Author report

We thank the reviewers and editors for their time and effort. The reviewers have been particularly helpful in suggesting appropriate and helpful alternatives to address their concerns. We're pleased that the reviewers found the manuscript useful and interesting overall. We will address the limitations and shortcomings pointed out by the reviewers, particularly the representation of the modelled processes.

In this document, we first address each of the reviewers' more general concerns one by one. The second section is the list of minor comments, in order of appearance in the manuscript with replies. The reviewers' suggestions and comments are in gray, while our replies are in black text. Suggested edits to the manuscript are in *italics*.

Overall, the major concern was the equation used to represent the physical, chemical and biological processes. We have taken the reviewers' suggestions and replaced the formal equation with a conceptual diagram and have added further text to detail this.

Some of the concerns related to the assessment of mixing processes using temperature gradients in the BML. We agree that a more detailed study of the physical processes would possibly provide more insight, however we're limited by the absence of accurate depth-defined tidal velocities. Without this information, any further investigation would be burdened by a range of assumptions negate the benefit of this further work. However, a recent paper in this issue (Thermocline mixing and vertical oxygen fluxes in the stratified central North Sea, Rovelli et al.) investigates this mechanism in a similar region. We have drawn links to their paper here and feel it provides more information than we could provide ourselves.

In particular, we were glad to see the reviewers acknowledge the usefulness of this study. Although it is a short survey, investigating multiple simultaneous processes (mixing, sinking and remineralisation), we believe that we have made a good attempt to resolve each separately. An individual glider will necessarily have difficulties differentiating between spatial and temporal changes. These instruments are not the sole answer, but they have contributed here to answering the problem. This short term deployment isn't intended to provide a description of mechanisms covering the entire seasonal cycle in the North Sea but rather highlights that mechanisms of oxygen consumption/depletion/renewal can occur on short time scales. This variability in water column oxygen concentration is higher than previously thought, and furthermore, the response to inputs is more rapid than previously thought.

Major Comments

Reviewer 1

We're glad the reviewer found the manuscript to be a pleasant read and thank them for their useful comments on the theoretical representation of the oxygen dynamics. We have taken the reviewers major suggestion to remove the mathematical representation and implement a graphical box approach instead. This has the benefit of also answering many of Rev. 2 and 3's comments.

One of the main issues I have is with the mathematical basis of the methodology used. Conventionally, a partial derivative is represented by the symbol ∂ rather than δ, and should be changed throughout the manuscript. The processes considered are represented by an advection-diffusion equation, given by equation (1). The advective terms (u∂O2/∂x and similar terms) have the wrong sign. Furthermore, a physical system described by a partial differential equation, such as equation (1) also needs boundary conditions. In fact, I believe the authors mixed the boundary conditions erroneously in the equation. The term 'ase' represents air-sea exchange, which is typically a boundary condition. The exchange with the seabed is probably also important and is not mentioned. The biological processes, as long as the are relevant in the water column should appear as a term in equation (1).
After stating equation (1), the authors eliminate the terms that are considered insignificant. The diffusive terms should be ignored, not the coefficients of diffusivity (line 24/8697 and 1/8698). It is probably more correct to say that the terms are negligible because the gradients are small (well-mixed layers). In fact, the coefficients KH and Kz are termed eddy diffusivity coefficients, which implies that equation (1) is averaged over turbulent time scales. In this case it is fair to assume that w = 0. (It is, by the way, the convention to write velocity vectors as in bold face, but the velocity components are set in regular type.) This leaves the simplification of equation (1), i.e. equation (2) wrong. A vertical advective term could play a role, of course, but then it represents a process such as upwelling or downwelling, which, as far as I understand is not meant here. As before, the simplified equation (2) also requires boundary conditions to specify the problem, and also here they are included in the differential equation, in particular the term RBenthic. In fact, I think the flux of oxygen through the thermocline, that the authors describe later, is in fact a diffusive flux and should be described by the term Kz∂ 2O2/∂z2 .
Since the analysis the authors carry out is mainly depth-averaged and restricts itself to the BML, it may be beneficial to remove the mathematical formulae altogether rather than fixing the mistakes, and instead, present a (graphic) box orientated budget model. Such a model would help the authors and the reader to get a picture of what and where the various fluxes, sources and sinks are defined.

We agree with the reviewer's general comments. There were some formatting problems in the conversion between latex and word during the multiple edits before submission which have not helped in clarity of the equation or it terms. The suggestion of using a box diagram to describe the relevant terms is a good one and has been implemented. Several parts of the text have been rewritten to reflect these changes.

