>>>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>>>

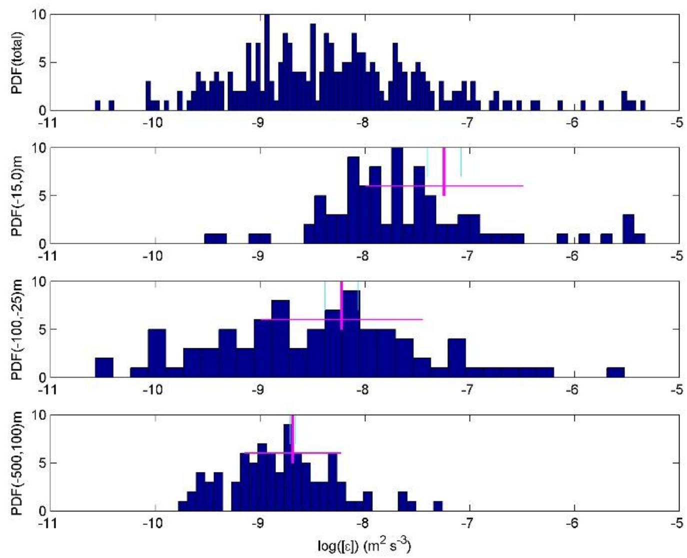
Anonymous Referee #3
Received and published: 20 September 2020

This is an observational study of diapycnal mixing and the corresponding nutrient flux in the upper ocean across a quasi-latitudinal transect in the Northeast Atlantic Ocean. The data cover a rather long distance from 30deg.N to 62deg.N. The measurements were mainly temperature and conductivity profiles (from which the density or potential density profiles were obtained) with a carefully modified CTD system, and the turbulent kinetic energy dissipation rate and the diapycnal diffusivity were estimated based on the overturning (Thorpe) scale analysis. In general, the methodology of the analysis is reasonable, and useful information on turbulent mixing characteristics along the transect is obtained. However, I cannot recommend this manuscript for publication in the present form due to the major concerns as detailed in the following.
>>>Thank you for the appreciation of the outline and methodology.

First of all, I find the major point that the authors try to make (i.e., "nutrient availability for phytoplankton in the euphotic surface waters may not be affected by the physical process of global warming") is not convincing at all. For me, the point is not even relevant to what the data have shown. Obviously, the exact response of the upper ocean to global warming could be rather complicated, and I do agree with the authors that the global warming may not necessarily lead to a change in vertical turbulent exchange, but the results presented in the manuscript are by no means evidence for this. One may expect to see clear trend of upper ocean mixing (and corresponding material fluxes) at a certain location under continuing warming, but in such a large region covering more than 30 degrees, the underlying dynamics controlling diapycnal mixing could be very different from place to place, thus the spatial difference in mixing seen along the transect cannot be simply taken as a result of the difference in stratification (or "warming" by solar radiation).
>>>There is general consensus that upper ocean convection and interior ocean internal wave breaking are the dominant turbulence generating mechanisms in the upper 50 – 100 m and in the deeper 100(50)-500 m stratified waters, respectively. These mechanisms are universal and not particularly location dependent, but do depend on variations in stratification. Turbulence is such an intermittent process that variations over four orders magnitude occur, at the same location (e.g. Gregg, JGR1989). Such variability is found in the present observations too, e.g. as indicated in old l.242 and 243. This is much larger variability than particular variation in turbulence generation processes in the ocean interior, away from boundaries. In other words, the sources may not greatly vary their energy content along a transect compared with turbulence intermittency. The trend in spatial difference in mixing along a transect can be taken as a result of difference in stratification. Along these lines we have added text (l 233-235) 'As will be demonstrated below, this is considerably less spread in values than the natural turbulence values variability over typically four orders of magnitude at a given position and depth in the ocean (e.g., Gregg, 1989).'

More technically, although I do appreciate the authors' efforts in estimating turbulence and mixing characteristics from carefully conducted CTD measurements (via overturning scale analysis), I cannot be convinced by the subtle mixing (and flux) variability revealed by their estimates. As well acknowledged by the authors, even with the microstructure measurements one cannot expect to get an estimate with insignificant uncertainty. I agree that the overturning (Thorpe) scale analysis could be very useful in getting a rough estimate of mixing intensity when more direct measurements are not available, but using it to reveal subtle spatial (or temporal) variability could be misleading. For this purpose, direct

microstructure measurements are certainly much more reliable. On the other hand, ocean turbulence is certainly a stochastic process with both significant dynamical variability (which could be taken as deterministic linked to certain dynamical processes generating turbulence) and intermittency. As such, for the purpose of evaluating spatial variability of turbulent mixing, one should look at turbulence statistics. How many data points are used to get the reported averages? How does the PDF in each corresponding depth range look like? What are the confidence intervals of the reported averages? Are the noted differences/variabilities really significant?
>>>We thank the reviewer for the appreciation of our efforts in estimating turbulence from carefully conducting CTD measurements. We like to point out that our near-surface data do show the same trends as upper 100 m microstructure profiler observations obtained a few years earlier along the same transect (Jurado et al 2012). In general, microstructure measurements are indeed more reliable for turbulence measurements, but in this case do not provide significantly different results. We note that turbulence is not by any means a deterministic process, even though its dominant generator, e.g. tide, may be deterministic. We have now additional CTD sampling numbers. The CTD sampled at 24 Hz, so 7 m ensemble averaging vertical intervals contain about 200 data points and we collected 3 to 6 CTD-casts per station. Below we add PDF of the entire dissipation rate data, averaged per vertical interval as indicated. As for the errors: they are about a factor of 3 for mean dissipation rate.



(New)Fig. A2. Probability Density Functions of logarithm of vertically averaged dissipation rate in comparison with latitudinal trend extreme values. (a) Distribution as a function of latitude for all data. (b) As a, but for the upper 15 m averages only. The mean value is given by the vertical purple line, with the horizontal line indicating +/- 1 standard deviation. The vertical light-blue lines indicate the best-fit value of the trend for 30° and 63°N. (c) As b, but for averages between -100 < z < -25 m. (d) As c, but for averages between -500 < z < -100 m.

To conclude, I agree that the reported analysis gives useful information about mixing characteristics along the sampled transect, but without clear information of the underlying mechanisms and robust constraint on the reliability and significance of the reported mixing variability, one cannot be led to the points that the authors try to make. In particular, the results presented in the manuscript do not seem to lend any support to the authors' argument on the global warming impact on upper ocean mixing and nutrient flux trend. The authors may choose to simply emphasize their mixing estimates from the overturning scale analysis, with clear indication of the underlying uncertainties.

>>>We adapted part of the discussion to better relate to comments by the reviewers, including toning down the relation with climate change, and we hope that as such we now made our point more convincing..