

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 2

"Technical note: Turbulence measurements from a Light Autonomous Underwater Vehicle" by Eivind H. Kolås, Tore Mo-Bjørkelund, and Ilker Fer

The authors report on a successful test deployment of a small AUV combined with a turbulence microrider. They show that issues arising from thruster vibrations can be overcome in high mixing environments, by post processing. The resulting turbulent dissipation rates are plausibly reliable (while uncertainty limits have not been estimated). The paper is written in a clear manner and will fit the journal, after a number of necessary additions and revisions.

Thank you for the positive and constructive feedback. Following both reviewers' comments we provided substantial revisions and clarifications that improved the previous version of the manuscript.

The program of the paper is well distilled in the title and the first two sentences of the abstract (technical note; combination AUV - MR; data quality), so that it seems natural to structure the major remarks accordingly:

(1) aspects of the combination lightAUV - MR: pro and contra, future potential.

(2) aspects of a technical note: complete and precise descriptions and practical hints.

(3) aspects of data quality to be expected.

Your structured and thorough comments have been very helpful and are much appreciated. Below we reply to each of your remarks.

(1)

The paper does not talk much about why the combination of light AUV and MR was used, or why other researchers should take this step. There is one passage in the course of the paper: the light AUV is lighter, cheaper, easier to handle than heavy AUVs (L.53f). I'd recommend to inform potential users in a condensed short discussion (might be in the Introduction

or Conclusion): what past gap in turbulence measurements can be closed by this system; what is its future potential; why and when should a researcher owning a glider-MR or heavyAUV-MR switch to lightAUV-MR?

We agree with the reviewer. We now include a short discussion in the introduction about the potential advantages of this system. In addition, we elaborate on the potential usages of this system in the discussion.

In this study, we mounted a self-contained turbulence instrument package on a Light AUV, and collected measurements in the Barents Sea in a frontal region where Atlantic- and Arctic-origin waters meet (Figure 1). The Light AUV is lighter than REMUS and the Autosub Long Range AUV. With a weight in air less than 35 kg (depending on the number of instruments on it), it is usually portable by one person, and enables easy deployment and recovery. In addition, the Light AUV is significantly more affordable than other AUVs, and with an open access software and ease of hardware configurations, it is a desirable and highly versatile product.

What are the constraints of this system? Is it really 100m depth range, order 10 hours of mission duration, 1m/s speed, cold water environment, high turbulence environment, short-distance communication only via acoustic modem, epsilon uncertainty of factor 2 to 5; as might be inferred from different parts of the paper? Are these final constraints or is there potential for pushing the limits?

The LAUV system is constrained to 100 m depth (although the MR is 1000-m capable). The mission duration largely depends on the operating speed and is usually around 12 h. Another version of the LAUV with larger battery that can last up to 72 h is also available. Speeds can reach 4 knots (about 2 m/s), yet the normal operating speed is about 1.5 m/s (without the MR). The water temperature is not a constraint. This particular MR/LAUV setup is only suitable for high turbulence environment, but improvements on the MR integration can potentially reduce the background noise contamination. 97% of dissipation estimates from the two sensors agree within a factor of 2. Communication can be done over the horizon via satellite (Iridium), which was done during the mission in question. Short-distance communication can be done through Wi-Fi and acoustics.

We now include more details on the LAUV constraints in Section 2.1 (see the snapshot from the revised manuscript in the next comment). In addition, in the discussion section, we elaborate more on the limitations and potentials.

(2)

There are some open technical questions that users probably would be interested in.

For the following comments to section 2.1, we now include more details as suggested. A snapshot of section 2.1 is inserted below. We also answer each comment separately below.

