Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-385-AC1, 2018

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

Jørgen Bendtsen and Katherine Richardson

jb@climatelab.dk

Received and published: 29 October 2018

Anonymous Referee #1

Reviewer 1: The paper presents data from an intensive series of stations and transects in the eastern North Sea, reaching from the shelf into the Norwegian trench, to suggest that the edge of the shelf is a site of significantly higher new primary production compared to deeper and shallower regions. The results are certainly interesting, as this shelf edge region is relatively isolated from the open ocean and so is far less

Printer-friendly version

Discussion paper

influenced by typical shelf edge processes (e.g. internal tides and breaking internal waves). The results appear to be generally sound, but there is a lack of detail in key areas that needs to be addresses.

General Points: 1. The turbulence data presented is incomplete. Sections on turbulence parameters are presented (Fig. 5), but there is almost no consideration of the typical temporal variability in turbulence. Were the microstructure stations single profiles? Normally turbulence data is collected over a series of profiles to try to capture the chaotic nature of mixing events, and I would expect to see turbulence present with confidence intervals reflecting any variability. Are tidal flows important here? In which case, was there any attempt to provide some average turbulence measurement over a tidal cycle? There is a short statement in the discussion that implies additional data was collected to indicate the amount of temporal variability – if that is the case, it should be included more explicitly in the paper. Also, there is very limited presentation of the nitrate flux data – one profile, and plots of the max flux along transects. A section of the fluxes would be very useful. The paper at times mentions quite strong fluxes below the SCM, which implies a divergence in nitrate flux that needs to be considered.

Response: We would like to thank the reviewer for a careful positive review with constructive comments and criticism of our manuscript. The reviewer's concerns put in focus several important issues that we now address in more detail in the revised manuscript. Specifically, we have added more information from the two time series stations (T1, T2) which were only briefly described in the original version of the manuscript. We use time series data from T1 (and further details in Supplementary information and figure S1) to estimate the significance of the epsilon-estimates and for analyzing the temporal variability at the shelf edge. The time series data set includes 107 profiles made in three sequences at one site over a 22-hour period with a time interval of about 3 minutes. With this data set, we analyse the variability between the two simultaneous measurements from the two shear-sensors and we estimate the uncertainty associated with the differences between the two shear probe measurements.

In the Supplementary Information we show that the differences are in qualitative accordance with a normal distribution characterized by the absolute deviation of the samples. This allows us to apply a quality criterion on the data included in the study, i.e., that the differences between sensor readings should be less than three times the absolute deviation. We apply this criterion to all of the measurements used in the study. A very limited number of measurements did not meet the criterion described above. This supports the validity of our approach of using the average value of the two shear probes from a single profile as providing a representative turbulence value for the stations visited on our transects. The relatively close station spacing on our transects as well as the spatial distribution also indicates a consistent distribution pattern for vertical mixing parameters in the area. In addition, we use the time series data from T1 to consider the temporal variation between subsequent profiles separated by only 3 minutes. These new analyses are described in the text in the revised manuscript and the details provided in the revised Supplementary Information section and in Supplementary figure 1.

Further information on spatial variability is also now included in figure 6, where the measurements from four stations, separated by 5 km, and made within four hours are presented. In addition, we have added a new figure 8, as suggested by the reviewer, where data from the second time series station are analysed (this time series station was previously only described in the text). We also analyse temporal variability in water column characteristics in relation to tides and other energy sources. This analysis shows that increased mixing in the boundary layer is in phase with tidal energy input but also that energy from non-tidal currents may be important in this area. For example, a short-term change of T, S and O2 can be associated with advection of ambient water masses, i.e. not directly related to tidal flow.

Nitrate fluxes are now shown for four stations in figure 6 and the spatial distribution of the maximum nitrate flux is also now added to figure 7. Finally, average values for nitrate concentration in three depth intervals are presented in the new Table 2.

Reviewer 1: 2. The discussion is somewhat unsatisfactory. Quite a broad range of alternative processes are suggested as underpinning some of the observations, but they are often vague and rather descriptive. Some better quantification of these would help to determine how likely they are as playing important roles.

Response: We believe that explaining the observed distributions of, for example, nitrate, in our study requires knowledge of seasonal nutrient dynamics in this area that we do not have. Therefore, our considerations of nutrient distributions are to some extent qualitative and we refer to previous studies to provide background information on these aspects. Nevertheless, we have now considered the specific points raised by the reviewer below and clarified the considerations in the Discussion section.

