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# *Interactive comment on* "Turbulence measurements suggest high rates of new production over the shelf edge in the north-eastern North Sea during summer" *by* Jørgen Bendtsen and Katherine Richardson

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Anonymous Referee #2 Reviewer 2: The manuscript presents an extensive characterization the spatial variability of different variables related to primary and new production across the eastern North Sea shelf. The main conclusion pointed out by the authors is that nitrate turbulent fluxes into the photic layer (ie. new production rates) are enhanced close to the shelf edge, with potential implications for the ecosystem, as enhanced transfer towards higher trophic levels. The larger turbulent fluxes at the shelf edge do not relate to localized internal wave breaking (as reported for other locations,



i.e. the Celtic Sea shelf, Sharples et al. 2007), but to a penetration of the nutricline into the bottom boundary layer following isopycnals, which in turn seem to adjust to the baroclinic flow of Atlantic Water along the shelf edge. The dataset presented is impressive, with a unique collection of biological, chemical and physical parameters, and the results are certainly interesting. The quality of the figures and writing are overall good. However, the manuscript has some significant weak-points that need to be addressed before acceptance. My main comment is that, in my opinion, the results do not convincingly support the main conclusions, at least in the form in which they are presented now (see below).

General comments

1. The main conclusion that the shelf edge is an area of localized nitrate fluxes leading to new production (and increased fishing activity) is not convincingly supported by the results, at least in the way in which they are presented and discussed. If I interpret the text and figures correctly, the integrated values of chlorophyll and primary show a distinct cross-shelf distribution, being minimum close the shelf edge (Page 11, line 6, Figure 7). The authors must explain and discuss why this happens and how this relates to their statement that new production and transfer towards higher trophic levels is enhanced at the shelf edge. I could understand that larger NP may not necessary result in larger PP but this needs to be discussed at least. From figures 7 and 8 it is not entirely clear if f-ratios are larger there because primary production rates are relatively low or because nutrient fluxes are larger. Reporting mean/median values of PP and FNO3 at the different regions (shelf, shelf edge, Norwegian Trench) in Figure 9 would definitely help. Also, a statistical analysis/error assessment would be needed to show that the differences between regions are significant, particularly in the case of turbulent nitrate fluxes, which are highly uncertain due to the chaotic nature of turbulent mixing. Hence, the presentation and discussion of the results need to be significantly improved. Until then, the title of the manuscript ("Turbulence measurements suggest high rates of new production over the shelf edge in the north-eastern North Sea during

summer") is not justified.

Response: We would like to thank the reviewer for a careful positive review with constructive comments and criticism of our manuscript. We reply to all the comments below: a) We have added a new panel of the spatial distribution of the nitrate flux in figure 7c. b) We have added more text explaining the chlorophyll and PP distribution around the shelf edge region. In the end of section 4.3 we add: "Thus, the tendency to increased chlorophyll and PP on either side of the shelf edge could also be explained from the gradual build-up of biomass as nutrients are transported away from the shelf-edge region by isopycnal mixing. Alternatively, the tendency to low values could also be explained by a larger grazing pressure above the shelf edge. Thus, a full explanation of the tendency to low chlorophyll and PP above the shelf edge area cannot be determined from these data." c) To clarify that it is primarily an increase in nitrate fluxes that explains the variation in the f-ratios, we have added a new figure 7c where the nitrate flux is shown. From this figure, it can be seen that the elevated nutrient fluxes are in accordance with the distribution of the f-ratios. d) we have now assessed the distribution of the nitrate fluxes in a more quantitative manner by including Table 2, where the median values are related to the distributions of the parameters across the shelf edge. The table supports the discussion of the distributions shown in figure 7 and 8. Based on the statistical analysis in Table 2, we have now modified the depth range of the shelf edge zone to be between 80 - 130 m. The nitrate flux is largest in the depth interval representing the shelf edge (80-130). However, the average values are not significantly larger than in the other two depth intervals. This does not rule out that the fluxes are larger at the shelf edge, because of the transient nature of short-term term mixing events. We now show and analyse data from a time series station at the shelf edge at Tr4 (Fig. 8) where temporal changes associated with tides and other short-term variations can be seen. Thus, even though the average value is not significantly higher at the shelf edge the fact that the largest fluxes are observed in this area supports the interpretation of figure 7 and 8.



