Author comment to Anonymous Referee #1

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 data from the PAP site in the northeast Atlantic, with estimates of seasonal NCP and new production that will be useful for the scientific community. A particle tracking method is used to examine source regions of material within a sediment trap, and the manuscript demonstrates that the site shows high flux attenuation by calculating the export ratio and transfer efficiency. Time-series stations such as the PAP site are of great importance in evaluating changes in the environment and understanding these important biogeochemical processes. With further clarification and inclusion of the errors involved, and mixing influences this paper could be a valid contribution to productivity estimates in this region. If the following issues are addressed, I support the publication of this manuscript.

We thank the reviewer for the positive comment, and have addressed the errors involved with calculating NCP and the contribution from mixing in the responses below.

Specific comments:

p.5176 line 25. The error associated with the calculation of DIC from calculated TA and measured CO2 should be quantified. The error quoted as 3 μmol kg^{-1} is the measurement uncertainty if DIC were measured discretely, which is not the case in the method presented. Please quantify this error and calculate how this error propagates into the NCP estimates.

We have calculated the total error associated with the calculation of DIC from estimated TA and measured fCO2 of ± 0.85 mol C m^{-2}. This 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). The propagated probable error is larger than the uncertainty due to monthly variation in NCP$_{MLD}$ of ± 0.27 mol C m^{-2}, and we have chosen to use the propagated error to give a conservative estimate of the uncertainty in our NCP estimates. We have therefore updated the uncertainty in NCP estimates in the results section.

The text has been revised as follows:
"A different source of uncertainty in NCP and new production estimates come from measurement uncertainty, which propagates into calculated values. However, for new production, the variability associated with measurement uncertainty is negligible compared to the natural variability. The measurement uncertainty for the NO3 sensor given by the manufacturer (Satlantic ISUS V3 sensor) was 2 μmolkg kg^{1}. This gives a propagated probable error in the NO3$_{MLD}$ estimates of < 0.05 mol N m^{-2} (calculated from the square root of the sum of squared errors), which is considered negligible compared to the uncertainty associated with monthly variation of ± 0.14 mol N m^{-2}. Therefore we present the uncertainty related to monthly variation in NO3$_{MLD}$. Conversely, the total uncertainty associated with monthly variations in NCP$_{MLD}$ was ± 0.27 mol C m^{-2}, 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). We have therefore chosen to present the propagated probable error as a conservative estimate of the uncertainty in our NCP estimates.

p.5178 line 11 Please include justification/reference for why all particles are assumed to have a sinking speed of 100 m d^{-1}.

Observations at the PAP observatory have estimated the particle sinking speed as between 60 and 180 m day^{-1} (Riley et al., 2012; Villa-Alfageme et al., 2014). The value we choose of 100 m day-1 falls in the middle of this range. Faster particle sinking speeds would result in a smaller source region and vice versa for slower sinking particles.

The text has been revised as follows: "…, which is chosen because it falls in the middle of the range observed for particle sinking speeds at the PAP observatory of between 60 and 180 m day^{-1} (Riley et al., 2012; Villa-Alfageme et al., 2014)…"

p.5177 line 4 and figure 3 How can mixing be assumed negligible? Kortzinger et al. (2008) demonstrate that mixing plays an important role in increasing NO3 and DIC concentrations in the mixed layer during winter months at the PAP site. Using the method presented, the positive NCP and new production values in figure 3 are not necessarily solely due to biological drawdown, but are actually where the biological drawdown exceeds mixing and remineralisation. This assumption should be discussed further, and made clearer to the reader.

This is a valid comment, and an issue we discussed frequently in the preparation of this manuscript. 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 calculation of seasonal NCP and new production 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).

Figure 3. Why are the productive periods (grey shaded) different for NO3MLD and NCPMLD? Using the MLD it should be possible to determine if the mixed layer is deepening, and therefore give an indication of when high concentrations of DIC and nutrients are being entrained in to the mixed layer. The summer months before the mixed layer deepens may be a better period to calculate NCP and new production

The period NO3$_{MLD}$ and NCP$_{MLD}$ are positive, is when there is a net decrease in NO$_3$ or DIC concentrations, respectively, in the mixed layer caused by biological drawdown. As can be seen in the monthly climatology in Fig. 2, DIC concentrations starts increasing from August, which causes the sign of the NCP$_{MLD}$ to change, from positive to negative. NO$_3$ concentration has a very small decrease from July to August, before there is a small increase from August to September. Therefore the NO3$_{MLD}$ becomes negative in September, one month later that for NCP$_{MLD}$. These months with very small changes in concentrations have error bars that are almost as large as the NO3$_{MLD}$ and NCP$_{MLD}$ itself, and it is therefore little use in speculating

into a reason for the difference in timing of negative values.

