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Ocean Science Discussions

Interactive comment on "Internal tide energy flux over a ridge measured by a co-located ocean glider and moored ADCP" *by* Rob Hall et al.

Anonymous Referee #1

Received and published: 28 March 2019

This paper explores the errors in calculating the internal tidal energy flux using a combination of a moored ADCP and an ocean glider operating in a 'virtual mooring' mode. Observations were collected on the Wyville Thompson Ridge and the semi-diurnal tidal energy flux calculated together with the available potential energy (APE) and the horizontal kinetic energy (HKE). The results compare favorably to the modelled internal tide energy flux, ellipse diagnostics, APE and HKE. The unique part of this manuscript is the estimation of bias and error introduced by temporal and spatial aliasing – a consequence of the glider not being co-located with the ADCP and sampling within a certain sized watch circle. The authors use an idealized internal tidal field and a monte-carlo sampling approach to quantify the increasingly low bias in energy flux associated with increasing the size of the gliders watch circle (taking into consideration the internal tidal

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wavelength). They show that their flux calculations from the Wyville Thompson Ridge have an acceptably low bias and error and that for most-mid latitude slope locations a <10% error should be achievable.

General comments: Given the rapidly increasing use of marine gliders I think that this is a very topical issue and will be an interesting read for large parts of the oceanography community looking to use gliders. I find the paper to be well written, suitably concise and logically structured. The figures are good quality. I would support its publication.

Specific comments: I have one main question to the authors. In section 4.1 the observations are compared to a regional configuration of the Princeton Ocean Model. This all looks very good. I was then expecting the 'glider sampling error' to be quantified using this regional model. Instead an idealized internal tide field is used. Whilst I do not dispute that using an idealized model is an important thing to do and produces metrics that are most generally representative of other areas, I wonder whether a similar analysis should have been done using the regional POM configuration? Otherwise, I'm not sure what section 4.1 really adds to the paper?

Your idealized setup has a flat bottom, spatially uniform stratification and does not contain any other dynamics (e.g. waves passing through that have been remotely generated, waves of other frequencies). The regional POM contains more of these real world variables and I can see value in attempting a similar analysis using its output, perhaps targeting regions of weak/strong energy flux, steep slope vs deep channel, spring v.s neap. The resulting error/bias analysis would be more unique to the region, but would give a more realistic error bound on the ADCP-glider flux calculation. Have the authors considered doing this here?

Minor comments: Page 18. I feel that the description of the idealized internal tidal field should be moved into the main body of the text and not relegated to an Appendix.

Page 18, line 16. Do you really mean 'uniform stratification (N2=constant)'? Figure 7 suggests that this is not the case.

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