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Biogeosciences Discussions

Interactive comment on "Diapycnal oxygen supply to the tropical North Atlantic oxygen minimum zone" *by* T. Fischer et al.

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Reply to Reviewer #1

Thank you very much for your helpful comments. Our replies follow after each of the comments.

In the paper, Fischer et al use 3 different methods to compute the vertical eddy diffusivity in the North Atlantic Oxygen Minimum Zone and resultant fluxes of oxygen. Two of these methods are completely independent. The contribution of the diapycnal fluxes to the total oxygen budget are assessed and found to be up to a third of the isopycnal fluxes. The attention to detail in computing the vertical eddy diffusivity is commendable and the application of these methods to the OMZ is an interesting science question.

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Thus I recommend this paper for acceptance after revisions. My detailed comments are below.

1) Introduction: It isn't clear to me if there is any other similar work involving concurrent measurements of open ocean oxygen and turbulence profiles . If there isn't, the authors should emphasize the uniqueness of their measurements. If there is, then that should be cited. I also think it would be worthwhile to cite other examples of combined turbulence and dissolved substance measurements in the open ocean.

Answer: To our knowledge there is no similar reported work regarding the oxygen budget; Rovelli et al., unpublished, used microstructure turbulence with combined fast oxygen sensor profiles in the North Sea in 2010 to calculate oxygen fluxes. In that sense, in this study for the first time oxygen profiles and turbulence profiles have been used to infer a large-scale flux profile for the open ocean. There are studies combining microstructure measurements and measurements of different tracers. Turbulence profiles and nutrient profiles have been used in various shelf regions to estimate nutrient supply to surface waters [Sharples et al. 2001, 2007, Rippeth et al. 2009, Hales et al. 2005, 2009, Schafstall et al. 2010]. Kock et al. [2012] used microstructure turbulence and nitrous oxide gradients on the shelf and open ocean of the tropical Atlantic to estimate gas flux to surface waters. Oxygen fluxes to the sediment have been estimated at benthic boundary layers by eddy correlation techniques [Berg et al. 2003, Kuwae et al. 2006, Brand et al. 2008]. We add this information to the introduction.

2) P14301-L3: What was eps/nuN2?

Answer: For convenience, the distribution of the turbulence activity parameter eps/nuN2 is shown in the figure attached to this reply. The average eps/nuN2 of all data available for this study is 41, the median is 30. However, 30 out of 400 datapoints show eps/nuN2 > 100 and indicate energetic turbulence according to Shih et al. [2005]. Further following Shih et al. [2005], Gamma values < 0.2 should be chosen for this subset, with Gamma being determined as a function of the turbulence activity parameter itself. When doing so, the average K_rho changes only marginally, K_rho is reduced by 2.5%.

To provide this information to the reader, we add two sentences to P14301 L4: "The average value of the turbulence activity parameter (eps/nuN2) calculated from all data available for this study amounts to 41, less than 8% of the eps/nuN2 values exceed 100. When taking into account the eddy diffusivity parameterization suggested by Shih et al. [2005] for situations of eps/nuN2 > 100, the average K_rho reduces by only 2.5%."

3) P14301-L7: Later on you present profiles of K_MSS, but you don't actually use those profiles in any way? Is that correct?

Answer: We used the average K_MSS profile only to motivate our assumption that K does not significantly change with depth from 150m to 500m.

4) You just use a mean epsilon and a mean N2 to compute K, correct? Do the results change if you take the mean of the K profile?

Answer: We chose the complete depth range 150m to 500m to average epsilon and N2 in order to apply comparable procedures to calculate K_rho from MSS and ADCP, and because that depth range seems vertically homogeneous in epsilon and N2 (Figure 4). To answer your question concerning the effect of depth binning, we used the available 45 microstructure stations and subdivided them to 20-meter-bins, then calculated single K_rho for the bins first, then averaged the single K_rho values. We find the mean of the K profile on average 10% larger than the K from the mean profile. However, there is a bin size dependent bias on K that arises just from the measurement uncertainty of N2 and the fact that computation of K involves a division by N2. This bias we find to be of the same sign and similar magnitude as the reported K enhancement: e.g. in our setting a normally distributed error of water density of rms 0.001 kgm-3 generates 5% increase of mean of K profile vs. K from mean profile, an rms of 0.002 kgm-3 generates 20% increase. Both rms are in the typical range of density measurements precision.

To avoid such bias from N2 uncertainty, it is useful to use larger bins. Ferrari and Polzin [2005] and St.Laurent and Schmitt [1999] used 100m-bins for their K estimates in the Eastern North Atlantic thermocline. If we follow this approach, we find the expected bias in K due to uncertainty in N2 is in the range of 1 to 3%, and the mean of the K profile is on average 4% larger than the K from the mean profile.

