

Response to **Referee #2**

Note on color-coding: Reviewer's comments are in black, responses in blue, and potential/suggested additions in green.

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 summer 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.

We agree with the reviewer that the interpretation of short-term studies in the context of seasonality and seasonal processes has to be carefully weighted and validated. In that respect, we do stand by our core data and remind the reader on several occasions throughout the Result and Discussion section, that the consideration of the BBL O₂ replenishment via turbulent transport and the O₂ budget refer to only our observational period. We present our study as a process-oriented study, not a seasonal study.

Based on our study, however, we hypothesize that the physical processes described and proposed in this study are relevant for the O₂ dynamics in the thermocline and BBL and the potential occurrence of O₂ depletion in the central North Sea.

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 aim of the BBL O₂ budget of this study is to disentangle and discuss what we consider to be the main processes/pathways controlling the BBL O₂ dynamics during our observational period. For these 3 days, we feel that an assessment of the variability of the referenced data used in snapshot BBL O₂ budget would be very speculative, due to the lack of long term monitoring data.

The values for BBL loss are now shown in SI Figure 2, and give a depletion rate of $-0.42 \mu\text{mol kg}^{-1} \text{d}^{-1}$ ($R^2 = 0.6$). This observed O₂ loss in the BBL are extremely comparable to those reported in Greenwood et al. (2010) for nearby North Dogger. We now provide more information about core dataset in the methods section and results section and we added a figure in the supplement (see specific comment to section 3.3).

Benthic remineralisation rates were taken from a parallel eddy covariance (EC) study performed by McGinnis et al. (2014); the data cover the same exact time and the same location, and thus provide a solid reference for the benthic remineralisation during our observational period. We only refer to the mean benthic O₂ uptake reported by McGinnis et al (2014), and refer the reader to the publication for more information regarding the dynamics. The mean SUR, $-10 \text{ mmol m}^{-2} \text{d}^{-1}$, also compares well with the ex situ diffusive uptake rates from ex situ O₂ microprofiling at the Oyster Grounds (Neubacher et al. 2011) and with the modeling effort of Meire et al. (2013), also at Oyster Grounds. They reported average rates uptake of $-9.8 \text{ mmol m}^{-2} \text{d}^{-1}$ and $-8.6 \text{ mmol m}^{-2} \text{d}^{-1}$, respectively.

We now explicitly relate to those studies in the text:

“The SUR was consistent with the average SUR at Oyster Grounds reported by Neubacher et al. (2011), $-9.8 \text{ mmol m}^{-2} \text{d}^{-1}$, as well as with modeled SURs at the same site (average $-8.6 \text{ mmol m}^{-2} \text{d}^{-1}$; Meire et al. 2013).”

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.

We do agree with the reviewer that presenting the physics and the fluxes could be sufficient for a paper. However, we are confident that section 4.4 presents an important yet overlooked aspect of the interaction between primary producers and the physical environment. The mechanism we propose is a new hypothesis which we feel is relevant to present here. Therefore, we feel it could be a key aspect for the O₂ dynamics as it could further promote bottom water isolation and therefore low O₂ conditions in climate change scenario under the current climatic projections. The section was shortened (one paragraph removed) and we have revised the final consideration in the light of climate change and O₂ depletion in the North Sea to relay the above message more clearly.

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 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?

We acknowledge that other studies might have not presented such layer but a thicker surface boundary layer (see reviewer's comment to 9914L14). We base our water column description on our observational period, when we detected the presence of a second vertical mode near-inertial wave. Further discussion on the occurrence and development of the layers requires seasonal data of vertical density profiles. As we do not have such data as supporting evidence, any consideration on the transition layer occurrence in the summer time would be too speculative, and behind the scope of this study.

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.

We are pleased that the reviewer feels that our study can contribute to the current knowledge of O₂ dynamics in the North Sea. We are very grateful for the reviewer's comments, and feel like they are all easy to incorporate within our revised manuscript.

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.

