Author comment to Anonymous Referee #2

We thank anonymous Referee #1 for his/her constructive criticism and valuable comments. In the following we address the points raised, with referee comments in boldface and author responses in normal typeface.

General comment:

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.
We thank the reviewer for the positive comment, and have addressed the issue regarding particle tracking in the response below.

Specific comments:

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.
We agree with the reviewer, and have revised the text accordingly: "The C:N ratio was high (12) …"

Introduction
p 5170, l 26 (and elsewhere): delete 'full depth'
We have deleted "full depth" throughout the revised text.

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.
We have deleted the Sabine reference in the revised text, and the sentence now reads: "and therefore quantifying the biological carbon pump is key in understanding the global carbon cycle (Falkowski et al. 1998)."

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 gold standard'.
We agree that this sentence can be misunderstood, and have revised the text to: "It can be challenging to compare between techniques and there is …"

p5171, l24 – p 5172, l6 may be shortened
We go into a bit of detail explaining the differences in NCP and export flux in this paragraph. We felt it was important to explain these differences, because the study computes both NCP and export flux, and compare the two variables in the results and discussion sections. We therefore believe that it is important to give the reader some background, and that these sentences can be justified.

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?
The reviewer is correct, and we have inserted the correct reference and updated the reference list.

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.
The manuscript has been revised as suggested: "… and has often been used to describe the efficiency of the biological pump …"

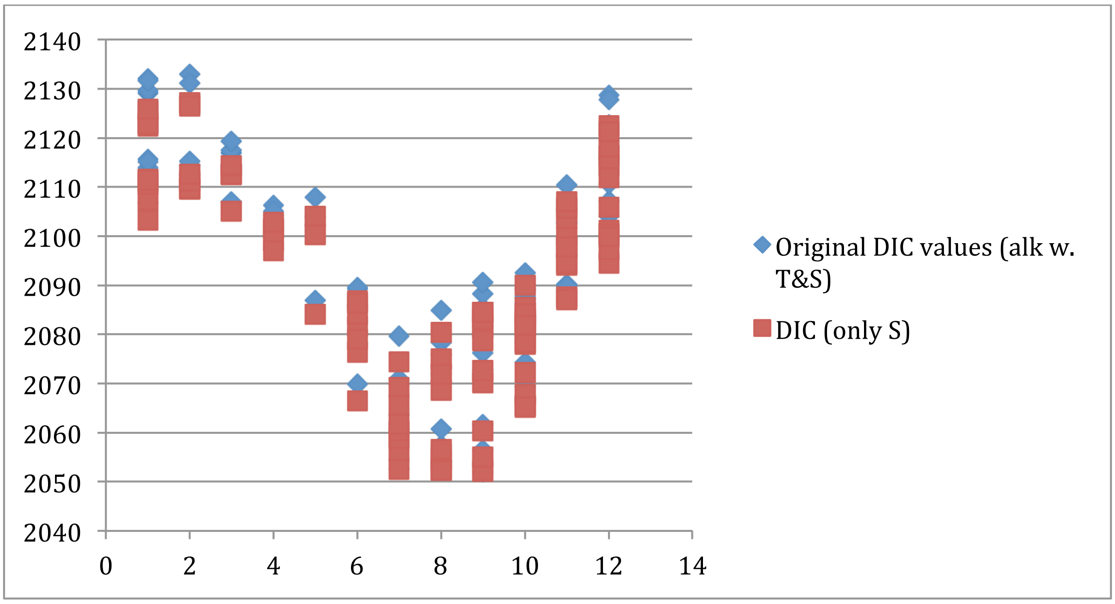
It is noted in p 5171, l 27 – p 5172, l 1 that one of the differences between NCP (as calculated in this study) and export flux (calculated as POC flux at a nominal depth), is that the NCP estimate will include the contribution of DOC. However, have added to the revised manuscript that the POC-based metric of calculating export flux and transport efficiency does not include the DOM pathway. Revised text on p. 5172, l 23: "It should be noted that the POC-based metric of calculating export flux and transfer efficiency does not include the contribution from DOC."

Data and methods
p 5173, l24: rewrite: "(2010). Briefly …"
The text has been revised accordingly.

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?
a) We use analytical grade (AnalR or NORMAOUR) formalin as directed by the JGOFS protocols for preservation. There are numerous unpublished studies that show that somewhat surprisingly formalin does not affect POC. The most cited paper (Knauer et al 1984) indicated some losses.
b) Kähler and Bauerfeind worked with shallow sediment traps, which are much more susceptible to swimmer contamination. At PAP traps are at 3000m and 100mab and have few swimmers therefore negligible DOC leaching.

