Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-385-RC1, 2018

Biogeosciences Discussions

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 #1

Received and published: 21 September 2018

Review of "Turbulence measurements suggest high rates of new production…." By Bendtsen & Richardson.

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 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.

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. 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.

(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).

(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.

(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)?

(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?

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.

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.

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

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.

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

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

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.

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

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.

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

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

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?

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).

14. Page 10, line 7: units needed for 4.76 and 1.72.

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

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.

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?

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

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

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

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

22. Fig. 8: the different colours presumably indicate different transects. Legend needed.

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