We understand the reviewer considerations here. The abstract was structured as such to better relate to the foci of the special issue, as advised by the associated editor. As the reviewer pointed out on his general consideration of this manuscript, studies based on short datasets in a seasonal settings struggle to present their results as in terms of seasonality due to the lack of long term supporting evidence. For such reason, we believe that presenting the O₂ fluxes or the results from our snapshot O₂ budget quantitatively would overpraise our results. While we can speculate that the processes described and investigated in this manuscript are relevant for the O₂ dynamics in the BBL during the stratification period, we feel it is not appropriate upscale our rates to the entire summertime.

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.

To improve clarity the sentence was reformulated and reads:

“In the center region of the thermocline we observed substantially suppressed turbulence compared to the thermocline-BBL interface. Therefore an upward shift in the production layer could lead to further isolation of the bottom water and thus further promote the seasonal occurrence of lower O₂ concentrations”

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.

We agree with the reviewer. The former formulation could mislead the reader. The section was modified accordingly and refocused to the Shelf Sea and coastal hypoxia.

“1.1 Hypoxia in shelf seas and coastal regions

The distribution of dissolved oxygen (O₂) in shelf seas results from the complex interaction between biological processes (photosynthesis and respiration) and physical processes (O₂ flux pathways) occurring within the water column and at the seafloor. O₂ is therefore regarded as an important indicator of ecosystem functioning for aquatic organisms (Best et al., 2007) as well as for benthic activity (e.g., Glud, 2008). Changes in the O₂ concentrations can have severe impacts on the shelf ecosystems. O₂ concentrations below 62.5 μmol L⁻¹, which is generally regarded as the threshold of hypoxia (Vaquer-Sunyer and Duarte, 2008) were shown to impose significant stress on aquatic communities leading to increased mortality among fish communities (Diaz, 2001). This also highlighted not only the ecological but also the economic impacts of O₂ depletion, leading to increasing concern regarding the occurrence of hypoxia and hypoxic events. In fact, as reviewed by Diaz and Rosenberg (2008), hypoxia in coastal environments is spreading and so are the reports of unprecedented occurrence of hypoxia in several shelf seas and coastal regions (Grantham et al., 2004; Chan et al., 2008; Crawford and Pena, 2013).”

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?

The section was intended to provide an overview of “Oxygen depletion in the North Sea”, we thank the reviewer to pointing that out. We have restructured the section to provide a more rounded description:

“In the North Sea, the occurrence of low O₂ levels in bottom waters has already been reported in the past (e.g., North Sea Task Force, 1993; Greenwood et al., 2010). More recently, monitoring studies in the central North Sea for the 2007 – 2008 period have shown that O₂

concentration in the bottom waters at the Oyster Grounds and North Dogger can drop as low as $163 - 169 \mu\text{mol L}^{-1}$ (60 – 63 % saturation) and $\sim 200 \mu\text{mol L}^{-1}$ (71% saturation), respectively (Fig. 1; Greenwood et al., 2010). Comparable field observations were also reported in the summer of 2010 (Queste et al., 2013). The authors also reviewed the available historical O₂ data in the North Sea (1900 – 2010), revealing a clear increase in O₂ depletion after 1990.

While the reported O₂ levels were still above the hypoxic threshold, growing concerns of hypoxia developing in the North Sea have highlighted the need for more detailed studies on the O₂ dynamics and driving forces (Kemp et al., 2009). In fact, since 1984 surface water temperatures in the North Sea have increased by 1 – 2°C, greater than the global mean (OSPAR, 2009, 2010; Meyer et al., 2011). On seasonal time scales, climate projections indicate longer duration of the stratification period and stronger thermocline stability (Lowe et al., 2009; Meire et al., 2013), with some projection also suggesting earlier onset of stratification (e.g., Lowe et al., 2009). Due to the semi-enclosed nature of the North Sea, earlier onset and longer stratification increases the length of time that the deep water is isolated, potentially allowing lower O₂ concentrations to develop (Greenwood et al., 2010).“

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.

We agree, the eutrophication aspect of section 1.2 was removed

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 concentration/saturation in shelf seas.

We state that the distribution is “largely” controlled by physical processes. Obviously respiration and primary production control the production, utilization of O₂, recycling of nutrient, etc. however, the distribution and specifically the fluxes are strictly physical processes.

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.

We agree with the reviewer, the original title of this section and that of section 1.1, 1.2 we too general therefore misleading. The section is now more appropriately title “Physical controls on oxygen dynamics”. We respectfully disagree, however, with the reviewer suggestion to present all the processes that affect O₂ dynamics in shelf seas within this context. We thoroughly cite appropriate references for this information in other section of the manuscript.