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.
This is a very relevant issue raised by the reviewer. In Lee's T-S to Alk relashionship, the

salinity coefficients are several orders larger than those of the temperature-part. Thus, the seasonality is driven mainly by salinity. To verify this, we have recalculated Alk from salinity alone (following Nondal et al. (2009)), and there was still seasonal variation in calculated Alk values. Furthermore, this change in the calculation of Alk had only a negligible effect on the seasonal variation in DIC values (figure below).



For this reason, and since Lee originally included temperature to account for the nutrient cycle, we chose to keep Alk values obtained with Lee's relationship. We also note that the main issue in Friis et al. (2003) concerned the use of zero intercept during the normalization of Alk values to constant salinity. This normalization is not used in this manuscript.

p5176, l 6 (and elsewhere!!) Kon"rzinger has an o-Umlaut, also Kan"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.
The text has been revised accordingly.

p5176, 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.
This is the same issue raised by reviewer#1, and the text has been revised to include the error from calculated Alk (from temperature and salinity) instead of measured Alk as in the previous version of the manuscript:
"… which is smaller than the total error associated with the calculation of DIC from estimated TA and measured fCO2 of ±0.85 mol C m^{-2}. The latter was determined by propagation using the method described in Dickson and Riley (1978), together with the errors in the estimated TA values (±6.4 µmolkg^{-1}; Lee et al. 2006) and measured pCO2 (±2 µatm; Wanninkhof et al., 2013)."

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.
This is the same issue as raised by reviewer#1, and we repeat the response given on this issue: It is stated in Sect. 2.3 that the monthly changes in DIC and NO_3 can be attributed to changes caused by air-sea gas exchange (for DIC), physical mixing processes and biological drawdown. The physical mixing processes, such as vertical entrainment, diffusion and

advection are difficult to account for without proper measurements. In Kortzinger et al. (2008) they also assume that the contribution from these three mixing processes are small and negligible in the calculation of NCP, but acknowledges that a "full mixed layer budget cannot be constructed". Only a simplified budget is possible, under certain limitations and for restricted periods. We have followed the same rationale, calculating NCP and new production for the period when the MLD is stable and where biological drawdown is believed to play a dominating role in monthly changes in DIC and NO_3. We do, however, acknowledge the limitations in this approach and will elaborate on the uncertainties associated with mixing in the manuscript.

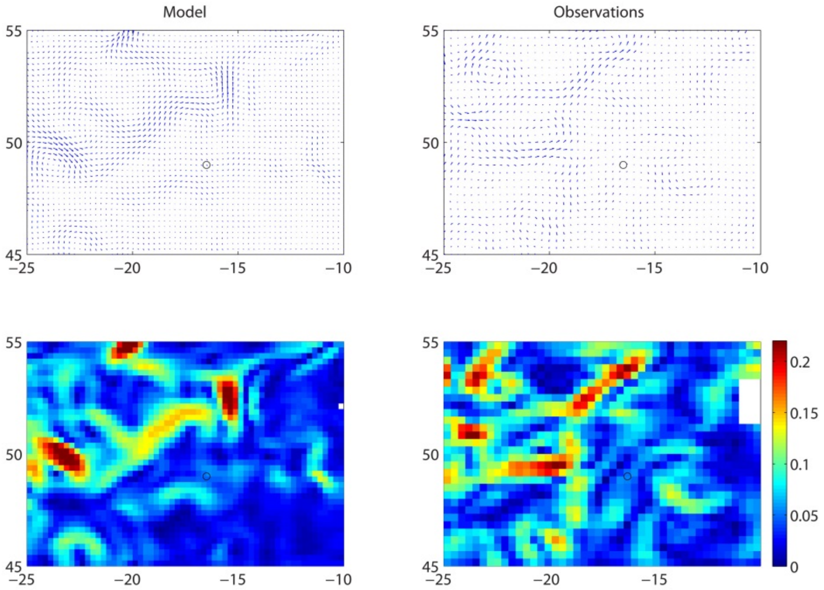
The manuscript has been revised as follows:
Physical mixing processes, such as vertical entrainment, diffusion and advection, will to some degree contribute to monthly DIC changes, however are difficult to quantify without information on vertical and horizontal gradients. Following the approach by Kortzinger et al. (2008) we have performed a simplified budget calculation for the summer period when the mixed layer is relatively stable and the biological drawdown in DIC (and NO_3) is strong. Therefore the contribution of ΔDIC_{mix} was assumed negligible, and ΔDIC_{BP} was assumed to be largely determined by NCP (excluding the effect of calcification).

