Review of Referee #1

Summary:

This study demonstrates the observed turbulent energy dissipation rate along a transect in the North Atlantic, which includes the microstructure observations of the deep ocean turbulence. The authors compare the microstructure measurements with the finescale parameterizations and the Thorpe sorting method, which can be estimated using the standard CTD data. Thought these comparisons, they suggest some recommendations and restrictions for applying both parameterizations in a deep ocean weakly turbulent environment. While the paper is well-motivated and contributes to the science, I believe the manuscript requires substantial revision before it can be considered for publication.

We thank the referee for taking the time to read our manuscript and providing constructive feedback. Please find the original review in black, and our response and proposed changes added in blue.

Major comments:

I found this paper is concise and readable and the methods on the whole easy to follow. The comparison between the microstructure measurement and parameterization in the deep ocean is a good contribution to the existing literature, and most of the method is sound and adequately explained. However, for readers slightly removed from this field, I think that it would be useful to provide a further discussion in the Introduction about previous comparisons of finescale parameterizations and Thorpe methods to direct microstructure measurements.

Thank you for the suggestion. We have extended the discussion of previous comparisons in lines 61-65 with *" i.e. locations where sensor limitations are of lesser concern. Few studies do make a comparison between direct measurements of ε and one or more parameterizations in a low energetic environment, such as the Arctic Ocean (Fine2021,Baumann2023). However, these studies often only use shear-based observations of ε and are thus unable to consider values below the shear noise floor. Whereas for the few studies that are concerned with thermistor-based ε estimates in the deep sea (e.g., Scheifele2018,Yasuda2021), a comparison with parameterizations is not within the scope of those studies."*

Section 8 of the current manuscript is mostly a summary of the results, but I think more discussion should be provided on the reasons why the finescale parameterization and Thorpe scale method deviates from microstructure observations in some regions of the present study, based on a discussion in the Introduction about previous comparisons. For

example, the overestimations of the parameterization shown in Figure 5 and Figure 6 are currently interpreted as being due to iso-neutral stirring, but the influence of other physical mechanisms (e.g., double diffusion) should also be discussed.
We have added a T/S diagram of the profiles as Fig. B1. Most of the data is susceptible to double diffusion (R_rho>1), but in the individual profiles of T and S (not shown) there are no consistent thermohaline staircases visible. However, the T/S diagram shows interleaving variability in most of the profiles where also the triple decomposition is showing an increased signal of isopycnal stirring. This supports the idea that isoneutral stirring is at least dominating over double diffusive processes. This discussion has been added in lines 473-476.
It has also been pointed out by previous studies that there the limitations of the finescale parameterization due to the fixed R_omega
Yes, we acknowledge that there are limitations to the fixed R_omega and that a variable R_omega could lead to better results: '*It can be argued that the best choice to gain optimal results would be to use a variable R_omega (Sun et al, 2024)'* (Lines 214-215)
and that the low-resolution of the CTD may not capture small overturns leading to the overestimation of Thorpe-method (Sheehan et al., 2023).
Thank you for suggesting this reference. It has been added in line 379 and lines 549-554 of the revised manuscript
After the revision of the introduction and discussion part, I believe that this paper will more accurately provide the science significance of this study in the context of other literature.

My other comments are mostly on details that could be improved.

Minor comments:

Line 35: Units used in parentheses should not be italicized here.
Corrected

Line 39: 'temperature variance'. Consider replacing it with "thermal variance dissipation rate'/'the rate of decrease of thermal variance' etc.
Replaced with 'thermal variance dissipation rate'

Line 54: 'fine-scale'but 'finescale' in the other parts. Please check for consistent spelling.
'Fine-scale' has been replaced for 'finescale' in lines 54, 63, and 470 of the revised manuscript

Line 71: ‘strain rates’. Please rephrase it ‘strain variance’ or ‘strain’.