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.

We did not observe any clearly quantifiable horizontal gradients in density during our survey at the Tommeliten site, which included towed near-seafloor CTD transects. Over our observational period, we also did not observe any change in the BBL temperature or salinity over the tidal cycle that would suggest advection of different water masses. Based on that we believe that our assumption of $K_{\rho} = K_z$, which is generally established in such conditions, is justified.

SECTION 3:

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

This is a stylistic choice to better guide the reader across the sections. As the result section reflects the dense Methods section, we believe that an introduction paragraphs at the beginning of the section will increase the readability. We leave the final decision to the associate editor and editorial board.

9914L4: "oceanic background" could just be hydrographic

Yes, the term "hydrographic" is more appropriate within this context.

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.

The layers were separated based on temperature changes. Salinity was on average 35.08 with little variation throughout the water column (35.04 to 35.1) and thus contributed very little to the observed stratification. We have added additional information on salinity in the Result section and added a description of the layer separation to caption of Fig. 2. The additional sentences read:

"Water column layers were identified based on the temperature profiles. A 0.2°C and 1.5°C decrease from the surface boundary layer average temperature (3–6 m depth) was used to determine the depth of the surface boundary layer – transition layer interface and the transition layer – interior interface, respectively. Correspondingly, a 0.2°C from a 50-60 m depth average temperature was used to locate the interior – bottom boundary layer interface."

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

The section was updated accordingly and reads:

"The O₂ profiles were generally characterized by near saturation in the SBL and transition layers, with O₂ concentrations in the 238 – 243 μmol kg⁻¹ range, and undersaturated (~80%) in the BBL, where

the O₂ concentration was ~243 $\mu\text{mol kg}^{-1}$ (Fig. 2c,d). The stratified interior was oversaturated by up to 115%, with a well-established O₂ maximum at ~39 m depth with concentrations up to ~315 $\mu\text{mol kg}^{-1}$.”

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

As we realize that such information is relevant to a specific audience, we have now added the spectra plot as supplementary information (SI Figure 1).

9916L23: There is no figure 6.

The whole paragraph refers to Fig. 4. The typo was removed from the text.

SECTION 3.3:

9917L14-18: How accurate is your assessment of $d\text{O}_2/dt$, a figure showing the observed 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.

Our assessment of $d\text{O}_2/dt$ is limited to the short O₂ timeseries collected during our observational period. We agreed with both reviewers that these data were not properly introduced.

This has now been revised and we provide a better description in the methods and results section (see below) and we have added a figure of our O₂ timeseries with the fitted linear regression curve in the supplement (SI Figure 2).

We observed variable O₂ concentrations over 52 hours, but an overall decreasing O₂ concentration trend. Such trend was quantified via linear regression to be $-0.42 \mu\text{mol kg}^{-1} \text{d}^{-1}$ ($R^2=0.60$). Despite the limited amount of data the inferred O₂ loss rate, once expressed as areal rate, was about $-15 \text{mmol m}^{-2} \text{d}^{-1}$ and thus within 2% of the rate observed by Greenwood et al. (2010) for the North Dogger, which were based on an extensive mooring study over almost two years. This gave us confidence that our estimates were realistic for the in situ condition at Tommeliten during the mid-late summer stratification period.

The text additions read:

“The POZ lander was also equipped with a Winkler-calibrated O₂ optode sensor (Aanderaa Data Instruments AS, Bergen, Norway) which recorded BBL O₂ concentration continuously at 1 min intervals.”

“The apparent BBL O₂ loss of $-0.42 \mu\text{mol kg}^{-1} \text{d}^{-1}$ was determined from the POZ lander O₂ optode time series (SI Figure 2) over 52 hours, ($R^2=0.60$). Though over a short time interval, the apparent BBL O₂ loss was about $-15 \text{mmol m}^{-2} \text{d}^{-1}$ and thus within 2% of the nearby North Dogger average presented by Greenwood et al. (2010).”

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 dO_2/dt and not a spatial change?

