>>>We are grateful for the comments on our manuscript from the reviewer. We feel that this new version of the paper is much stronger as the result of the comments we received on the original manuscript. We have addressed all of the comments and have detailed our response to specific comments below. Our response to each comment is bulleted and in italics below the relevant comment behind>>>

Anonymous Referee #1

Received and published: 4 September 2020

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.

>>>We thank the reviewer for the appreciation. We have now attempted to substantiate support for our interpretations. Uncertainties to the results are further explained.

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.

>>>In reply to point (1) We are aware of the near-lognormal PDF of turbulence dissipation data, actually it is one of the reasons to plot our data in log-fashion. We do not agree that the number of profiles cannot say anything about average quantities, as the spread is clearly given. Of course one can

compute average values from that, also considering that every 24 Hz sampled profile is binned in 7 m vertical bins (200 data points), that are again grouped in several layers (down to 500 m, or 70 bins). We wonder if the reviewer hereby discards all observational oceanographic turbulence work? Much effort goes into such observational work. Point (2) Yes, that is precisely what we indicated in the original manuscript: two (to four) orders of magnitude variability. The statistics is thereby given: the spread around the mean, considering the instrumental and methodological error of about half an order of magnitude. Point (3) We do not agree with this statement, because all sampling is done in the upper 500 m where the local water depth was at least 1100 m, and, except for 3 stations, most stations were over (much) deeper waters >2000 m. So, sampling was well away from bottom topography, in the NE-Atlantic where semidiurnal tides, and inertial motions, dominate the internal wave field, in summertime under overall moderate-good weather conditions across the entire survey. As a result, the dominant convection (in the upper 20-30 m) and internal wave induced mixing (in the stratified layers below) are much less variable across the transect due to different forcing than due to the highly intermittent occurrence of turbulent bursts as the reviewer correctly indicates above. Those bursts are inherent to turbulence, and less so dependent on the generation process. We added text to better explain this, lines 419-421:' If shear-induced turbulence in the upper ocean is dominant it may thus be latitudinally independent (Jurado et al., 2012; deeper observations present study). There are no indications that the overall open ocean internal wave field and (sub)mesoscale activities are energetically much different across the mid-latitudes.'

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.
>>> The outcome of our paper is the suggestion that climate change might not affect fluxes as strongly as current paradigm suggests. The intention is to inspire discussion/further research. The nutrient budget and comparison to the net community production have been described by Mojica et al (2016), which we will not repeat in our paper which is more oriented to physics processes than biology. We explained this better now. Our manuscript is an extension of that work.

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.
>>>The reviewer is right, we should have given the precision and detection limits. Without that info, the interpretation is not well substantiated. However, the accuracy is much better than assumed by the reviewer, as it is e.g. 0.028 µM for phosphate. We added the information now in the Methods section of the revised manuscript.

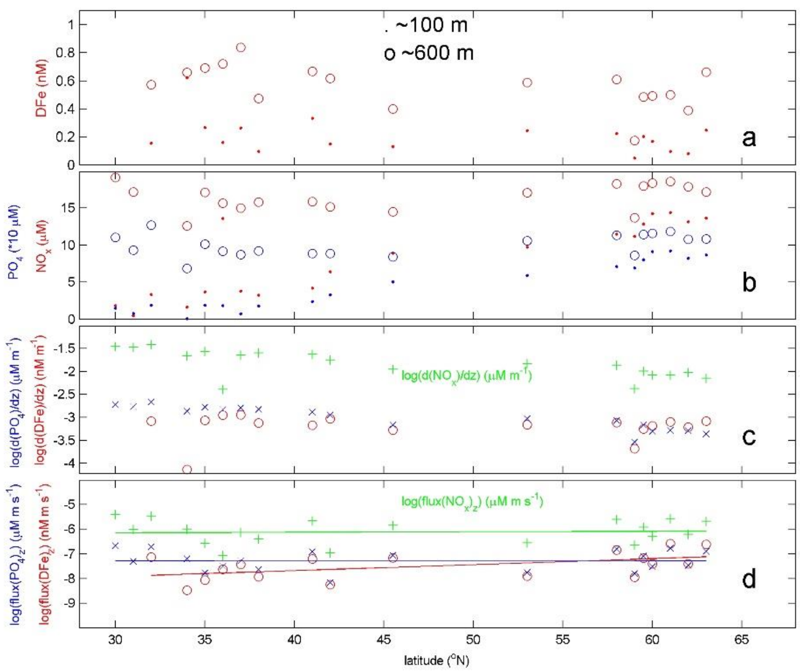
Absolute and relative precision for reasonably high concentrations in an in-house standard that is often measured.

	S.D. (µM)	*N*	*concentration*	*rel SD*
PO4	*0.028*	*30*	*0.9*	*3.1%*
NO3	*0.143*	*30*	*14.0*	*1.0 %*
Si	*0.088*	*15*	*20.99*	*0.42%*

The method detection limit was calculated during the cruise using the standard deviation of ten samples containing 2% of the highest standard used for the calibration curve and multiplied with the student's value for n=10, thus being 2.82. (M.D.L = Std Dev of 10 samples x 2.82)

	µM
PO4	*0.007*
NH4	*0.010*
NO3+NO2	*0.012*
NO2	*0.003*
Si	*0.008*

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.
>>>We are happy to compare with works from others, noting that Cyr et al. presented work at 2 stations in an estuary, which may be difficult to compare with the open ocean. We choose to graphical display macronutrient phosphorous representing other nutrients that show similar latitudinal trends. Attached is a version of Fig. 9 demonstrating the little extra information if we conclude NOx. We have to rescale panel c. We have now given global figures for nitrate fluxes. As mentioned, in this paper we are mainly interested in latitudinal and stratification trends and trends for phosphate fluxes precisely represent those for nitrate fluxes (blue and green lines in panel d in the figure below).



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