Response to reviewer 2

12th February 2018

The authors thank the reviewer for their careful reading of our discussion paper, and for their helpful and constructive comments regarding its content and improvement. The text of the review is reproduced below in black type; our comments are in blue; and changes to the original discussion paper are presented in italics.

General comments

This is a largely methodological paper that uses Seaglider derived depth averaged currents to calculate tidal velocities and amplitudes in a shelf sea environment. These tidal currents are compared to modelled tides and output from current meters before being used to define tidal mixing fronts. A combination of a simple model and hydrographic observations has been used to explain the positions of the fronts.

I think there is value in the methods presented within this work and that it has been done well. I do however have some concerns about how well some of the later analysis supports the conclusions and find that the general story gets lost because of this.

Introduction

Is the mixing front visible in satellite data? If so it would be interesting to add SST or a front map to figure 1 to highlight the co-location.

The mixing front is not really visible in satellite images taken during the deployment, and glider temperature observations record only a slight temperature decrease with distance offshore. In some years, particularly in the summer months, the front has a modest surface expression (Sheehan et al., 2017) but that does not appear to be the case at the time of our glider observations.

Method 2.1

I would be interested to see if the glider altimeter depths compare well to the GEBCO bathymetry. Given that the depth is so important to your analysis then glider depth might be more accurate.

We take bathymetry from the GEBCO database because we trust it more than bathymetry as determined by the glider, which could be inaccurate. The altimeter is a rudimentary instrument used primarily as a piloting tool; it does not continuously record the glider's height above the seafloor and it does not accurately detect the bottom on each dive. What is more, the response of the altimeter can differ with the composition of the sea floor (sand, sediment, rock etc.).

Page 5, line 8 *All bathymetry data used in this study were extracted from the GEBCO dataset (GEBCO_08 grid, version 20100927, www.gebco.net; resolution 30 arc-seconds). While it is possible to estimate bathymetry from the glider's altimeter* *observations, we believe that bathymetry from a databank for a well-studied region such as the North Sea is likely more accurate.*

How have you combined the M_2 and S_2 tides? You often use M2-S2 as a concatenation and this suggests that you subtracted S2 from M2. You also need to be consistent in you notation for this combination throughout the text.

The zonal components of the M_2 and S_2 tides have been added together, as have the meridional components of the M_2 and S_2 tides. The abbreviation has been changed to M_2+S_2 throughout in order to make this clear and the abbreviation has been defined.

Page 5, line 25 *Combined M_2 and S_2 (hereafter denoted M_2+S_2) zonal and meridional velocities were then calculated at the time of each glider dive.*

You have interpolated the velocities to give "along-track" glider velocities and then reference Figure 4 which shows "meridional" and "zonal" velocities. Have you assumed that the along track direction is zonal? This needs to be made clear as the glider tracks are rarely completely zonal.

The along-track velocity time series comprises estimates of tidal velocity at the time and location of each glider dive: it is the location of the data points in time and space that we describe as along-track (i.e. following the glider's track), not the velocities themselves. The text has been amended to make this clearer.

Page 5, line 26 *Tidal velocity was linearly interpolated zonally onto that dive's location to construct a time series of tidal velocity along the glider's overground track – that is, a time series of tidal velocities with data points at the time and location of each dive. These are hereafter referred to as along-track velocities.*

How were estimates extracted from the TPXO model? I assume the TPXO model has some interpolation or smoothing and as such the points within it aren't entirely independent. In contrast the glider DACs are independent from each other. Have you accounted for this difference between models and observations?

The TPXO European shelf model has a 1/30° grid of amplitude and phase estimates for 11 tidal constituents. Velocities are extracted from the TPXO model using the Earth and Space Research Tidal Model Driver software for Matlab (esr.org/research/polar-tide-models/tmd-software), which interpolates between TPXO grid points to make predictions. The reviewer is correct that the DACs are independent. We do not apply smoothing or filtering prior to the harmonic analysis.