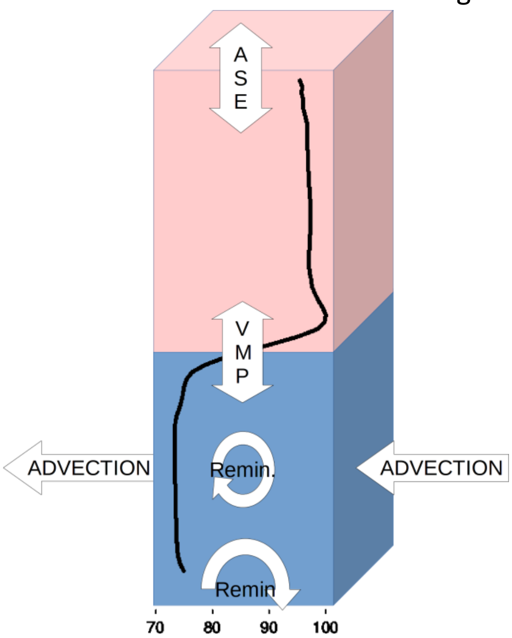


Fig. 1: *A conceptual representation of the processes affecting oxygen supply and consumption to the bottom mixed layer during the glider survey. The water column is separated in two layers, the SML and BML (red and blue). The observed mean oxygen saturation profiles is overlaid on the water column to illustrate the position of the oxycline and deep chlorophyll maximum (indicated by the mid water peak). ASE: air-sea exchanges; VMP: vertical mixing processes; Remin. : remineralisation of organic matter.*

If the authors insist on keeping the mathematical model (in corrected form), then it is required to make a connection between the model description and what the glider observes. In its present form, the observations from the glider are plugged into the model equations. Equation (1) and the derived equations probably assume a Eulerian reference frame, although not explicitly stated. It is questionable whether or not the glider observations can be considered to be taken in an Eulerian frame. Also when a simplified box/budget model is included, it should be explicitly stated how this model relates to the observed parameters.

While the glider does move in space, it moves relatively slowly compared to the length scales of the water masses in the North Sea. Furthermore we have clearly identified the periods when we think this to be valid and when not. As the equation was replaced by the conceptual model, this is no longer an issue.

Section 5 “Supply of oxygenated water”, which is an important part of the analysis did not convince me. The authors conclude in this section that, based on the observation, the vertical mixing (!) is the largest potential source source of oxygen input in the BML. In the authors’ terminology, mixing would be represented by turbulent diffusive fluxes, not the advective term. Nevertheless, the vertical diffusive term is reasoned away, whereas the advective term is still present in equations (3) and (4).

Figure 6 shows an increase in AOU with two or three dips. These dips are associated with mixing events where oxygen rich water from the SML is injected into the BML layer. If horizontal advection were to be negligible, how can it be that the temperature in the BML drops? (see line 5/8700). The argument the authors put forward in favour of the vertical mixing hypothesis is the observed temporarily increased stratification in the temperature (Figure 7). Although it may be vertical mixing, I think it could be equally likely that the observed time series is affected by spatial variation. In the end of the day, the glider does move with respect to the water column.

I do agree with the authors that, if oxygen rich waters enter the BML, the AOU would go down. However, with everything else being equal, I would expect the AOU to increase gradually at a rate of 2.8 μmoll/dm3 /day after the mixing episode. The graph in Figure 6 would then take the appearance of a staircase. This clearly does not happen and the AOU increases rapidly until (slightly more than?) pre-dip levels. Is this increase solely due to the remineralisation of organic matter? I am suspicious, however, as in all cases (three if you count the last small dip associated with the mixing event during the early hours of 20 Aug.) the disappearance of the dip coincides with the disappearance of the vertical temperature gradient in the BML. Are they causally related? To maintain the interpretation of vertical mixing, the authors should provide more support for it. In my opinion it is equally likely that the glider crosses a patch of water (eddy?) with different a temperature and oxygen concentration. It may not be easy at all to make this distinction, given this dataset, though.

The value of high resolution data such as this is that it sparks debate and is open to different interpretation. However, we feel that the view of a stair case response where AOU would remain at a constant rate (of 2.8) after addition of new carbon and oxygen overlooks the biological response to the new addition. It is most likely that the addition of new material would lead to increased oxygen consumption, thus resulting in the curvy profile that has been observed in figure 6.

Regarding the question of advection, it is, as the reviewer states, difficult to make the distinction. We feel strongly that if advection were the dominant mechanism we would not observe such a rapid return to near identical T & S properties, and instead would observe a sharp decline as we did at the end of the survey. Because we felt we could not exclude in the influence of advection within that period, it was excluded from the analysis. Furthermore, if it is indeed a small localised spatial process rather than temporal, we still observe reduced AOU in tandem with weakened stratification. This remains in agreement with our overall assessment that small scale vertical (in our opinion temporally variable over spatially) is significant over the survey period/transect. Reviewer 2 is also in agreement with our assessment: “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" and "I agree with the authors that internal waves and shear spikes are the likely source of mixing across the pycnocline".

In this light, even the two-day trend of increasing AOU becomes questionable, but this issue could have been resolved if the trend kept increasing, had the glider flown in the opposite direction too.
I think the fundamental problem here is that this dataset, which shows a time varying oxygen concentration, does not allow to observed changes to be decomposed into a local rate of change and a horizontal varying component. It is a real pity, as this contribution could have provided interesting data on the oxygen consumption in the North Sea, but because of the mingling of time and space dimensions in the observations, the conclusions drawn in this manuscript do not have a strong foundation. Perhaps the authors can provide additional proof for their case by calculating the diffusivity coefficients for temperature and oxygen, as an indication that this process is at the heart of the observations.