2.1 Light Autonomous Underwater Vehicle

The Light AUV was developed at the Underwater Systems and Technology Laboratory at the University of Porto (Sousa et al., 2012). It is commercially produced by OceanScan Marine Systems and Technology Lda. Our AUV (sketch shown in Figure 2) is an extended version compared to the standard Light AUV. It is 100 m pressure rated, and equipped with a pumped CTD (SBE-49 FastCAT), a Nortek Doppler Velocity Log (DVL1000), an attitude sensor (Lord Microstrain 3DM-GX4-25), an acoustic modem, a Fluorescence sensor and a dissolved oxygen optode. The accuracy of the measurements from the AUV are $\pm 0.002^\circ\text{C}$ for temperature, $\pm 0.0003 \text{ S m}^{-1}$ for conductivity, 0.3% RMS (root mean square) of the measured value for horizontal flow speed past the instrument (measured by DVL1000), $\pm 8.5^\circ$ for yaw at the observation latitude and $\pm 2.0^\circ$ 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 AUV is controlled by the on-board software DUNE Unified Navigation Environment, and is configurable both in hardware and software. Expected mission duration is between a few hours to 48 hours, largely depending on the operating speed. While maximum speed can exceed 2 m s^{-1} , a normal operating speed (without the turbulence package) is about 1.5 m s^{-1} . The AUV communicates via satellite (Iridium), WiFi and acoustics. It can be remotely controlled within WiFi-range (about 200 m range), which can be useful during deployment and recovery. While deployment is easily done from ship using a crane (see Figure

4

3), recovery is best done from smaller work boats to avoid damaging the instrument. The turbulence package was mounted below the AUV using custom-made brackets, and connected to the AUV using a bulkhead connector and a custom-made cable. Due to the extra drag caused by the turbulence package, the operating speed during our mission was about 1.1 m s^{-1} . Before deployment, the AUV was programmed to follow the frontal zone by tracking the maximum temperature gradient at different depths, which it successfully did.

For section 2.1:

- What mission duration can I expect? (battery endurance? reliability? see leak after 5 hours)

Mission duration can be between a few hours to 48 hours with the standard battery, depending on the operating speed (1-2.6 m/s). The version with extended battery capacity can last up to 72 hours.

In this LAUVs lifetime, there has been three leakages from a total of about 1000 deployments. The cause of the leakages was a batch of bad antennas. The system is reliable.

- Is remote control during the mission needed/possible at all? How? At what range?

Remote control is possible when the vehicle is at the surface and within WiFi-range (200 m from the mothership). It can be remotely controlled using a smart phone (need to download an application). It is not necessary to remotely control it, but it can be useful during deployment and recovery.

- Size/weight of the AUV (Fig.2 can only give a hint)?

Length: 2.6 m, diameter: 15 cm, weight: 35 kg.

- How is the handling and constraints for deployment and, particularly, recovery?

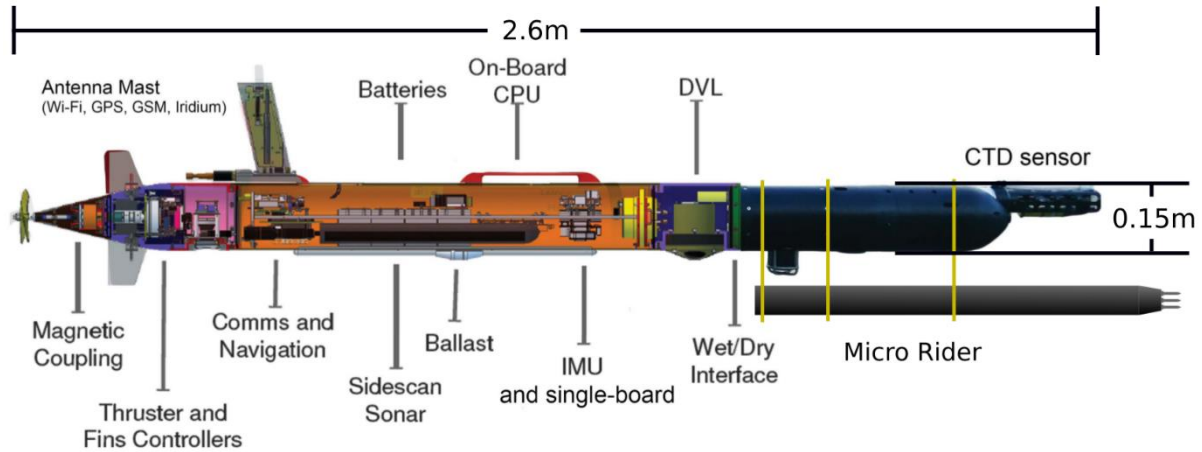
Deployment can be done from a crane on board the mothership, however, one needs to recover the vehicle from a work boat or similar. This adds constraints on the weather conditions.

