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Discussions

Interactive comment on "Links between surface productivity and deep ocean particle flux at the Porcupine Abyssal Plain (PAP) sustained observatory" *by* H. Frigstad et al.

Anonymous Referee #2

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Frigstad et al. present observations from the Porcupine Abyssal Plain (PAP) time series station / observatory and related interpretation. The dataset presented covers up to a decade of data of observations from automated devices in the surface mixed layer (euphotic zone) and from sediment traps at 3000m. The PAP site is one of the very few open ocean time series sites outside oligotrophic waters. Though parts of the data have been published earlier this is a timely overview, providing also interesting additional analysis of the combined dataset. Using data from Argo, remote sensing and ocean circulation models this, by nature, spatially limited data are set into a wider context. Running such a site and putting such a dataset together is clearly a significant effort of the group lead by the senior author, Richard Lampitt.

I recommend publication with minor corrections as indicated below.

My largest concern is related to the interpretation of particle tracking, NPP and flux. See the comments on section 2.4, 4.2 below.

Abstract

p5170: The Redfield C:N ratio is 6.6. The observed C:N ratio of NCP was 12, I suggest to not confuse younger readers and the speak about 'C:N-ratio of 12' and not to refer to Redfield here.

Introduction

p 5170, l 26 (and elsewhere): delete 'full depth'

p5171, l 9-10: The Sabine reference for the phrase 'biological carbon pump is key to understanding the global carbon cycle' is not justified. In the last paragraph Sabine speaks about potential biological feedbacks to OA in a very general sense. Hence you shouldn't use that reference here. You should refer to papers that give evidence to this statement and not just use such a phrase themselves in either the intro or the outlook! The Falkowski reference is well suited here, perhaps refer to Volk and Hofferts centennial paper in addition, or some significant post-1998 overview paper.

l22-24: Is it really the 'multitude of methods . . . that lead to a poor understanding of NCP. . .'? I understand Quay rather in the following sence 'Unfortunately, there are only a few sites where multiple NCP methods have been compared (e.g., JGOFS study sites, BATS and ALOHA time series sites).' (p2). Multiple methods may be rather an advantage, in the absence of a âĂŽgold standard'.

p5171, l24 – p 5172, l6 may be shortened

p 5172, l 13-15: Is the correct reference for Lampitt et al 2018 given? From my memory (but the senior author should know better), the Royal Society paper from the same year is referred to here, right?

l 21-24: suggest to write: 'transfer efficiency has often been used to describe the efficiency'. Rational: a) the POC based metric ignores DOM, b) see papers by Marinov and co-authors

Overall, DOM as a pathway is ignored completely in the paper. Its role in sequestration may be less understood, but you might want to mention this pathway (and your ignorance of it in the analysis) at least once in the intro.

Data and methods

p 5173, l24: rewrite: '(2010). Briefly . . ..'

Same paragraph: Perhaps mention at least two more details: a) why is formaline addition not an issue for POC measurements (with reference to a study that gives respective evidence). b) what about losses of POC to the supernatant in the cup until splitting of samples, losses e.g. to DOC (see e.g. Kähler and Bauerfeind, L&O, 2001). This is (evidently ?) no issue in your traps?

p5175, l23-24: I am not really sure about the meaningfulness of Lee's T-S to Alk relationship. In particular the T-part. See e.g. Friis et al., 2003, GRL. In your data, (Fig. 2) what is driving the seaonality of ALK? T or S? How sensitive is your DIC seasonality to the computed ALK. What if you assume no seasonality of ALK, e.g. by taking the annual mean of your computed ALK together with your seasonally varying pCO2 data? Hopefully, that gives almost identical DIC values, compared to the presented ones. Please check into this.

p5176, l 6 (and elsewhere!!) Ko\"rzinger has an o-Umlaut, also Ka\"hler has an a-Umlaut. Please check the ms carefully for correct spelling of authors! Go back to the original papers to check, if needed.

l 24-27: You should include the error from ALK-S-T in your error budget, and refer to what I proposed above in the text.

p 5177, l 4: 'contribution from delDICmix was assumed negligible': can it be? If I recall

correctly Gruber et al. 1998 (DSR ?) used 13C-CO2-data at Bermuda to constrain the role of mixing to a seasonal surface ocean DIC budged. There it was important, I think. I suggest, that you at least discuss this limitation of your estimate briefly, in particular in terms of sign for C:N – NCP estimate, and mention the Gruber and related studies.

l 7 vs. p 5176 l 28-29. This is a little unclear. I suggest you rewrite Equ3 to explicitly include the gas exchange term.

l 21-23: Please clarify in the text whether you computed NPP, or downloaded it from the web site.

Section 2.4: The particle tracking analysis is done here much better than in some older papers of the senior author which used moored current meters. This is acknowledged by the reviewer. However, here and also later in the paper, you seem to take the transports in the model to be fully consistent with the real ocean patterns and distribution of NPP as seen by the satellite. Why should that be the case? The best you may hope for, I think, is that the applied physical model has the right statistics of transports compared to the real ocean. Whether the eddies (etc.) are at the right place at the right time in the model vs. the real ocean is not known. Hence, the combination of particle tracking, remote sensing, and the deep traps stands on somewhat slippery ground. You need to mention and discuss that – unless you can provide hard remote sensing evidence (e.g altimetry, sst, sss patterns) supporting that your model behaves perfectly in that sense. Recently, Jamie Wilson and co-authors had a very nice paper (currently in review in BGD, I think) demonstrating how deficient similar combinations of models and reality can be.

p 5179, l 24-25: Could you explain a little further from your data why there is no seasonal signal in CO2-fluxes?

l 25: delete: 'also'. There is no (causal) relationship between the two issues, I think.

p 5181, l 11: rewrite: 'for surface sea-water pCO2 . . .'

p 5183, l8: Schneider was not the first to report this. Please check for example papers of Cindy Lee from the early 80s, e.g. Lee and Cronin, 1984 and Wakeham et al. 1984. Please do not cite only convenient references, but also the original literature.

l9: rewrite 'ratio may influence . . .by about 20 ppm . . .'

l 17: 'basin was too complex' is awk and not to the point. I guess GS's mixing model did not resolve more than 2 or 3 endmembers? Please check and present carefully.

p 5183 l 22 to p 5184, l 9. I think this paragraph can be deleted. You follow an idea that does not work out well, for reasons published by others decades ago.

p 5184, l 17; '64 and 207' is a little awk, explain with one more sentence why the difference is so large.

l21: is De La Rocha and Passow the appropriate reference for 'export ratio'. This term is much longer in use! Also the reference of that paper given reference section is not complete!

l 22: '115', please give error bar of your mean value

Section 4.2 & Conclusion. See my comments on section 2.4. You need to discuss the issue stated above.

References: See my comment of o\" and a\" for Korzinger, Kahler, (but Koeve is correct, :-)

Overall, I enjoyed reading the paper.

Interactive comment on Biogeosciences Discuss., 12, 5169, 2015.