Response to Referee #1

Major Comments

1. Section 3.3.1 needs some clarifying and expansion. First, as I understand the section and figure 5, you are simply arguing that the submesoscale eddies at 500 m depth are generated via baroclinic instability. This seems plausible, but should be put in the context of recent work on the topic (eg. Hetland 2017, and Wenegrat et. al 2018). Likewise, it was not clear to me whether the focus throughout on the mechanism being a topographic Rossby wave was meant to distinguish this in some way from the basic baroclinic instability mechanism over a slope (in which case this needs clarification), or whether it was just a particular way of introducing why baroclinic instability can happen over a slope (which I would argue is unnecessarily complicated and could just be replaced throughout by 'baroclinic instability').
It would also be good to dig a bit deeper in this section into related questions such as:

- *Why is this mechanism not generating as active an eddy field at shallower depths in EXP1? A possible explanation might be the dependence of the instability on the Slope Burger number, such that the stronger stratification at shallower depths suppresses growth (Wenegrat et al. 2018).*
- *Is the instability trapped between the bottom and the pycnocline? Ie. what sets the vertical scale?*
- *What determines the separation of the eddies off the topography into coher- ent vorticies?*

Thank you for suggesting the paper written by Hetland (2017) and Wenegrat et al. (2018). We modified the section « 3.3.1 Unstable Topographic Rossby Waves » in « 3.3.1 Baroclinic instability at depth ». We re-wrote this section in order to make the process at play more clearer for the reader.

2. The motivation of the study mentions both the Persian Gulf and Red Sea outflows, however the study is really only focused on the 200 m depth range (ie. the Persian Gulf water). For instance, both the detailed case studies and the particle tracking are focused only on the 100-300 m depth range. This choice may reflect the fact that it is only in this depth range where there are substantial differences between the experiments. However, the most active submesoscale eddy field is at 500 m depth (eg. figure 3).

As such, I would suggest that the particle tracking analysis should be repeated for 500 m depth. While there are likely not significant differences between experiments at this depth, the findings would have implications for the accuracy of lower-resolution models in capturing the spread of Red Sea water.

In our configuration, the velocity field of mesoscale eddies reaches 300 m depth, typical of the Gulf of Oman. In the Gulf of Aden, the mesoscale eddies can reach 1000 m depth. So, the detachment of the bottom boundary layer should occur at depth as well and then, intense and long-lived submesoscale eddies should be created below 300 m depth and have an impact on the dispersion of particles (or the Red Sea outflow Water). That is why, we did not repeated the particle tracking at 500 m depth. However, we added couple of lines of discussion about this point in the conclusion (p. 19, l. 4).

3. The differences in particle dispersion between the 3 experiments are being at- tributed to the submesoscale eddies in the interior. However, an alternate hypoth- esis would be that the differences arise due to boundary layer dynamics (absent in EXP1). Histograms are shown for vorticity field sampled by the particles (eg. figure 13 b, c, d), showing heavier tails in EXP2 and EXP3, which is interpreted as evidence of the role of submesoscale eddies. However, this same sort of pattern could also occur if the particles were randomly sampling the underlying flow field (which would have a heavier tailed vorticity distribution in EXP2 and EXP3). A bit more analysis of this section would make the argument for the role of submesoscale eddies more convincing. For example, one could look at the changes in the particle sampled vorticity distribution relative to the changes in the underlying distribution across the whole domain. You could also try comparing distributions between particles which make it to the right-hand side of the domain to those that don't.

As you suggested, we compare now the distribution of vorticity associated with particles which reach the right-hand side of the domain to those which don't (See Fig. 14 and the text p. 16, l. 18).

Minor Comments

1. You have high resolution in the vertical (100 σ levels), and moderately high- resolution in the horizontal, with moderately steep topographic slopes. Are hydrostatic pressure gradient errors a concern for this setup? It would be good to comment on this (eg. grid stiffness etc) in section 2.2.

Yes, the hydrostatic pressure gradient errors are a concern for this setup. We added a comment about this (p.3 , l. 28), in particular, regarding our sensitivity experiments.

2. You should comment a bit more on the choice to model the dispersion of dense water as a passive tracer. As I understand the setup, really what you are intending to say is that the passive particles are meant to act as a proxy for high-salinity water. I assume that this choice was made because introducing a salinity gradient in the initial condition would be problematic with the re-entrant domain.

We did not add a dense water directly in our initialization for several reasons. Firstly, as mentioned in the MS now (p. 6, l. 10), there is no submesoscale velocity structure clearly associated with the fragment of Persian Gulf outflow Water (observation made from the VM-ADCP). It indicates the PGW can be modeled as a passive tracer. Secondly, as pointed out, the periodic boundary condition in the x-direction would have been problematic. Thirdly, we are not interested in the dynamics of the outflow itself. We added some comments (p.5 , l. 25).

3. Given that you only have 3 runs, I would suggest renaming them with more in- formative names, which is very helpful to the reader. For example, you could choose to name them NO-BBL, BBL, and BBL-CAPE, or any other variant that immediately conveys the setup.

We re-named the experiments in the text, captions and figures as you suggested.

4. In section 3.5 you introduce two different definitions of the diffusivity (equations 13 and 14). Please clarify why these two definitions are given, why they don't agree, and if possible clean this section up a bit by using only one.

As mentioned in the MS, the first definition allows to estimate a diffusivity coefficient for short times. This is the case of our ballistic regime. Then, thanks to the second definition, we can estimate a diffusivity coefficient over a long time period.

5. The final paragraph of the manuscript feels out of place, and not well supported. For example 'the vertical motions then are of importance to the uplift of nutrients in the ocean and then onset of algae blooms' is extremely speculative when considering an instability at 200 m depth. As this paragraph really is just laying out a variety of future work the authors plan to carry out, it is not entirely relevant to the bulk of the manuscript, and I'd suggest removing it.

The final paragraph has been rewritten thanks to the referes suggestions.

6. In some of the figures the subplots lack labels/scales on the axes. For instance, figure 8 shows an eddy in plan view, without axes labels. The moving focus region between subplots would make it hard to label with absolute position, however you could at least add some scale to the x-y axes (ie are the subplots showing a 10km x 10 km region? 100 km x 100 km?).

This has been modified.

7. Figure 10: Describe the meaning of the dashed lines in the caption.

This has been added.

8. The wording in the abstract connecting the findings here to the Persian Gulf Water and Red Sea water is a bit too strong. I would suggest rewording to: ...and their potential impact on the spread of Persian Gulf Water..' and 'This shows the potentially important role of submesoscale eddies...'.

This has been modified in the MS.

9. Spelling: 'without' near line 15 in the abstract.

We modified this.

10. I assume that the black contours in Figure 2 (b) and (c) are density, however this is not indicated in the caption.

This was added in the caption.