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Interactive comment on "Drivers of summer oxygen depletion in the central North Sea" *by* B. Y. Queste et al.

Anonymous Referee #2

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The manuscript "Drivers of summer oxygen depletion in the central North Sea" by B. Queste et al. brings together the exciting topics of new autonomous technology and shelf sea biogeochemistry. The authors use a 3 day glider survey in the North Sea, near Doggers Bank, to estimate an oxygen budget for the region. The authors attempt this by taking into account the oxygen utilisation in the BML (AOU), and compare this to an estimate of the oxygen supply via diapycnal mixing, and an estimate of oxygen consumption via remineralisation of organic matter taken from the literature. The glider-derived estimate of AOU is considerably higher than the estimate of oxygen consumption based on the literature, which the authors claim signifies either an unknown oxygen sink, or a high rate of remineralisation during the 3 day study.

I found the paper content to be very interesting and enjoyed reading it. It is well written,

flowed well and the results nicely presented. There are some issues with the paper that need to be corrected before it could be published. One of the main issues is with equation 1 being incorrect. I think there has been some misunderstanding here with what Kz(dO2/dz) represents. The authors argue that the horizontal flux can be ignored as the area is relatively isolated from horizontal gradients, but they also argue that the vertical flux of oxygen due to vertical mixing, Kz(dO2/dz), is negligible. They then later instead mistake w(dO2/dz) as the term responsible for the vertical flux of oxygen and estimate it as a supply mechanism. This is in fact an advective term and does not represent the flux or transfer of any properties vertically, so it can be reasoned away along with the advective terms in the U and V directions, but Kz(dO2/dz) (the actual vertical flux) should not be. The vertical flux is not negligible unless there is no mixing or no vertical gradient in oxygen – both of these are not the case and the authors later go on to describe the vertical flux as the supply mechanism of oxygen to the BML. Also equation 1 should use partial derivative delta and not the one the authors have used. It may also be worth clarifying that the 'bio' term in equation 1 (and 'R_benthic' term in equation 2) includes fluxes at the seabed due to remineralisation in the sediments, as this is likely an important sink of oxygen in the BML and the authors do mention this later in the paper.

Another issue is the use of chlorophyll as a marker for DCM POC export. Chlorophyll degrades quickly and it is only a marker for 'live' DCM POC export - which is likely due to the turbulent erosion of live cells from the DCM. Sinking biomass from the DCM is likely dead organic matter and would not be picked up by a chlorophyll signature; this would have a large contribution to remineralisation in the BML. This needs to be clarified in the manuscript as the flux of chlorophyll from the DCM is not equal to the POC flux. The authors state that vertical mixing is unlikely to be the mechanism exporting biomass for remineralisation but is the mechanism for injecting nutrients, however, if an exchange of water is occurring and nutrients are being injected upwards then biomass (live and dead) will also be exported downwards due to diapycnal mixing. If there is an upward flux then there must also be a downward flux.

The authors need to clarify how they define the BML. This is important when they describe max temperature gradients in the BML signifying the occurrence of diapycnal mixing events. If they are defining the BML by depth, then they will capture the displacement of the thermocline, and thus warmer water in the 'BML', due to the barotropic tide. However this would not be a diapycnal mix event, only the movement of density/temperature gradients up and down. If they have defined the BML by a particular isotherm/isocline then the value of this isotherm should be stated and it would be helpful to have it sub plotted on to Fig 3a.

There are some interesting features in the oxygen and density around the pycnocline (Figure 3). An increase in DO at pycnocline and decrease just below the pycnocline is observed. The authors use sections of up and down casts to make a composite profile of oxygen. Any error due to hysteresis or optode lag would be largest when the glider is crossing a large gradient, and therefore biased if using either all up or all down cast to capture the pycnocline. It would be good to highlight that these features are real by perhaps providing a couple of example up and down oxygen casts, indicating where the authors take the pycnocline from, or some clarification on how the pycnocline/oxycline is resolved. Queste (2014) contains details on lag correction and calibration figures but I think a figure/linear regression equation and Rˆ2 value is needed for oxygen and chlorophyll as the glider values are being used directly for C budgets later in the manuscript.

I agree with the authors that internal waves and shear spikes are the likely source of mixing across the pycnocline and therefore I wouldn't expect the tidal and wind velocity to necessarily show correlation with the timing or duration of these mixing events (Figure 8). Boundary driven mixing (i.e. wind at the surface and tidal stirring at the bed) rarely reaches this far (Simpson et al., 1996), but if the maximum density gradient is in a state of marginal stability certain processes can 'tip the scale' and cause mixing (Palmer et al., 2008; Burchard and Rippeth 2009). The Burchard and Rippeth 2009 paper that the authors mention suggested that a sudden change in the

direction and/or forcing of the wind (which you have on 19th August), acts as a 'trigger' for shear spikes and thus mixing to occur when the wind stress and bulk current shear (du/dzˆ2) align. This mechanism has been shown to result in considerable diapycnal fluxes of nutrients and carbon across the thermocline in shelf seas (e.g. Williams et al. 2013). If you have the SML and BML tidal velocities then you can easily produce a time series of the approximate bulk shear. This would be much more useful than providing the mean tidal current speed, which really doesn't tell us much about the stability of the water column and wouldn't be expected to correlate with mid water mix events. If you wanted to go further and show the likelihood of marginal instability and mixing you could even estimate the Richardson number from your bulk shear estimate and the buoyancy frequency across the pycnocline (from your glider density measurements). It is probably beyond the scope of this paper but would be a simple and useful method to observe instability and mixing and would add some weight to the comments on diapycnal mixing events.

