Ocean Science Discussions

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Comment on os-2021-71

Anonymous Referee #1

Referee comment on "Impact of acoustic Doppler current profiler (ADCP) motion on structure function estimates of turbulent kinetic energy dissipation rate" by Brian D. Scannell et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-71-RC1, 2021

General Comments

This manuscript examines the use, and potential bias, of the structure function method applied to high frequency (1 Hz) data collected with a moored ADCP for over a year. It should be noted that the paper is limited to assessing the impact of ADCP motion on the estimates of the dissipation rate, and makes no attempt to describe or explain the seasonal variability in the dissipation rate itself. The paper is very well written, comprehensive, and timely given that improvements in ADCP technology have enhanced the ability to measure velocity fluctuations with these instruments.

The manuscript presents two significant conclusions that are worthy of publication. First, the authors derive an equation for the bias introduced to the dissipation estimates due to the presence of background velocity shear. Second, the authors use synthetic data to demonstrate that the periodic motion of the ADCP adds only a small bias to the estimated dissipation rate (less than 10% for good mooring performance). Because the structure function method is based on velocity differences, the insensitivity to instrument motion is not surprising, but the manuscript helps to quantify the bias. These results will help to objectively quality control similar ADCP measurements. The authors also provide useful guidance on achieving better dissipation estimates from a moving ADCP.

I recommend this manuscript be published with some minor improvements. The overall suggestions I have are:

1. There should be better connections drawn between the observational data and the synthetic data. There is some nice discussion starting on line 398, but I think more conclusions can be drawn, specifically in how these results would help in data processing and quality control of these specific measurements.

- Can you quantify the approximate bias in the measurements from the results of the synthetic data and your measured shear and/or attitude variations? Are there measurements / intervals that you would reject in your turbulence analysis as a result?
- What is this bias relative to the estimated dissipation rates? How does this bias compare to the minimum measurable dissipation rate achievable due to the Doppler noise?
- Do your conclusions suggest that averaging the dissipation rates across all four beams minimizes the introduction of any bias?

2. Include a review and a discussion about the results in comparison to other studies that used moving ADCPs. For example, surface drifters (SWIFT buoys and others) have recently been used to estimate the dissipation rate in tidal channels (Guerra et al. https://doi.org/10.1016/j.renene.2019.02.052 0960-1481/ and https://doi.org/10.1016/j.renene.2021.05.133).

- These drifters commonly only use a central "vertical" beam to estimate the dissipation rate. Could your synthetic data analysis be extended to include the vertical beam? This would be an interesting application of your results.

3. I think the paper would benefit from having some of the observational results moved from the appendix to the main body (e.g. Fig A1). This is a personal preference to help the reader get a feel for the data and its variability. This helps to motivate the paper and visualize key variables that are used in the analysis. (Specific comments below)

Specific Comments

- Abstract: I suggest removing the sentence starting on line 3 about microstructure profilers. While these instruments are indeed the dominant method used to measure the dissipation rate, they are not used in this study so I do not think they need to be mentioned in the abstract. The introduction would be a better location to summarize the benefit of ADCPs over other technology (microstructure profilers, ADVs) for mooring deployments. Microstructure profilers require a background flow and consume a lot of power, whereas ADVs require significant correction for motion (compared to ADCPs).
- Line 38: Can you quantify the smallest dissipation rate that can be measured with an ADCP?
- Line 90: "a function of the noise of the velocity of observations" is vague. Why not specify two times the Doppler noise variance like in Wiles et al (2006).
- Line 92: "homogeneous" or "isotropic"?
- It would be useful to include a schematic of the mooring line in Section 3. This should

show the relative positions and orientations of the ADCPs with respect to each other and the total water depth. It would also highlight that the middle instrument was in an open frame unlike the spherical buoys used for the top and bottom instruments. It is not critical to the results but helps visualize the configuration.

- Line 110: It would be useful to explicitly give the equation for ε^b as a function of S and r_n.
- Line 114: "When using…" This sentence is confusing. The illustration and quantification in the paragraph that follows is helpful, but I think it would be beneficial to include a plot of the dependency of ε^b on S, r_n and r_{max}. This is a key result of the manuscript that quantifies the bias as a function of shear and instrument configuration. Perhaps a figure of ε^b versus S for the three combinations of bin size and r_{max} (i.e. 0.1 m bins with r_{max}=0.92, 0.1 m bins with r_{max}=1.92, 0.2 m bins with r_{max}=1.92) would be sufficient.
- Section 3.1: I propose including Figure A.1 in this section to give a sample of the measured speeds and instrument attitude. This helps to visualize the variability within the 5-minute bursts and motivates the definition of the parameters used in sections 3.2 and 3.3. It may be useful to also include the dissipation rates in this sample time series (or elsewhere in the paper).
- Figures 1 and 2: I find these plots a little hard to interpret, but the text does a good job of summarizing the major conclusions. I wonder if simpler plots of the coloured parameters (e.g. oscillation range, oscillations per burst) versus U and H_{m0} would be more intuitive and better show the trends.
- Section 4: Figures 3-6 are well described, but some additional references to line types and colours would help guide the reader. My suggestions are in italics

  - Line 295: "to increase with $\Delta\Phi_H$", *as indicated by the varying line types*
  - Line 310: "beam 3" *(yellow)*
  - Line 313: "for all bins in both beams" *(red line overlying blue line)*
  - Line 353: "range of the beam average values" *(light gray shading)*
  - Line 365: "maximum of 0.09" *(black line)*
- Figures 4 and 6: Is it possible to show the range of values from the observations in these plots of the synthetic data? (e.g. the 50 or 90% $\Delta\Phi_H$ values presented in table A2 could be plotted as vertical bars in Fig 4b and Fig 4c for each of the ADCPs). This might help show that there is expected to be only small biases in the estimates of the dissipation rate from the measurements.
- Line 382: "separation distance" *(eq. 8)* [i.e. add referemce to equation]

- Line 413: can you clarify what you mean by "the linear length scale dependence of the velocity difference between bins"? Are you suggesting that the method used to eliminate waves (eq 6) also handles shear?

Technical Corrections

This paper is extremely well written with very few technical errors. The couple I noticed include:

- line 53: “us” should be “is”
- line 84: “anticipate” should be “anticipates”