As the reviewer states, the crux of the issue is teasing apart spatial variability (horizontal advection) and the time varying component. Where we disagree with the reviewer is that we feel we have clearly separated the period where the glider was exposed to advection, as illustrated in the figures. This leaves this, admittedly short, period as representative of vertical processes. Regarding the duration of the study, we are choosing to not generalise our results to the entire season, or to the entire North Sea. Instead, we put forward that this is evidence of a spatially and temporally patchy event that causes enhanced remineralisation and requires further investigation. This study does contribute small but significant information on these processes and describes similar results to those presented in a recent paper focusing on the same region of the stratified area of the North Sea (Thermocline mixing and vertical oxygen fluxes in the stratified central North Sea, Rovelli et al., in this issue). We do not believe a longer deployment, or a return transect would inform us further on these processes; it would simply provide more confidence. What this short term deployment does is to highlight mechanisms of enhanced variability in the oxygen signal that were previously ignored due to under sampling.

Reviewer 2

We're very pleased with Reviewer 2's response to the manuscript. Their comments are both detailed and insightful. We have done our best to address them as detailed below. Some of reviewer 2's comments address others detailed above, particularly those relating to the equation, these have mainly been addressed with the added conceptual diagram.

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.

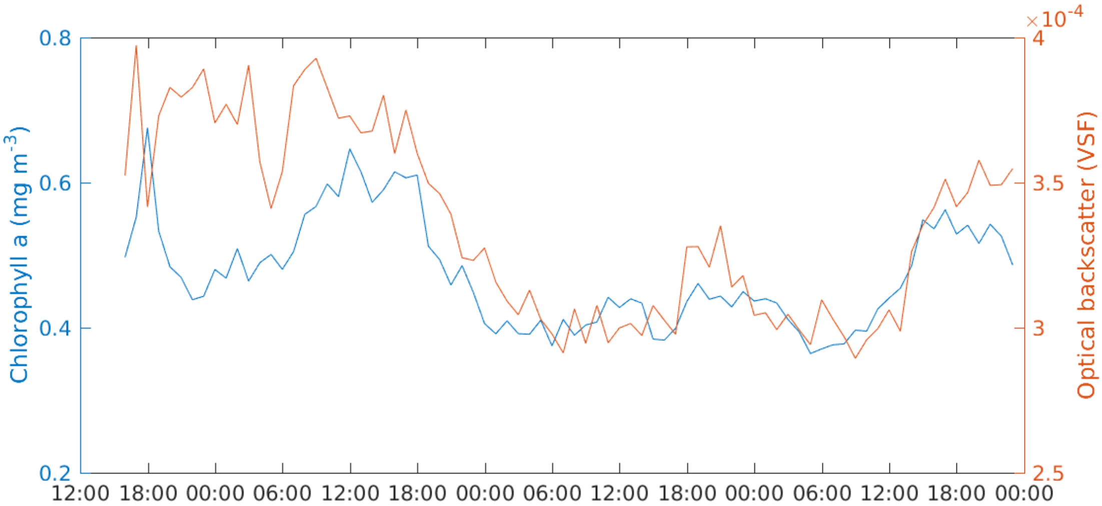
These issues are now better represented in the conceptual model and with edits to the text.

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.

While it is true that measurements of chlorophyll *a* are not a direct measurement of POC they are appropriate and have been used elsewhere (Weston at al., 2004). The empirical 50:1 ratio accounts for an estimate of dead organic matter (ie. not picked up by the fluorometer). However, we agree with the reviewer that this needs to be made more evident to the reader and have therefore amended the text:

Although chlorophyll \it{a} is not a direct proxy for POC due to the non-fluorescence of decayed organic matter and lysed cells, the 50:1 was determined empirically and therefore also accounts for non fluorescing organic matter.

We have compiled a figure to further reassure the reviewer that there is a strong link between optical backscatter (~ POC) and chlorophyll a in the BML of the North Sea. We feel the figure would be redundant in the manuscript as the 50:1 ratio accounts for POC in the region. This figure indicates mean concentrations of chlorophyll *a* and mean backscatter in the BML (defined here using the 7°C isotherm).



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.

We do highlight that mixing is not the dominant mechanism for exporting organic matter downwards from the DCM, but dominates for nutrient and oxygen fluxes. This is because dead/sinking organic matter likely dominates the POC flux (rather than mixing). Furthermore, the reviewers statement is incorrect here. Mixing events have a limited impact on larger particles that are adapted for neutral buoyancy at set densities (ie. at the termocline). While nutrients and oxygen are chemical properties of the water and therefore act as tracers (on very short time scales), larger *particulate* matter does not due to independent buoyancy properties.

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.

In the figures, we only plot the isobars below 45db – significantly below the thermocline specifically to avoid the issue mentioned. This is indicated both in text and in figure captions. Although there is vertical displacement of the thermocline, this does not impact the figure.

We did try to illustrate the data as the reviewer suggested during development of the manuscript but it made it difficult to represent the gradients in temperature. Using a fixed isopycnal or isotherm means that the vertical gradient does not vary since neither bottom or limiting isotherm change (however the distribution of isotherms varies greatly in between).

