Dear Editor, Dear Ilker,

The best wishes for 2021 to you too!
We much appreciate your critical comments. You will find ou replies below, and a ‘track changes’ version with the items and text inserted.

Also on behalf of my co-authors,

Best regards,

Hans

Topic Editor Decision: Publish subject to minor revisions (review by editor) (01 Jan 2021) by Ilker Fer
Comments to the Author:

Dear Hans,

Happy 2021! Thanks for the revised submission of your manuscript.

I think you have addressed the comments of the reviewers satisfactorily and I do not see the need to send the manuscript out for further comments. I doubt that you and reviewer 3 will converge; however, together with the open discussion and concerns raised, your findings and analysis can be documented in the literature.

I have some other concerns, which must be addressed before the manuscript could be published. I list them below together with some minor issues.

Thank you,

Ilker

1- Description of the Thorpe scale analysis (Section 2.2):

Using Eq(1) in the form of Thorpe 1977 but with the 0.64 factor from Dillon is misleading. In this equation, “d” must be replaced with the Thorpe scale, LT which is the r.m.s. of d over the overturn (or over constant vertical scale of 7 m as you calculated). Although not ideal, it is acceptable to use a fixed vertical averaging scale. Please revise throughout by introducing LT = rms(d), and in line 223, LO/LT = 0.8.
>>>Done as you suggest, although it was meant to be introduced two paragraphs later, and we did prefer to use |d|_rms over L_T.

Also because potential density is introduced for analysis (line 211) (and also C_T and S_A earlier), the exact definition of N using in situ density is confusing. Please start the description using sigma_theta from line 204 (and N approximated as sqrt(g/rho0 dsigma_theta/dz)). Also, because rho changes in space, use partial derivative with z.
>>>Yes, that is true, it came from an attempt to keep the paper readable also for non-physical oceanography colleagues. We have restructured largely following your suggestions. So, we now use potential density from (old) l.204 and give the practical definition of N. But we retain the other, exact definition, and insert some text to warn people, following e.g. Gill (1982) and King et al. JGR2012, to use that definition for deeper waters. As buoyancy frequency is not a vector but a scalar, and although we agree it may vary over (x,y), we retain the total derivative following Gill (1982).

2. Vertical scale for N (100 m) versus LT (7 m):

This leads to at least two issues. Firstly, the Thorpe scale analysis is typically based on a background stratification over the overturn scale. There is a mismatch (and inconsistency) between 7 m and 100 m. Secondly, when mixed layer depth is about 30 m (Fig 7a), calculating N over 100 m will always give you unrealistically large stratification in the mixed layer. Your analysis (of epsilon, Krho and turbulent fluxes) in the upper layer (0-15 m averages) will be biased. All green data points in Fig 5 are likely in error. Furthermore, given that MLD is 30 m or so (i.e., > 15 m), by definition N2 = 0 (or at least cannot be resolved by density profiles), and the application of the Osborn approach (for Krho) in the upper 15 m is not allowed since N2 leads to singularity. One approach is to exclude the 0-15 m results from the paper.
>>>The 7 m is chosen on the basis of the Ozmidov scale, as a compromise. The 100 m scale refers to N computed over raw potential density profiles, not the reordered profiles. N computed over the reordered profile using the 7-m scale mends this (yielding very much the same result as using 100-m scale over raw profiles), under (Thorpe method's) assumption that the difference between raw and reordered profile is entirely attributable to mechanical turbulent overturning.
The MLD is defined, following previous conventions, as the level where a nonzero threshold (DeltaT>0.5 degrC) is passed, so in practice N^2 is nonzero: If one carefully investigates the potential density profile, the 'MLD' is not (everywhere) fully mixed and using a criterion of 0.001 degrC of a reordered profile demonstrates an 'MLD' of only about 5 m. Recall that we use 7 m scales for computing N from reordered profiles.
As the paper is also aiming at a readership of marine biologists who work in the euphotic zone and as the results from 0-15 m are consistent with those deeper down, which indirectly confirms the improvement of the CTD-modification which is also supported by previous ner-surface microstructure observations by Jurado et al (2012), we prefer to leave them in. However, triggered by your comment, we now include more cautionary notes on these data.

