Response to referee comment on "Technical note: Turbulence measurements from a Light Autonomous Underwater Vehicle" by Eivind Hugaas Kolås et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-107-RC1, 2021

We thank both reviewers for their constructive comments and useful suggestions, which helped to improve the manuscript. Below we provide a point-by-point response to all comments raised by the reviewers. Reviewers' comments are reproduced in *italic type in red* followed by our response in regular type, black color. We note that this is the discussion response to the reviewers' comment. Our final response at the next stage will detail the changes made in the manuscript thoroughly.

Response to Reviewer 1

This paper reports on experiences with integrating microstructure shear sensors on a lightweight autonomous underwater vehicle (AUV). A particular challenge to overcome is that small AUV fly less stably, being actively propelled and maneuvering, contaminating microstructure spectra. The authors report good data quality down to about 5e-8 W/kg dissipation, which makes it fit only for measurements in turbulent boundary layers, which I do not see as too big a constraint given the lightweight nature of the AUV in the first place. This technical note is very thorough and I only have a few comments and further questions that the authors may wish to consider during revision.

Thank you for the positive comments.

75: DVL was downward looking? DVL1000 I assume means a 1 MHz instrument? With 1MHz the DVL probably did not have bottom lock (Though I have not checked the bottom depth at your location) - how is the trajectory (see Fig 1b) so well-constrained? What is the navigation accuracy? Were there other navigation aids apart from inertial?

Indeed, the DVL1000 is a downward looking 1MHz instrument. It did not have bottom lock during this mission as the depth was about 250 m. The trajectory is only constrained by inertial navigation, and the expected drift is about 15% of distance travelled.

We now added two sentences explaining this:

±2.0° for pitch and roll. As the depth was about 250 m, the DVL1000 did not have bottom tracking during this mission. The AUV trajectory was only constrained by inertial navigation, and expected drift is about 15% of distance traveled. The

147: Why is this used as opposed to the methodologies used by Moum et al. 1995 or Fer 2006 (iterative integration)?

Thanks for pointing out this inaccurate processing description. The upper integration limit is set by the smallest of several criteria that are explained thoroughly at the ATOMIX wiki page https://wiki.uib.no/atomix/index.php/Iterative_spectral_integration_algorithm (see a snapshot below). The 5th criteria described here is the iterative integration. Yet for most cases, the limiting factor is constrained by the electronic noise because the other wavenumber criteria are typically larger than where the noise starts to dominate the shear spectrum. This is determined by fitting a low-order polynomial (the 1st criteria). We now rewrite this sentence as; "…and the upper ($k_u < \infty$) integration limit is usually determined from a minimum in a low-order polynomial fit to the wavenumber spectrum in log-log space.".

Description from the ATOMIX wiki:

The upper limit of integration, k_u, is set by the smallest of a number of criteria that are listed next.

(1) The wavenumber range that is dominated by electronic noise can usually be determined from a minimum in the spectrum. Real shear is at wavenumbers smaller than this minimum while electronic noise is usually at higher wavenumbers. The spectral minimum may be found by fitting a low-order polynomial to the spectrum in log-log space. Third order is often sufficient. The wavenumber of the spectral minimum, $k_{\min}$, sets one of the limits on k_u.

(2) Another upper limit is $k_{150} = 150$ cpm that is imposed by the spatial resolution of a commonly used shear probe. You may use a different value if your shear probe has a spatial resolution different from that reported by Macoun and Lueck, 2004[2] At the wavenumber of 150 cpm the spectrum derived from the commonly used shear probe is boosted by a factor of 10. At higher wavenumbers the spectral correction is more than a actor of 10 and such large corrections are not recommended.

(3) The cut-off frequency, f_A, of the anti-aliasing filter used by the shear-probe sampler sets another upper limit of spectral integration, namely $k_A \leq f_A/U$. Because most filters have a transition range from passing to attenuating a signals, it is wise to set this limit to value slightly smaller than the cut-off frequency. For example, $k_A \leq 0.9\, f_A/U$.

(4) The user may impose an upper limit to exclude wavenumbers that have contaminations that are not correctable, $f_{\lim}$. For most instruments this limit is usually set to ∞, but it may be prudent to set this limit to a finite value in some cases.

(5) The final wavenumber limit, k_{95}, is the wavenumber at which the variance of shear is resolved to 95%. There is not incentive to integrate the spectrum beyond this limit because the correction that must be applied amounts to only 5%. This wavenumber is $k_{95} = 0.12\,(\varepsilon/\nu^4)^{1/4}$ and the factor of 0.12, is nearly identical for all of the common approximations to the shear spectrum, such as the approximations to the Nasmyth [3] [4] [5] , the Panchev-Kesich [6] , and the Lueck [5] non-dimensional universal spectra.

Thus, the upper limit of spectral integration is

$$k_u = \min(k_{\min},\ k_{150},\ 0.9k_A,\ k_{\lim},\ k_{95}).$$

The last of these upper limits, k_{95}, presets us with a conundrum because it requires the rate of dissipation which is what we are trying to estimate by way of the integration of the shear spectrum. Clearly, we need to bootstrap this process by starting with a reasonable (but certainly a rough) estimate of the rate of dissipation.

184 and 230, regarding data quality differences between dw/dx and dv/dx: You could consider adding a remark on how stratification and/or violations of isotropy may play a role or not, given that it seems to be worse at lower dissipation (lower buoyancy Reynolds number - but how low?).

Thank you for suggesting this. From our two vertical microstructure profiles used to compare to the MR-AUV data, we see that the buoyancy Reynolds number is large (>100) for the entire water column, and very large in the upper 60 m (>10^4). This is consistent with the estimates from the LAUV dissipation measurements using the buoyancy frequency at the measurement level. It is unlikely that the difference between the two probes is caused by vertical stratification or anisotropy at probe separation scales. We now add a sentence about this; "In weakly-turbulent regimes the assumption of local isotropy may be violated, and the dissipation estimates from the orthogonal probes deviate when the buoyancy Reynolds number ($\frac{\varepsilon}{\nu N^2}$) is about 200 or less (Yamazaki and Osborn, 1990). The AUV mission is conducted within the weakly-stratified upper surface layer with large buoyancy Reynolds numbers (>10^4, not shown), and we do not expect differences caused by vertical stratification or anisotropy at probe separation scales.".