During the writing of the manuscript and data analysis we came to the conclusion that this was the best way to represent the vertical gradient. Displacement of the thermocline is not captured within the figure, and as we are not looking for a mean BML value (which would also be impacted by not fully resolving the bottom 3 m of the water column), this is not an issue; as we are looking at a gradient, displacement of the BML simply moves this gradient up and down but does not affect the slope or extent.

As we agree that it may not be immediately clear the reader, we have added further detail in text to hopefully alleviate this issue.

"[AOU is shown at depths beginning several metres below the pycnocline to account for tidal vertical displacement....]. *As we are not measuring mean BML temperature value, vertical displacement of the thermocline does not impact assessment of vertical temperature gradients. As we are looking at a gradient rather than absolute values, displacement of the BML simply moves this gradient up and down but does not affect the slope or extent."*

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 R^2 value is needed for oxygen and chlorophyll as the glider values are being used directly for C budgets later in the manuscript.

We thank the reviewer for highlighting the value of our processing technique, however we view a full explanation of the technique here to be outside the scope of this paper and not relevant to the broad readership interested in the overall rates of oxygen change. As the reviewer has confirmed, further details are available in Quest 2014. Figure 4 does contain example mean profiles and show that the method does replicate the feature of the oxygen profile well including super-saturation at the DCM.

We include in this reply a figure from Queste (2014) to reassure other reviewers and the editor of the quality of the data correction but would rather not include it in the manuscript.

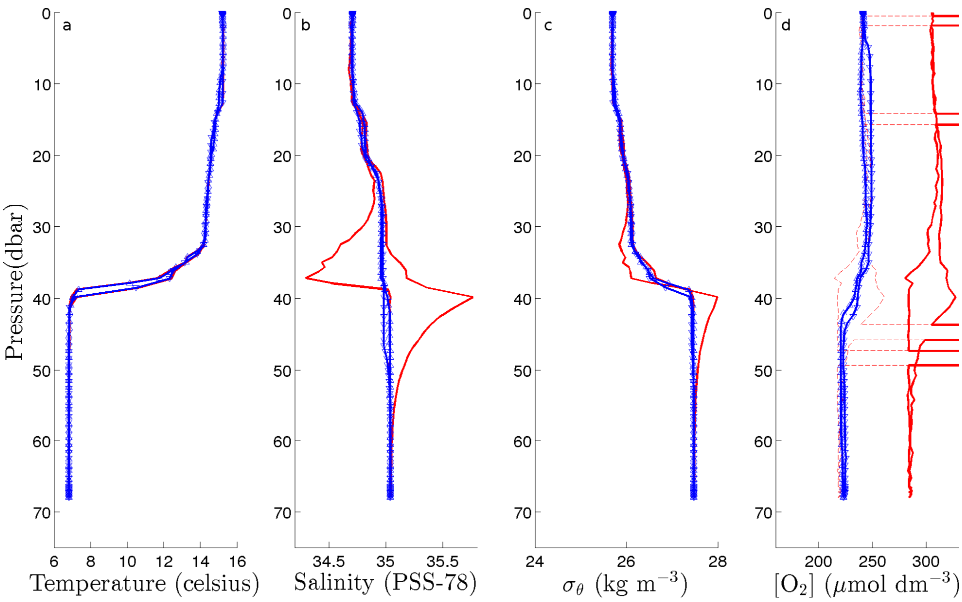


Figure 1: Temperature (a), salinity (b), potential density (c) and DO profiles (d) of the final dive for SG510. Glider observations as output by the iRobot processing are in red. Glider observations after calibration by the UEA toolbox are in blue. The dotted red line is iRobot processed DO offset for ease of comparison with calibrated data. The greatest difference between the iRobot processed data and the UEA toolbox data is located near the thermocline. The UEA toolbox provides additional corrections for thermal lag and inertia of sensors as well as corrects offsets in timestamps for each sensor providing synchronous temperature, conductivity and oxygen optode phase measurements for calculation of seawater properties.

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.

These are useful comments for further work, but as suggested by the reviewer, we agree it's beyond the scope of the paper. Furthermore we don't have direct measurements of independent BML and SML tidal velocities as the glider measured only depth averaged current and the tidal toolbox does not properly resolve the differences between SML and BML. We hope that the reference to Rovelli et al.'s paper in this issue helps alleviate this issue, particularly considering their overall estimates agree with our findings.

Regarding the shear estimates, it is indeed something that we have been wondering about – I'm trying to get an MSc. Student to investigate this further (out of curiosity) but do not feel the results would be within the scope of this paper. The analysis involves many assumptions that would make the shear estimates difficult to correlate adequately to what we've observed with the glider due to the low resolution and source of the wind and tidal estimates. If the reviewer is interested, they are welcome to contact me and these can be forwarded when they are complete.

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.

We are happy that this reviewer agrees with our main points. We will add a few sentences to say that remineralisation rates may vary through other factors. A longer study would hopefully resolve AOU and NCP over the entire summer; unfortunately we have not had the chance to redeploy a glider in the same region yet. It is very unfortunate that this deployment had to be terminated so early.

"The 50:1 conversion ratio, as well as the amount of sinking organic matter, is likely to vary on a seasonal level though changes in community composition. Species composition and bacterial cycling within the DCM will also likely impact how labile sinking organic matter is. This estimate would benefit from further in situ studies of sinking rates and organic matter export from the DCM."