We understand the reviewer concern over the potential contribution of horizontal advection to the O_2 balance. Although with our measurement setup we cannot quantify horizontal advective O_2 fluxes our data does not suggest that such fluxes would significantly contribute to the O_2 balance (see paragraphs below for details)

The temporal O_2 variability in the BBL was continuously recorded by an optode mounted on our POZ lander (SI Figure 2) simultaneously with current velocities (Figure 3). We reported that the strongest velocity signal was due to the tides and inertial currents.

If horizontal O_2 gradients were elevated at the Tommeliten site during our observation period, than we would have likely observed variability in the O_2 concentration on tidal and or inertial frequencies in the POZ O_2 time series. The fact that such periodicity was not observed suggests that there were no large horizontal O_2 gradients.

Additionally, mean currents in the BBL were only about 2 cm/s and thus small compared to the tides. This, in conjunction with weak horizontal O_2 gradients, suggests that horizontal advective O_2 fluxes are likely to be small.

SECTION 4:

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

This is a stylistic choice to better guide the reader across the sections. As the Discussion section merges considerations crossing disciplines, we believe that the introduction paragraph will provide the reader the tools to efficiently follow the points raised in the discussion section. The associate editor should take the final decision on the subject.

SECTION 4.1:

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

We made the figure available in the supplementary information (SI Figure 3).

SECTION 4.2:

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

We have expanded the sentence to improve readability:

“Based on the above, we can argue that O_2 dynamics during the stratified period are more complicated than previously regarded. To maintain an excess of O_2 in the thermocline, primary producers require adequate nutrient entrainment from the bottom water to fuel potential new production. The resulting increase in productivity and subsequent export to the bottom water could therefore boost the carbon turnover estimates substantially.”

9920L15-17: 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.

The reviewer is correct; it might not be all new production, but rather recycling. We tuned the sentence 9920L15-17 down:

"The resulting increase in (new) productivity and subsequent export to the bottom water could therefore boost the carbon turnover estimates substantially."

SECTION 4.3:

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

We are, of course, aware that the hydrology differences between the generally well-mixed southern North Sea sites and the seasonally stratified central North Sea.

The whole paragraph (9921L21-9922L7) provides evidence of the influence of tidal forcing on both vertical transport of constituents (O₂, OM, macronutrients, ...) and on primary producers and resulting primary production. In such context, the study by Blauw et al. (2012) provides evidence of a close correlation between tidal motions and phytoplankton biomass (from Chl.a concentrations), which seems to suggest a physical control over primary production. In Section 4.4 we then expand the concept to migrating phytoplankton (armored dinoflagellates which are observed in central North Sea – Reid et al., 1990) and hypothesized that under low/lower turbulent mixing (i.e., stronger stratification) they could bypass the physical constraints of stratification and shift the depth of primary production.

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?

The sentence was reformulated accordingly:

"The flux of O₂ from the DCM production zone downward to the BBL could set the lower limit of the BBL O₂ concentration, and thus the O₂ depletion level, during the stratification period."

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.

We do believe the reviewer misunderstood us here. Indeed we assume a 1:1 ratio C:O₂. If there is no isolation (fully mixed waters) production and remineralization are likely to balance out if there is no influx of nutrients as the system will recycle matter (no new production). However, turbulence transport would have limited effect on POM, and thus you would still expect a SUR and thus a net O₂ loss in the BBL, but at a much slower rate

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?

We argued that the same turbulent transport that supports the O₂ export from the DCM to the BBL also supports BBL nutrient import to the DCM, and this could drive additional new production. We are aware that this is an oversimplification, as we are, conceptually, not separating new production from recycling. However, the main point here is that migrating plankton can overcome stratification by actively swimming towards the interface with the BBL to access nutrients. In such scenario, the physical transport limitations would not necessarily impede primary production, but only mainly the O₂ flux towards the BBL. We now mention explicitly the fact that in such scenario, migrating algae species would still be able to access nutrients from the BBL.

“Migrating phytoplankton could therefore access BBL nutrients in this scenario, i.e., primary production rates would be comparable, but the result would be an evident further decrease in the BBL O₂.”

“Of course, whether such scenario could be sustained over the whole stratification period is not known and requires further assessment.”

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

This paragraph was removed. Accordingly, we have revised the final paragraph to link with the previous section and to streamline our conclusions.