The MLD climatology is also shown in Fig. 2, and the MLD shallows rapidly from around 250m in March to 50m in May. The MLD remains fairly stable at 50m until October. We chose to use the difference in MLD between consecutive months, and not to integrate over a fixed time period. We find the strongest NCP_{MLD} and $NO3_{MLD}$ before the onset of shallow stratification in summer, however believe it gives value to show the monthly changes, and the stabile MLD and nutrient depletion in summer indicates that there is not a large contribution from entrainment during these months.

Figure 3. Please include a table of the different years used to calculate the monthly values and the inter-annual variability. This would be useful to the reader and might also explain why some of the error bars are so small in the figure.
We have compiled a table (se below) with monthly mean, SD and number of samples (n) for each year we have data (taking the years vs. months would mean that the table is very large). The means and SDs are identical to the values given in Fig. 3, and from this table it is clear that for most months we have data from 2-4 different years. This is also described on p. 5176 l. 3-16, where the method of calculating the $NO3_{MLD}$ and NCP_{MLD} is explained. However, one small error was discovered in the compilation of this table. For NCP_{MLD} there is only data from one year (2004) in March and April, and therefore we cannot calculate the SD, and consequently there is no error bar in Fig. 3. This is now explicitly described in the revised manuscript. In addition, we added which months are lacking error bars in Fig. 3 in parenthesis in the text:
"However, for most months of the year data from at least two to four different years were available, and NCP_{MLD} and $NO3_{MLD}$ was estimated two to four times for each of these months. This allowed the quantification of the average seasonal NCP_{MLD} and $NO3_{MLD}$ and variability (expressed as one standard deviation; SD) from the PAP time series. With the exception of March and April for NCP_{MLD}, when there was only data for one year (2004) and consequently SD could not be calculated (error bars missing in Fig. 3). The monthly changes in equations 3 and 4 (below) were computed in a circular manner i.e. the change in the 12th month is the difference January minus December. For $NO3_{MLD}$ there was only data for one year in December, and consequently the SD could not be calculated. For NCP_{MLD} there was data from two different years in December and three years in January, however they coincided only for 2004. Therefore, the variability could not be determined for December for either NCP_{MLD} and $NO3_{MLD}$ (error bar missing for December in Fig. 3)."

In addition, we add which months are lacking error bars in the caption to Fig. 3:
"Figure 3. Monthly changes in MLD integrated NO_3^- (top) and NCP (bottom) with $\pm$ 1 SD (vertical bars). The shaded grey area indicates the months over which the seasonal new production (0.37 $\pm$ 0.14 mol N m^{-2}) and NCP (4.57 $\pm$ 0.27 mol C m^{-2}) were calculated. For NCP_{MLD} no SD could be calculated in March and April, and SD is also lacking for December for both NCP_{MLD} and $NO3_{MLD}$ (see Sect. 2.3 for calculations)."

Seeing as how the information in the table is identical to Fig. 3, and we have expanded on the which months there are no error bars due to missing data we feel that adding this table to the revised manuscript would not justify the extra space this would require.

	Jan	feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Okt	Nov	Des
NO3$_{MLD}$												
	-								-	-	-	-
Mean	0,179	0,126	0,107	0,064	0,031	0,029	0,016	0,002	0,013	0,161	0,070	0,142
SD	0,372	0,072	0,109	0,032	0,001	0,017	0,008	0,001	0,012	0,109	0,229	NA
n	3	3	2	2	2	2	2	2	3	3	3	1

NCP$_{MLD}$

	-							-	-	-	-	-
Mean	0,713	0,294	2,351	0,768	0,667	0,265	0,220	0,202	0,261	0,934	1,463	1,532
SD	0,822	0,267	NA	NA	0,010	0,041	0,027	0,153	0,244	0,404	1,072	1,065
n	3	3	1	1	2	3	3	4	4	4	4	2