Reviewer 2: 2. The mechanisms that cause the nutrient fluxes to be larger at the shelf edge are not sufficiently discussed. In particular, it is not clearly shown if larger nitrate fluxes are related to enhanced turbulent dissipation, reduced stratification or enhanced nitrate gradient. I think this is important for the interpretation of the NP dynamics in the area. Additional figures showing the nitrate and buoyancy frequency distribution would help. In the discussion (Section 4.3), the authors point out that the dynamics of the shelf edge in the study area is different from similar locations, where enhanced turbulence and nutrient supply is sustained by internal tide dissipation at the shelf edge (eg. Sharples et al 2007). The authors say that they have carried out some time-series measurements to study the internal wave activity at the shelf edge and they have not found any signal of enhanced mixing (why not show this data at least as Supplementary Information?). They suggest, instead, that the enhanced nutrient fluxes at the shelf edge relate to the deepening of the nitracline at the shelf edge, reaching the bottom boundary layer. This deepening would be related to the baroclinic flow of the nutrient-rich AW at the shelf slope. This could be a very interesting point of the manuscript but it needs to be more clearly demonstrated with data allowing for a more thorough characterization of the site's dynamics, i.e. some current measurements (if available), or at least discussed in more depth with additional support from the literature.

Response: We referred only to the mixing of nutrients in our comments on the time series stations. We have clarified that we cannot identify the specific mixing process, thus we cannot disregard the influence from internal waves or tides based on the present data set. After our references to the papers of Sharples, Burchard and Rippeth in the start of section 4.3 we clarify this by adding: "Mixing associated with wind and tides (e.g. Burchard and Rippeth, 2009) as well as breaking internal waves (Sharples et al, 2007; 2009) has been shown to be important for vertical nutrient fluxes in shelf areas. The specific physical processes behind increased turbulent mixing cannot be identified from the present data set. Measurements on the time series station at the shelf edge showed that elevated mixing occurred in phase with the tidal energy input but also that additional energy sources likely contributed to the elevated mixing, e.g. energy from non-tidal currents. Short term variability associated with advection of ambient water masses was also observed. This could possibly be related to sub-mesoscale eddies or other transport processes occurring below the pycnocline. The time series station T2 at Tr4 showed an important feature where mixing associated with the bottom boundary layer increased and intersected the bottom of the euphotic zone. Thus, the combined effect from a deep nutricline and elevated mixing provide a mechanism for increased diapycnal nutrient fluxes along the shelf edge."

We have added a new figure from the time series station at the shelf edge on Tr4 which includes the distribution of the buoyancy frequency. N2 is also now shown in figure 6. The nitrate concentration has now been included in figure 2d where the distribution along Tr4 is shown. Nitrate concentrations at stations at the shelf edge are also shown in figure 6.

Reviewer 2: 3. Lack of important information: the authors have omitted some relevant information in the methods section and others (see specific comments). Also, at least two figures, which are very relevant for the scientific content of the manuscript, must be added: (1) the distribution of nitrate concentration along at least one of the transects and (2) a comparison of the modeled vs. measured PP values at in situ conditions for the stations where they are available.

Response: We have added the information to the Methods section and Supplementary information, as described below. (1) We now show the nitrate distribution along Tr4. (2) we explain the calculation of PP in more detail and include references to two papers on uncertainties related to photosynthetic parameters. The photosynthetic parameters are derived from laboratory experiments and, therefore, the uncertainty on these parameters are the relevant measure in this context. This is discussed in detail in section 4.2. To further clarify this issue, we have now included the following paragraph in the section. "Primary production estimates at individual sites are dependent upon the value for maximum rate of photosynthesis (PBmax). However, PBmax (and all other measured photosynthetic parameters) represent the physiological condition of the phytoplankton