“In the light of climatic changes, studies have suggested that O₂ loss in the North Sea bottom waters would mainly result from a strengthening of the stratification and O₂ solubility reduction with increasingly warmer waters (e.g. Meire et al., 2013). The findings of this study suggest there might be an additional level of complexity based on the interplay between the tidally-driven physics, water column structure, biogeochemical cycling and active phytoplankton migration in the central North Sea. The proposed mechanism could contribute to the observed decreasing O₂ levels in the North Sea water column, however, further detailed studies are obviously necessary to validate and fully quantify this effect at the seasonal level.”

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 significant role in vertical exchanges at the thermocline.

The main purpose of the image is to locate the Tommeliten site, which in the literature is not as well represented as the North Dogger Bank and Oyster Grounds. We understand the reviewer concern but we feel that in-depth descriptions and visualization of the general North Sea and central North Sea specific features are already well presented in other studies, Queste et al. (2013) being one of them, or

in the well-cited review by Otto et al. (1990). We have added the location of the North Dogger and Oyster Grounds to provide the reader reference points to other studies in the central North Sea, specially that of Greenwood et al. (2010), which is highly relevant to this study and the associated O₂ budget in the bottom water.

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.

Fig. 4c shows both upward and downwards O₂ fluxes (white and grey dots, respectively). In the 33 – 37 m range, the average flux reflects the alternating upwards and downwards fluxes that were observed. At both 35 m and 37 this resulted in reduced net fluxes during the observational period. We have added a further sentence the figure caption to avoid misunderstandings:

“Note that in the center interior (33 – 37 m) the average reflects the combination of the variability of the observed upward and downwards fluxes”

Is Fig. 6 missing?

The text refers to Fig. 4, and not Fig. 6. We have now removed the misreference from the text.

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

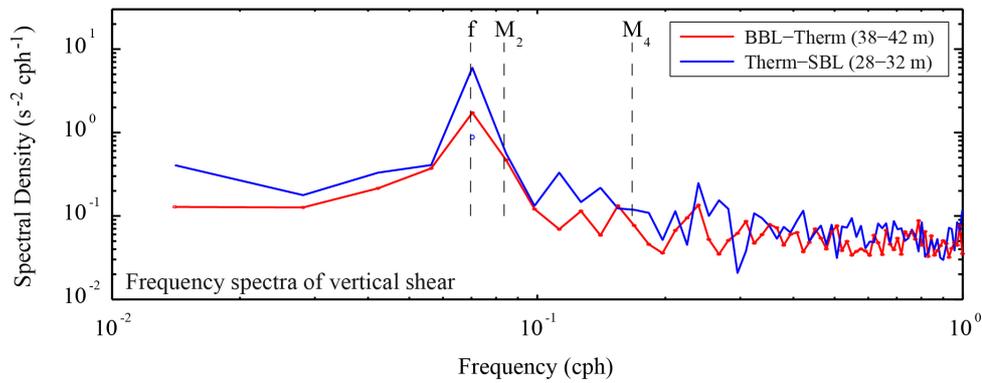
We thank the reviewer for noticing that. The discrepancies were corrected

Added References

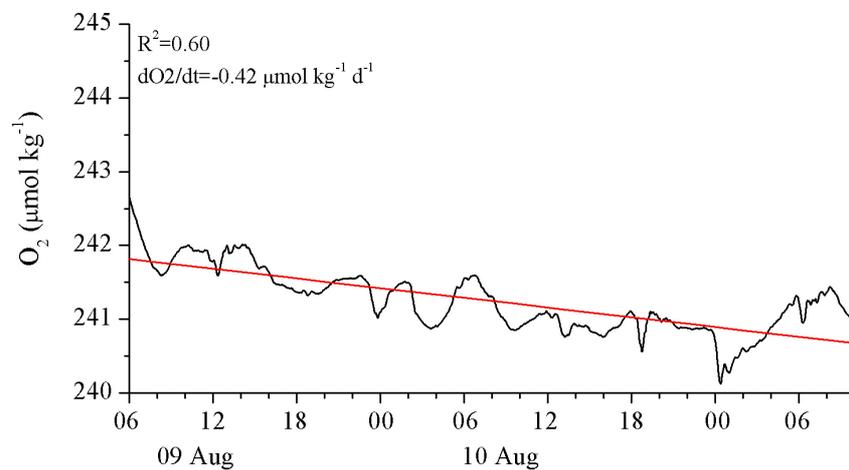
Meire, L., Soetaert, K. E. R., and Meysman, F. J. R.: Impact of global change on coastal oxygen dynamics and risk of hypoxia, *Biogeosciences*, 10, 2633–2653, doi:10.5194/bg-10-2633-2013, 2013.

