Dear editor,

We thank the reviewers for their constructive help in improving our manuscript and reviewers #2 and #3 for accepting our revised manuscript as is for publication.
Below you will find our latest replies to further queries by reviewer #1, in blue and italic.

Best regards,
Hans van Haren
(also on behalf of the coauthors)

Review (#1) for the reviesed manuscript # os-2020-73" Diapycnal mixing across the photic zone of the NE-Atlantic" by van Haren et al.

As indicated in the previous review of this manuscript, this paper discusses 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. While the new version of the manuscript is somewhat improved, in particular by adding statistical and measurement uncertainty, I still find that results presented in the manuscript are not sufficient to support the authors' interpretations and conclusions.

Remaining major concerns.
My major remaining concern continues to be the authors' claim "the lack of correspondence between turbulent mixing and stratification along the transect suggests that nutrient availability for phytoplankton in the euphotic surface waters may not be affected by global warming", which is not supported by their results. As stated in my first review, comparing the strength of thermocline mixing in different oceanic regions (at different latitudes, here) cannot lead to any conclusions on local changes of the strength of turbulent mixing due to locally increasing stratification. As I am detailing in the response below, the added wording does not help to justify their claim.
Furthermore, my second concern related to the presented vertical nutrient fluxes due to turbulent mixing has not fully been addressed. Although adding accuracy and precision of the nutrient measurements has improved the manuscript, it remains unclear if the presented nutrient fluxes are relevant for near-surface primary production and thus for the biological carbon pump as a whole. In my earlier review, I suggested to the authors to compare the results to previous studies and to provide a comparison to nutrient uptake in the upper ocean. Unfortunately, it seems that my suggestion was misinterpreted by the authors.

We disagree with lack of proof for our general conclusion, which is now further weakened and elaborated in its wording. As will follow in responses in more detail below, the objections given by the reviewer are either not relevant or inaccurate. We acknowledge that we should have been more clear on these two points (internal wave characteristics and nutrient fluxes), as we do now, below and in the manuscript. In response to the reviewer's last sentence above: No, it was wrongly indicated, as the reviewer admits below.

I provide further details to these concerns in my reply to the responses below. For clarity, I am presenting my former remarks, then the reply by the reviewers followed by my reply.

Reviewer's previous major remarks: (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.

Reply by the authors: 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.

Reply by the reviewer: I do not want to get into a (perhaps endless) discussion on internal wave forcing and energy fluxes. However, even if the energy residing in the internal wave field were comparable at the different latitudes (which the authors do not demonstrate in this study nor was this shown in the study by Jurado et al. 2012) it can not be concluded that the energy fluxes into internal waves and from internal waves into turbulence would be the same. In fact, this would be rather unlikely, because internal wave-wave interaction processes are dependent on latitude (see e.g. Henyey et al. 1986 or Gregg et al. 2003). Wave-wave interaction at higher latitudes is more efficient in fluxing energy to turbulence than at lower latitudes and we would thus expect an elevated flux of internal wave energy to turbulence at 60°N compared to 30°N. Furthermore, parametric subharmonic instability of the M2 tide leads to elevated energy flux into turbulence and enhanced mixing in the region between about 20°-30° away from the equator (see e.g. Hibiya et al. 2007). This process may be responsible for the somewhat elevated eddy diffusivities in the southern part (30°N-32°N) of the transect. I am mentioning these two processes because the added text, in particular "no indications that the open-ocean internal waves field … are energetically not much different across the mid-latitudes" does not help to understand the claim that eddy diffusivities across the transect are of comparable magnitude.
Apart from issues with the added sentence, I strongly disagree with the statement in the authors response above "… internal wave induced mixing (in the stratified layers below) are [is] much less variable across the transect due to different forcing than due to the highly intermittent occurrence of turbulent bursts …" In the thermocline, internal wave energy is dissipated by turbulence. Certainly, the bursts of turbulence are highly variable, but if resolved adequately in time, they merely reflect the dissipation of internal wave energy. There are numerous factors that impact local internal wave energetics and their energy flux to turbulence (see e.g. McKinnon et al. 2017 for a recent review). Additionally, wind stress curl may efficiently excite near-inertial waves if rotating anticyclonically at frequency close to the Coriolis frequency even at low wind speeds.
I retain my position that the authors should remove the discussion on mixing and nutrient fluxes in a changing climate from the manuscript. The data analysis presented in the study does not allow to draw conclusions on this matter.

