Response to reviewer 1

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.

The paper presents some new glider observations from which (i) tidal currents have been estimated and (ii) the position of tidal mixing front as been established.

My main problem with the paper is that as it is currently written it is poorly focused although the title implies that the paper is methodological but at times it appears to be making claims as to deepening understanding re- shelf sea fronts, which I am not convinced it does.
We have clarified in the introduction that the paper consists of two parts – method and demonstration – and that we consider the method to be a key result of the work.

Page 3, line 34	*The method for calculating tidal velocities from DAC observations, outlined in section 2, is a key result of the work with potential applications beyond that presented in section 3.*

More so I believe as presented, the paper actually obscures at potentially novel scientific contribution.

Much of the analysis is based on the (now rather old) H/U3 theory for the positioning of shelf sea fronts and appears to push the theory further than it was ever intended. Firstly the theory was derived to explain the position of shelf sea fronts in terms of the balance between the stratifying influence of buoyancy input as surface heating. Yet the measurements presented were taken in the autumn, when the net buoyancy is negative and so contributes to mixing and not stratification. As such I would argue that the application of the model in the paper is not correct.
The h/u^3 theory, as the reviewer points out, was developed for fronts in summer and is a better predictor of frontal location at that time. We use it in this study primarily because, as RC3 acknowledges, we are using new methods (i.e. gliders) to revisit established ideas. While h/u^3 may not be as effective a predictor of frontal location in autumn as in winter, it is an important part of how we have come to understand fronts; we believe that it deserves to remain in the paper, even if it serves to confirm expectations that it is most suitable in summer.

There are also problems with the application of the model:

1) Use of the ancient air-sea heat flux parameterisations (eg. Ivanoff, 1977) – there are much more up to date parameterisations available. This is particularly important as these types of simple models have always struggled to get convection (the consequence of the negative buoyancy flux) correct.

The model is indeed a classic and well-used model; more sophisticated models exist with different air-sea flux parameterisations. Our goal is not to develop the closest simulation of the observations, but rather to explore the processes. The very simple model offers the best opportunity to do this, and indeed it is surprising how well the simple classic model performs.

2) The spring-neap motion of the front in response to changing U3. Note that there is an important feedback here – between turbulence driving mixing and stratification which limits the impact of the turbulence and so limits the spring-neap excursion of the tidal mixing front (I believe Simpson and Bowers, 1984 talk about this).

The reviewer is right to highlight that stratification limits the impact of turbulence and therefore spring-neap frontal excursion. Additionally, Simpson and Bowers (1981) highlight that the spring-neap frontal excursion is a more readily observed feature during frontal development in spring and into early summer. We have expanded on this point in our discussion.

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 – i.e. in late spring and early summer (Simpson and Bowers, 1981). Furthermore, some of the additional mixing energy available at spring tides is expended reducing stored potential energy on the stratified side of the front rather than moving the front itself, limiting the extent of spring-neap frontal adjustment (Simpson and Bowers, 1981).*

3) The model does not include the stratifying influence of freshwater which I believe to be important here.

We agree that the influence of freshwater is important in the region. It helps to maintain the salinity gradient between relatively fresh water of primarily coastal origin and relatively saline water of primarily oceanic origin. The absence of the zonal salinity gradient in the model is, we propose, the cause of the divergence between the model and reality in the final weeks of the deployment. We now discuss this in the context of water masses in section 3.2. We do not include freshwater in the model in order to isolate when heating-stirring interactions are the dominant control on frontal location. Adding salinity into the model would not allow us to do this.

Page 11, line 17	*The heating-stirring model cannot reproduce these water masses: they are not formed locally by heating-stirring interactions and their distribution in the northern North Sea is controlled by advection. The temperature distribution created by these water masses is such that a horizontal temperature gradient is maintained. In particular, the bottom front, which is the most dynamically significant feature of the frontal system (Hill et al., 2008; Sheehan et al., 2017) is maintained by the presence of the Atlantic-*

influenced CAW.

In particular it this later point which could form the basis of an interesting story, if the paper goes down the science route. The fact that after the disappearance of the thermal stratification there is still a lateral salinity gradient points to the development of seasonal stratification influencing shelf sea residual circulation even after the disappearance of seasonal stratification. Although not totally up to date with the literature, this is not a topic which I have seen discussed in the literature and so would be well worth pursuing.
We agree that the existence of the lateral salinity gradient is noteworthy. We have expanded the discussion of this in the paper.

Page 12, line 11 *The results of a one-dimensional heating-stirring model, and comparison of these results with glider hydrographic observations, demonstrated that salinity gradients and the distribution of water masses are important controls on frontal location in the region, in addition to surface heating and primarily tidal mixing. A water mass distribution exists which gives rise to a frontal boundary in temperature and salinity. In the absence of significant surface heating, this is the primary source of a frontal boundary and therefore the primary control on frontal location. In summer, heating-stirring interactions modify the water masses, enhancing the background temperature gradient such that heating-stirring interactions become the primary control on frontal location. This situation persists until the autumn: the observations presented in this study capture the period during which, in 2013, the front transitions from being primarily a tidal mixing front to being primarily a front between different water masses.*

Page 12, line 21 *Water mass distribution and attendant spatial gradients of thermal and haline buoyancy are likely to be important in shelf sea where significant incursions of oceanic water are found, such as the northwestern North Sea, the South China Sea (Shaw, 1991; Su, 2004}, along the eastern coast of the United States (Blanton et al., 1981) and around Antarctica (Moffat et al., 2009). Mixing fronts in such regions may persist during periods when local heating-stirring interactions would not promote frontal formation, and the controls on frontal location may change over an annual cycle.*