Neubacher, E. C., Parker, R. E., and Trimmer, M.: Short-term hypoxia alters the balance of the nitrogen cycle in coastal sediments, *Limnol. Oceanogr.*, 56, 651–665, doi:10.4319/lo.2011.56.2.0651, 2011.

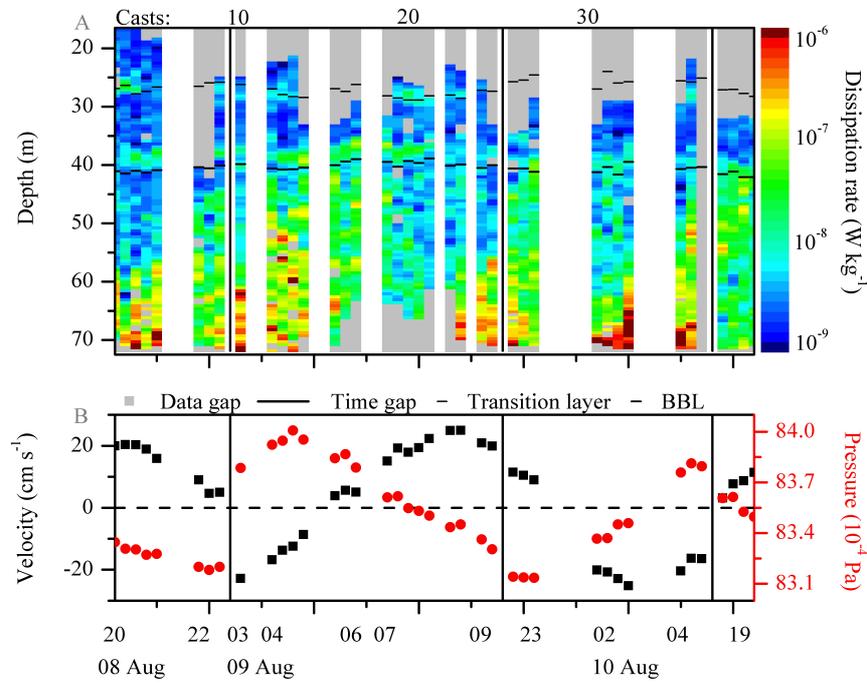
Supplementary Information



SI Figure 1. Frequency spectra of vertical shear of horizontal velocity calculated from the ADCP data for the maximum shear layers. Note that the spectra clearly indicate near-inertial oscillation (f) to be the main contributor to the detected enhanced shear.

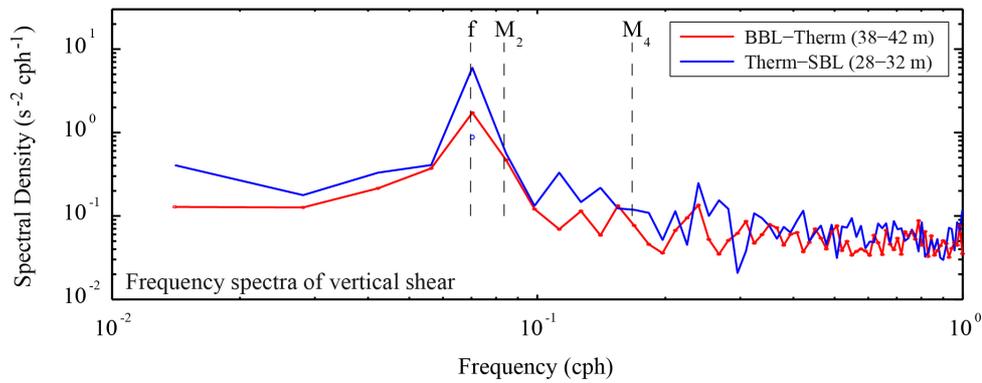


SI Figure 2. Bottom water apparent O_2 loss as estimated from near-seafloor O_2 timeseries from the POZ-Lander.