There seems to be a misunderstanding by the reviewer. Previous observations (van Haren, 2005b, and Hibiya et al., 2007) have shown that a diurnal critical latitude enhancement of near-inertial internal waves only occurs sharply equatorward of |30°| (not 32°) latitude. The present observations are all made poleward of this latitude. Likewise, earlier observations (van Haren 2005a) have demonstrated that the Henyey/Gregg model does not hold close to the equator, where the internal wave regime drops much more rapidly in a non-gravity wave system. The H/G model is not varying very much across mid-latitudes, maximum by a factor of 1.8 between 30° and 63°, compared with the observed variations and errors (factor of 3) in turbulence dissipation rate. Naturally, other processes like interaction between internal waves and mesoscale phenomena may be important, but these occur in a similar fashion across the sampled ocean far away from boundaries. As mentioned in our revised manuscript, all observations were made (i) under similar summertime weather conditions, providing comparablenear-inertial internal wave generation, and (ii) far away from major topographic features, i.e. r from internal tide sources, (which, if important, would have definitely given different results at our station at the latitude of Porcupine Bank, for example). The above is claried even better now in the manuscript.

Reviewer's previous comment: As a revision strategy, … 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.

Reply by the authors: 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.

Reply by the reviewer: I have two issues with the revised version and the response above. First of all, my last suggestion to refer to the publication by Cyr et al. (2015) was misunderstood, because I did not want to point to the results of that particular study but rather their table 1 listing about 20 published studies on vertical nutrient fluxes by turbulent mixing on page 2326. I should have been more detailed with my comment, here. This list also includes several studies in the subtropical and subpolar North Atlantic. In particular, the results by Martin et al. (2010) are of relevance here. Their measurement program was conducted in boreal summer in an area (PAP, 49°N 16°30'W) very close to the transect which data are analyzed here. The study by Martin et al. revealed vertical nutrient fluxes due to turbulent mixing that are very similar to the numbers reported by the authors here. However, additional analysis by Martin et al. showed that these nutrient fluxes account for only about 2% of the nutrient uptake within the euphotic zone. They conclude that other processes must be responsible to supply nitrate to the euphotic zone that are much more relevant than vertical fluxes due to mixing.
In the study by Mojica et al. (2016), no numbers for the calculated vertical turbulent nutrient fluxes are given. This is different from the study here. When comparing these numbers with previous studies, it appears that they are too small to sustain significant production in the euphotic zone. Is it possible that rare intense mixing events relevant for the vertical supply of nutrients were not captured by the measurement program (see e.g. Hummels et al., 2020)? A discussion of this issue needs to be added to the manuscript.

In reply to the reviewer's comment on rare mixing events:
If important, rare mixing events would show up in the data, for example in data by Martin et al. (2010) who stayed in the same station for a prolonged period. At least they would see differences in day/night turbulence, whereby we note that convection is also a vertical turbulent exchange process. As mentioned previously, ocean mixing is characterized by high intermittency, of a puff here and a puff there, which causes single dissipation rates to vary over at least four orders of magnitude. The same spread in values characterizes 'rare events' as observed by Hummels et al. (2020) in the equatorial Atlantic, the same area studied by Alford and Gregg (2001), an area well outside our transect.