Reviewer 1: For instance: (i) On page 12 denitrification is suggested as a mechanism for reducing the shallow water nitrate, with a global mean rate from Yool 2007 mentioned. There are shelf/coastal estimates available, and a quick calculation could be done to assess the feasibility of this process.

Response: We originally referred to the global nitrification rate of Yool et al. (2007) to explain the potential for recycling of ammonium in the water column. In this revised version of the manuscript, we have also included a reference from Fan et al. (2015) on denitrification rates and show that the apparent loss of nitrate recorded here could be explained by these rates.

Reviewer 1: (ii) The mechanism for the elevated turbulence at the shelf edge is never discussed. It seems to be a boundary-layer process – is it due to a slope current or tides? Also, the boundary turbulence seems fairly consistent along the transect (e.g. Fig. 5c) – so is the shelf edge nitrate flux really a result of increased turbulence, or is it because the sloping isopycnals bring the nutricline down towards the turbulence (almost implied on page 15). The latter idea seems to be suggested by Fig. 9 (though without better information on the turbulence data, I'm not convinced that the bed turbulence over the shelf edge is significantly greater than bed-driven turbulence elsewhere

– in which case the deepening of the nutricline is vital).

Response: The reviewer's points here are in accordance with our understanding of the processes at the shelf edge. We have added more material to clarify and support this point. A figure has been added (Fig. 8) showing the temporal variability at the shelf edge where mixing in the bottom boundary layer is seen to increase and elevated mixing in periods reaches the bottom of the euphotic zone. Thus, interference between a deep nutricline and bottom mixing may provide a mechanism for enhancing diapycnal nitrate fluxes. We have also added Table 2 in which it is shown that the nutricline is significantly deeper above the shelf edge than in the deeper areas. This is also seen in figure 2d (where we have added the nitrate concentration along Tr4) and shown in figure 3 (where we have added isopycnals to illustrate the link between the nutricline, chlorophyll and the density fields) and in the conceptual figure 9. Thus, the deepening of the nutricline, together with increased mixing in the bottom boundary layer, is probably an important mechanism for the elevated nutrient fluxes above the shelf edge. We speculate on this in the Discussion, in particular in relation to figure 9 where the potential dynamic feedback between currents along the shelf-edge, the depth of the nutricline and nutrient fluxes into the euphotic zone is outlined. Thus, this proposed mechanism is, indeed, a result of the deepening of the nutricline and elevated mixing above the shelf edge area.

Reviewer 1: (iii) Isopycnal transport of organic material is suggested as a way of supplying nutrients (page 13), but is not estimated in any way – some reasonable numbers would help in determining its likely use.

Response: It is difficult to provide better information than the time scales for the decay of organic matter we describe in the text. The relevant time scales are of the order days to weeks and we refer to a previous study where we analysed these time scales; Thus, even small cross-shelf transports may contribute with isopycnal fluxes. However, we do not have any measurements of the labile fraction of organic matter in the area. In addition, we have only limited information on the cross-shelf exchange. So, rather than speculating further on this issue, we prefer to relate to the information on the time scales described in the text.

Reviewer 1: (iv) On page 15, “other transport processes” apart from vertical turbulent mixing are required, and motility of phytoplankton is suggested. This again is rather vague – why not quantify the possibility (e.g. use the turbulence data and an estimate of phytoplankton swimming speed to get a Peclet number)?

Response: We have now added an example at the end of Section 4.4, based on swimming speeds of dinoflagellates (Raven and Richardson,1984), to illustrate the potential of diel vertical migration to provide access to nutrients. In addition, we now also estimate the associated Peclet number in the area north of the shelf edge where vertical diffusion coefficients are very low and show that Pe » 1.

Reviewer 1: (v) A link to a coastal bloom off Norway, seen in a MODIS image, is invoked on page 15. Why not show this image, rather than simply assert its likely relevance based on the proximity of the sampling?

Response: We have followed this suggestion and added the MODIS-derived fields of chlorophyll a and SST in figures 1a and 1b, respectively. We refer to the satellite images in the discussion.

