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Interactive comment on "Thermocline mixing and vertical oxygen fluxes in the stratified central North Sea" *by* L. Rovelli et al.

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The manuscript "Thermocline mixing and vertical oxygen fluxes in the stratified central North Sea" attempts to quantify oxygen fluxes in and around the bottom mixed layer of the Tommeliten site of the North Sea in late summmer based on a short investigation relying on microstructure measurements. The authors present the idea that fluxes between the bottom mixed layer and a mid-water layer are greater than previously thought. The implication being that there is a higher turnover than previously thought but that remineralisation of injected DCM matter masks the oxygen influx into the BBL. This would also imply a much greater rate of BBL respiration than previously described in the literature. Although I believe this is quite possible as I have also observed similar processes (and come to the same estimates of respiration! Queste et al., also in

discussion for the same issue), the authors of this manuscript encounter the same hurdles: it is difficult to reassure the reader of the validity of a short term measurement in context of seasonal processes, particularly when observing dissolved oxygen which shows high spatial and temporal variability.

The budget itself needs strengthening. The paper focuses on quantifying one term, the flux at the BBL interface, which seems to be well constrained. Benthic remineralisation rates and pelagic respiration are taken from the literature, which is acceptable, but have been taken out of context and without any assessment of variability. It is the overall dO2/dt rate which I currently find problematic: it is taken from observations which are poorly described in text, not shown in figures and not backed up by numbers. How did you calculate this rate?

The paper as a whole reads ok. The sentence construction is sometimes clumsy, although it never impedes understanding. The paper is well structured, although I feel some sections of the introduction lack a bit of detail (detailed further below). My main issue is with the final section. The biological perspective (Sec. 4.4) seems to me tenuous, but also not necessarily relevant to the paper. The results and preceding discussions are, in my opinion, more than sufficient for a paper. I feel this work would come across as stronger without and instead focused solely on the physics and the fluxes.

I would have liked to see some comments from the authors regarding the observed vertical density profile. My understanding (admittedly based on other sites further west, ie. North Dogger) is that the these waters usually exhibit a clear two layer regime in August. Can the authors guess at the origin of the "intermediate layer"; is it a remnant of a recent storm, a tidally driven process, or advection of an intermediate watermass?

Not being a turbulence expert, I find it hard to comment on the methodology employed for assessing turbulence and fluxes and hope another reviewer will be able to better cover this aspect.

Overall, I feel this paper is an interesting contribution to the ongoing oxygen debate within the North Sea and provides much needed estimates of turbulent fluxes at the thermocline but requires considerable revisions to be acceptable for publication.

ABSTRACT:

I feel the abstract focuses too strongly on the results of Sec 4.4 which I feel is the weakest part of the paper. Instead of 50% of the abstract focusing on Sec.4.4, I would rather see some numbers coming from your flux estimates or comments regarding the high amount of cycling between the DCM and the BBL.

9906L17-19: "Due to the substantially lower turbulence levels in the central region of the thermocline as compared to the higher turbulence observed at the thermocline-BBL interface..." The sentence is unclear.

SECTION 1.1:

L5: Slightly oversimplified. Not sure what eutrophication has to do with deep waters. OMZs (deep water), eutrophied shallow regions such as the German Bight and the central North Sea all exhibit low oxygen, but from quite different mechanisms.

SECTION 1.2:

The section title is "distribution" but you don't mention the actual distribution of O2 in the North Sea. I would also expect a (brief mention) of North Sea hydrography and how the section you're referring to is classified as a seasonally mixed region (ie. only relevant to the North Sea above 56N). Where and when have we seen low O2 before?

9907L15: What is the relevance of eutrophication in the central North Sea? It is a big issue in coastal regions and in the south, but it is irrelevant nears the Tommeliten site.

SECTION 1.3:

9908L1: I'm not sure I agree with that first statement in the context of shelf seas, particularly with oxygen. Biology plays a very important role in defining O2 concentra-

tion/saturation in shelf seas.

In a section entitled "controls on oxygen dynamics" I would expect a breakdown of the processes that affect oxygen in shelf seas: the vertical transport, but also horizontal advection, primary production and remineralisation and air/sea exchanges (which dominate in the surface layers). The relative importance of each will be very different compared to mixed regimes or OMZs.

SECTION 2:

Section 2 is too far out of my field of expertise for me to comment.

9912/L18-20: Quantify density gradients, reassure the reader what you're saying is true.

SECTION 3:

There should not be text under Sec3 if subheadings (ie. 3.1, 3.2) are coming later.

9914L4: "oceanic background" could just be hydrographic

SECTION 3.2:

9914L14: What criteria is used to separate the layers? I struggle to see the difference between the surface layer and transition layer in Figure 2.

9914L24,25: I would like to see the saturation values accompanied by the corresponding concentrations

9916L6-7: Spectral density function is not shown. Why not, I see no problem with adding it in terms of number of figures.

9916L23: There is no figure 6.

SECTION 3.3:

9917L14-18: How accurate is your assessment of dO2/dt, a figure showing the ob-

served values wouldn't be a bad thing. Did you observe a linear decline? Is it uniform throughout the water column? Is it an artefact of sampling at dawn, night or dusk? How good of a fit is your linear regression? Since your entire budget relies on this value, I would expect much more justification here.

9917L22: Over what distance did you observe no horizontal density gradients? It would have to be large to show no horizontal advection. If it's large, how do you justify saying you're measuring dO2/dt and not a spatial change?

SECTION 4:

There should not be text under Sec4 if subheadings (ie. 4.1, 4.2) are coming later.

SECTION 4.1:

9919L24: Data not shown. Again, there is sufficient space for figures. Maybe these additions would help give the reader more confidence?

9920L13-15: I would rephrase this sentence as it is not very clear at the moment.

9920LL15-17: I'm not sure I agree here. You're arguing there is possibly more production than anticipated, but not necessarily new production, so the impact on export is more limited... I think Weston 2005 discussed this pretty well.

SECTION 4.3:

9921L25-28: The southern North Sea is an incredibly different regime, I'm not sure I see the relevance.

9922L8: They help regulate, but they are not the only mechanism. Maybe rather say it sets the lower limit on how depleted oxygen concentrations can get?

9922L10: Only if the amount of OM is equal to the amount of O2 injected. This assumes no difference in O2 concentrations between the BBL and DCM.

SECTION 4.4:

9923L14-23: You were previously arguing that nutrient supply was proportional to O2 flux. If you reduce O2 flux here, wouldn't you also reduce OM production, and therefore SUR and pelagic respiration as well?

9923L24-28: Paragraph isn't very clear.

FIGURES:

Fig.1: I would suggest a map projection that is more indicative of actual relative distances at 56N. The bathymetric contours also fail to highlight some of the important features in the North Sea; ie. the Dogger Bank which is known for generating internal waves which play a sgnificant role in vertical exchanges at the thermocline.

Fig.4: Is there possibly an anomaly in the data, panel C at 35m? The averaged value seems off relative to the other points indicated.

Is Fig. 6 missing?

References need checking in text; for example, Queste has been cited with different dates for the same paper.

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