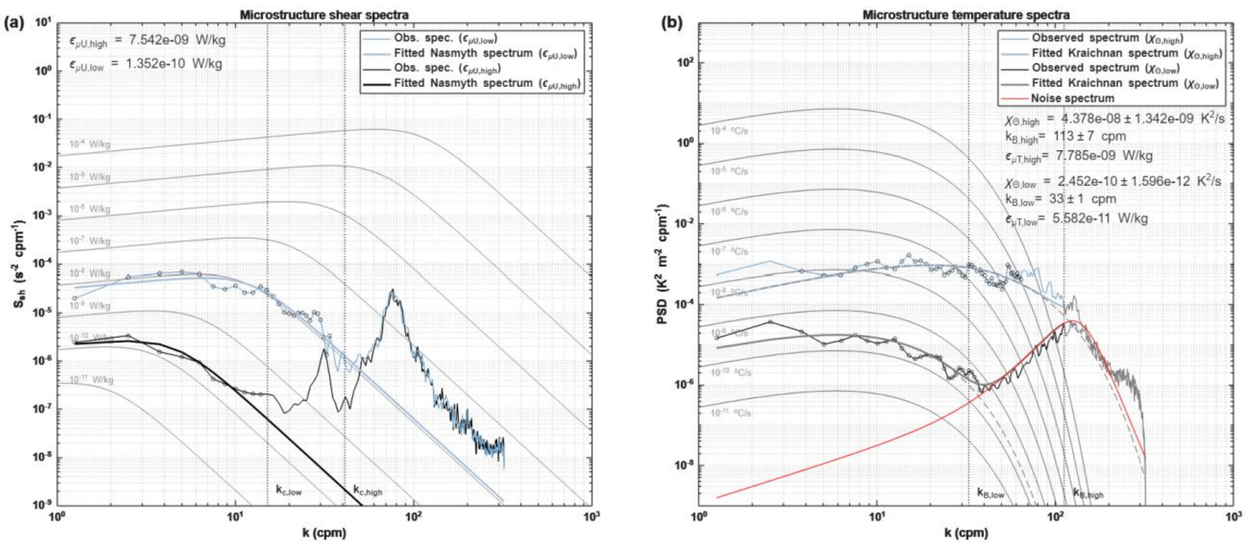
Rephrased as ‘strain’

Line 97: Estimates of εμU (εμT) were yielded from one distinct shear (FP07) probe? If they were obtained from multiple probes, are they averaged between them?

Yes, unless there was a large discrepancy between the two simultaneous estimates, the two estimates are averaged. This is described in line 626 of the revised manuscript, together with the other quality control metrics.

Section 3: Showing some example spectra (or composite spectra) for the shear and FP07 method in the appendix would be very helpful. Plotting the observed spectra with the fitted theoretical curves and some characteristics wavenumber (e.g., Batchelor wavenumber) would also make it easier for readers to follow the explanation of the methods for the estimates of εμU and εμT .

Thank you for the suggestion. Example spectra have been added as Figure A1.



Line 125: (Fer et al. (2014)) - > (Fer et al., 2014)

Corrected

Figure 3: typo in the legend of Figure 3b.’ perrcentile’ -> ’ percentile’. Also, plotting the lines of shear probe noise floors in Figs. 3a, c as well as Figs. 3b, d would be helpful.

The typo has been corrected. Also the lines showing the different methods for defining the noise floor, as was shown in Tab 1, have been added to panels 3a,c.

Table 1: Why the values of the mode (Scheifele et al., 2018) is smaller than the 5th percentiles (Piccolroaz et al., 2021). Please confirm it.

Thank you for spotting this, indeed the values have been swapped. This has been corrected.

Line 155: The citations should be inside the parentheses.
Corrected

Line 156: Please include references.
A reference to Piccolroaz et al (2021) has been added for the factor of 1.3 and a reference to Technical Note 40 of RSI has been added for the noise curve.

Line 157: What is 'x %' in this sentence?
Thank you for spotting this. The 'x%' has been replaced by '0.49%'

Line 160: Was the correction method to compensate for the insufficient temporal response (Lueck et al. 1977; Gregg and Meagher 1980; Oakey 1982; Gregg 1999) used in this study? Did you use the same correction method as Piccolroaz et al. (2021)? This should be clearly stated in the main manuscript.
The time response has been corrected using a double-pole transfer function. The first part of line 168 has been rewritten to: "Whilst the limited, speed-dependent time-response is corrected for using a double-pole transfer function (Vachon and Lueck, 1984), ..."