community at the time of sampling. This means that PBmax may vary as a function of time of sampling (Richardson et al., 2017) or during different light conditions (e.g., photo-inhibition). Normalisation of the photosynthetic parameters with chlorophyll also represents an uncertainty in the PP estimates at individual stations as, for example, division with low chlorophyll values (e.g. some surface values were ~0.1 mg chl m-3) may result in large uncertainty of the normalised values due to relatively large absolute errors. This uncertainty error has been shown to potentially have a significant impact on the estimation of photosynthetic parameters (e.g., Kumari, 2005; McKee et al., 2015). Finally, the fact that photosynthetic parameters were determined from incubations carried out on only one water sample from each sampling depth represents a source of uncertainty with respect to the estimates of PP at individual stations. Therefore, in order to compare PP estimates from the stations we sampled, we applied average values (median for all stations) of photosynthetic parameters in the surface layer (5 m) and in the SCM in the calculation of PP. The uncertainties associated with the photosynthetic parameters are further considered in the Discussion. Surface values were assumed to represent the photosynthetic parameters in the upper 10 m and average values from the SCM were assumed to represent the parameters for the water column below 10 m."

Reviewer 2: 4. Structure: The structure of the manuscript is not always linear. I suggest some reorganization of the text/figures (eg. see specific comment 17)

Response: We have considered the specific comments in 17.

Reviewer 2:Specific comments 1. Abstract: “Estimated nitrate fluxes due to turbulent vertical mixing into the euphotic zone were up to 0.5 - 1 mmol N m-2 d-1 over the shelf-edge (f-ratios > 0.1) while values of < 0.1 mmol N m-2 d-1 were found in the deeper open area north of the shelf-edge.” If this refers to figure 8, those numbers are not easy to read from this figure. A logarithmic scale must be used. Mean/median values (and uncertainties) could be reported in Figure 9.

Response: The maximum nitrate flux is now also shown in figure 7c and the large values in the shelf area are seen by the color shading (yellow-red) while the low values in the deeper open area appears as blue. We report median values in the new Table 2.

Reviewer 2: 2. Section 2.2. Important information is lacking in this section. What was the final vertical resolution of the TKE dissipation rate? How many casts were performed at each station?

Response: The resolution was $\sim$3 m and this information is now added to the Methods section. In general, one cast was made at the relatively closely spaced stations and average values from the two shear sensors were reported. A quality criterion is, in the revised manuscript, now placed on the measurements used in the analyses. The derivation of this criterion is explained supplementary information, figure S1 and described in the Methods section.

Reviewer 2: 3. Section 2.3. How many nutrient and chlorophyll profiles/samples were analysed? "In some cases/At some stations" are very vague expressions. What was the intended horizontal and vertical resolution for nutrients? How were the sampling stations chosen?

Response: We have added the following to section 2.3: "In total, 649 water samples were analyzed for nutrients.". The distribution of nutrient samples in the vertical is described in the Methods section and it can now also be seen in figure 2d along Tr4 (shown with small bullets). The number of chlorophyll samples used for the calibration is reported in the end of the section: (n=205).

Reviewer 2: 4. Section 2.4, Page 5, lines 11-12. The goodness of the fits to eq. (3) is not sufficiently demonstrated. The authors should provide any measurement of this goodness and/or some plot of the data and fitted lines.

Response: We apply the model in Eq. 3 for calculating PP. The parameters are determined from the incubations and the three parameters at the two depth levels are shown



in Table 1. Uncertainties associated with the photosynthetic parameters are discussed in section 4.2 and we also added a new paragraph about this, cf. our response to the general comment no. 3, above. In the end of Section 4.2, we evaluate the uncertainty from the photosynthetic parameters and assess the associated uncertainty on the PP-value to be +/-30%.

Reviewer 2: 5. What is the difference between PBmax and PBmax*?

Response: In a simple PP-model without photoinhibition, then PBmax becomes the asymptotic maximum value of PP in a PP-Irradiance diagram (e.g. following the traditional simple PP-model of Webb et al (1974), Oecologia, 17, 281-91). However, the maximum of the PP-Irradiance curve is not exactly at PBmax when photoinhibition is included, cf. Eq. 3, and, therefore, PBmax* is used to quantify the maximum of the curve. We have clarified this in the parenthesis in the end of the paragraph.