Of these results we add to P14301 L9: "Using narrower depth bins to calculate K_rho,MSS only marginally changes the result. At the same time growing measurement errors in N2, which go in hand with narrower depth bins chosen, introduce a bias to the K estimate from the Osborn parameterization."

5) I also think you need to be clearer about the uncertainty estimate. So you have three profiles of epsilon (I didn't see how large the bins were, so I'm not sure how many points you had). So essentially all the epsilon values are averaged together to make one epsilon value, is that right? How were they averaged? A regular mean? A logarithmic mean? A bootstrap mean? The uncertainty in K is partially from sensor uncertainties and spectral estimation, but is also a function of the statistics of the turbulence. Is it essentially assumed that the vertical profiles through the measurement region are a time series, thus reducing a lot of the spread in the dissipation distribution? The averaging procedure should be clearer, which will help with the discussion of the uncertainty.

Answer: We clarify as follows: The microstructure probe delivers one estimate of epsilon per meter depth, which leaves us with roughly 1000 epsilon values in the 150m to 500m depth range per station of 3 profiles. These epsilon values are arithmetically averaged, following Davis [1996]. To estimate uncertainty of the epsilon average we follow an approach used by Ferrari and Polzin [2005] in a similar situation where they calculated vertically averaged K_rho values from microstructure measurements in the Eastern North Atlantic. We find a vertical decorrelation scale in our epsilon profiles of 5m (comparable to the findings of Ferrari and Polzin [2005] in water depths < 800m), which leaves us with roughly 200 degrees of freedom to reduce the spread of the epsilon distribution. Nonetheless the epsilon uncertainty remains the main contributor to the total uncertainty of the K estimate, together with sensor uncertainties that have to be accounted as systematic errors for the duration of an MSS station. Concerning average K, there is a minor contribution from uncertainty in N2, and there certainly is a contribution from the choice of Gamma, but of unknown amount. In total, the quantifiable errors add up to a 60% uncertainty in K_rho,MSS on a 95% confidence level.

6) P14303-L11: What does resp. stand for?

Answer: To avoid misleading, we rephrase the sentence to "...statistical independence of the oxygen gradient and Krho_bar from MSS/ADCP ..."

7) P14303-L14: You need to replace “grad” with the proper symbol throughout the text.

Answer: Will be done.

8) P14304-L1: I’m not sure that I agree with bootstrapping at this point in the analysis. Per my comment above, the confidence intervals should probably be computed on each value of K. Then some kind of appropriate averaging should be done to get the average for the entire box. The reason I say this is because the bootstrap is good for the statistical distribution of the turbulence. Here, you are averaging over a broad spatial area and you need a representative K_MSS/ADCP. There is probably some physical reason for the variation in K, not just statistics.

Answer: Thank you for this comment. We rethought our approach and now agree that bootstrapping is not adequate here, regarding the partly dependent and inhomogeneous data we find in the study area. The average <K> for the entire box was calculated according to the procedure described in section 3.1; which is arithmetic averaging after objective mapping (here smoothing with Gaussian weights). This procedure partly amends the effects of irregular sampling inside the box, and we assume here, that the smoothed data represent the distribution of K in the analysis box. We change the way we estimate the confidence interval of <K>: of the originally 400 K values contribut-

BGD
9, C8987–C8998, 2013
Interactive Comment

ing, the smoothed K field retains 86 degrees of freedom. After grouping dependent K values according to their position, 86 independent K values with their individual error distributions serve to calculate the distribution of <K>. We find the 95% confidence interval of <K> as [0.8 1.1]*10ˆ-5, which is slightly wider than before. But still this interval only represents the errors we were able to quantify, i.e. a lower limit for the uncertainty, while the true uncertainty will certainly be larger.

9) P14304-L3: Do you really know your diffusivity to 3 significant digits? To me, both your estimates of K are 1x10ËĘ-5 m2/s. I can appreciate all the effort that went into explaining why they are different, but unless you can be convincing that you know your diffusivity to 3 significant figures (or even 2), I'm not sure the entire set of arguments is necessary.

