Response to reviewer 3

12th February 2018

The authors thank David Bowers for his careful reading of our discussion paper, and for his 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.

This paper is about using a glider to study the position and movement of a front at the north-west entrance to the North Sea. The front is a boundary between mixed and stratified water and seems to have a mixture of causes: tides are important in creating the mixed water; the stratification is produced by a combination of surface heating, freshwater input and currents from the Atlantic.

The authors will likely disagree with me, but it seems to me that gliders are a solution still looking for a meaty problem to get their fins into, at least in shelf sea oceanography. Fronts could be just what they are looking for. Fronts are not always straightforward to find and so moorings, if misplaced by a few kilometres, might miss them altogether. Ships, for reasons of cost, are limited and satellites can only see the surface. Programming a glider to make repeated transits of a front (with a generous allowance for frontal movement), as has been done in this work, can lead to useful new knowledge.

We agree with the point made by the reviewer and we have incorporated it into the text.

Page 3, line 14	*The front is bottom-intensified and has only a limited signature at the surface (Hughes, 2014; Sheehan et al., 2017). Consequently, the front may be more readily observed from a profiling glider than from satellite observations of, for instance, sea-surface temperature.*

It's some years since I've worried about these problems but the observations presented here, are among the best set of observations of the autumn retreat of a front that I have seen for a while. There are a few things I would invite the authors to comment on. One of the most important things which moves a front in a shelf sea is the tide itself. The front will be in a different place at low water slack, say, than high water slack. The difference can be a dozen kilometres or so. I don't think the authors have corrected their observations to allow for this. Is that right? If so, it's not a big issue: it will introduce noise into their observations rather than bias, but it would be interesting to know how easy it would be to do this with glider measurements.

We have not attempted to correct our observations of frontal location for tidal displacement. To estimate the uncertainty this adds to our observations, we calculate mean zonal tidal displacement: we integrate zonal velocity over half a tidal cycle. We take as the velocity amplitude the mean absolute zonal velocity amplitude over the deployment.

The correction could be performed from our data. It would first be necessary to estimate zonal M_2+S_2 displacement at the time and location of the observation; this would be a function of the phase of the spring-neap cycle and position on the section. Secondly, one would need to chose a phase of the M_2 tide as a baseline phase – perhaps 0 or π radians. By comparing the phase of the zonal M_2 tide at the time and location of the observation to the baseline phase, one could then calculate what proportion of the total zonal M_2+S_2 displacement to apply as a correction. We have decided not to apply this correction to our data. It would require detailed explanation and illustration that would likely distract from the message of the second part of our discussion paper.

Page 8, line 17	*Observations of frontal location are not corrected for zonal tidal advection of the front. Instead, we acknowledge a zonal uncertainty in frontal position of ± 2 km (0.04° longitude), that being the mean zonal tidal displacement during the deployment.*

The appropriateness of the h/u3 criterion for a front which may have other causes than heating and stirring has been commented on by another reviewer and I won't dwell on that. The results shown in figure 5a are impressive, I think. It's a very nice set of observations of the autumn retreat of the front compared to a simple theoretical prediction. One thing I don't understand about this figure is why there are several yellow spots on each crossing. Where there several fronts?

There are several yellow dots for many crossings of the front (as opposed to just one frontal position being produced by the heating-stirring model) because the real world is a little noisier than the model. Specifically, the front frequently extends over a wider zonal distance that the distance between glider dives, which were only some 300 m apart; therefore the glider observes multiple locations at which the top-bottom temperature difference equals 0.5°C. This is one reason why, when calculating the rate of offshore frontal movement, we use a line of best fit instead of joining up the dots, as could be done with the model output. (The other reason is to calculate an average rate over the entire deployment.)

Page 8, line 16	*On a number of crossings of the front, the top-bottom temperature difference equals 0.5°C at a number of points (Fig. 5a). This is because the front often covers a zonal distance wider than that between glider dives.*

Of course, in the autumn, heating is no longer important: the tide and wind together are eating away at the buoyancy stored over the summer. The cooling of the surface in the autumn is helping and there may be an influence from the Atlantic. The authors might like to construct their own model with these processes in (not now, but for a future paper) and see if this fits the observations better?

This is as much a methods paper as anything and I have a couple of questions about that. First, the authors have used u from the glider and h from a data bank to test front position as measured by surface-to-bottom temperature difference, also measured by the glider. Why those choices of data sources, I wonder. Could everything be determined from the glider? Does it know how deep the water is that it is gliding through? Or would it be better to use current velocities from a model? We all do this – select data from wherever we think is best, but maybe in this case some justification of the choice would be good.

It is possible to estimate the depth of the water column from glider observations. The glider carries an altimeter with which it measures distance to the bottom. Adding this to the depth measured by the glider's pressure sensor at the time of the altimeter observation gives the depth of the sea floor. 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.).

We use tidal velocities from the glider rather than a model in order to demonstrate a potential application of our method and because the comparison of the glider- and TPXO-derived tides proves that the two data sources are of comparable accuracy.

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.*

Finally, I'm a little surprised that the water velocity is so close to the glider velocity that the glider velocity can be used to give the depth-averaged current. Does the glider not move relative to the water to glide through it?

Similarity between the dive-average current and the glider's velocity should not influence the accuracy of a dive-average current observation. While underwater, the glider cannot communicate with a GPS satellite and so can estimate its position only by dead reckoning. On surfacing, the glider is able to compare its position as estimated by dead reckoning with its actual position as determined by GPS. The difference, along with the duration of the dive, is used to calculate the dive-average current. The accuracy of these observations is improved post-deployment by optimising a hydrodynamic model of the glider's flight path.

Page 5, line 12	*DAC observations are obtained incidentally during a glider's flight as the glider is advected by the flow over the duration of a dive-climb cycle. On surfacing, the glider compares its actual, GPS-determined position with its position as estimated by dead-reckoning, the difference being attributed to advection by the DAC. The accuracy of DAC observations was improved post-deployment by optimising the hydrodynamic model of the glider's flight (Frajka-Williams et al., 2011)*

I think this is an interesting paper using new methods to tackle an old problem. Thank you for letting me read it.