Reviewer 1: Specific Points: 1. Page 2 line 7 (also discussion, page 11 line 29): Linking localised NP to higher trophic levels needs to be more nuanced that implying a simple “more production leads to more fish”. Scott et al note that the increased chl arises due to internal wave mixing, and the internal waves might also affect prey aggregation – i.e. the correlation with chl is not causal, but chl and prey aggregation are both a result of internal waves.

Response: We have clarified the paragraph. We agree with the view that Scott et al related increased NP to increased mixing, and this was also the intention with the paragraph.

Reviewer 1: 2. The introduction/background is very much focused on the North Sea. However, the issues being investigated have much broader significance – it would raise the profile and readership of the paper if a stronger, broader context was provided rather than such a localised one.

Response: We have followed the suggestion by the reviewer and added a paragraph in the end of the Introduction where we relate to the more general implications of shelf-edge processes and to conditions in similar shelf-regions.

Reviewer 1: 3. Page 3, line 18: a 1 km station spacing was used (which is impressive), bit how does that fit alongside the tidal excursion?

Response: A rough estimate of the tidal excursion, based on SST-evolution in forecast models of the North Sea, is $\sim$5 km, and this has to be considered when samples from closely spaced stations are analyzed. The station spacing was gradually decreased along a section of Tr2 for analyzing sub-mesoscale changes in plankton communities. This aspect is not the focus of this study and, therefore, not discussed further in this manuscript.

Reviewer 1: 4. Page 4, line 10. The mixing efficiency is assumed to be constant, but there's a good deal of recent literature that suggests this is not the case (e.g. Shih et al., J. Fluid Mechanics, 525, 193-214, 2005; Bouffard & Beogman, Dynamics of Atmospheres and Oceans, 61, 14-34, 2013). Both provide a way of estimating efficiency knowing the turbulence intensity – I suspect that the region of data in this N Sea study probably sits where efficiency = 0.2, but it would be good to check this.

Response: The reviewer's comments have now been considered. We have added a paragraph in the Methods section where we show that the range where a constant mixing efficiency of 0.2 is valid encompasses the values we apply in the calculation of the nutrient fluxes into the euphotic zone. We have also added the two references brought to our attention by the reviewer.

Reviewer 1: 5. Page 4, line 16: “the depth of the SCM was sampled” – do this mean the peak of the SCM?

Response: Yes, and we have added a comment to clarify this.

Reviewer 1: 6. Page 4, line 20. Nutrient analyses are mentioned, but no methods – I assume standard methods, but at least cite the usual papers.

Response: We have added more information on the nutrient analysis and included a reference to Grasshoff et al. (1983).

Reviewer 1: 7. Page 4 line 25: Why assume that the deep fluorescence signal is not chlorophyll? If you have boundary-driven turbulence acting at the base of the SCM and nutricline then it will draw chl down into the deeper water.

Response: The background value was determined from a deep station (522 m) where a relatively constant fluorescence value was observed between 100 m and 500 m. We see no reason to assume that chlorophyll would be uniformly distributed throughout this deep layer. Therefore, we treated this relatively small background fluorescence as being derived from an unknown source and it was subtracted from the fluorescence signal before the calibration. We have reformulated the sentence to clarify this.

Reviewer 1: 8. Page 5, lines 14-17. I’m not sure why this scaling of observed PAR to the MODIS product was done.

Response: We have added "during the day" to clarify this. The integral in Eq. 3 includes the daily variation of the insolation and this influences the integrated primary production significantly because of the non-linear terms in the equation. It has also been clarified by adding the time and depth dependence, i.e. (t,z), of the variables in the integral.

Reviewer 1: 9. Page 6, line 25-26: The assumption of Redfield is a critical part of the results of the paper. Some justification needs to be made to show that the assumption is OK, or to indicate the likely variability of C:N.

Response: The Redfield ratio is characterized by a C:N ratio of 106:16, and this ratio is widely used in observational and model studies, although variation of the ratio is known to occur. Thus, applying a constant ratio introduces an additional error-source in the calculations. We have added the original reference of Redfield et al. (1963) where a general relationship between the elemental stoichiometry of C:N:P in plankton is documented.

Reviewer 1: 10. Page 7, lines 3-5. I struggled to decipher this sentence, please clarify.

Response: We have clarified the introductory sentences.

Reviewer 1: 11. Page 7, lines 17-20. Re-phrase – this is a very long sentence with inconsistent use of brackets.

Response: We have reformulated the sentence.