Page 6, line 18 *To compare the results of our method with an established alternative, estimates of M_2 and S_2 amplitude, phase and velocity, were extracted from the TPXO inverse model European shelf solution (0.1° resolution; Egbert et al., 1994; Egbert and Erofeeva, 2002; Egbert et al., 2010) using the Tidal Model Driver software for Matlab (available at esr.org/research/polar-tide-models/tmd-software).*

Method 2.2

You explain the statistics and process of binning the data after you have introduced the binned data. However I think you do need to maintain the discussion of the accuracy at the end of this section and so some restructuring is required.

The final paragraph of section 2.2 has been moved into section 2.1 so that discussion of the accuracy of the ellipses and the number of dives needed in each bin comes directly after the method has been explained. All discussion of the method and related considerations (number of dives, number of bins etc.) is now in the same section, and section 2.2 is just a discussion of the ellipses as presented in Figure 3.

Frontal location

The structure of this section is very abrupt, with little initial introduction. Paragraph 2 is difficult to follow and again would benefit from restructuring. The information is all there but the story gets lost.

Section 3 has been given a longer introduction that we hope better introduces the work on frontal location. Paragraph two of this section in the discussion paper has been restructured and amended to improve the writing.

Page 8, line 9 *We apply the glider-derived tide presented in the previous section to study the location of a front in the north western North Sea. Specifically, we investigate the extent to which the location of the front may be explained by heating-stirring interactions, a principal component of which is tidal speed. Furthermore, this analysis serves as an illustration of a potential application of the method (section 2).*

Why have you focussed a large part of this section on work by Simpson and Sharples, which you say is not appropriate for this region? It would be nice to see more discussion of Hughes which appears to be more suitable to this work.

The theory of frontal location stems in no small part from the work of Simpson and Sharples (and co-authors), hence the prominence of their work in our discussion. We believe that this work is of great relevance to our study, not least because we are seeking to understand similar phenomena. Hughes (2014) refines the conclusions of Simpson, Sharples and co-authors – principally by providing a value of h/u^3 applicable to our study region and attributing this updated value to local surface heat fluxes – but does not change the fundamental concepts on which we rely. We therefore believe that the papers of Simpson, Sharples and co-authors, as updated by Hughes (2014), are important references for our study and receive appropriate attention. As suggested by the reviewer, more discussion of Hughes (2014) has been added.

Page 9, line 6 *Hughes (2014) using a heat flux appropriate to the northwestern North Sea, concluded from a modelling study that the critical value for frontal location in the region should be 3.4; the applicability of this value was confirmed by examination of 28 years (1982 – 2008 inclusive) of satellite observations of sea-surface temperature (Hughes, 2014). The higher critical value is attributed to the reduced heat flux and enhanced wind mixing at the latitudes of the northwestern North Sea compared with the latitudes of the Celtic Sea (Hughes, 2014) the site of much previous work* *on the h/u^3 criterion (e.g. Simpson and Hunter, 1974).*

In the final paragraph you say that there "does not appear to be adjustment" and then explain the effects of the adjustment, is there adjustment?

"Are" (discussion paper: page seven, line 11) has been changed to "would be" to make clear that we are presenting a potential reason for the lack of adjustment, rather than seeming to imply that there is adjustment.

Page 9, line 19 *There does not appear to be adjustment of frontal location with the spring-neap cycle, although the effects of such adjustment would be much greater immediately after frontal development.*

Comparison with model output

Again the story gets a little lost due to the structure here. I think a short section linking the observation of the front to this analysis of it. Say upfront what it is you're trying to get out of the comparisons.

Why was this model chosen?

We address the above two comments together. The opening of section 3.1 has been expanded to both better motivate our use of the model and to explain why we have chosen to use the Simpson and Bowers (1984) heating-stirring model

Page 9, line 30 *We compare the observations of frontal location with the output of a numerical model of heating-stirring processes to identify which factors control frontal location during the period of the glider deployment. We use the open-source, one-dimensional heating-stirring model of Simpson and Bowers (1984, see also Elliott and Clarke, 1991, and Simpson and Sharples, 2012). The model is straight-forward to run; it's may be readily adapted to suit the study region and to work with the glider-derived tide described in section 2; and it includes only the physical heating-stirring processes used to describe frontal location by Simpson and Hunter (1974) and Simpson and Bowers (1984), and described in section 1. Consequently, the model allows us to investigate the extent to which heating-stirring interactions influence the location of the observed front (Fig. 5).*

Why has the 1st of November been selected as a representative date?

