemical variations at the Porcupine Abyssal Plain Sustained Observatory (PAP-SO) in the northeast Atlantic Ocean, from weekly to inter-annual time scales” by S. E. Hartman et al.***

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Responses to Reviewer’s Comments Hartman et al., Biogeochemical variations at the Porcupine Abyssal Plain sustained observatory (PAP-SO) in the northeast Atlantic Ocean

We would like to thank the reviewer and for the positive and constructive review of our manuscript. Here we address each comment, and list additional changes/updates that were made to the manuscript. References used within the responses have been listed

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at the end of this document.

The changes outlined in the document are further highlighted in the accompanying word document.

Re: Reviewer specific comments

1) The MLD is calculated using density criteria, while in Hartman et al., (2012) temperature criteria is used. I would like to see arguments for the different choices and possible differences in the calculated MLD.

The reviewer refers to an earlier paper where MLD was calculated using a temperature difference (in that paper the conclusion was that the new production estimates decreased from 2003 to 2005 irrespective of the two different MLD criteria used). In the current paper we followed the method of Holte and Talley (2009) by using their algorithm to calculate the MLD based on density difference. "Before deciding on a MLD definition an inter-comparison of many definitions commonly used in the literature was done such as density differences, temperature differences and density gradients (Kara et al. 2000; Thomson and Fine 2003; Montegut et al. 2004). A subset of the global density profiles calculated from the gridded temperature and salinity fields was use to compare the different methods. The depth of the mixed layer was estimated through visual inspection of over 3000 profiles (following a similar approach used by Fiedler (2010)). The Holte and Talley (2009) density difference algorithm gave the closest match with the visually estimated MLD (RMSD 29.38 m)." This has been added to the method section & the appropriate references added.

2) There is a mix between paragraphs which refer to both time periods and paragraphs that deal with one time period eg: page 12425, line 8-13.

We have tidied up the results section by dealing with the earlier then the later period for each variable, and it should now be much clearer (see text).

3) What about the Redfield ratio for the earlier time period? The paragraph refers to

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Fig4 and the actual time period should be stated in the figure text.

The Redfield ratio for the earlier time period was dealt with by Kortzinger et al., 2008. We acknowledge that the figure legend could be clearer and this has been changed to reflect the time period covered.

4) At p.12425 (last paragraph) it is pointed out that wind speed peaks before the peak in nitrate and pCO<sub>2</sub>. This is difficult to see since the figures consist of data from two time periods. The effect could be illustrated by adding a symbol or separate figure.

As it is so unclear in the weekly data presented we have removed this comment from the text.

5) In the same paragraph the average wind speed is mentioned is this the annual average, please clarify?

This is the annual average and has been clarified in the text.

6) The manuscript is lacking in a discussion of error estimates. The reader doesn't get an idea of precision in nitrate, chl or pCO<sub>2</sub> measurements. Some error estimates are mentioned in the conclusion paragraph but the authors should elaborate more around these values. Also the error introduced by calculating Ct and At should be briefly mentioned. We have put precision and uncertainty estimates for each measurements and calculated variables into the method section. Specifically:

For nitrate data: "Nitrate concentration measurements were initially made using wet chemical NAS Nitrate Analysers (EnviroTech LLC, USA) precision 0.2  $\mu\text{mol l}^{-1}$ , as described in Hydes et al. (2000) with twice daily sampling frequency and internal calibration as described by Hartman et al. (2010). From 2010 additional higher frequency inorganic nitrate measurements were made using UV detection methods (ISUS, Satlantic), precision 1  $\mu\text{mol l}^{-1}$ ."

For Chl data: "The quoted precision for the fluorometers is 0.04% and the text has been changed. We have also noted that the fluorescence output can only provide an

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approximation of chlorophyll a. The fluorescence/chlorophyll a calibration ratio changes throughout the year, due to variations in the phytoplankton species composition."