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 ~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.

The Rovelli et al. paper in this issue has directly measured Kz in a similar region and has produced estimates of the fluxes. A sentence had been added to the discussion to put our measurements in the context of their results. Interestingly, beyond the measurements of Kz , the oxygen consumption rates they calculated are very similar to those that have been observed here.
"A recent study by \citet{Rovelli2015} observed much greater vertical mixing than is traditionally thought to be the case within the North Sea. Using a similar budget based approach, they identified consumption rates 5 times greater than those measured by \citet{Greenwood2010} (\~2~\unit{\mu mol~dm^{-3}~day^{-1}}). These rates, as well as high vertical mixing rates, are in agreement with our findings."

Reviewer 3

Reviewer 3 provides an interesting and different perspective to Reviewers 1 and 2, we have attempted to address their comments as best as possible. Many of the comments displayed below have been addressed in the comments and replies of reviewers 1 and 2.

The story is portrayed clearly but the quality of the presented results could do with some fine-tuning. I am a little torn about the conclusions drawn by the authors given the very short time series of data – 3 days. More below.
Unfortunately I am not an expert on oxygen dynamics and so most of my comments relate to specific areas of the manuscript and the technical merit of the paper. I really hope someone with a broader O2 knowledge will be able to review the paper as well since Sections 6 and 7 could not be adequately reviewed by me. I would request that a oxygen expert review this paper to assess its scientific impact and uniqueness (value adding aspect) given the short time series the manuscript is based on – see first major point for more on this.

General/major points:

1. My biggest concern relates to the fact that the time series of observations is very short (3 days). The conclusions made about short-term variability of oxygen depletion in the N Sea are from just this one short time series. . . ie. a longer time series of high resolution measurements showing reproducibility of such events would be much more convincing to understand their stated impact and importance. In other words, can we better quantify the importance of this observed 3 day time series on the monthly to seasonal to annual O2 dynamics and rates? In addition, I expect that a thorough understanding of the presented oxygen scenario require an idea of what the O2 conditions where like before and after this time series was collected. Defining a new unknown O2 sink from just this time series alone may be taking the dataset too far but I am willing to convinced otherwise?

We feel that reviewers 1 and 2 have produced a thorough review, with reviewer 2 in particular demonstrating biogeochemical expertise. The 3 day time series highlights particularly process and rates of change. In themselves, these rates allow only very limited extrapolation to the North Sea annual system, however they contribute to recent other short duration work that indicates that instantaneous rates are much higher than longer term estimates of state change would suggest (Rovelli et al. In this issue).

The oxygen concentrations at the start of the deployment are consistent with those which have been previously observed in this region (Queste et al., 2013) however, the main point of this paper is about the rates of change, which are locally derived. We have amended text in the introduction to highlight the spatially varying nature of oxygen depletion as shown in Figure 2.

"These processes can be bothspatially and temporally variable. This study takes place in 2011, where we observed a different spatial patter than in 2010. The oxygen depleted area experienced an overall northward shift but with similar intensities of oxygen depletion (Fig. \ref{nsea2_context})."

2. The use of the term 'depocenters' seems a bit loose throughout the manuscript and is also not specific enough – more detail needs to be provided to suggest it is a depocenter and what may be leading to it being clasified as an area subject to depocenters. Depocenters are normally formed when the water column shear stress/bottom shear stress is very low that allows for deposition mostly in dips in the topography. Is the tidal and mixing (advective and vertical) not too high to clasify these as depocenters? I am normally familiar with this term being used in report writing (to generalise areas and remain unspecific) and not in scientific publications, which needs to be much more specific and detailed on the characteristics and features.

The term depocentre has been previously described in scientific literature pertinent to this region most notably in the papers by Van Raaphorst who has published extensively on the role of sediment remineralisation in the North Sea. It is possible this term is has only really been used in reference to papers pertaining to the North Sea? We have provided a list of publications that the reviewer might find of interest, all referring to depocentres.

Greenwood, N., Parker, E. R., Fernand, L., Sivyer, D. B., Weston, K., Painting, S. J., … Laane, R. W. P. M. (2010). Detection of low bottom water oxygen concentrations in the North Sea; implications for monitoring and assessment of ecosystem health. Biogeosciences, 7(4), 1357–1373. doi:10.5194/bg-7-1357-2010

Ruardij, P., & Raaphorst, W. V. (1995). Benthic nutrient regeneration in the ERSEM ecosystem model of the North Sea. Netherlands Journal of Sea Research, 33(3-4), 453–483. doi:10.1016/0077-7579(95)90057-8

Tyson, R. V., & Pearson, T. H. (1991). Modern and ancient continental shelf anoxia: an overview. Geological Society, London, Special Publications, 58(1), 1–24. doi:10.1144/GSL.SP.1991.058.01.01

Van Raaphorst, W., Malschaert, H., & Van Haren, H. (1998). Tidal resuspension and deposition of particulate matter in the Oyster Grounds, North Sea. Journal of Marine Research, 56(1), 257–291. doi:10.1357/002224098321836181

