REVIEWER 3

This paper provides a standardized method of processing measurements of epsilon from point velocimeters. It provides clearcut recommendations for all aspects of processing, from QC and processing of velocity data through the fitting of the inertial subrange spectra to retrieve epsilon. This paper, and papers like it, provide a valuable opportunity to make turbulence measurements more accessible to the oceanographic community.

We thank Reviewer 3 for their feedback and respond to their comments below.

General comments

To echo other reviewers, I think code examples of your processing methodology would be extremely useful in allowing readers to actually implement your suggested best practices. Please consider it. A similar field turbulence methods paper (Zippel et al; doi: 10.1175/JTECH-D-21-0005.1) provided their code. It doesn't need to be perfectly commented or organized!

Prior to writing this manuscript, the development of the Working Group's Terms of Reference examined the possibility of including code. Ultimately it was decided against distributing software (see Terms of Reference). The logic for this decision was the overarching objective being to document the best practices so that researchers can then validate different processing workflows against the benchmarks at any checkpoint (e.g., fitting algorithms at Level 3). Our chosen strategy avoids the pitfall of having the working group's activities become obsolete because the code cannot be maintained and updated to keep up with new flavour of programming languages. It also intrinsically recognises that there may be other ways to arrive at the same answer.

There are many different ways to approach parts of the analysis such as spectral fitting and selection of the wavenumbers that define the inertial subrange. You make a case for your chosen method, but I don't think that enough evidence is always given to justify your choices as "best practices". Please see specific instances in the line-by-line comments

There are indeed processing steps for which the results were insensitive to the choice of techniques/method. In those situations, we elected to recommend the easiest to implement by users. We have also added some introductory text to emphasize that this best practices represents a starting point towards developing standards as the community implements and develops new techniques. The data format was specifically designed to facilitate testing at intermediary points of the processing workflow and build upon the working group's activities [L54-58].

Line-by-line comments

Line 26: Is it possible to briefly explain the physical reason that microstructure profilers are better suited for low energy environments and point-velocity better for high-energy?

We have added more text to highlight that the difference is attributed to shear probes being able to resolve the viscous (smallest) scale of turbulence, while the point-velocity sensors can typically only resolve the larger scales of turbulence. The viscous subrange are the last to be negatively impacted by flow properties (e.g., anisotropy), and thus lowers the resolvable ε that can be estimated.

Line 57: Since Shcherbina et al, 2018, at least a couple of papers have shown pulse-coherent ADCPs to be a viable method for obtaining field measurements of dissipation rate, so perhaps the phrasing "on the cusp" is inaccurate. Please see Zippel et al. (2021); "Moored Turbulence Measurements Using Pulse-Coherent Doppler Sonar"; doi: 10.1175/JTECH-D-21-0005.1

The Zippel et al. citation was added. The text was also reworded to highlight that smaller bin sizes of ADCPs now allow for direct computation of the wavenumber spectra.

Line 70 u_rms should be defined

This term has been defined on L77.

Line 76: Please define and explain "ambiguity velocity".

As suggested we have clarified "The ambiguity velocity defines the maximum (unambiguous) along-beam velocity that can be measured (for a given transmit pulse)".

Line 84: Because you go on to also describe the viscous subrange and "large turbulent scales", perhaps a description of the turbulence cascade would be helpful here.

We added some text to situate the inertial subrange within the other subranges on L93-95 by briefly stating the energy cascade model.

Line 94: "The largest scales [of the inertial subrange]" – please specify in order to avoid confusion with the largest scales of turbulence in general.

The sentence was reworded as "being more comparable" to the smallest scales. The largest scale of the inertial subrange according to the cited references in that sentence are L/3, thus about a third of the largest scales of turbulence "in general".

Line 127: Do you mean "highest" rather than "high"?

No, high not the highest. A prime example is the Tidal_MAVS dataset in Figure 1. We are not even close in resolving the highest wavenumbers within the inertial subrange, but because it's an energetic site, we resolve ample of the inertial subrange to estimate $\hat{\varepsilon}$.

