Response to reviewer 4

12th February 2018

The authors thank Emma Heslop for her careful reading of our discussion paper, and for her 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 assessing the methods and application of using a glider data from a single transect occupation, spanning several months, to determine tidal velocities, frontal position and the controls on this position. The paper was divided into 2 parts; assessment of the method and then use of the data. The method appears more leading edge than the application of the data to the problem.

This is a novel use of glider data to determine tidal velocities and the location of a front for analysis of drivers; this is an application worth highlighting for the reasons given and demonstrates that gliders are able to cost effectively provide data on ocean variability at sub seasonal scales. This is important to advancing our understanding of the interplay, at different scales, of drivers of ocean variability and improving model representation. Overall I think this work has value in highlighting this method and application for gliders. I have provided a couple of comments and a couple of minor edit notes.

1. The glider accuracy of the glider estimated DAV is dependent on the gliders internal compass and flight model used for the calculation, many authors including those cited in the paper recommend a procedure for correcting or calibrating the glider compass (Merckelbach, Smeed & Griffiths, 2010, Todd et al. 2011). For example 'swinging' the glider in a cradle/table, which produces a compass correction curve, similar to those traditionally produced for ship compasses (e.g. used with Spray/Slocum), or in situ spiral calibration flight (e.g. used with Seaglider). The article mentions visually inspecting the data, however it is worth expanding on this point. Although the comparison of the results is compelling and suggests the data is not overly affected by compass error, it is presumably one of the sources of error.

No compass calibration dive was recovered from the JONSIS line mission. In addition to the original visual inspection, we repeated the analysis using DACs from only east- and westbound occupations. Tidal ellipses from each sample show no systematic offset; we include the comparison plot below (Figure 1).

Page 5, line 16	*DAC observations were visually inspected to ensure that there were no systematic errors due to the glider's compass calibration, and the method described in this section was repeated using DAC observations from only east- and westbound occupations. No systematic difference between results obtained from the two samples was found, indicating that the observations are not affected by compass error.*

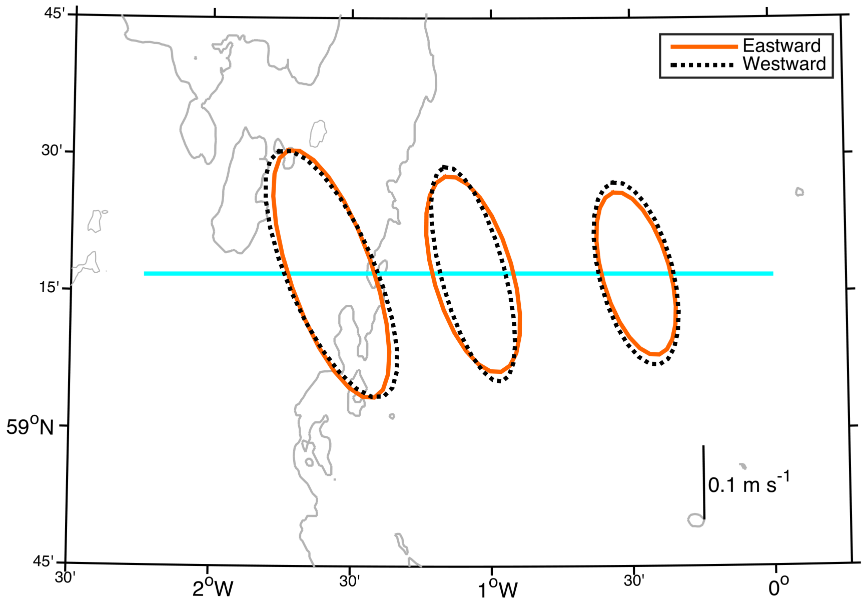

Figure 1 Tidal ellipses from only east- (orange, solid) and westbound (black, dashed) occupations

2. A significant part of the paper is about the novel method and potential benefits to other areas. Could it be worth summarising recommendations for future projects?

We thank the reviewer for an excellent suggestion, and we have added in recommendations for future projects.

Page 8, line 1

1. *Obtain repeat occupations of the same transect.*
2. *Set the transect length so as to avoid aliasing the spring-neap cycle – i.e. avoid individual occupations lasting around one or two weeks.*
3. *Optimise the hydrodynamic model of the glider's flight (Frajka-Williams et al., 2011) to obtain accurate DAC observations.*
4. *Do not attempt to resolve more constituents than may be accurately resolved given the length of each binned, discontinuous time series.*

3. I am not a tidal expert, however the model used seemed potentially old and so the utility/reason for selecting this model could be better explained. If it is to only to indicate when heating is dominant, is the variability that we see between glider and model in the earlier part of the study a result of mixed dominance/drivers in this period?

The reviewer is correct that we use this simple model to identify when heating-stirring interactions are the dominant control on frontal location. We have improved our justification of model choice in the revised manuscript.

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 better 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).

Minor edit notes

Comparison with model output: 4th paragraph, line 6.

"Tidal stirring becomes ever more dominant" – change every to ever

Comparison with model output: 4th paragraph, line 8.

"(main front ±1.59 km day-1 ±0.08 km day-1; excludes the secondary front…)" – some suggested re-wording, as it took a couple of minutes to work out what this meant.

Page 11, line 2 *(1.59 ± 0.08 km day^{-1}; rate excludes the secondary front that emerges on 15th November 2013 around 0.1° W*