Weston, K., Fernand, L., Nicholls, J., Marca-Bell, A., Mills, D. K., Sivyer, D. B., & Trimmer, M. (2008). Sedimentary and water column processes in the Oyster Grounds: a potentially hypoxic region of the North Sea. Marine Environmental Research, 65(3), 235–49. doi:10.1016/j.marenvres.2007.11.002

3. The presentation of chl-a glider data needs some improvements. Firstly, using only the manufactures calculations to derive chl-a is by my experience innacurate, especially because regionally specific fluorescence to chl-a ratios exist and should be taken into account. Bottle samples should be collected in situ with the glider profiles in order to filter, acetone and then read on a fluorometer calibrated with pure chl-a standard. These bottle 'gold standard' chl-a values are then regressed against the Seaglider derived ones and corrected appropriately. Were bottle samples collected during the SG mission?

We accept that manufacturer calibrations alone are imprecise, however in comparison with other published calibrated data this values are consistent. Unfortunately no samples were collected as the glider was recovered (during a storm) following a software failure and it was not possible to do a simultaneous CTD cast.

Simple fluorometry as suggested by the reviewer is also unfortunately inaccurate by 20-30%. When calibrating a glider, due to the inherent spatial variability of chlorophyll *a*, the sensor

generally needs to be calibrated alongside the CTD's sensor rather than separated by a few tens or hundreds of meters. We have found in previous studies that numerous glider dives and CTD profiles are required to get a robust calibration. We feel that the manufacturer's calibration is the best available for this study. Furthermore, there are a number of assumptions in our method with approximations at each stage, most notably the chlorophyll to carbon conversion. To reflect the uncertainty in these estimated, we have amended the summary to highlight these assumptions and included an error estimate in our derived consumption rates. We hope this will be satisfactory to the reviewer.
"Although chlorophyll \it{a} is not a direct proxy for POC due to the non-fluorescence of decayed organic matter and lysed cells, the 50:1 was determined empirically and therefore also accounts for non fluorescing organic matter. Based on the variability of chlorophyll \it{a} within the DCM, the calibration of the glider sensor and the variability of the POC to chlorophyll \it{a} ratio (< 25\%), we estimate the error of our oxygen consumption estimate to be $\pm$1~\unit{\mu mol~dm^{-3}~day^{-1}})."

The quenching of the chl-a fluorence data should be corrected as best possible when presented in a scientific publication. There are numerous ways to correct for quenching, including using the available backscatter data to correct it – very handy to have that available and should be utilised (see Berenfeld and Boss, 2003; Sackman et al., 2008; Swart et al., 2014).
As highlighted above there are a number of approximations that need to be made when looking at chlorophyll *a* fluorescence. However quenching is not a significant concern for the DCM and especially not in the BML of the North Sea. The impact of quenching is discussed in text in relation to Figure 3, and the fact that quenching is *not* corrected is stated in text. This was chosen as correcting for quenching in non homogeneous layers (ie. DCM) often requires extrapolation of euphotic depth chlorophyll values and would overestimate chlorophyll *a* in the SML. As is clearly visible in Figure 3, where we can observe the diurnal effect of quenching, quenching only affects chlorophyll *a* down to approximately 25m depth, which is a typical euphotic depth for the central North Sea at this time of year.

Lastly, Section 6.2 refers a lot of the chl-a data and DCM but all these references are to Fig 3d. One cannot clearly see the variations of this chl-a with the section alone – esp. when you start referring to diurnal variability and links with the winds enhancing chl-a through nutirent supply. . . these statements are too definitive given the way the data is currently displayed. In order to really see the variations a 1D time series of integrated chl-a through the water column should be displayed. If this doesn't reveal any variability then the DCM should be somehow isolated and plotted as a 1D time series. Hence, coming back to the quenching – if you provide an integrated time series, then the data has to have the quenching addressed first as this will bias the time series.
We have amended the text to be less definitive. The text also described how quenching was not affecting the DCM as fluctuations of quenched chlorophyll at the surface and fluctuations of chlorophyll fluorescence at the DCM are out of phase. The main thrust of the paper is not to explain the mechanisms of mixing at the DCM. The main focus of this section is to estimate the carbon exported into the BML, from BML observations.

Fluctuations in chlorophyll a at the DCM (Fig. \ref{nsea2_sections}) are not due to quenching as periods of high DCM chlorophyll a concentrations are present when surface chlorophyll a fluorescence is affected by high light intensity. Instead, it is possible that these fluctuations in *chlorophyll a fluorescence at the DCM may be caused by increases in nutrient supply linked to stronger winds that occurred in the mornings (Fig. \ref{nsea2_wind}).*

If the reviewer is still in disagreement, we offer to remove the portion of text detailing variability at the DCM as it does not impact on our assessments throughout the paper. We included it as we felt it was an interesting element to highlight within the paper due to the importance of the DCM in recent studies of the North Sea.

4. Wind and tide data: I did not find enough details on the wind and tide data you used, especially for the wind. What is the temporal and spatial resolution of the winds used? How did you co-locate this with the glider locations and what are some of the assumptions?

"The comparison was made by extracting data from the closest point in time and space to the glider's location from the ECMWF 6-hourly high resolution data. Linear interpolation was used in time between the ECMF data."

