Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2019-15-AC2, 2019

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.

Rob Hall et al.

robert.hall@uea.ac.uk

Received and published: 21 May 2019

Response to Reviewer 2

Specific points:

1) The only thing that I would recommend is using the existing model to assess some spatial decorrelation scale for internal tide amplitudes and energy at the mooring site. The authors describe that there may be considerable variability in space and they have a tool to calculate it. As is though, the manuscript is fine. Whether or not to do this, I leave to the authors.

If we understand correctly, the suggestion is equivalent to asking: How close to the mooring site does the glider's target location have to be to ensure that measured den-

sity and measured velocity are correlated? This is indeed an good question and of interest when planning glider missions. However, in our experience, if environmental conditions allow a glider to hold station then it will be able to successfully center its dive cluster over the desired target location. It is therefore unlikely that a glider would achieve a tight dive cluster, but spatially separated from the mooring. This question will be considered in a future study when a longer co-located timeseries is available.

Minor points:

1) 1/16th wavelength diameter watch circle should be emphasized in figures and text because that appears to be relevant to these observations. 80 km tidal wavelength. 5 km watch circle.

An additional Monte Carlo case has been included ($d/\lambda = 0.05$) that is a close match to estimates for the WTR. This new case is used to estimate the glider sampling error for the observations, instead of interpolating between the $d/\lambda = 0.03125$ and $d/\lambda = 0.0625$. This new case is highlighted in red in Figure 8 and in bold in Table 2.

2) Emphasize earlier on about the inability to separate S2 and M2. Maybe you want to call it D2. The discussion of spring-neap in the summary is good. Maybe this could go in the intro. 40 h analysis window limits this further than the 3 day observation period implies.

We now use the term D2 where appropriate, and have included a new paragraph in Section 2 to explain D2 the M2-S2 separation issue, 'As the glider was on-station for only 40 hours, the co-located timeseries is not long enough to resolve the internal spring-neap cycle. As a result, M2 harmonic fits to the glider and mooring data (Section 3) are contaminated with S2 variability. To acknowledge this, we refer to the estimated M2 component of the co-located timeseries as D2 following Alford et al. (2011). The comparative numerical model (Section 4.1) only includes the M2 tidal constituent so we refer to model diagnostics as M2.'.

3) P4 L10. As you note the profile ends count as 1 observation. Only at the mid point are you doing better by a factor of 2.

To clarify this point, the text has been changed to 'This yielded approximately 12 profiles (six independent samples near the surface and seabed; 12 independent samples at mid-depth) per semidiurnal tidal cycle.'.

4) P4. Please explain up- and downsampling. Linear interpolation?

Yes, linear interpolation is used for up- and downsampling. To specify this method, the text has been changed to 'linearly upsampled' and 'linearly downsampled'.

5) P7. Bottom friction plays an unclear role (for me at least) in the separation of barotropic and baroclinic. Friction applies to the total velocity and lowers the depth mean. Calculate rms difference for various depth mean flows.

Yes, we agree that the frictional bottom boundary layer effects depth-mean velocity and so potentially causes a deviation from barotropic velocity. However, as the mooring did not have a near-bed downward-looking ADCP we cannot measure the near-bed velocity gradient. Data from the acoustic current meter (approximately 16 m above the bottom) do not show any substantial decrease in current velocity relative to the lowest bin of the ADCP (approximately 100 m above the bottom). We expect any error to be small compared to error from the linear interpolation and nearest neighbor extrapolation required to obtain full-depth velocity profiles. We state that 'barotropic velocity [is] assumed here to equal the depth-mean velocity' to acknowledge this. We have also calculated the r.m.s. difference between the D2 component of mooring depth-mean current velocity and the D2 component of glider dive-average current (DAC) velocity. This is 1.2 cm s-1 and 0.8 cm s-1 in the along-slope and across-slope direction, respectively. These r.m.s. difference values have been included in Section 4.2, with maximum current velocity values for comparison.

6) Fig 3. Which mode has a phase shift near 550 m?

Baroclinic modes and mode eigenspeeds are calculated from the observed buoyancy frequency profile and used to estimate the M2 mode-1 horizontal wavelength over the WTR. This analysis shows that mode-1 horizontal velocity reverses at 505 m. A sentence has been added to the text, 'Mode-1 horizontal velocity, calculated from the observed buoyancy frequency profile, reverses at approximately 505 m, slightly above the pycnocline.'.

7) Fig 4b should be solid lines according to caption- not interpolated.

The phase profiles in Figure 4b have been changed to solid lines.

8) Fig 4c should have only a portion dashed- should be mostly solid.

This confusion stems from the fact that solid/dashed line differentiation in the caption only applies to Figure 4d. To avoid this confusion, N2 has been changed to a green line in Figure 4c.

9) Table 1. For angles you could use bearings.

Done.

10) Section 4.1 has pretty good model-data comparison. Are you doing same or better than other models that you have referenced? Also on P17.

All the studies of the FSC/WTR region that we reference used observational datasets [with the exception of Hall et al. (2011), which also has a numerical modelling component]. There have been other regional model studies, but these have tended to focus on mesoscale eddies (e.g., Oey, 1998) or the FBC and WTR overflows (e.g., Riemenschneider and Legg, 2007; Stashchuk et al., 2010). The internal tide studies by Gerkema (2002) and Hall et al. (2013) used simplified 2-D (x,z) numerical models and were both motivated by observations further northeast in the wider section of the FSC.

11) Fig 5a green flux vector is hard to see.

The flux vector has been changed to a darker shade of green to distinguish it from the yellow contour shading beneath.

12) Fig 5. How about energy density from model compared to your observations?

APE and HKE values for the observations and model are stated in Table 1 and compared in Section 4.1.

13) Fig 5. How was the magenta representative box decided?

The dotted magenta box in Figure 5d encompasses all of the glider dives that were used to calculate the DAC tidal ellipse (deeper than 500 m). The figure caption has been changed to 'The magenta ellipse is calculated from DAC velocity and is representative of the area that contains all the dives deeper than 500 m (delineated by the dotted magenta line).'.

14) P12 L25. Also gets you faster dives.

This is typically true, but not always. It is possible to operate a Seaglider with a steep glide angle but low thrust. As Section 5 is on spatial sampling error, we feel that including a comment on temporal sampling may confuse the reader.

15) Fig 8. Use colour to highlight most relevant choice for these observations [in] Table 2 will be handy.

An additional Monte Carlo case has been included (d/λ = 0.05) that is a close match to estimates for the WTR. This new case is used to estimate the glider sampling error for the observations, instead of interpolating between the d/λ = 0.03125 and d/λ = 0.0625. This new case is highlighted in red in Figure 8 and in bold in Table 2.

16) P18 L28. Explain the choice behind modal amplitude decay.

The velocity amplitude decay rate, $u_n = u_1 e^{-0.5(n-1)}$, where n is mode number, results in an internal tide beam if velocity phase is equal for each baroclinic mode. The appendix text has been changed to 'Velocity amplitude decays with mode number, un

$= u_1 e^{-0.5(n-1)}$, where u1 is the mode-1 velocity amplitude. This decay rate results in a well-defined internal tide beam if velocity phase is approximately equal for each baroclinic mode. However, a different random set of baroclinic mode phases (ÏŢn) is used for each scenario simulated so internal tide beams are only apparent in a subset of scenarios. u_1 = 0.28 m s^{-1} yields a mode-1 vertical isopycnal displacement amplitude of 50 m, but energy flux error and APE error are not sensitive to absolute amplitude.'

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2019-15, 2019.