Reviewer 2: 6. FNO3 calculation. If I understood correctly, the FNO3 fluxes into the photic zone at each station are reported as the maximum of the FNO3 across the nitracline. Thus, the reported fluxes are the result of a point by point multiplication of "measured" Kv values and calculated NO3 gradient. Kv has generally a patchy distribution in space and episodic in time, so that the fluxes calculated in this way my contain spurious values. How did the authors deal with this? Did they apply any averaging to the "measured" Kv values? How many casts were done at each station? The robustness of the FNO3 calculation must be assessed through a more thorough error analysis.

Response: FNO3 is calculated as the maximum flux into the euphotic zone. It was found that using the nitracline specifically as a relevant boundary may lead to an underestimate of the flux into the photic zone. We analyze this finding and document that the maximum flux is, in general, located a little deeper than the nitracline. We agree with the considerations about Kv and vertical variation and this motivated the use of Eq. 5. There are still fluctuations but these are not influenced by uncertainties associated with Kv and division of low values of N2.

Reviewer 2: 7. Page 7, lines 7-23, Fig. 2. The authors could identify the different water masses with a text label in Figure 2. Also, the authors may outline the main circulation patterns of the different water masses in Figure 1 and provide some geographic indications (name of the countries and some topographic) features to facilitate the orientation of the reader.

Response: We have added the names of the countries to fig 1b as suggested by the reviewer. We find that water masses are well represented in figure 1a and figure 4. They could be added to figure 2a, as suggested by the reviewer, but it would make the figures less harmonic, in our opinion, so we have avoided adding more information in the figure. The water masses can be identified from the TS-relations described in the text. We also chose not to add more information about the general surface circulation in figure 1 as these figures already contain many layers of information.

Reviewer 2: 8. Page 7, lines 7-23, Page 8 lines 1-23. Though extremely relevant for the study and extensively described in these lines, nitrate distributions are not shown in the manuscript. The authors must at least include the nitrate distribution of transect 4 in Figure 2.

Response: We have followed the suggestion by the reviewer and added the nitrate distribution along Tr4 in figure 2. All nitrate measurements are also shown in figure 4a-c and observations are also now shown in figure 6 at four stations along the shelf edge.

Reviewer 2: 9. Page 8. I don't believe that adding a new subsection (3.2.1) is necessary here.

Response: We have removed the heading of subsection 3.2.1 and the section is now simply a part of section 3.2

Reviewer 2: 10. Fig. 3: there is some overlapping between the red circles and orange



squares and in some cases it is difficult to know whether some points are lacking or hidden. You could use different sizes

Response: We have increased the size of the orange squares so it is easier to see the overlapping points.

Reviewer 2: 11. Figs. 2. and 4. In the methods section, the authors say that sections 2 and 4 were repeated to study the temporal variability. Are the distributions presented in Figs. 2 and 4 a mean of the different occupations, or how were they calculated?

Response: We carried out repeated measurements at two fixed geographic locations on Tr2 and Tr4, respectively, (referred to here as "time series stations"). Both data sets are included in the revised manuscript. We apply measurements from time series 1 (107 casts in three sequences with about 3 min intervals and over 22 hours down to 62 m) to analyze the statistical significance of the epsilon-values (described in this revised mansucript in Methods and Supplementary material and shown in Figure S1). In addition, we include the time series data from the shelf edge at Tr4 over a 36h period in the new Figure 8 to show the temporal variability and the influence from tidal currents on mixing in the bottom boundary layer.

Reviewer 2: 12. Fig. 6. The vertical distribution of FNO3 is very difficult to appreciate in this figure because it follows the logarithmic variability of Kv. The authors may use a log-scale for FNO3 too (also in Fig. 8). The largest FNO3 are shown for the lower boundary layer, due to larger values of the diapycnal diffusion coefficient. The nitrate gradient however is very weak here, so I doubt whether these large fluxes would actually different from zero if the uncertainties in the nitrate gradient calculation and Kv were accounted for. Error bars should be added to the nitrate flux.

Response: Figure 6 has now been expanded so it shows four stations taken within a few hours across the shelf edge at Tr4. In this area, there is a vertical gradient in nitrate, as seen in Fig 6 and also from the new figure 2d. Thus, increased fluxes here are due to the combined effects of increased mixing and nitrate gradients. We now describe uncertainties of epsilon in the supplementary information and uncertainties in nitrate determination are described in the Methods section. However, from the large nitrate gradient and the relative increase in the epsilon-values in the deeper part of the water column (i.e. below $\sim$40 m in fig. 6) it can be seen that the increased nitrate flux is significant. We have not attempted to show error-bars on the figures because they already contain several curves.

