

Review of Referee #2

The authors provide a very detailed analysis of a series of microstructure profiles collected in the North Atlantic. They provide a detailed breakdown of the data and technical analysis of the performance of the instrument and comparison against non-microstructure approaches to estimating turbulent parameters. Through this they provide advice on the applicability and analysis of both microstructure and CTD data for turbulence. This includes considering the limitations of both fine scale parameterizations and Thorpe scale analysis which the authors interpret as being driven by the methods not accounting for isoneutral mixing processes. In general, this is a very detailed manuscript which is well written and provides interesting insight. The discussion nicely summarises the results and recommendations from this study however it would be much stronger if there was a discussion of how the recommendations made here compare with the results of other studies.

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.

We have added extra discussion on how the results compare with other studies in the Introduction (l.61-65 and in section 8, Discussion & Conclusions (l. 533-538, l.548-554)

Detailed comments

Line 72 – space missing between deep and sea

Space added

Line 157 – “Only x% of” missing value

Thank you for spotting this. The ‘x%’ has been replaced with ‘0.49%’

Line 193 – I suggest to clarify what you mean by shear as you did collect microstructure shear data so I assume you mean LADCP based shear. I suggest “shear” > “large-scale shear”

Thank you for the suggestion. ‘shear’ has been replaced for ‘large-scale shear’

Around line 275 – Is it true that your CTD is measuring at 8cm vertical resolution? I would be concerned about the impact of the thermal mass of your platform and the flushing time of CTD. I saw you quote the response time given by SeaBird in the appendix however that is not the time for the sensor to give the “true” value. I think the more relevant value is the settling time which can be quite a bit longer. These potential constraints should at least be mentioned.

The CTD is sampling at a rate of 64Hz (approx. 0.012m resolution, depending on fall speed), which is effectively a higher resolution than 0.08m (depending on fall speed). As you already indicate, there are several factors that influence response length scale, such as the sampling frequency, profiler fall speed and sensor response times. This is extensively described in Appendix C1.2. From this, it follows a typical value of 0.06m. Because of variations in this value, the more conservative estimate of 0.08m is chosen. Additionally, we note that the CTD instrumentation was mounted on the free-falling VMP. The thermal mass of this platform is not of concern, as the profiler is monotonously falling and thus not affected by e.g. ship-heave as would be the case for a standard ship-tethered CTD-rosette.

Line 349 – I think the full stop is meant to be a comma, as what follows isn't a sentence
This sentence was complicated, it has been rewritten completely.

Line 366 – I think the labels for alpha for each method are the wrong way round
Thank you for spotting this

Line 510 – The statement that the values are “likely unrealistically high” should be justified or removed. Whilst the authors might believe this, if it is to appear in a published paper it need to be driven by either data or some other evidence e.g. a scaling that shows that such values are not consistent with our existing understanding of the deep ocean. The authors compare with the profiles of Groeskamp et al 2020 however that approach also contains many assumptions and it is not clear to me a priori that one approach is more robust than the other.

The part of the isoneutral diffusivities has been moved to the Appendix D2, including most of this discussion. Nevertheless, in response to this comment, we agree with the reviewer that the method of Groeskamp et al 2020 is also based on many assumptions. This is clearly illustrated in the first part of the sentence. For further justification, we have added a reference to Abernathey et al 2022, Tab 9.1. For example, estimates from tracer experiments at large depths show very small isoneutral diffusivities, rather than the large values obtained in this study.

The sentence ending with “... are likely unrealistically high.” has been rephrased to “...are, when compared to previous diffusivity estimates (see e.g. Abernathey (2022), Tab. 9.1, and references therein), very high.”

Line 613 –Typo lenght > length
Corrected