

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.

Anonymous Referee #1

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General comments: I have read the paper “Biogeochemical variations at the PAP-SO in the northeast Atlantic Ocean from weekly to inter-annual time scales” with great interest. The authors have used new biogeochemical data from PAP-SO and nearby from the time period 2010 to 2012 and compared them with a previous dataset from 2003–2005. Time series data are of particular value for process understanding and it is to wish that the data series from PAP-SO will continue into the future. The language is good and clear.

Specific comments: The MLD is calculated using density criteria, while in Hartman

C5114

et al. (2012) temperature criteria is used for MLD calculation. I would like to see arguments for these different choices and possible differences in the calculated MLD. There is a mix between paragraphs which refer to both time periods (2003–2005 and 2010–2012) and paragraphs which only deal with one time period. E.g. at page 12425, line 8–13, which only deals with the latter time period (I assume). What about the Redfield ratio for the first time period? This paragraph refers to Fig. 4, and the actual time period should be stated in the figure text. At p. 12425, in the last paragraph, it is pointed out that wind speed peaks before the peak in nitrate and pCO₂. This is difficult to see since the figures consist of data from two time periods, and the effect should be illustrated in some way, e.g. adding some kind of symbol or shape in the figures to focus at specific changes. Or possible a separate figure. In the same paragraph the average wind speed is mentioned; is this annual average? Please clarify. The manuscript is lacking a discussion of error estimates. The reader doesn't get any information about precision of the nitrate, chlorophyll or pCO₂ 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. When the air-sea CO₂ flux is discussed it is claimed that the long term wind speed values have increased and that high wind events are starting earlier in the winter. Please add relevant references for these statements.

Technical corrections: p 12422, l 18: Nightingale (2000) should be changed to Nightingale et al. (2000); p 12422, l 20: Weiss and Cohen (1974) should be changed to Weiss (1974); p 12427, l 19: “. . .start of the 2011/2012 winter (Fig. 5b) coinciding with an earlier increase . . .”; p 12427, l 20: “mixing (Fig. 3b) . . .”; p 12427, l 27: “. . . low sea-water pCO₂ (Fig. 2a) and high wind speed (Fig. 5b) . . .”; p 12430, l 4: “additional 1 m measurements of pCO₂ . . .”. Please rewrite as this sentence is not understandable; p 12435, Figure 1: It would be preferable to include the general circulation pattern in this figure; p 12438, Figure 4: The figure has to be made clearer, e.g. use “spring” in figure and explain that this is April–June in figure text. Consider putting the rates in the figure text and not in the figure;

C5115

C5116