- *Is there light AUV versions with higher pressure rating?*

Not currently, but a 200 m version is in the making.

- *Particulars about the MR mounting, possibly a drawing.*

The following figure illustrating the setup has now been included in the paper.



- *Is there anything known about self-oscillation of the system (maybe from the MR vibration sensor while thruster off)?*

No, there is unfortunately no information about the self-oscillation of the system available.

For section 2.2:

- *How severe would have been the consequences, if the MR had not been in TE configuration? Referring to Fig.4 it seems that resolving wavenumbers up to 82 cpm instead of 164 cpm could still work to some degree.*

We agree with the reviewer that in this particular dataset, the standard MR configuration could work satisfactorily to some degree. The LAUV used in this paper has the capability of moving at speeds exceeding 4 knots (>2 m/s). Due to the drag of the MR mounted below the AUV, the actual maximum operating speeds were limited to about 1.5 m/s. However, as higher speeds generally caused more artificial vibrations and vehicle oscillation, we decided to not go faster than about 1.2 m/s. We agree that for operating speeds no more than about 1 m/s, the standard MR configuration may have been sufficient. However, with a better integrated MR (for instance a MR mounted inside the wet-nose section of the AUV) higher speeds would be achievable with reduced drag. We now elaborate on operating speeds in section 2.1, and states in section 2.2 that these modifications allow reaching wavenumbers up to 130 cpm at 1.5 m/s.

- *(L.106) Is there more known about the flow deformation around the system?*

The flow field around AUVs with similar shape and cross-section as the LAUV we used, has been modeled (Mostafapour et al. 2017; DOI: 10.29252/jafm.11.02.28302). Although this cannot fully represent our case, their figure 4 shows the flow deformation around the AUV hull. According to the velocity contours, the shear probes protrude sufficiently away from the body of the AUV and are expected to sample flow with negligible deformation. The additional flow deformation due to protruding instruments (such as the CTD) is unknown. We will insert this citation and the clarification in the revised version.

For chapter 3:

- (L.128) Has the spectral loss due to the high pass filter been corrected during post processing? And akin to this question, has the spectral loss due to the finite size of the airfoils been corrected?

Yes, both have been corrected for. The high-pass filter correction is obtained by multiplying the spectrum by $1 + (f_c/f)^2$ where f_c is the high-pass cut-off frequency and f is the frequency. This correction only affects the spectrum near the cutoff frequency of the high-pass filter, f_c . The correction due to the size of the shear probe is done by multiplying the spectra by the factor $1 + \left(\frac{k}{k_0}\right)^2$ where $k_0 = 50$ cpm and k is the wavenumber expressed in units of cpm.

We now clarify these corrections in section 3.

- (L.130) How is despiking done?

The initial despiking is done by comparing the absolute shear and vibration time series to a 0.5 Hz low passed time series. When the ratio between the absolute and the low passed time series of shear or vibration exceeds a set threshold (a value of 8 typically works fine), the spike ± 0.04 s is replaced by the average value over 0.5 s before and after the spike.

We now include these details in the processing description (Section 3)

- (L.135) 'calculating the shear frequency spectrum': this is probably averaging of the 8 spectra and correcting for the windowing loss; or is there more?