The figure 8 caption has been amended to indicate the resolution of the ECMWF data.

Also, there is likely some sort of lag between the observed wind and the mixing events. . . can be 2-18 hours depending on the region, the depth of MLD and extent of stratification. Perhaps there were no correlations with winds if you have not applied a lag to the mixing events? MLD should be displayed on Figure 3 sections. This should tell you straight away if there may be a link between the wind and mixing. Perhaps a Brunt-Vaisala Frequency section may reveal something? Overall I don't think the authors have done a good enough job at relating winds and mixing with the available data to make the statements they use in the text.

As stated earlier the main thrust of this paper is not to unambiguously define the source of mixing, particularly in such a short data set, the discussion of mixing is included here because it is natural to include the meteorological and tidal forcing.

Both reviewers 1 and 2 have also put forward some interesting suggestions as to the mixing processes and seem to agree with our suggestion that shear spikes (which are linked to winds) dominate. As discussed in reviewer 2's comments and reply, further investigations into shear stress would not be possible without accurate tidal velocities for both the SML and BML. A further assessment of wind considering lag as the reviewer suggests could be attempted, but would have very little relevance considering the short duration of the survey.

5. Figure use and quality:

Figure 3: some panels could be removed, namely density if you rather plot the density contours on one or more of the other plots instead, which would be useful. Figures will hopefully be bigger since the ones on BGS Discussions are tiny. Add MLD to plots. Maybe I am wrong but I do not think its the norm to display the 650nm scatter data in raw Beta units as this is hard to interpret or compare to other work. The data should be converted to conventional backscatter?

We feel showing all three of temperature, salinity and density is an appropriate and efficient method of showing their relative contribution and the mixed layer depth in this two layer system. It is our intention to have the panels be formatted to make best use of a full page in the final version of the manuscript. We hope that when the figures are larger, this will no longer be an issue. We will be happy to place the panels side by side if this increases the size of panels during publication.

Indicating MLD would also be misleading here as MLD is different to thermocline depth, however we are only interested in the BML in this study. We feel the figures are more descriptive as they are. One could instead plot the 7°C isotherm if we wished to highlight the BML, however we do feel that a thin black line would be lost in the rapid change from red to blue and would mask the gradient without truly adding any additional information.

Regarding the units of the backscatter data, the units displayed are those representative of a volume scattering function (often abbreviated VSF). The reviewer is possibly referring to Formazin Turbidity Units (FTU)? VSF is a measure of reflectance whereas FTU is a measure of absorbance. I am unsure of what the reviewer refers to with "conventional backscatter". Backscatter can be used as a proxy to POC however it is highly dependent on a good measures of water inherent optical properties and POC samples which we do not have available.

We have added the following details in the figure caption "...*optical scattering as a volume scattering function at 650~nm...*". We are awaiting a reply from manufacturers concerning the possible conversion to other units but have not had a reply yet. In the mean time, we do not feel it is a critical issue as we do not refer to absolute values of backscatter and only use the panel to point out relative changes in backscatter.

Figure 4 and 5 are hardly used and discussed. Fig 4a-d are not even refered to in the text as far as I could see. They must either be more used in the manuscript or left out or included into other figures where possible. Figure 4 in fact could be discussed more and reveals more about whats hapenning than revealed by the authors in the text. Figure 5 really does not reveal much and can be removed.

We feel that figure 5 is important to highlight the variability in the SML of the North Sea. We agree it is of little relevance to the BML but we feel it still provides interesting context. As neither Reviewers 1 or 2 have asked to have figure 5 removed, we will leave it in to avoid the risk of removing a figure that they have found useful.

The reviewer is correct in saying that Figure 4 could be better utilised, *additional references have been added to the figure at relevant places in text.*

It is worth noting though that Fig 4d has been specifically mentioned P8696/L21. Figure 4 is also referenced as a whole (without indicating individual subpanels) multiple times, most notably P8696/L8-12 when discussing stratification and vertical profiles of temperature and salinity (panels a to c).

It seems Figure 8 was included in a hurry. There are no y-axis labels and units. There is no wind speed vector magnitude indicator. I would recommend representing wind speed here as wind stress to understand its force on the sea surface, as is the norm. I don't believe adding the bathymetry plot (c) reveals anything and should be dropped. These small fluctuations in the bathy (72-75m) in my opinion don't have bearing on anything proposed in the manuscript or does it say something about the presence of depocenters? Fig 8 could do with the same vertical line indicators on it as displayed in Figs 6 and 7 that indicates the higher 'mixing' events so reader can relate time of events.

I am perplexed by the reviewer's comments as the version I obtained from the Biogeosciences website has an x-axis for all three panels and a y-axis for panels *a* and *c*.

The absence of a y-axis on panel b seems to me natural as the figure does not have a second dimension as such. We have however included windspeed magnitude vectors in the bottom left of panel *b*.

We shall make the windspeed magnitude vectors more evident by changing their colour and copying the 5 m s^{-1} label from the figure legend to the figure vectors.

