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Interactive comment on "Diapycnal mixing across the photic zone of the NE-Atlantic" *by* Hans van Haren et al.

Anonymous Referee #1

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Review for manuscript # os-2020-73 "Diapycnal mixing across the photic zone of the NE-Atlantic" by van Haren et al.

Formal review:

The authors discuss dissipation rates of turbulent kinetic energy, eddy diffusivities and vertical turbulent nutrient fluxes inferred from upper-ocean hydrographic and nutrient data taken during a cruise on a transect from 60°N to 30°N along about 17°W in the North Atlantic. Inferred eddy diffusivities and vertical turbulent nutrient fluxes in the upper thermocline (<500m depth) did not vary with latitude. However, from south to north stratification in the upper thermocline weakened by a factor of 5. The authors claim that the lack of correspondence between turbulent mixing and stratification (temperature) suggest that nutrient availability for phytoplankton in the euphotic surface waters may

not be affected by global warming.

While this paper is fairly well written and addresses scientifically relevant question such as an advancing quantitative understanding of the role of mixing in sustaining biological production in the near surface layers of the ocean, the current version of the manuscript has major deficiencies. In particular, I find that the results presented in the manuscript are not sufficient to support the authors' interpretations and conclusions. Furthermore, a statistical analysis of uncertainties inherent to the results needs to be added.

Major concerns

Personally, I per se agree with the statement that climate warming and associated increase of upper-ocean stratification will not necessarily lead to a decrease of turbulent mixing in the thermocline or a decrease of vertical turbulent nutrient fluxes. Certainly, there are also arguments that support an enhanced energy flux into internal waves due to increasing stratification (which has also been suggest by several previous publications, e.g. DeCarlo et al., 2015). However, to me, the data analysis presented here does not permit to draw any conclusions on this issue. This is because (1) the data is inadequately resolving average mixing quantities. Turbulent mixing in the ocean exhibits a near log-normal frequency distribution and elevated mixing events occur infrequently. However, these elevated mixing events are dominantly responsible for the vertical turbulent fluxes of solutes in the ocean. The 60+ profiles (I am guessing here as no numbers are provided in the manuscript) that may represent turbulence conditions over a period of 3 to 4 hours at the 15 to 20 individual stations are certainly inadequate to draw any conclusions on average turbulence quantities at different latitudes. The variability of turbulent mixing is also reflected by (2) the individual estimates of vertical turbulent nutrient fluxes available from the limited individual stations along the transect. Fluxes vary by three orders of magnitude (Figures 7, 8, 9). Again, an analysis of their statistical uncertainty would show the ambiguity of any trend analysis. Finally, (3) I cannot approve the approach chosen here as a whole. Comparing the strength of upper thermocline mixing at different latitudes cannot lead to any conclusions on local changes of the strength of turbulent mixing e.g. due to locally increasing stratification. The regions where measurements were taken combine very different external forcing and internal wave environments making it impossible to relate mixing strength to a single parameter.

As a revision strategy, I would advise the authors to remove the discussion on mixing and nutrient fluxes in a changing climate from the manuscript. Instead, the focus could be shifted to a detailed discussion of an upper-ocean nutrient budget including statistical uncertainties and a comparison to the net community production.

Some specific comments

Line 294 – 299, discussion of nutrient fluxes in the mixed layer and Fig. 7. I find the discussion of macro-nutrient fluxes in the mixed layer erroneous. First of all, vertical gradients of macro-nutrients are mostly insignificant. Macro-nutrient concentrations determined by a QuAAtro autoanalyser usually have accuracies of 0.1mM if CRM standards were used (please add details of uncertainties inherent to the nutrient concentrations to the methods section). To me, the differences between macro-nutrient concentrations measured at 10m and 25m depth are mostly smaller than measurement uncertainties.

Line 300 – 303, discussion of nutrient fluxes below the mixed layer. As stated in the above, individual estimates of nutrient fluxes vary by three orders of magnitude and a statement about how the nutrient fluxes vary with latitude (i.e. with stratification) is inadequate. What may be interesting to the reader is the magnitude of average regional fluxes that could be compared to previous estimates (see e.g. Cyr et al., 2015). Presented results should also include nitrate/nitrite fluxes as the relative vertical turbulent fluxes of reactive nitrogen species and phosphorous could be of interest to a broader scientific community.

Literature

Cyr, F., D. Bourgault, P. S. Galbraith, and M. Gosselin (2015), Turbulent nitrate fluxes in

the Lower St. Lawrence Estuary, Canada, J. Geophys. Res. Oceans, 120, 2308–2330, doi:10.1002/2014JC010272.

DeCarlo, T. M., K. B. Karnauskas, K. A. Davis,and G.T.F. Wong (2015), Climate modulates internal wave activity in the Northern South China Sea, Geophys. Res.Lett., 42, 831–838, doi:10.1002/2014GL062522.

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