The 1-s second fft length is overlapped by 50%. For each 8-s record we average 15 spectra and correct for the windowing loss. We now specify in the text that the 1-s fft length is also half overlapping, and that the final spectra shown for the 8-s portion is an average.

- (L.150) The underlying assumption to exclude the larger shear sensor value if surpassing a factor of 5 is probably that the previous despiking was not perfect, and the factor 5 is from experience. Is there a rationale for this, e.g. 'at 8s-segments a factor 5 anisotropy between horizontal and vertical is so rare, that we can safely assume a particle collision happened to the sensor with larger signal'?

Thank you for pointing to this. The reason for using a factor of 5 is, as you say, based on experience, and to simply allow for a security net when one noisy estimate from a probe survived the despiking. This threshold is routinely implemented in our processing, however, for this particular dataset, the two probes always agreed to within a factor of 4. We now removed the mention of factor of 5 to avoid confusion, and state that the final dissipation estimate is an average of the two probes, and that they always agreed to within a factor of 4.

- (L.156) Is there a criterion why change rates of 10° roll/s, 5° pitch/s, and 2 RPM/s have the same consequence?

The final screening done here is based on visual control of the timeseries, where abrupt rate of change of flight behavior can lead to contaminated dissipation estimates. The chosen thresholds delineated these and are used to remove outliers. We've produced scatter plots of rate of change vs dissipation, and decided on rate of change thresholds based on those. A

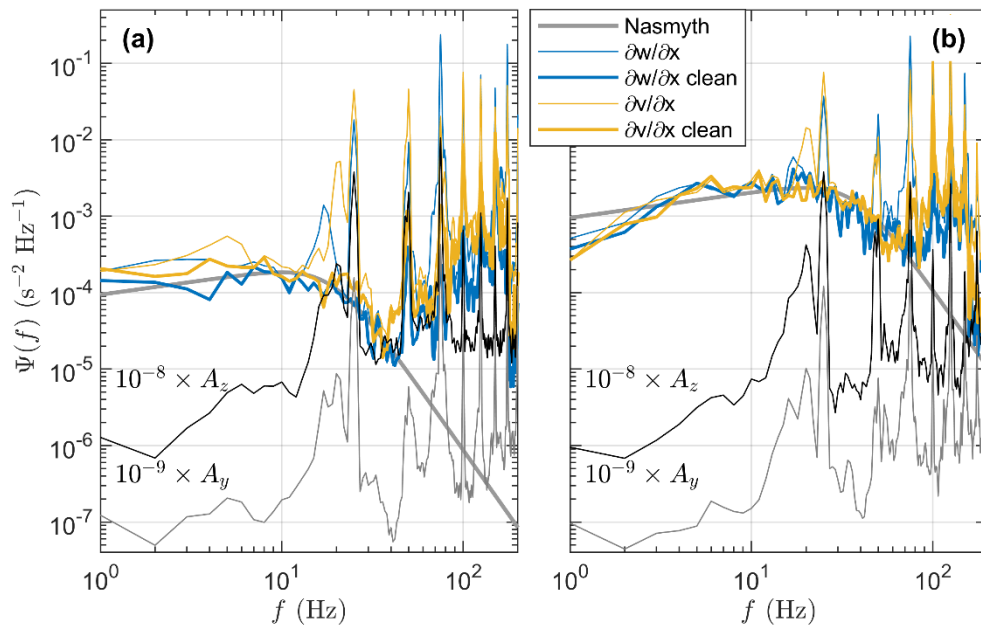
change in roll is generally a much smaller disturbance on the shear sensor than a change in pitch (because of how the MR is mounted on the AUV), thus a higher value is allowed. There is no criterion why the different thresholds on the different variables should have the same consequence.

We now specify in the processing description that the thresholds in the final screening is based on visual inspection of the rate of change vs dissipation estimates.