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.
The changes in DIC concentrations caused by air-sea gas exchange are calculated from the air-sea flux and MLD in Eq. 2. We have revised the text to show how this is included in the terms of Eq. 3 (the contribution of air-sea gas exchange is added to the DIC concentrations because the flux is positive throughout the year):
".. corrected for the effects of air-sea gas exchange ($\Delta DIC^{GasCorr} = \Delta DIC_{obs} + \Delta DIC_{gas}$)"

l 21-23: Please clarify in the text whether you computed NPP, or downloaded it from the web site.
The NPP data were downloaded from the website listed in Table 1, and this is already stated in the text. We have clearified this sentence:
"The NPP data were downloaded … ".

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.
To demonstrate that the NEMO model produces currents that are consistent with observations, we have plotted below the surface currents (geostrophic + Ekman) derived from satellite altimetry and wind data (downloaded from http://www.oscar.noaa.gov/). The mean observed current vectors (top panels) and speed (lower panels) for 2008 are plotted alongside the modelled currents in the figure below. The model reproduces well the main features of the circulation in the region, i.e. the band of strong northeastward currents in the NW quadrant of the domain and the relatively quiescent SE quadrant. The magnitude of the currents are also in a very similar range in the model and observations. Note that satellite observations can necessarily only supply information on the surface currents and so the

analysis we present in the manuscript would not be possible without relying on modelled currents. The paper mentioned by the reviewer by Wilson et al. discusses the issues of accurate model circulation in the context of thousand year spin-ups and attempts to reproduce the 3-D distribution of phosphate in the oceans, i.e. very different time and space scales than we consider here.



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

The air-sea CO_2 flux does not show corresponding winter to summer variations as for example pCO_2 in Fig. 2, largely because of the balancing effect of the seasonal cycles in pCO_2 and wind speed (U_{10}). During spring and summer the reduction in wind speed is compensated by the effect of increased ΔfCO_2 leading to overall small variations in air-sea flux of CO_2 throughout the year.

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 …'

The text has been revised accordingly: "The sediment fluxes had high…"

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.

The text has been updated to include the Lee and Cronin (1984) reference: "Studies have shown an increasing C:N of sinking material due to preferential remineralization of nutrients (Schneider et al. 2003; Lee and Cronin 1984) …"

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

The text has been revised accordingly: "ratio may influence atmospheric CO_2 concentrations 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.

We agree with reviewer, and deleted this part of the sentence as it is not essential to the argument. The revised text now reads: "However, the deep ocean remineralization rates of Anderson and Sarmiento (1994) did not include the Atlantic Ocean."

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.
We agree with reviewer that this paragraph does not give added value to the manuscript, and have deleted it in the revised text.

p 5184, l 17; '64 and 207' is a little awk, explain with one more sentence why the difference is so large.
The Lampitt et al (2008) is an integrated estimate based on the deployment of the Pelagra sediment trap during summers of 2003 to 2006, while the estimate from Tomalla et al. (2008) is based on a simple sample close to the PAP-site (48.6N;where material was collected using a pump). The samples therefore represent different time (and also spatial scales, due to the location not being identical), and therefore represent the natural variability in POC flux. The different time scales is emphasized in the revised text:
"determined to be in the range between 64 and 207 mg C m^{-2} d^{-1} (based on measurements from a single cruise and long-time trap data; Lampitt et al. 2008, Thomalla et al. 2008)."

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!
We have changed the reference in this sentence to Dugdale and Goering (1967), which is the reference given in the introduction where the term is introduced. The revised text now reads: "… divided by the NPP (Dugdale & Goering 1967)."

l 22: '115', please give error bar of your mean value
The standard deviation of the literature values given in the text is ± 61 mg C m^{-2} d^{-1}. This uncertainty is added to the revised text:
"Using an average of the above values for POC flux out of the surface layer of 115 ± 61 mg C m^{-2} d^{-1}".

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

References: See my comment of on" and an" for Korzinger, Kahler, (but Koeve is correct,
:-)
These references have been corrected in the reference list as well

Overall, I enjoyed reading the paper.
Thank you!

References (not cited in article):
Kähler P, Bauerfeind E. Organic particles in a shallow sediment trap: substantial loss to the dissolved phase. Limnol Oceanogr 2001, 46:719–723

Knauer, George A.; Karl, David M.; Martin, John H.; Hunter, Craig N. In situ effects of selected preservatives on total carbon, nitrogen and metals collected in sediment traps. Journal of Marine Research, Volume 42, Number 2, May 1984, pp. 445-462(18)
Nondal, Gisle; Bellerby, Richard G. J. ; Olsen, A; Johannessen, T. Optimal evaluation of the surface ocean CO_2 system in the northern Atlantic using data from voluntary observing ships. Limnology and Oceanography: Methods. 7, 2009, 109-118