Answer: Indeed 3 digits are not necessary, particularly after the changes to the <K>MSS,ADCP estimate which is now (0.95+-0.15)*10ˆ-5. We still keep 2 digits to do the comparison of <K>MSS;ADCP to <K>TRE, which was (1.2+-0.2)*10ˆ-5. Both uncertainty estimates only represent a lower limit to the true uncertainty. The true uncertainty would include at least the set of possible biases we give in the text of section 4.1, but we cannot quantify their contribution. Thus we conclude that the two <K> estimates most probably are not significantly different, even if the given minimum confidence limits could suggest a statistically significant difference. The small difference might diminish further by the estimated small contribution of salt fingering to the diapycnal mixing, which would enhance the value of <K>MSS,ADCP. In the abstract, we just state <K> = 1x10ˆ-5.

10) P14306-L1: I think you need to be a little clearer in explaining the implication of the region of zero flux. One thing I was not clear about is if the oxygen profile is always constant. From my understanding, the oxygen profile is the average from all the profiles taken over all the field surveys, is that correct? If it varies very little over time, I think you need to emphasize that, because that is the only reason that your argument that the regions of zero flux mean you can assess the OMZ halves separately and also

ignore the surface sources of oxygen. Also related to this: what is the scale of your turbulent overturns (Thorpe scales or Ozmidov scales, for example) compared to the scale of these zones of zero flux? Do they ever get large enough that they can create countergradient fluxes?

Answer: Ozmidov scales are O(10cm) throughout the study region and depth range, and much smaller than the extent of the zones in which the oxygen gradients are not significantly different from zero (i.e the zones of zero flux). The zones of zero flux have been localized by reading the average diapycnal oxygen flux profile (Fig.6), which had been calculated from the individual flux profiles at 400 stations. In that sense, a surface of zero flux indeed represents an isopycnal surface in the analysis box, which exhibited no average net oxygen flux during 2008 to 2010. The coincidence of locations of zero oxygen flux in the average flux profile (Fig.6) and locations of oxygen extrema in the average oxygen concentration profile (Fig.4) is rather a result than an assumption. Despite their variance, individual oxygen profiles all exhibit the deep oxygen minimum and most of them also the shallow minimum (P14295 L8-12 for more detail). So the basic shape of the oxygen profiles may be seen as constant throughout the analysis box during the analysis period. Stramma et al. [2008] observed a long-term oxygen trend in this region in the layer 300m-700m of -0.35 μmol/kg/yr, which would likely not be observable within the time span 2008-2010 with the measurement accuracy of dissolved oxygen.

To clarify the explanation of the average oxygen flux profile (Fig.6) we change the text now reading: "Main features of the diapycnal oxygen flux profile are two surfaces of zero average flux which coincide with positions of zero oxygen gradients: at the oxygen maximum at about 200m depth and at the OMZ core at about 450m depth (Fig.4). In between these two surfaces, there is maximum downward flux of oxygen located at the deep oxycline. The depth interval of our data only allows oxygen flux calculations for the depth range 150m to 500m of the water column, encompassing the OMZ core and the deep oxycline."

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9, C8987–C8998, 2013

Interactive Comment

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Interactive Discussion

Discussion Paper

11) P14307-L24: Related to the above, I think there needs to be more explanation of what the divergence of the flux means. In a basic Fickian diffusion model, “zero diapycnal contribution at the maximum oxygen gradients” seems counterintuitive, because that is when the maximum flux is. It makes sense in this context, but at first glance it is a bit confusing, as the fact that you are saying that the flux into the region of maximum gradient = flux out of it, thus there is no addition to the net oxygen there, is not clear.

Answer: Probably the confusion arises from not making clear that the flux divergence profile basically gives information on local diapycnal flux divergence = local diapycnal oxygen supply. Later in the text and in the conclusions we also look at the oxygen supply to finite water bodies that are enclosed between two isopycnal surfaces, which is equivalent to integrating the flux divergence profile along the according density range.

We rephrase P14307-L24 to make the local aspect clearer: "Maximum local diapycnal supply happens at about the OMZ core - with about 1.5 to 2 μmol/kg/yr. Other distinguished locations in the flux divergence profile are the surfaces where local diapycnal supply is zero, which coincide with the locations of maximum oxygen gradients above and below the OMZ core."

Further we change P14308-L15 to: "Diapycnal oxygen supply to the water layer surrounding the OMZ core (e.g. between isopycnals 26.9 and 27.1) is estimated to be about a third of the demand, with large uncertainty."

12) P14308-L9: Doesn’t the consumption affect the fluxes by helping to create a gradient in the oxygen? It seems to me that you can’t consider these terms independently.