For chapter 4:

- (L.171f) The cleaned spectra additionally show extra removal of signal. Fig.4 shows a factor 1.5 to 2 reduction in the frequency band from 1 to 8 Hz, although there is no relevant vibration. Similar for the band between 25 and 50Hz. What is the reason for this? And what is the bias in estimated epsilon caused by this?

Thank you for pointing this out. Indeed, the cleaned spectra show an extra removal of signal even when vibration is not relevant. This is because the Goodman algorithm relies on the squared-coherency between signals produced by shear probes and vibration sensors, thus is always bigger than zero even if signals are completely incoherent. Correction for this bias was not applied when processing our data. The bias (in the integrated shear variance) can be corrected for by dividing the spectrum by $1 - \frac{1.02N_v}{N_f}$ where N_v is the number of vibration signals used and N_f is the number of fft-segments used. This result can be obtained from Nuttall, A., 1971: Spectral estimation by means of overlapped fast fourier transform processing of windowed data. NUSC Tech. Rep. 5291, [Available online at <https://apps.dtic.mil/dtic/tr/fulltext/u2/a182402.pdf>], Naval Underwater Systems Center; Lueck (2022, personal communication). In our case N_v and N_f are 2 and 15 respectively, which would increase the spectra by a factor of about 1.16. The figure below shows an example where we correct the cleaned spectra by a factor of 1.16. This is not as much as the factor 1.5 to 2 you mention, and we can only assume the remaining difference between the uncleaned and cleaned signal is correctly removal of noise even if the vibration signal is relatively low.



The resulting bias on the dissipation estimate can be slightly larger, carried over from the estimate of upper cutoff wavenumber and other processing steps. A comparison of the Nasmyth spectra multiplied by a factor of 1.16 and the Nasmyth spectra calculated from higher epsilon, shows that a factor of 1.16 offset in the spectra translates to an increase in epsilon by a factor of 1.22 to produce the same spectra. Hence the Goodman bias has a slightly higher impact on the epsilon estimates than on the spectra, but this is not systematically quantified here and can be nonlinear. We did not reprocess our data set to remove the bias induced by the Goodman algorithm; however, we recommend this correction when the bias correction method is scientifically reported. We are aware of a study in progress led by Rolf Lueck on this topic.

We now comment on the bias introduced by using the Goodman method in the results and elaborate on this in the discussion.

For chapter 5:

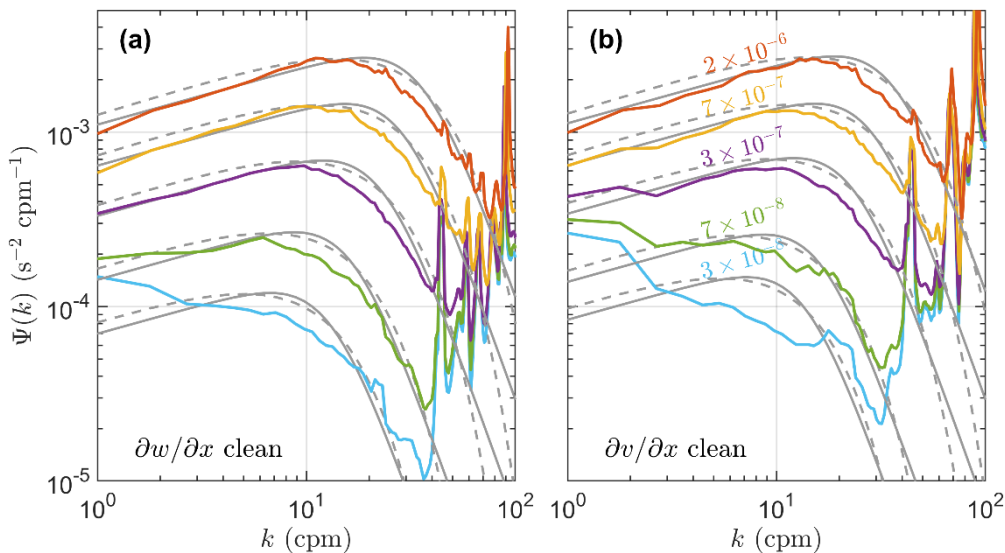
- (L.214) Is it clear that vehicle vibrations are the source of the 10-30Hz frequencies? If yes, integrating the MR into the AUV instead of using brackets (as proposed in L.239ff and L.250f) would not have much effect. What is the contribution of the brackets, of the MR body, of the shear sensor shafts? Only the part of the brackets possibly could be remedied.