In any case I would recommend repeating the analysis using 7 m vertical scale for both N2 and LT. I would also recommend screening the data (excluding epsilon and Krho) over segments when there are very few displacements in a 7-m window, and when N is less than a noise level (hence Krho and turbulent fluxes undefined).
>>>N (from the reordered profile) and LT were calculated over 7 m. The turbulence data are screened when they fall below threshold (cf x-axis limits and gaps in profiles in Figs 3,4 panels c,d).

3. Turbulent fluxes (and Fig 8-9): I find the presentation of figures 8-9 confusing. Some data points are excluded without mentioning. Take the vertical gradient value, first red circle data point in panel c. It has no corresponding data in panel a. The next data points near 33N in panel a do not have a corresponding gradient in panel c but have a flux value in (d). Please carefully go through what you plot and ensure the dataset is correctly presented. At latitudes 60-63N blue dots and circles are almost collocated in panel b, hence zero vertical gradient, yet there are substantial gradient values (blue crosses) in panel c. This is all very confusing.
In Fig 9, panel a, the second and third set of data points have the opposite sign gradient (dot and circles are reversed). But their turbulent fluxes are plotted positive. This is erroneous. The authors appear to have ignored the sign of turbulent fluxes (also in other occasions. e.g. DFe at 53N and PO4 at 30N, these must have a gradient sign opposite to other data points.
>>>Thank you very much for your critical eye, you struck some embarrassing mix-up of errors due to mismatching of data-files and Matlab's inadequacy of blocking imaginary data (logarithm of negative values). We have gone through it all carefully and mended the errors, whilst only plotting downgradient values.
Yes, we agree that in Fig. 8 only few gradient values represent upward fluxes and that those gradients are very weak within the standard deviations of the measurements. But, as stated above, we would like to retain these results from the euphotic zone also because they are consistent with those from deeper down. We put in extra cautionary words.

All in all, we replotted Figures 1,3,4,5,8,9 and 10

Minor points:

Li52: delete "near the surface"
>>>OK

Li 54: [average] temperature decreased
>>> ‘average’ inserted now

Li 111: end the paragraph with a concluding sentence that the sampled dataset is adequate for a discussion on the variability of turbulence, stratification and turbulent nutrient fluxes with latitude.
>>>Concluding sentence inserted

Li 175: [A]bsolute [S]alinity, S_A (that is capital first letters, and subscript A)… [potential] density anomalies…
>>>Modified as suggested

Li 231: should be Gregg et al (2018)
>>>Yes, thank you

Li 238: downgradient turbulent fluxes, with z traditionally defined upward has a minus sign, i.e. -Kz d()/dz.
>>>Added ‘downgradient’ and minus-sign

Li 246: Where does 7x10-5 come from? If it is half of peak-to-peak raw variations of 1.4x10-3, it should be 7x10-4? Note this value (7x10-5) is mentioned again in Fig 3’s caption.
>>>The 7x10^-5 follows from standard deviations determined across short sections of near-homogeneous layers in the potential density profile. This corresponds to noise-variational amplitudes of 1.4x10^-4 in raw data, see also the small-scale variations in (detrended) raw temperature data in Fig. 2b for an indication. Sorry for the small misinformation in the manuscript. The correct threshold is 7x10^-5 as indicated.

Li 304: dissipation rate[s]
>>>Yes, modified

Fig 3: In practice a proper application of Thorpe scale analysis with detection of overturns (even with very high-quality density measurements) cannot resolve epsilon values of 10-11 or less, yet panel c shows such small values. It must be an artifact of using dz=7 m and that 7 m window having a few non-zero d values, resulting in a low epsilon. A data screening excluding segments with few data points can improve this.
>>>Yes we agree, the minimum level is about 10^-11, so we blocked panels c in Figs 3and 4 at minimum level now. Data gaps also indicate values below threshold.

Fig 3. Li 748: relaced average over with r.m.s calculated over
>>>Yes, indeed

Fig 3, 4 and throughout as needed: x-label for N should be log(N) as with other parameters. Also in the caption of Fig 3 (and other captions where log is mentioned) note that it is logarithm base 10.
>>>Modified now; we follow the mathematics ISO 80000 recommendation to write log_10(x) as lg(x), now indicated in the caption of Fig. 3 and used throughout.