Concerning the vertical nutrient fluxes comment by the reviewer:
Our cruise transect entails a latitudinal gradient from 30 to 63 degrees N, which also includes the latitude at which the study by Martin et al. was conducted. It is promising that our vertical nutrient fluxes indeed match those obtained in the study by Martin et al. (2010), which gives confidence in the methods applied. The comment by the reviewer that Martin et al. (2010) state that such nutrient flux only accounts for 2% of the uptake within the euphotic zone can however not be generalized. The nitrate uptake assays by Martin et al. (2010) spiked 100-200 µL stock solution $K^{15}NO_3^-$ of 1 µmol/100 µL concentration to 2 L sample water in order to obtain approximately 10% of the ambient dissolved nitrate concentration. As such, a minimum spike of 100 µl per 2 L sample implies there was a minimum of 5 µmol/L nitrate at their location. Given the calculated uptake of nitrate of 0.1 µmol/L/day, this implies (i) the population could be sustained by about 50 days on the existing nutrient inventory without any additional flux, and (ii) that most likely something else must have been limitating the phytoplankton growth (as otherwise the inventory would be depleted much faster). Inventories along our transect were much lower and the two studies are thus not directly comparable. We suspect that in the case of Martin et al. (2010), either a past upwelling event or lateral advection (their study site is close to the European continental shelf) led to the enhanced inventories, and consequently higher nitrate uptake rates were possible. Thus, while the vertical turbulent fluxes are similar, it is unlikely the uptake rates should be similar.
Furthermore, particularly in the southern part of our cruise transect remineralization of N in the form of ammonia likely supplied most of the N-demand. A small flux of nitrate from below may be sufficient to balance losses of N via export to the deeper waters. For example, Gaul et al. (1999), found that nitrogen regeneration could dominate nitrogen supply and recycling efficiencies up to 100% under post-bloom situations. Our earlier work largely along the same transect (Mojica et al. 2016) demonstrated (i) that phytoplankton cell losses generally matched the production, and (ii) that viral lysis was equally responsible for those losses as grazing. Viral lysis has been shown to release labile organic matter, strongly stimulating bacterial recycling of nutrients (ammonia and phosphate), and at the same time zooplankton are known to contribute substantially to ammonium release.

In our opinion, the relatively low turbulent flux in the stratified waters of our summer cruise aids to a low F-ratio, rather than an important nitrate flux is missing. That being said, supply from above via aeolian deposition as well as nitrogen fixation by diazotrophs can play an important role, but this is beyond the scope of this paper that focusses on differences (or lack thereof) in observed turbulent fluxes along a meridional transect.

References

Alford, M. H. and Gregg, M. C.: Near-inertial mixing: Modulation of shear, strain and microstructure at low latitude, J. Geophys. Res., 106, 16,947-16,968, 2001.

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.

Gaul, M., A.N. Antia, W. Koeve (2015), Microzooplankton grazing and nitrogen supply of phytoplankton growth in the temperate and subtropical northeast Atlantic, Mar. Ecol. Progr. Ser., 189, 93-104.

Gregg, M. C., T. B. Sanford, and D. P. Winkel (2003), Reduced mixing from the breaking of internal waves in equatorial waters, Nature, 422, 513–515.

Henyey, F. S., J. Wright, and S. M. Flatte (1986), Energy and action flow through the internal wave field - an eikonal approach, J. Geophys. Res., 91(C7), 8487-8495.

Hibiya T., M. Nagasawa and Y. Niwa (2007) Latitudinal dependence of diapycnal diffusivity in the thermocline observed using a microstructure profiler, Geophys. Res. Lett, 34, L24602.

Hummels, R., M. Dengler, W. Rath, G. Foltz, F. Schütte, T. Fischer and P. Brandt (2020), Surface cooling caused by rare but intense near-inertial wave induced mixing in the tropical Atlantic, Nature Comm., 11, 3829.

Martin, A. P., M. I. Lucas, S. C. Painter, R. Pidcock, H. Prandke, H. Prandke, and M. C. Stinchcombe (2010), The supply of nutrients due to vertical turbulent mixing: A study at the Porcupine Abyssal Plain study site in the northeast Atlantic, Deep Sea Res., Part II, 57(15), 1293–1302.

MacKinnon, J. A., et al. (2017) Climate Process Team on Internal Wave–Driven Ocean Mixing. Bulletin of the American Meteorological Society 98, 2429-2454.

Mojica, K. D. A., Huisman, J., Wilhelm, S. W. and Brussaard, C. P. D.: Latitudinal variation in virus-induced mortality of phytoplankton across the North Atlantic Ocean, ISME J., 10, 500-513, 2016.

van Haren, H. 2005a. Sharp near-equatorial transitions in inertial motions and deep-ocean step-formation. Geophys. Res. Lett., 32, L01605, doi:10.1029/2004GL021630.

van Haren, H., 2005b. Tidal and near-inertial peak variations around the diurnal critical latitude. Geophys. Res. Lett., 32, L23611, doi:10.1029/2005GL024160.