The phrase “Vehicle vibration” was inaccurate in our paper. We also mean to include vehicle motion such as roll and pitch. One of the major issues with mounting the MR below the AUV as we did, is the reduction in AUV maneuverability. The additional off-center drag due to the MR caused more pitch than during a dive without the MR, and subsequently the AUV rudders made more adjustments than during a dive with no MR. This behavior could be reduced (if not avoided) by mounting the AUV in a more streamlined fashion. We cannot quantify the individual contributions to vibration from the brackets, the MR body or the shear sensor shafts.

The phrase “vehicle vibration” is now clarified in our discussion, and we elaborate on the problems with pitching rudders.

- Fig.5: Suddenly the theoretical Panchev-Kesich spectrum pops up, only in the left panel, without having been introduced before. Its only usage in the paper is in L.218 stating that the spectra resemble both Nasmyth and Panchev-Kesich. However, inspecting Fig.5a, it seems the cleaned spectra fit the Panchev-Kesich spectral shape much better than Nasmyth. (Only the estimated epsilon would result a little lower after a thorough fit.) If so, much of the discussion on early rolloff and spectrum averaging (L.219-224) would be obsolete (L.184 in Results is affected, too). Instead, the interesting question could be discussed why Panchev-Kesich shows the more similar spectral shape.

Thank you for pointing this out. The Panchev-Kesich spectrum is now introduced earlier in the Results section and is now included in both panels of Figure 5 (see attached figure below). In addition, we now produce Panchev-Kesich spectra following the same averaging as for the Nasmyth spectra. In the original version, the Panchev-Kesich spectrum was presented as a reference, plotted using the average dissipation value (hence the roll-off was not smeared out as it would be after averaging over several spectra covering one decade range of dissipation values). Now, the individual Panchev-Kesich spectra (from the individual epsilon estimates) are bin-averaged similar to the Nasmyth spectra and shear spectra. As you point out, the roll-off in our observations does fit the Panchev-Kesich spectra better than the Nasmyth spectra. Yet there is still a systematic offset at the roll-off, and we think this is worth some consideration, hence retain a modified version of the discussion in line 219-224. For wavenumbers between 1 and 10, the Nasmyth spectra fit our observations better for $\epsilon > 1e-7$. A discussion about similarities between our observations and the two spectra is now included.



(3)

- The expected uncertainty of resulting epsilon is not stated/estimated. The resulting effect of basic MR uncertainty plus vehicle noise plus post processing in sum will probably exceed the typical factor 2 for calm platforms.

Thanks for bringing this up. A recently study (Lueck, R.G., 2022: The statistics of turbulence

measurements. Part 2: Shear spectra and a new spectral model. J. Atmos. Oceanic Tech., submitted) estimates the 95% confidence interval around a mean spectra as the factor $e^{\pm 1.96 \times \frac{5}{4} (N_f - N_v)^{-\frac{7}{9}}}$ where N_f is the number of fft segments and N_v is the number of vibration signals. Hence, the 95% confidence interval around a mean spectrum in our data with $N_f = 15$ and $N_v = 2$ is about [0.72 1.40]. The 95% confidence interval carried over to our epsilon estimates, including an additional 10% sensor sensitivity calibration uncertainty, becomes about [0.61 1.63]. We now include this information in the discussion.