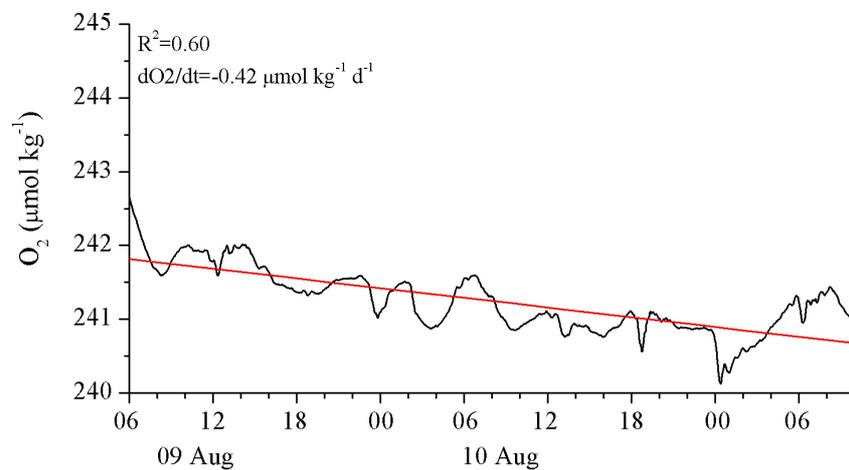


SI Figure 3. Tidal referenced turbulence contour plots. (A) Turbulence contour plot of all MSS90 casts together with the temperature layers. Thin and thick dashed lines represent the transition layer – interior interface and the interior – BBL interface, respectively. Gray spots indicate data missing due to uncompleted profiles (casts 16-23), unsuccessful profiles (cast 36), or flagged as bad based on spikes, collisions and suspected contamination due to ship activity. The vertical black lines indicate the transition (time gaps) between consecutive profile ensembles. (B) Background information on bottom current, and hydrostatic pressure during the casts. Both velocity and pressure data were collected by the deployed POZ lander. Note that as a result of the time gaps between the consecutive MSS90 casts (see Fig. 3A) the time scale is not linear.

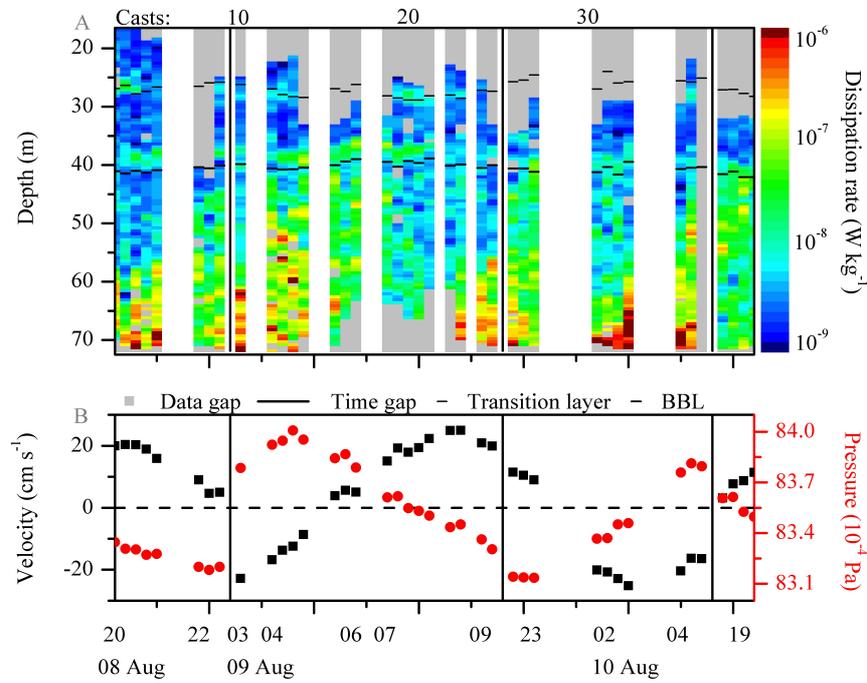
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