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Comment on egusphere-2022-361

Anonymous Referee #1

Referee comment on "Turbulent kinetic energy dissipation rate and associated fluxes in the western tropical Atlantic estimated from ocean glider observations" by Peter M. F. Sheehan et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-361-RC1, 2022

In this manuscript, Sheehan et al. describe results from a Seaglider mission in January-February 2020 in the western tropical Atlantic. The paper focuses on reporting dissipation rates of turbulent kinetic energy using two methods: a direct microstructure method, with which temperature fluctuations are measured at centimetre-scales, and an indirect method based on the meter-scale vertical overturns detected in the temperature profiles (the so-called Thorpe method), also measured with a microstructure probe. The most interesting pattern in the observations is a local enhancement of turbulent dissipation below a subsurface layer of high-salinity associated with the subtropical underwater. The authors claim that this is the first study reporting microstructure temperature measurements from this particular type of glider. Another highlight of the paper is that, contrary to some previous assessments, the direct and indirect methods for estimating dissipation rates agree well in their dataset. Finally, the authors also report and discuss heat and salt fluxes driven by the measured turbulent motions and compare them with those potentially caused by double-diffusive instabilities, for which the conditions are favourable in the region.
Although the paper has some strong points (it describes novel dataset with some interesting results, it is well written), it has also -in my view- some major weaknesses that need to be addressed before publication. My general feeling is that the authors missed several good opportunities to make the paper more relevant and useful for their colleagues. I list my comments below with some suggestions of further analysis/discussions that could potentially improve the impact of the article:

(1) Microstructure Method. Overall, I missed a more extensive and direct acknowledgement of the limitations of the different methods used. The authors use a nowadays standard method to estimate energy dissipation rates from temperature microstructure measurements (the Batchelor fitting method). However, from my experience and the available literature, the method has important limitations and the results may be highly dependent on some user choices. In my opinion, those aspects are relevant to discuss. One key limitation is the fact that the dissipation rates detectable with

the method are upper bounded due to the limited time response of the FP07 sensor. For typical glider along-path speeds of 40 cm/s, maximum dissipation rates measurable are about 10-7 W/Kg. The authors report values larger than that in the upper mixing layer, but I haven't seen any mentioning as to how the sensor limitation may affect their results, in particular those values, and the comparison with the Thorpe-scale method. I would like to see some of the temperature-gradient spectra in the upper mixing layer and the fit parameters, to confirm that those values correspond to real dissipation. My experience is that for high epsilon values the roll-off in the temperature gradient is probably imposed by sensor limitations, such that the Batchelor fitting is not really meaningful (see also Paterson and Fer 2014). Further, the temperature spectra are typically partially corrected for the sensor time-response. The best parameters for the adjustment are not well stablished and can differ from sensor to sensor. It would be good to report explicitly what exact correction is used here. I also have a general vocabulary concern. The authors make a difference between what they call "spectral" methods for calculating turbulent dissipation, as opposed to non-spectral methods, like the fine-scale and Thorpe methods. In my view this is not entirely correct since, for example, the fine-scale method often involves spectral calculations. In my opinion, what distinguishes the two kind of methods is the scale at which measurements are done. In this regard, I think it would be more correct to use the term "microstructure method" as opposed to "indirect methods" (or something similar). I would recommend to change this throughout the manuscript.

(2) Thorpe-scale method. Regarding the Thorpe method, its applicability to non-vertically profiling instruments like gliders has been questioned in the past (e.g. Thorpe et al. 2012), since the method is based on measuring the vertical size of turbulent overturns, which is not entirely achievable with a slant-wise measuring trajectory. Although your results are encouraging, I still think it would be better to acknowledge and discuss the potential impact of this limitation more explicitly. Also, the authors contrast their results (in which the microstructure and Thorpe methods agree well), with a previous study (Howatt et al. 2021) in which the Thorpe method tended to overestimate dissipation. They speculate that this difference may be due to the different probe resolution: in the present study a microstructure probe is used, which allows a much better resolution of the smaller overturns, whereas Howatt et al. 2021 use a standard CTD probe. While this is just a mere speculation, the authors have the available data to evaluate this hypothesis: they could apply the Thorpe method to the glider regular CTD data and compare the results with those of the FP07-Thorpe method. I think not doing that is a missed opportunity, since it would be very useful for future studies to know whether the disagreement comes from the different spatial resolution or not. I have another concern about the method, in particular about what you describe in line 145 regarding the aggregation of overturns until they reach a scale of 2 m. Sorry, I am not familiar with this approach, but it seems a bit weird to me, why do you need to impose a minimal overturning scale if those appear smaller in the data? This could bias high the lower dissipation rates, couldn't it?

(3) Salt fingers. I have two main comments regarding the double diffusion diffusivity calculations and fluxes. First, I think there is a clear missed opportunity here of using the microstructure data to assess the role of double diffusion more directly. I missed, for example, a figure showing some of the staircases, but more importantly, you could use the thermal variance dissipation rates to directly assess the rate of heat mixing in the salt-finger favourable zone using the Osborn-Cox method. Several authors (e.g. St Laurent et al. 1999; Schmitt et al. 2005) have reported levels of thermal variance dissipation in thermohaline staircases, and heat diffusivities above those predicted by the Osborn model (your eq. 7). In most microstructure studies based on shear probes this information is missing, but you have it and you could compare the results with the flux law estimates.

