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Comment on os-2021-107

Anonymous Referee #2

Referee comment on "Technical note: Turbulence measurements from a light autonomous underwater vehicle" by Eivind H. Kolås et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-107-RC2, 2022

"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.

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.

(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?
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?

(2)
There are some open technical questions that users probably would be interested in.

For section 2.1:
- What mission duration can I expect? (battery endurance? reliability? see leak after 5 hours)
- Is remote control during the mission needed/possible at all? How? At what range?
- Size/weight of the AUV (Fig.2 can only give a hint)?
- How is the handling and constraints for deployment and, particularly, recovery?
- Is there light AUV versions with higher pressure rating?
- Particulars about the MR mounting, possibly a drawing.
- Is there anything known about self-oscillation of the system (maybe from the MR vibration sensor while thruster off)?

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

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?
- (L.130) How is despiking done?
- (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?
- (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' ?
- (L.156) Is there a criterion why change rates of 10° roll/s, 5° pitch/s, and 2 RPM/s have the same consequence?

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?

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.
- 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.

(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.
- 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.

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.

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.

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.

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.

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

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

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.

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?

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.

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.

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

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.

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.

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

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