- The comparison between the three epsilon timeseries and the two single vertical MSS profiles (Fig. 7c) seems questionable. AUV epsilon is extremely variable from noise level to very high values of 5e-6. The two MSS profiles differ a factor of about 50, one is near AUV noise level, one is near 1e-6. Trying to compare statistically would mean that we'd have to check the hypothesis if the two MSS values can stem from the AUV epsilon distributions. This hypothesis will certainly not be rejected, but: the question is, which imaginable MSS profiles would be rejected at all? As the basic distribution spans more or less the entire possible range, nearly any MSS measurement would not contradict the zero hypothesis. Two noise level profiles would not; two profiles of 5e-6 would not; maybe two profiles of 1e-5 would have cast doubts. That means that the two MSS profiles probably cannot support the statement 'dissipation from AUV agrees well with MSS profiles' (L.8ff, L.206f, L.247f) in a valuable manner, even if they don't contradict either. I'd recommend to comment the comparison (L.8ff, L.206f, L.247f) more cautious. The MSS profiles might confirm the high variability of epsilon in the region, and they might constrain the uncertainty of the AUV measurements to a factor of 5 or 10.

Thank you for addressing this. We agree with you on all accounts. The only way the two MSS profiles could potentially contradict the zero hypothesis is if they were off by a factor of 50 or more on the low epsilon side (say both profiles estimated 1e-8), suggesting the region was not as turbulent as the MR would have it. We have rewritten this comparison, and now make sure to point out the shortcomings of such a comparison.

Minor comments:

L.27f: '... the traditional methods limit the spatial and temporal coverage of the measurements.' Better: '... limit the horizontal and temporal resolution ...', if this was meant.

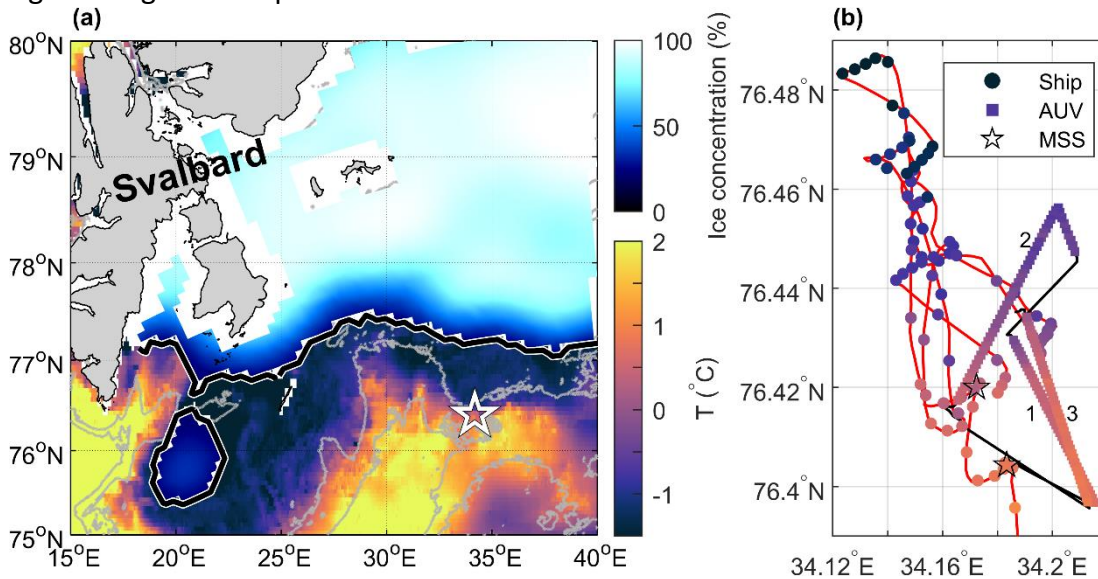
Revised as: "However, the vertical profiling limits the horizontal and temporal resolution of the measurements."

L.15-41: For better readability I'd propose to put L.23&24 to the previous paragraph, and to lump L25-31 into one paragraph. Thus there'd be 3 paragraphs: on turbulence measurement in general; on traditional and robotic platforms; on vibrations of robotic platforms.