We also agree that the bathymetry plot (c) does not reveal any significant features which would be capable of influencing our analysis of the data. This is particularly why we feel it is necessary to include it. In shallow systems such as the central North Sea, a large bathymetric feature would likely lead to increased vertical mixing and therefore would be the first suspect when trying to explain the vertical mixing processes we've observed. It is included so that this possibility is rules out and therefore show the reader that processes such as shear spikes are the likely cause.

We will also add vertical lines to Fig. 8 for more uniformity between all figures.

Minor comments

(R3) There is generally an over use of the semi colon - ; I think rather start a new sentence or restructure the sentence in many cases.

We will do a careful proof read of the article during submission; such a process will allow removal of superfluous semi colons.

(R1) Abstract: here terms are used "short-lived" and "small scale". Is it for the target audience clear what order of magnitude these relative qualifications indicate? Perhaps best to make it explicit.

We have added an indication of scale in brackets: *"[...] small scale events (< 200m / 6 hr) [...] localised or short-lived (< 200m / 6 hr) increase in oxygen [...]"*

(R2) 8694/10 – 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.

The text has been amended to indicate the source of the data and the reference to the paper that performed the study and the compiling of the data.

"Historical data originating from the ICES database (Queste, 2013) revealed a similar distribution of low dissolved oxygen (DO) in the BML during summer in the ND and OG regions."

(R2) 8694/12 – 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.

The text has been changed to:

"Queste et al. (2013) suggest that the same mechanisms likely lead to the depletion of oxygen at both the OG and ND sites. These mechanisms are thermal stratification preventing vertical resupply of oxygen; reduced advection slow replenishment of local BML oxygen and continuous remineralisation of organic matter in the BML leads to gradual decline in oxygen concentrations. At both sites, it is thought that the replenishment of oxygenated waters through advective processes is limited by local topography."

(R1) 8694/12: suggested → suggest (published knowledge)

Change implemented as suggested.

(R3) 8695/24: The 16min casts is an average. Please provide a range rather as this is very specific.

We thank the reviewer for asking this question as it allowed us to identify an inaccuracy in our information. Dives were 16 min on average, with cast durations being on average 7.6 min long with a standard deviation of 0.8. This has been amended in the text.

(R2) 8696/27 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.

Please refer to our reply to this comment in the major comments section of reviewer 2, where we have included a figure that illustrates the mean chlorophyll in the BML.

(R2) 8697/Eq1: See comments on vertical flux of oxygen term being presented incorrectly above.

Now corrected using the conceptual diagram.

(R1) 8698/25: "... would require a month." Like you do in the discussion I would emphasise here that it is just or not more than a month.

"would require a month" has been changed to "would require no more than a month. Continuous consumption at this rate over the entire summer would very rapidly lead to severe hypoxia."

(R1) 8699/18: To observe the supply...

Text amended.

(R3) 8700/ 3: change to 'freshening of 0.3 in the SML'

Text amended.

(R3) 8700/22: is '3m' a typo. If not its unclear what you are showing here. Max gradient of pycnocline is over about 10m and found 30m below sfc.

Text amended: *"spanning approximately 8 m (\~30 - 38m*)"

(R2) 8703/13: "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).

Text amended as suggested with additional references to back up the statement.

(R2) 8703/14: 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).

See detailed reply in the major comments section.

(R2) 8703/22: "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.

Text amended as suggested.

(R1) 8704/8: Figure 3d shows **(8705?)**
Text amended as suggested.

(R1) 8704/17: If this were the case **(8705?)**
Text amended as suggested.

(R2) 8705/19: "Unresolved mechanisms contributing to oxygen depletion" – like remineralisation of dead particles, as well as non-photosynthetic particles/heterotrophs?
We assume the reviewer is referring the previous comment where chlorophyll fluorescence does not resolve dead organic matter, and therefore also refer back to our reply in the major comments section.

(R1) 8710/7: Van der Molen... (Also some inconsistencies in the reference list in relationship to Dutch surnames starting with "van".
Document library has been updated and the changes should be reflected in the next latex compile.

(R2) Figure 3: This is quite difficult to see, would it be possible to represent plots 3 x 2?
We will work with the editors to identify the best ratio. We suspect 4 panels on the left, 3 on the right, and the caption in the inset, covering the whole page would be the best solution.

(R2) 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.
Indeed, there is an erroneous salinity value at the bottom, where a thermal lag correction caused an incorrect value (division by 0 as the glider stalled at the bottom). This will be corrected in a new figure.

(R2) 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.
Both figures could be included as subplots, however as it stands I have a preference for having both separate. I feel both figures are relatively complicated at first glance and each merits having its own clearly defined caption to ensure the reader understands what's going on easily. I will ensure both figures are on the same page and comparable in the final version.

(R2) Figure 8a & c: I'm not sure how much these figures add to the paper content, see notes above on bulk shear/Richardson number.
Reviewer 3 highlighted this issue as well. I agree that neither show features that would cause the mixing events discussed in the paper, but both are features that could potentially have a strong influence (and are therefore included to show that they do not contribute in this case). Both panels also provide contextual data that help clarify to the reader what the general region is like from a hydrodynamic and topographic point of view.

(R1) 8701/13: The direction of the tidal currents is not shown. Perhaps better to speak of currents than velocities.
Text amended as suggested. Tidal velocities where originally shown but later removed.