For pCO<sub>2</sub> data: "Although measured by different instruments, the two p(CO<sub>2</sub>) data sets were calibrated in a similar way to make them comparable: the sensor outputs were calibrated against p(CO<sub>2</sub>) values calculated from dissolved inorganic carbon (DIC) and total alkalinity (TA) from discrete samples taken at the mooring site during deployment/recovery cruises; and plausibility check were made with underway p(CO<sub>2</sub>) measurements around the PAP site (see below). The 2003-2005 data were previously published (see Körtzinger et al., 2008 for details) with a precision of 1  $\mu$ atm and an accuracy estimated as 6-10  $\mu$ atm. The 2010-2012 data have a similar precision (1  $\mu$ atm) and accuracy (6  $\mu$ atm). "

For calculated TA: "The TA was calculated from Argo temperature and salinity (30 m), following the relationship for the North Atlantic developed by Lee et al. (2006) with a uncertainty of  $\pm 6.4 \mu$ mol kg<sup>-1</sup> (Lee et al., 2006)." For calculated DIC: "Using TA and p(CO<sub>2</sub>) to calculate DIC introduces an error in the order of 6  $\mu$ mol kg<sup>-1</sup>."

7) When the air-sea CO<sub>2</sub> flux is discussed it is claimed that the long term wind speed values have increased and high wind events are earlier in the year. Please add relevant references.

There are signs that the wind speed is increasing and the intensity of storms is predicted to increase (Knutson et al., 2012) . We have added a reference that deals with this and we have reworded the text accordingly.

Technical corrections

1) Changes in references p.12422 Nightingale & p12422 weiss Both now changed, thank you.

2) Modification to text to include figure numbers as follows These have all been changed:

p12427 'start of the 2011/2012 winter (Fig. 5b) coinciding with an earlier increase...'  
p12427 'mixing (Fig. 3b)...' p12427 '... low seawater pCO<sub>2</sub> (Fig. 2a) and high wind speed (Fig. 5b)...'

3) p12430 'additional 1m measurements of pCO<sub>2</sub>...' please rewrite as sentence is not understandable

The sentence has been clarified to 'From 2013 additional measurements of p(CO<sub>2</sub>) will be made at the site, at the shallower depth of 1 m, and should further improve the SOO comparison'.

4) Reviewer comment: re p12435 Fig 1. It would be preferable to include the general circulation pattern in this figure.

As this is only a small part of the North Atlantic it doesn't seem appropriate to draw arrows on to represent the circulation. We propose that adding the bathymetry is more useful as you can clearly see the shelf break and ridge. We hope that this is satisfactory for the reviewer as an improvement on the previous Fig. 1?

5) Fig.4 has to be made clearer eg: use 'spring' in the figure with 'April-June' in the legend. Consider putting rates in the text and not the figure.

We have changed figure 4 to reflect both aspects of the reviewers comments and believe that it is now much clearer

added references:

Fiedler, P.C. (2010). "Comparison of objective descriptions of the thermocline." *Limnology and Oceanography-Methods* 8: 313-325.

Kara, A.B., P.A. Rochford and H.E. Hurlburt (2000). "An optimal definition for ocean mixed layer depth." *Journal of Geophysical Research-Oceans* 105(C7): 16803-16821.

Knutson, T.R. McBride, J.L. Chan, J. Emanuel, K. Holland, G. Landsea, C. Held, I. Kossin, J.P. Srivastava, A.K. Sugi, M. 2010. Tropical cyclones and climate change.

Nature Geoscience. 3. 157-163

Montegut, C.D., G. Madec, A.S. Fischer, A. Lazar and D. Iudicone (2004). "Mixed layer depth over the global ocean: An examination of profile data and a profile-based climatology." *Journal of Geophysical Research-Oceans* 109(C12): 20.

Thomson, R.E. and I.V. Fine (2003). "Estimating Mixed Layer Depth from Oceanic Profile Data." *Journal of Atmospheric and Oceanic Technology* 20(2): 319-329.

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