

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

Anonymous Referee #2

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REVIEW of “Turbulence measurements suggest high rates of new production over the shelf edge in the north-eastern North Sea during summer” by Bendtsen and Richardson

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, an statistical analysis/error assessment would be needed to show that the differences between regions are significant, particularly in the case

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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.

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.
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,

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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.

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

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.
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?
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?
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.
5. What is the difference between PBmax and PBmax*?

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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” K_v values and calculated NO₃ gradient. K_v has generally a patchy distribution in space and episodic in time, so that the fluxes calculated in this way may contain spurious values. How did the authors deal with this? Did they apply any averaging to the “measured” K_v values? How many casts were done at each station? The robustness of the FNO₃ calculation must be assessed through a more thorough error analysis.
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.
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.
9. Page 8. I don't believe that adding a new subsection (3.2.1) is necessary here.
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
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?

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12. Fig. 6. The vertical distribution of FNO₃ is very difficult to appreciate in this figure because it follows the logarithmic variability of K_v. 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 differ from zero if the uncertainties in the nitrate gradient calculation and K_v were accounted for. Error bars should be added to the nitrate flux.
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.
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.
15. Section 3.5, Page 10 Line 19. How does the extrapolation with equation 3 compare 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.
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 f-ratios 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. These numbers could be shown in Figure 9. This would also allow for a

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quantitative evaluation of the significance of the differences in NP between the different areas.

17. Sections 3.5 and 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.
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.
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.
20. Section 4.1. This section could be much improved if a comparison between modeled and measured PP values was shown.

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?
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)
23. Page 15, Lines 18-26. This paragraph does not match the section heading

Technical comments

1. Page 6, line 21 and Page 9 line 10. There are too much “)”
2. Page 17, line 7. Rippith → Rippeth

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

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