Reviewer 2: 13. Section 3.3. I would have expected to find a description of the spatial distribution of the nitrate fluxes here similar to previous sections.

Response: The new figure 2d contains the nitrate distribution along Tr4. These are described in the text at the end of section 3.1 along with a description of the other panels in figure 2. The nitrate fluxes are now shown in figure 7c.

Reviewer 2: 14. Section 3.5 / Figure 7. Vertically integrated quantities (Chlorophyll and PP) are reported in this section/figure. However, I could not find the integration depth in the manuscript. I guess that they have been integrated in the euphotic zone but this should be specified.

Response: This is now specified in the start of section 3.5: "The vertically integrated chlorophyll in the euphotic zone (50 m) ..."

Reviewer 2: 15. Section 3.5, Page 10 Line 19. How do the extrapolation with equation 3 compares with measured PP at local conditions at the locations where direct measurements are available? I suggest to add a new figure where modeled and measured values are compared.

Response: We have added more information on the photosynthetic parameters. This is described in our response to the general comment no. 3 above. We argue that the photosynthetic parameters are the relevant quantities to evaluate because these parameters are directly derived from the incubation experiments. Therefore, we have not included a new figure as suggested by the reviewer.



Reviewer 2: 16. Figure 7c and text. There is some overlapping of the color dots here and it is difficult to see whether there is a clear background tendency towards higher fratios at the shelf edge or there are only a few large values superimposed to a generally low background. How does this relate to the episodic nature of turbulent mixing? I would suggest to calculate average f-ratios for the shelf, the shelf-edge and the Norwegian Trench based on the mean (or median values) of PP and FNO3 in the different regions, instead of the point-wise calculation presented here. This numbers could be shown in Figure 9. This would also allow for a quantitative evaluation of the significance of the differences in NP between the different areas.

Response: The size of the color dots for the low values are slightly larger than for the larger values, to make it clearer to see the distribution. Regarding the episodic nature of the mixing, we expect that the spatial distribution will be influenced by this. However, because of the relatively large number of stations, we consider the distribution to reflect the general distribution of mixing in the area. We have now added a Table 2 where the parameters are calculated in different depth sections across the shelf edge, as suggested by the reviewer. The Table is analysed in section 3.6 where we have added the following:" The distributions were analysed across the shelf edge by dividing the stations into three depth ranges characterising the shallow area (50 - 80 m), the shelf edge zone (80 -130 m) and the deep area (> 130 m), respectively (Table 2). Although the shelf edge was characterised by the largest nutrient fluxes, the averaged values were not significantly higher than observed above the deeper areas. However, the depth of the maximum flux was found to be significantly deeper ($\sim$43 m) above the shelf edge than in the deeper area ($\sim$32 m). This can be explained by the significantly deeper nutricline at the shelf edge ($\sim$35 m) than observed above the deeper area ($\sim$27 m). Distributions of vertically integrated chlorophyll and PP support that minimum values are found above the shelf edge. However the low values are not significantly different from the larger values above the deeper and shallower part of the area.". The episodic nature of mixing is considered in the final paragraph in section 3.7.

Reviewer 2: 17. Sections 3.5 an 3.6 / Figures 7 and 8: The information about the spatial distribution of PP and integrated chlorophyll-a is somehow dispersed and repeated in these two figures/sections. On the other hand, in my opinion, the description of the spatial variability of the nitrate fluxes -which seems to be a central topic of the manuscript- is insufficient. I would replace the f-ratio in Fig. 7 by the actual nitrate flux and describe its variability and drivers (changes in nitrate gradient, stratification and TKE dissipation) in section 3.3, for example.

Response: We have added a new panel to figure 7 (Fig. 7c) where the flux is shown, as suggested by the reviewer. We have moved the previous figure 8 to the Supplementary information (Figure S2) because most of the information now is contained in figure 7. However, the figure shows the quantitative distribution more precisely and it also contains information of the temporal variation in, for example, PP along Tr4.