Due to the observed levels of oxygen consumption via AOU estimates, compared to the estimated sources and sinks of oxygen over the 3 day study, the authors argue that either strong reoxygenation events must occur to prevent the BML entering a state of hypoxia, or the seaglider surveyed the region during a short-lived or localised increase in AOU. I agree with the authors that vertical fluxes are likely to play a role both in both 'replenishing' oxygen and providing fresh organic matter to be remineralised in intermittent bursts associated with mixing events. I do think it is worth briefly mentioning that the rate of remineralisation of organic matter is also likely to be dependent on many factors – the size and type of the particles sinking for example, which is dependent on community composition which is known to shift over the summer and following the spring bloom. This is interesting as it raises the question of whether a glider could be used over a longer study to assess AOU or NCP over the entire summer, and observe how the remineralisation rate changes.

I agree that the authors can make the assumption that there is little horizontal oxygen flux during their 3 day glider campaign based on their results and what is available to them. This could be supported further by the model output they use – perhaps providing an estimate of the length and velocity scales (order of magnitude) when horizontal transport would be significant. However the vertical supply of oxygen is misrepresented both in equation 1 and throughout the manuscript. It is not clear how the BML is defined and so I'm not quite convinced at the moment that the maximum temperature gradient in the BML signifies a diapycnal mixing event. It is difficult for the authors to estimate vertical fluxes without the expensive measurements of Kz. However, I think the authors can address this by taking a typical value or range of Kz at the thermocline from the literature and multiplying this by their time series of glider-derived oxygen gradient. This will give them an oxygen flux estimate that takes into account the real eddy diffusivity and mixing at the thermocline, and would be a reliable estimate of the daily oxygen supply (and DCM chlorophyll erosion) that they could compare, and add some weight to, their glider-derived $\sim$2 +/- 1 umol dm-3 d-1 (and 5.7 mg m-2 d-1 respectively). Overall the authors show that the glider is an excellent tool for investigating oxygen dynamics, but the conclusions the authors make need to be backed up by a little more than what they have provided so far. I think if these issues can be addressed by the authors it would make the dataset and conclusions a worthy and interesting contribution to shelf sea biogeochemistry and oxygen budget methods.

Minor issues:

PAGE 8694 L10 – Reference for the historical dataset that highlights low DO conc = low oxy saturation is missing in text - in figure caption says this is model and ICES database. L12 – What mechanisms lead to depletion of oxygen at OG and ND? Low DO in BML during summer and sharp decline in summer oxygen saturation? Unclear.

Figure 3: This is quite difficult to see, would it be possible to represent plots 3 x 2?

Figure 4: These are mean profiles (over the entire glider survey?), therefore how can the bottom of the water column be unstable (higher density water over lower density

water)? I think an erroneous bottom value is skewing the results in this plot.

PAGE 8696 L27 Interesting – it might be worth noting that this is loss of 'live' sinking organic matter biomass – i.e. export due to diapycnal mixing. This is also hard to observe in Figure 3d and e, could you perhaps subplot the depth integrated chlorophyll over the thermocline (define by 2 isotherms), and compare against the BML-depth integrated BBP.

Figure 6 shows a nice trend of AOU, and nicely explained. Could figure 7 not be combined with this in a subplot? Not sure why it is a separate figure.

PAGE 8697 Equation 1: See comments on vertical flux of oxygen term being presented incorrectly above.

PAGE 8703 L13: "The DCM relies on 'small scale' mixing to provide nutrients from below": Does it? What do you mean by small scale? Increasing amount of evidence to suggest intermittent, enhanced mixing events sustain the DCM in shelf seas (e.g. Williams et al. 2013a; 2013b). Might be better to say that 'the DCM is sustained by turbulent fluxes of nutrients from the BML' (e.g. Sharples et al., 2001; Williams et al., 2013a).

L14: If wind is not responsible for mixing organic matter out of the DCM into the BML, how can it be responsible for injecting nutrients into the DCM from BML as you mention in the paragraph above? This doesn't make sense to me – if nutrients are being injected via wind mixing (upward flux) then organic matter would also be mixed out (downward flux), as this is a transfer of water and its associated properties (flux = Kz * property gradient). Also, you need to mention that you completely exclude 'dead' organic matter which would be a huge proportion of the remineralisation occurring in the BML. Much of the 'live' export you are seeing is likely due to turbulent erosion of live cells from the DCM, it is dead cells that will sink out (Ross and Sharples, 2008; Williams et al., 2013a).

L22: “if all this organic matter were to be remineralised within the BML this would equate to 33.58 mmol DO m-2 day-1. . .” – according to Redfield? If so please state and reference this otherwise it is unclear where these numbers come from.

PAGE 8305 L19: “Unresolved mechanisms contributing to oxygen depletion” – like remineralisation of dead particles, as well as non-photosynthetic particles/heterotrophs?

Figure 8a & c: I'm not sure how much these figures add to the paper content, see notes above on bulk shear/Richardson number.

Helpful references

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