Line 203: I think it would be worth going into more detail on how unwrapping can be performed, or at least providing a reference to a paper that does. Some wrapping is unavoidable, and even datasets with a lot of wrapping can still be fully usable once corrected.

As suggested, we have added some relatively recent references to recent works that outline a number of methods to undertake phase-unwrapping.

> *Line 204 More detail on selecting the max velocity during programming in order to avoid wrapping should be given. For example, if you've never sampled in a particular environment before, how should you best estimate the velocity range? Perhaps you could refer to the velocity ranges given in Table 1 as examples of what a user could expect across different environments.*

The phase wrapping is now described in its own section 4.1.1. We have expanded on ways to choose the maximum velocities (numerical modelling, tidal and hydrological predictions). We also highlighted that sometimes you still get it wrong despite having much of the required information in hand (e.g., phase-wrapped *Tidal Shelf ADV benchmark*).

> *Line 330: Even though these methods are detailed in Bluteau (2025), it would be good to at least summarize what they are here.*

Yes, we have added a paragraph at L356-364 summarizing the six methods tested by *Bluteau* (2025)

> *Line 333: Why was this method chosen of the 6? Perhaps more details should be given on the comparison analysis between the 6 stated methods?*

We added several sentences about the better accuracy of log-based methods for spectra with low degrees of freedom (i.e., computed from short time segments), and explained the advantage of our recommended logLAD method over the least-squares of log-transformed spectra [L368-371].

> *Line 365: How does a user decide on a value for A? Perhaps mention that you describe this further in Section 4.6.4. Also, why not base flagging directly on the deviation of the spectral slope from the theoretical -5/3? (I am not suggesting that is better, I'm just having some trouble following how A is derived)*

We refer the reader now to section 4.6.4 where the choice of A is discussed when flagging. We do not recommend flagging the deviation of the estimated slope from -5/3 because the estimated slope varies (within known bounds shown later) depending on the technique used and the amount of spectra averaging when estimating the spectra (hence A).

> *Line 375: Here, does log-transformed spectra refer to the (synthetic) observed spectra ($\hat{\Psi}$), and model to the theoretical spectra with the $-5/3$ slope (Psi)?*

Yes, the observed (or synthetic) spectra depending on the context. The sentence has been amended with the addition of equation 16. The identified inertial subrange from these two methods are now illustrated in Figure 6 d,e.

> *Line 376: Why is this particular strategy recommended?*

We have provided more information in §4.4.2 as to why we recommended this strategy. We chose the slope method partly for convenience since we are calculating the slope (β_1) to flag $\hat{\varepsilon}$. This method was less biased than the other methods when fitting spectra with limited degrees of freedom provided the fitting uses our recommended decadal range $\delta \geq 0.8$ (not shown). However, for spectra that are sufficiently averaged (i.e., degrees of freedom $d \geq 10$), all four methods returned acceptable results for locating the inertial subrange.

> *Line 399: Am I understanding that Figure 8 is showing that the introduction of noise does not cause substantial deviations in the ratio of theoretical to measured epsilon? I'm not sure that this alone is enough to justify your method of treating noise is optimal.*

Figure 8 (now Figure 7) shows by how much ε can be over-estimated based on which portion of the inertial subrange is fitted and spectra's noise levels. It is used to justify flagging $\hat{\varepsilon}$ based on an acceptable deviations from ε based on the fitted wavenumbers and noise levels. We have added a few sentences to explain why it was chosen over the other strategies found in the literature [L437-440].

> *Figure 8: Please define k, L_K, epsilon, and epsilon_m in the caption. Interpretation of figures is greatly helped by avoiding the reader having to hunt through the text for variable definitions.*

We added the word minimum in front of ε_m, and written in full k and L_k.

References

Bluteau, C. (2025), Assessing statistical fitting methods used for estimating turbulence parameters, *Limnol. Oceanogr.: Methods*, doi:10.1002/lom3.10729.