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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.

Anonymous Referee #1

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

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 au-

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thors 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.

P14301-L3: What was eps/nuN2?

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

P14303-L11: What does resp. stand for?

P14303-L14: You need to replace "grad" with the proper symbol throughout the text.

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.

P14304-L3: Do you really know your diffusivity to 3 significant digits? To me, both your

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estimates of K are 1x10ˆ-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.

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?

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.

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.

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

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?

Interactive comment on Biogeosciences Discuss., 9, 14291, 2012.