Reviewer 2: 18. Figure 8. The location of the shelf edge is not evident at all in this figure and this weakens the authors' main point (new production is enhanced at the shelf edge). I would suggest to represent the different variables as a function of the distance to the shelf edge instead of latitude. The smooth cross shelf distribution of FNO3 and the f-ratio outlined in Figure 9 and the abstract (see first comment) is not clear in this figure due to the large short-scale variability of these quantities. I would suggest to use logarithmic scale or even add a representation of FNO3 in figure 7, report mean values in Figure 9, and remove figure 8.

Response: We have followed the suggestion about adding FNO3 to figure 7 and now also report median values in Table 2. We have moved figure 8 to the Supplementary Information as described above in lines 644-648. We have kept the reference to latitude in the figure instead of distance to the shelf edge for simplicity. The location of the shelf edge along the five transects can be seen in figures 1 and 3.

Reviewer 2: 19. Figure 9. This figure is promising but it definitely needs more information. I would add mean values of primary production and nitrate fluxes (at least).



From Figures 7 and 8 it is very difficult to know if the larger f-ratios at the shelf edge are mostly due to enhanced nitrate fluxes or reduced primary production in this area. How were the f-ratios calculated, are they mean/median values or just an estimate of their order of magnitude? This is the main message of the manuscript and the authors should provide a solid quantification (and some error assessment) of the f-ratio.

Response: Median values of PP and nitrate fluxes are now reported in Table 2. Figure 7 has been improved by the addition of the distribution of FNO3max. From this, it can be seen that it is the enhanced nitrate fluxes that explain the distribution of the f-ratios. The f-ratios were calculated as the ratio of the maximum nitrate flux into the euphotic zone (converted to units of carbon by the C:N ratio) and divided by the PP. This was done for every station and is described in the end of section 2.5.

Reviewer 2: 20. Section 4.1. This section could be much improved if a comparison between modeled and measured PP values was shown.

Response: This is described in our response to the general comment no. 3 above.

Reviewer 2: 21. Page 14, lines 10. "Finally, estimates of new production imply a conversion from nitrate to carbon and a fixed ratio may not be representative for the different communities in the area." Is not there any quantification of plankton stoichiometry in the area available to assess the validity of the chosen C:N ratio?

Response: We rely on the Redfield C:N ratio and have added the original reference to the Methods section. The C:N ratio of phytoplankton has been investigated in numerous studies and, in general, a ratio of 106:16 is a good representation of the stoichiometry of the plankton. Variation is known to occur due to various causes, for example due to varying nutrient conditions and, therefore, previous values from the area would not necessarily be a better representation than simply using the "original" C:N ratio. We do not regard this to be the most critical assumption in our analyses and we have shortened the description of various error-sources in the Discussion accordingly.

Reviewer 2: 22. Page 14, line 29-30: “Mixing from tides (Sharples et al, 2007; 2009) and breaking internal waves (e.g. Burchard and Rippeth, 2008) has been shown to be important for vertical nutrient fluxes in shelf areas.” This sentence is imprecise. In Sharples et al. (2007) mixing is enhanced due to internal wave breaking (in particular to the dissipation of the internal tide) and in Burchard and Rippeth (2009) enhanced turbulence is due to the the alignment of the shear vectors induced by different sources (inertial oscillations, wind and tidal bed friction). Also, the Burchard paper is from 2009, not 2008. In general this section has great potential, but needs to be improved (see General comment 2)

Response: We have corrected the introduction to the section so now it reads:"Mixing associated with wind and tides (e.g. Burchard and Rippeth, 2009) and breaking internal waves (Sharples et al, 2007; 2009) ...."

Reviewer 2: 23. Page 15, Lines 18-26. This paragraph does not match the section heading

Response: We have added a new heading to this paragraph so it now is referred to as: "Vertical nutrient fluxes in the euphotic zone"

Reviewer 2: Technical comments 1. Page 6, line 21 and Page 9 line 10. There are too much “)”

Response: These sentences have been changed to avoid double parenthesis.

Reviewer 2: 2. Page 17, line 7. Rippith ! Rippeth

Response: This has been corrected.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-385/bg-2018-385-AC2-supplement.pdf



Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-385, 2018.