Reviewer 1: 12. Page 9, line 14. The highest nitrate flux is reported at a depth below the photic zone. This implies some divergence of the nitrate flux – where does it go if there is no sink for it?

Response: To answer this question, we would need more measurements from the area around the station and in the boundary layer. There is a temporal change, likely associated with the tidal currents, as shown in the new figure 8. Figure 6 in the original manuscript has now been replaced with a section showing more profiles taken across the shelf edge at Tr4 obtained over a short period. However, the divergence is likely associated with transient currents and an example of this is now shown and discussed in relation to the time series station in the new figure 8.

Reviewer 1: 13. Page 9, lines 26-30. I'm not convinced that the chl-normalised production rates are useful. Chl per cell in the SCM is likely to be higher than in the surface, so comparing chl-normalised parameters does not tell us much. Or does it and I have missed the point? Normalised per cell or per C would make sense (though not clear this is possible).

Response: Primary production is, as here, frequently calculated from the model of Platt et al. (1980) in Eq. 3. We have clarified this procedure by adding the integral over the vertical depth range (from the bottom of the euphotic zone to the surface) and during the day (24h, i.e. only daytime PAR-values contribute). Thus, the integral considers the vertical chlorophyll a concentration (we now specify the depth-and time-dependence of the parameters in the integral) and, therefore, the PBmax-values are normalised in the equation. We sample data from both the surface and the SCM (cf. Table 1) to take the potential vertical variation of the phytoplankton characteristics, as mentioned by the reviewer, into account - both in relation to photosynthetic parameters and chlorophyll content. The normalisation implies that when PBmax(z) is multiplied with the chlorophyll concentration in Eq. (3) then this variation is accounted for.

Reviewer 1: 14. Page 10, line 7: units needed for 4.76 and 1.72. Response: The unit has been added.

Reviewer 1: 15. Page 11 line 8. ". . .coastal upwelling. . ." This is rather vague. What mechanisms or evidence do you have?

Response: In addition to our measurements showing increased vertical fluxes, satellite images also indicate that coastal upwelling may be significant along the Norwegian coast. We now refer to the new figure 1b, showing SST from a MODIS-image and we have added the following to the text: ("also indicated by relatively cold Norwegian coastal water masses observed from satellite in Fig. 1b").

Reviewer 1: 16. Page 14, line 29. The two Sharples refs deal with breaking internal tides/waves. The Burchard & Rippeth ref deals with wind-driven shear spikes and mixing by inertial waves. This is an important aspect of the discussion – most regions of the shelf edge are reported to have high nitrate fluxes due to breaking internal tidal waves. In the present study this is not the case – which is worth pointing out.

Response: We have corrected the description of the references. From our data, we cannot identify the specific processes behind the mixing, and this is now clarified in the paragraph.

Reviewer 1: 17. Page 16, line 1: ". . .indicate increased mixing, upwelling, or eddy activity. . ." This is very vague. What evidence do you have, or is there citable work that supports this?

Reponse: We have reformulated the sentence to: "A tendency towards a thicker chlorophyll layer around the SCM and a deeper nutricline at Tr4 and Tr5 also indicates increased production and supply of nutrients near the coast." We now refer to the upwelling elsewhere in the text where we refer to the SST seen in the new figure 1b. Therefore, this is not repeated in this section.

Reviewer 1: 18. Page 20, caption to Fig. 5: (c,d) rather than (b,d).

Response: This has been corrected.

Reviewer 1: 19. Figs 1 and 7. The bathymetry contours are hard to read. Better labelling needed, also perhaps mark the shelf edge?

Response: The font size has been increased in the figures so it is easier to read the depth contours (it should now be easier to identify the shelf edge so no additional lines are included).

Reviewer 1: 20. Figs. 2, 3, 4, 5, 7, the colourbars need units.

Response: The units of the color bar have now been added to the figures and described in the figure legends.

Reviewer 1: 21. Fig. 3: parallel sections of density would help a lot in understanding the chl distributions.

Response: Contour lines of density have been added, as suggested, to all the panels in figure 3.

Reviewer 1: 22. Fig. 8: the different colours presumably indicate different transects.

Printer-friendly version

Discussion paper

Legend needed.

Response: Line legends for all the panels are shown in panel (f) and this information is now also added to the figure legend.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-385/bg-2018-385-AC1-supplement.pdf

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