We agree that it improves the readability and made the changes as you suggested.

Fig.1a needs more clarity. Isobaths may be negligible. The ice edge is unnecessarily hard to spot, a thicker black line with an overlying dashed white line might be a solution. The experiment location should not be directly on the edge of the plot; expanding to 40°E at least would better support orientation.

Agreed. Figure 1 is updated as shown below.



L.82-91: The entire paragraph about the particulars of reckoning the front location would fit a scientific paper on observing the frontal zone. For the purpose of this technical note, it could be distilled to a single sentence, saying that the AUV was programmed to follow the frontal zone and did this successfully.

Agreed. The entire section has been distilled and attached to the previous section.

L.92: replace 'MicroRider' by 'Turbulence package' (the term MicroRider is first introduced in L.93)

Agreed. We changed the title of section 2.2 to “Turbulence package”.

L.140: 'the shear probe signal ... was removed' sounds as if the raw shear timeseries was filtered before calculating the spectra

Indeed. We now write “the shear spectrum signal coherent with the accelerometer spectrum signal was removed...”

Equation (4) is a bit misleading, the second '=' is not correct. 'epsilon = left hand side of eq.4' is the exact equation, while 'epsilon estimated = right hand side of eq.4 plus correction for unresolved wavenumbers' is the practically used equation, trying to be as close to the exact equation as possible.

Thank you for pointing this out. We now changed our equation as shown below.

$$\epsilon = \frac{15}{2} \nu \overline{\left(\frac{\partial v}{\partial x}\right)^2} = \frac{15}{2} \nu \int_0^{\infty} \Psi(k) dk \approx \frac{15}{2} \nu \int_{k_1}^{k_u} \Psi(k) dk$$

In addition, we point out in the text that epsilon in the unresolved part of the spectra is corrected for by using the Nasmyth spectra, so that our final estimate is close to the full integration.

L.164: Is there a conceivable reason why the AUV should be 10% faster in 0°C water than in 1°C water? If true, what would that mean for missions in the tropics?

No, this is only because we set the AUV to use a constant RPM. The front is highly dynamic and it is not surprising if current changes direction across the front.

L.166: The main energy should be at 75Hz (the propeller having 3 blades), and indeed Fig.4 points to this. However, the system seems not to be perfectly symmetric, showing 25Hz and 50Hz as well.

Thank you for pointing to this. The propulsion system is not perfectly balanced around its rotational axis, and vibrations at 25, 50 and 75 Hz (and the harmonics of these frequencies) are induced by this off-center rotation. The main energy is indeed at 75 Hz, related to the 3-bladed propeller. We now include this information in our results.

L.169: 'by the propeller at 25Hz'. Better 'by the propulsion system at 25Hz' (see previous remark). Delete 'of the propeller frequency' for the same reason.

Agreed. We corrected as you suggested.

L.180 to 186: all remarks in parentheses should be deleted. This is information for the figure caption

Agreed.

L.190: what is the purpose of this sentence? The important differences between sensor 1 and 2 discussed in the following are systematic, while the fact that the single epsilon values are calculated from individual spectra only explains scatter.

We agree that this sentence is unnecessary. We rewrote the start of this paragraph.

L.197: 'The distribution is not log-normal'. I'd recommend to delete the sentence. Probably none of the distributions shown is log-normal; and the next sentence 'A second mode appears ...' already implies that Fig.6e is not log-normal.

Agreed. We deleted this sentence.

L.204: 'boundary layer': please state at some point in the paper (maybe the beginning of chapter 2) how deep the mixed layer is.

The mixed layer was about 60 m at the time and location of the mission. We now include a short description of the environmental conditions in the beginning of chapter 2.

L.204: 'maximum likelihood estimate': the arithmetic average should do. Or is this what you mean?

We used the maximum likelihood estimate but have now changed this to the arithmetic average as suggested.