My second comment concerns the latter method. I see that in your Figure 7, salt fingers diffusivities are zero except in the step layers, however it is not clear how you deal with those zeros in the calculation of mean diffusivities and fluxes. If you just neglect them, you may be overestimating the average diffusivities by quite a lot! But this is not explicit. Can you clarify that? That said, I am not entirely sure about the way in which the flux-law formulae should be applied, but my intuitive guess would be that the equations should be applied for the entire region of the vertical profile where the density ratio is favourable for salt fingers and the steps are observed, that is, applied to the background temperature-salinity gradients instead of the gradients in the steps, as you did. Not sure if I am wrong on that, but I invite you to dive a deeper into it and find out what is the most correct way of applying the formulae and report the details (how and where the diffusivities, gradients and fluxes were calculated; how did you deal with zeros.

(4) Discussion of the observed patterns. The main feature of your dissipation dataset is an enhancement in a thick layer below the salinity maximum. In my view, there is also a missed opportunity here of making this result more meaningful and relevant by discussing the possible drivers of this enhancement. Is that maybe due to internal wave interactions with the rapidly changing stratification? Do you have current measurements from the ship that indicate a local enhancement of shear or other data that could give some clue of what is going on? Not sure if further analysis on this is needed for the manuscript (which has a strong methodological component), but I think it could help to make the paper more relevant and interesting.

SPECIFIC COMMENTS:
-Line 8. Superscript missing in W kg-1
-Line 21. The Batchelor method applies only to scalars (e.g. temperature), not shear. The use in this sentence is not correct
-Line 25. These authors report 266 profiles in 50 different stations
-Line 61, the use of "," and "()" in this sentence, and other locations in the manuscript, seems weird to me
-Line 73. Why didn't you use the shear probe data in this paper? It feels also like a potential missed opportunity of a more thorough evaluation of the different methods.
-Line 95. This sentence "the rate of destruction of temperature-gradient variance" is not correct: note that chi units are K^2 per second, not (K/m)^2 per second. Temperature of thermal variance is destroyed, not the temperature-gradient variance. Remove "-gradient"
-Line 114-115. I am not familiar with this paper but the normalisation of the temperature gradient spectra with epsilon seems a bit unphysical to me, since the spectrum scales with chi not with epsilon. There is a proper way of normalising the temperature variance spectrum (see e.g. Dillon and Caldwell (1980))
-Line 123. You could perhaps define the meaning of the term (epsilon/N)^1/2. Also note the wrong spelling in "Tayor"
-Line 161 onwards. How was the critical value chosen? It seems a bit arbitrary. Why don't you use the alpha and beta coefficients to directly estimate the contributions of T and S gradients/variability to density stratification/variability? A more detailed description of the results of this quality check would be desirable. How many datapoints have been discarded? Where?
-Figure 3 or 4. It could be nice to see a distribution of LT here or elsewhere.
-Figure 4. Why are there no epsilon_T values in the upper mixing layer? What is the criterion to discard those values? (I guess it is related to the very weak stratification but you should mention it)
-Section 3.1. Uncertainties reported in this section have inconsistent values and units, also with respect to Figure 4. E.g. values in line 180, are much higher than those in 184 (and

have no units, while those in 184 do have units), and they also seem different from the error margins in the figure. Please, clarify how the standard deviations are calculated and improve the consistency.
-Line 195. There is no panel a in Figure 5.
-Lines 195-196. Not sure I see this modest peak you talk about, or maybe it is too modest to be worth mentioning?
-Line 199-200. Is there something wrong with this sentence? I do not get it.
-Line 205. Why? (see comment above)
-Line 206-207. I think you need a bit more clarity in this sentence. What makes the epsilon and kappa distributions similar?
-Line 216. Epsilon units missing
-Line 217. Is there an upward diffusive heat flux in the upper layer? Does that mean that thermal stratification is inverse?
-Line 235. "[...] and given that theoretical flux laws can overestimate κS in the real ocean [...]". This sentence needs a reference
-Line 245. By eye this difference looks greater. More details on how the averaging is performed are needed
-Line 258. "sampling and the necessary setting of a minimum Thorpe scale", why do you need a minimum Thorpe scale? (see comment above)
-Lines 262-263. "however, the lack of similarly high-resolution salinity observations means that the resolution of N used to calculate εT in Eqn. 5 will limit any improvement that a small minimum LT might otherwise have on estimate of εT". The N that you introduce in the εT formula is, in my view, a background N representative of scales larger than the turbulent overturns, so I don't necessarily see the lower resolution of N as a problem here.
-Lines 264-265. "Howatt et al. (2021), who reduce the difference between their two estimates of ε," I don't get the meaning of this sentence
-Line 285. See main comment (4)
-Lines 290-292. "The downward flux of heat due to double diffusion is almost four times that due to turbulent mixing; the downward flux of salt due to double diffusion is 7.5 times that due to turbulent mixing.". The differences in diffusivities (as reported in line 245), appear much weaker. More details are needed on this calculation.
-Lines 298-300. "We posit that this improved agreement is due to εT being calculated using the same high-resolution observations as were used to calculate $\varepsilon\mu$: other studies have compared $\varepsilon\mu$ to εT calculated using lower-resolution temperature observations." That seems plausible but you have the data to test this and I think you should (see main comment (2))

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