p.5192 figure 1. The MLD looks like it goes shallower than 30 m during the summer, would this influence your calculations of NCP and new production within the mixed layer as your sensors are at 30 m? Please clarify this and discuss if necessary.
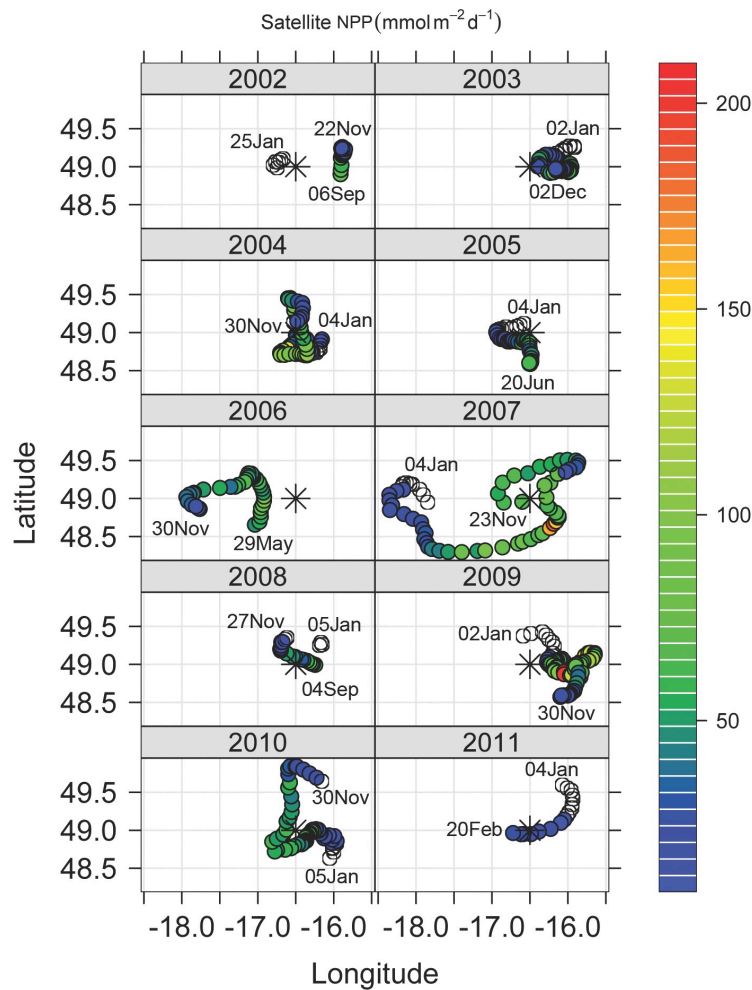
It is the difference in MLD between consecutive months that is used to calculate the NCP$_{MLD}$ and NO3$_{MLD}$, and from May-August this is shallower than 30m (23.7m, 17.1m, 16.3m and 21.3m, respectively). When the MLD is shallower than the sensors measuring NO$_3^-$ and DIC concentrations, this could imply that the drawdown from biological activity within the mixed layer is stronger (and thereby reaches lower concentrations) than what is actually measured by the sensors at 30m. However, the NO$_3^-$ concentrations in the mixed layer could become depleted, and thereby limit further biological drawdown. On the other side, the biological drawdown could be stronger at 30m than in the mixed layer due to more supply of nutrients than what is available within the mixed layer. Which of these processes would be dominating at the PAP observatory is impossible to determine without the nutrient data. The former process could imply that our estimates of NCP and new production are on the low side (because additional drawdown takes place within the mixed layer), however the latter could imply that our estimates are higher than within the mixed layer due to more readily available nutrients. This is an additional source of uncertainty, and we have added a short description of this in the discussion in Sect 4.1:

"… during this period of rapid stratification. From May to August the MLD is slightly shallower than the depth of the sensors at 30m, which could influence our estimates of NCP and new production. This uncertainty is impossible to quantify without measurements from within the mixed layer, although the effect is believed to be minor. It could potentially overestimate biological production due to more readily available nutrients at 30m than within the mixed layer, or lead to an underestimate due to additional drawdown within the mixed layer. …"

Figure 3 and figure 4. It would be beneficial to the reader if the units of CO2 flux, NCP, new production, Satellite derived NPP were all in mmol m^{-2} d^{-1} so that they are more easily comparable. Please make units consistent throughout the manuscript.

We agree with the comment by the reviewer and have converted the units in Fig. 4 to mmol m^{-2} d^{-1}.

The revised Fig 4 is shown below; here we have also included the other issues brought up by the reviewer regarding this figure:

Satellite NPP $(\mathrm{mmol\,m^{-2}\,d^{-1}})$

p.5199 Figure 4. Please add a time dimension to this plot, it is difficult to trace the particle with time without knowing which month the coloured dots are representing. It may also be useful to include a 100km box around the PAP site for reference, and to demonstrate that the source region is outside of the box in some years.
Time labels have been added to mark the start and end of data for each year in Fig. 4 (se above). We did experiment with adding a 100 km box around the PAP site, however there is all ready a lot of information on this plot and unfortunately the box made the figure too cluttered and difficult to interpret for the reader.

The caption to Fig. 4 has been revised as follows: "…The start and end dates of the data for each year is indicated in the panels".

Technical corrections:

p.5171 line 1 Is nitrate being measured, or nitrate + nitrite as it is in Hartman et al. 2012. On first use of NO3 please clarify this within the manuscript.
It is nitrate+nitrite, and this has been clarified in the revised text.

p.5171 line 15 closing brackets missing
The text has been revised accordingly.

p.5196 Figure 2. Add '(black dots)' to legend
The text has been revised accordingly, and now reads: "Monthly climatology (black dots)…"

p.5199 Figure 4. Make text on axes bigger, and add label to the colour bar including the units

See the revised Fig. 4 above. The font size for the axis labels has been increased, and a label has been added to the top of the figure with variable name and units (not under the color bar due to space constraints).