Line 206: (Waterman et al. (2013); Chin et al. (2016); Fine et al. (2021)) - > (Waterman et al., 2013; Chin et al., 2016; Fine et al., 2021)
Corrected

Line 213: I think that this sentence is not clear. How can we interpret the relationship between R_omega and the depths from Fig. 4? Please consider rephrasing this sentence (or revising Fig. 4).
We have removed this sentence.

Line 221: How did you define 'the surface mixed layer' here? Did you just omit the several bins close to the surface?
Yes, binning of the data for the finescale method was started at 100 dbar. 'the surface mixed layer' has been rephrased to 'the upper 100 dbar' to reflect this. We have also noted that the surface mixed layer was shallower than 100 dbar for all profiles.

Line 229: I think that it would be very helpful to plot vertical profiles of temperature, salinity, and R_rho and/or T-S diagrams to show the water mass variability in the present study. These plotting would be useful when you discuss how the region, where parameterizations were over(under)estimated, correspond to the finescale watermass variabilities. Also, when interpreting the results hereafter, it is important to indicate whether the water column is susceptible to double diffusion or not.
Thank you for the suggestion. A TS diagram has been added as Fig B1 and the following text has been added in lines 235-237: "*Finestructure watermass variability in the form of interleaving patterns are observed in the T/S diagram (Fig. B1). The depths where these*

patterns are observed correspond to the locations where temperature-based finescale estimates (severely) overestimate the measured dissipation rates ε_{vmp}."

Line 234: Why does salinity noise at weak stratification lead to the 'underestimation' of parameterization? Please explain it in more detail.

At this point we are not fully certain of the exact cause of the observed underestimation. The spectra of these segments are only minimally bluer than the segments that do agree well with the microstructure data, so that does not fully explain the observed underestimates. Also the next paragraph (l. 249-252), gives a possible explanation based on the shear-to-strain ratio, but unfortunately that is difficult to test with the absence of shear data (by calculating R). However, at this point flagging and removing these segments based on the proposed criteria seems to work well.

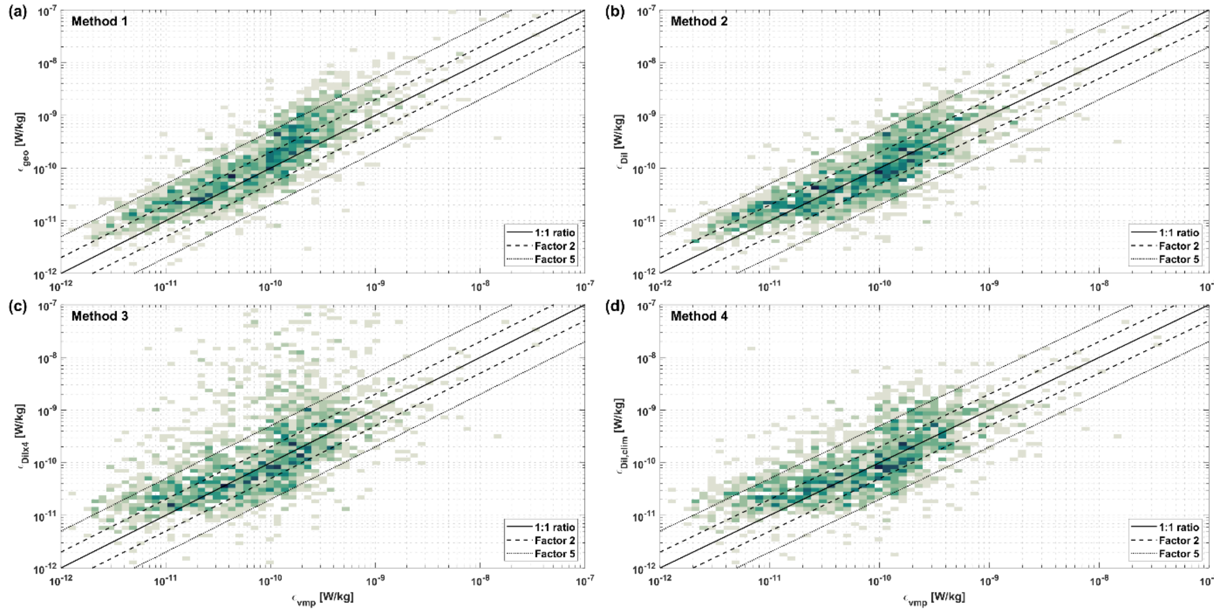
Line 305: App.D3 is not found in the manuscript.
This should have been App. C3 and has been corrected.