Answer: Indeed, it is a complex feedback. We try to disentangle the relations in the introduction P14293-L10 to L17. We do not consider the budget terms to be physically independent, and changing one of the terms will cause the system to move towards a new steady state and during that development probably the oxygen field and all other budget terms will have changed. But still it is possible to observe the budget terms independently from each other, because the terms are mathematically well defined and can - at least in principle - be calculated from the observed oxygen field and other observable quantities. However this does not imply that there cannot be found an imbalance in the observed budget terms. On the contrary, we might even expect a small imbalance between consumption and supply, as Stramma et al. [2008] observed a long-term oxygen trend of -0.35 μmol/kg/yr. But this expected imbalance is much smaller than the error margins of the budget terms in Fig.7, and thus could not be detected by observing the recent oxygen budget.

13) P14314-L1: How applicable is this expression for epsilon in other locations?

Answer: At this stage of our research in this parameterization, we can only assume that it is valid inside the covered parameter ranges and with similar physical constraints given, i.e. open ocean, internal wave breaking as main turbulence generating mechanism, no boundaries, fronts, strong shear zones near, 5 degrees latitude to midlatitudes. A separate paper is in preparation that describes and further explores this parameterization.

14) Fig. 1: Why is there a difference between the data from 2009 and this concurrent data? And why bother using the other data instead of your own to define the extent of the OMZ?

Answer: The reasons for the difference are not completely clear. We can speculate that presumably two main factors are: a) WOA is averaged over decades, while we recently sampled a region that experienced oxygen decline during the last 5 decades; b) The shown values are derived from the smoothed WOA gridded values by splining, which cannot reproduce the small scale oxygen minima. The great use of the WOA curve is that we have an educated guess what the shape of the OMZ is like, while our oxygen measurements do not fill the region. There are blank patches in our sampling, large enough that we couldn't interpolate our measurements. In the end a shape of the OMZ interpolated only from the measurements could look quite unrealistic and the extent would probably be underestimated. The WOA77 line is our best fit to the

60μmol/kg measurements, assuming that WOA isolines can describe the mean shape of the OMZ.

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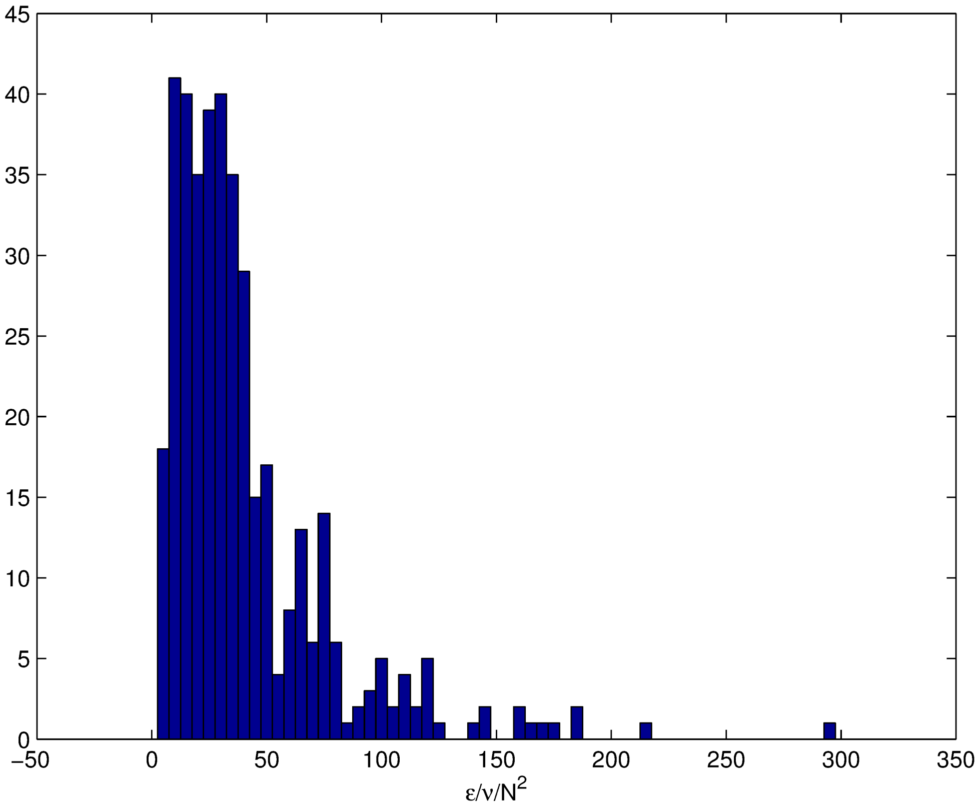

Fig. 1. Distribution of turbulence parameter eps/nuN2 in the tropical North Atlantic OMZ. Figure refers to comment 2).

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9, C8987–C8998, 2013