The explanation given in the discussion paper about how the velocities used in the heating-stirring model were chosen was not clear. This has been improved. 1st November was used because it falls midway between neap and spring tides.

Page 10, line 18 *We take as the tidal speed the meridional M_2+S_2 velocity amplitude midway between spring and neap tides (1st November), and use the glider-derived tide to capture the offshore decay in tidal amplitude.*

Comparison with observations

When you discuss the salinity gradient I think it would be interesting to show this, for example by a plot of the salinity gradient along a representative isobath, or something similar. This section is very interesting and a good argument for why the model doesn't hold up in October but I'm not sure it is sufficient to support your conclusion of a hybrid front. This is a qualitative assessment of the glider, which I would expect to be more thorough in order to be a full discussion item and to have such an influence on your discussion.

Based on analysis of the glider data carried out since the discussion paper was submitted, and in response to the reviewer's perceptive comment, we have re-written section 3.2 to shift the focus away from salinity – which is a little over-simplistic – and towards the hybrid front being formed between multiple water masses. The introduction has been modified accordingly. This does not change the conclusions of the study, but it does better reflect a situation in which the salinity distribution is more nuanced than our discussion paper implied.

Page 11, line 22 *The glider observations presented in Figs. 5 and 7 demonstrate that a front can exist in the northwestern North Sea independently of heating-stirring interactions. Such a front is clearly not simply a tidal mixing front. However, during the first part of the deployment, the observed frontal location compares well to frontal location as predicted from consideration of only heating-stirring interaction. We propose that the observed front is a hybrid between a tidal mixing front and a front which forms due to horizontal gradients between adjacent water masses in a region of complex water mass interaction.*

Do you have data to show that October (your early glider transects) is representative of summer as this section implies?

We emphasise that the observations presented in Figure 5a cover the period when the front makes the transition from its proposed summer state to its proposed winter state. We compare summertime frontal position in the model (when forced with a multi-decade average annual cycle) with summertime frontal position in the multi-decade average observations of Sheehan et al. (2017).

Page 11, line 28 *In summer, we propose that local heating-stirring interactions modify the water masses to the extent that the front is moved to a position as predicted by consideration of heating-stirring interactions alone. The present study does not include observations of the front at this time, but when forced with an annual cycle of multi-decadal mean meteorological values, the heating-stirring model places a tidal mixing front at approximately 1.5° W in summer; the front is found at the same location in multi-decadal summertime averages of JONSIS section hydrography (Sheehan et al., 2017). The observations presented in this study (Fig. 5a) capture the period in the annual cycle when, in 2013, the front makes the transition from being a front controlled by heating-stirring interactions to being a* *front controlled by the distribution of water masses.*

Figures

Figure 2a and 3 should be the same geographical area, the one highlighted in Figure 1.

The area shown in Figure 2a has been changed – as has that highlighted in Figure 1 – to match the area shown in Figure 3.

Why has the shaded area/zoomed in Figure 4 been selected? It appears to be when the fit is best, maybe a central point or even a period when the fit is worst would be more appropriate.

The zoomed area in Figure 4 had been selected at random. It has been changed to cover a period where the agreement between the two time series is not so good (12:00, 30th October – 12:00, 2nd November)

Technical corrections

You have used a mixture of Fig. and Figure throughout, be consistent.

"Figure #" has been changed to "Fig. #" throughout.

2.2 line 10: "Ellipses from both" – you have 3 data sources here not 2

3 line 19: "critical contour" – should it be "critical value"?

Page 9, line 9 *The higher critical value is attributed to the reduced heat flux and enhanced wind mixing*

3 line 32: When was the final occupation of the section?

Page 9, line 25 *It then widens considerably towards the end of the deployment, being spread between 1.4 and 0.8 °W at the time of the final occupation of the section (19th November – 1st December)*

3.1 line 38: glider has been spelt glided