Line 326: I'm confused by this condition. As far as I can tell, the condition R_rho>1 corresponds to the salt-fingering-favorable regime (e.g., Schmitt, 1994). Why did you not include R_rho<-1 (doubly-stable regime). Under the condition R_rho>1, I'm afraid that the temperature change would capture the structure of the staircases and intrusions due to double diffusion rather than mechanical turbulence. Please explain this part in more detail.
Thank you for raising this point. The intention of the R criterion is to filter out overturns where temperature is not the dominate factor (-1<R<1). You are right that it should include R<-1 as well. We changed the mention of R>1 in l.330 and in Eq 6 to be |R|>1, so that it also accounts for R<-1. We did not encounter this in our calculations, so adding the absolute value did not alter the results.

Line 358: Did the vertical size of the overturn larger than 25m exist?
Only in a few instances. The results are calculated per overturn and given on the mid-point depth of the overturn. The mid-point depth is used for the averaging, so there is no double counting of overturns/dissipation rates in the averaging process.

Line 377: Since the overestimation and underestimation were considered together in Table 2, it would be helpful to have scatterplots (eps_TP versus eps_VPM) to look at overestimation and underestimation separately.

These plots have been added as Fig C2.



Subsection 5.5.2: As shown in Fig. 6, the criterion 4 mainly works as a rejection of overestimate beyond a factor of 10 (When assuming the criterion 4 leads to the removal of the overturn for which eps_TP< 1/25 eps_VMP and eps_TP > 25 eps_VMP). What is the cause of these overestimations?

We are not quite sure. Overestimations are also more abundant than underestimations it seems. This suggests that the Thorpe scales are so large that they lead to overestimation of dissipation compared to the VMP measurements. And as these overturns are otherwise removed by the Ozmidov scale criterion, this suggests that the turbulence might not be isotropic. Further study is needed to better understand this phenomenon. For now, criterion 4 seems to do the job to account for this.

Line 385: Does St.2 and 3 in this line refer to St. NP2 and NP3?

Yes, this has been corrected.

How can we interpret from Fig.2 that ‘parameterized estimate (εTP?)’ tends to be more similar to than ? Please consider rephrasing it.

The last part of this sentence has been removed.

Line 393: ‘about 30-40% is removed’ by which method among Method 1-4?

This is mainly by methods 3, and 4 if this is applied. The text ‘(mostly by criterion 3 and 4 if applied)’ is added in line 403-404.

Line 453: What did you use as D in calculating /in Fig7? Is it the same as in Eq.12? If so, please define here.

Yes, a reference to Eq 12 has been placed at the end of line 455.

Line 462: I think that it is necessary to discuss the possibility that the high values of is affected by the double diffusion rather than the isopycnal stirring. I understand that it is difficult to conclude it, but it would be useful to discuss various possible mechanisms other than the mechanical turbulence.
See our response to the next comment.

Line 465: I understand that it is difficult to see horizontal T/S variations from the observations due to the sparce locations, but I think it would be useful to show the vertical distribution of temperature, salinity, and Rrho. Is there any interleaving patterns in temperature and/or salinity in the vertical profiles of T,S?
It is indeed difficult to assess horizontal variations with the current dataset. We have added a T/S diagram of the profiles as Fig. B1. Most of the data is susceptible to double diffusion (R_rho>1), but in the individual profiles of T and S (not shown) there are no consistent thermohaline staircases visible. However, the T/S diagram shows interleaving variability in most of the profiles where also the triple decomposition is showing an increased signal of isopycnal stirring. This supports the idea that isoneutral stirring is at least dominating over double diffusive processes. A comment discussing this has been added in l473-477.

Line 510: Does ‘St. 4-8’ mean St. NP4-8?
Yes, it does, it has been corrected.

Line 542: ‘temperature-based’
Corrected