Response to reviewers' comments on the manuscript "The influence of a submarine canyon on the wind-driven downwelling circulation over the continental shelf: egusphere-2024-2386"

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October 2024

1 Response to Reviewer #3

1.1 Specific Comments

1. The literature review misses important previous work on downwelling including frontal instabilities. Here are some examples.

   Thank you. Your recommendations were added to the introduction section. Other relevant references regarding wind-driven downwelling processes have been also included.

2. The wind-driven downwelling front develops over time and gradually moves away from the coast (see Kämpf, 2019). What are typical distances of upwelling fronts from the coast? How typical is it that a downwelling front actually comes close to a shelf-incised canyon? Given this, rather than a locally produced wind-driven upwelling front, wouldn't it make more sense to consider current-driven downwelling (driven by on offshore sea-level gradient) as forcing for your model? The results are probably different as this current could affect deeper portions of the canyon.

   We appreciate your comment. Nonetheless, the focus of the paper is to address the response of a downwelling front, and the associated circulation, generated in response to the wind forcing. This be an important part of the dynamics of mid-latitude Eastern Boundary systems, where local storms events could induce strong events of downwelling (Austin & Barth 2002). These storm can tilt isopycnal from the coast offshore up to about 30-40 km. Please note that we have reproduced the typical wind-driven downwelling results from other studies in our no-canyon experiments. Thus, introducing the submarine canyon in our simulations allowed us to highlight the impact of having a submarine canyon in the coastal circulation of an idealized eastern boundary margin during winter conditions.

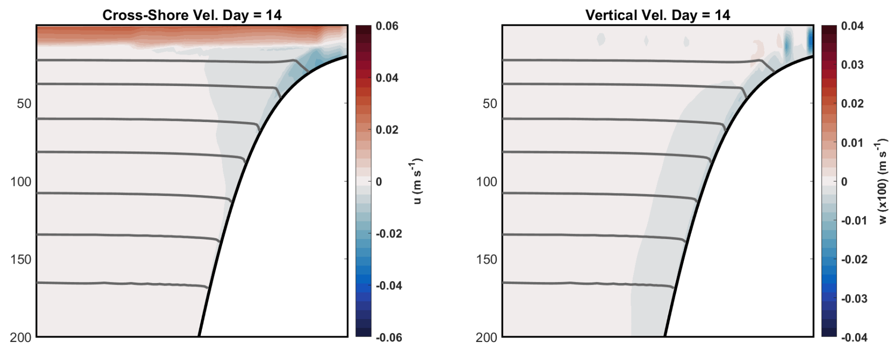
   We have included some literature of the case you mention including a current-driven downwelling in realistic (Jordi et al. 2005) and idealized cases (Klinck 1996, Spurgin & Allen 2014). In this revised version we have also included new references of your studies (Kämpf, 2006, 2007, 2009, 2012, 2018, 2019). Thank you.

3. . Most results are affected by instabilities. These instabilities are key features of the results that require explanation, further analysis, and references to previous studies such as Feliks and Ghil

(1993). But are these instabilities frontal instabilities? Some results of the cross-shelf velocity component u in Figure 3 and Figure 5 show negative values just above positive values near almost vertical isopycnals. These disturbances resemble overturning (i.e., forced convection) cells, rather than horizontal disturbances. Downwelling induces extreme situations of unstable density stratification. Could it be that the simulated instabilities are rather a side effect of the vertical turbulence scheme used? The authors should explore whether the instabilities disappear when using difference turbulence schemes, grid resolutions and/or vertical density stratifications.

We appreciate your comment. The instabilities appear just at the location of the downwelling front. We have attached a complementary animation showing the evolution of the cross-shore and vertical velocity in the x-z plane. As it can be seen there, isopycnal in the shallow shelf case (which is the one that most rapidly evolves) does not present isopycnals going into unstable distributions. Rather, they form a front near the bottom and then start to oscillate as baroclinic instabilities develop, with vertical velocities in the entire column. We believe these are not representing overturning. The downwelling wind forcing is not that strong and the lighter waters does not seem to penetrate below the denser water. Thank you for pointing out to the paper by Feliks and Ghil (1993) – great reference to include in the introduction and discussion.

You can download the movie at Downwelling Movie Isopycnal. It is also reproduced here:



4. Why are the instability patterns horizontally tilted (see Figure 2)?

   Considering that the northward flow is horizontally and vertically sheared around the core of instabilities in both, the intermediate and shallow shelf experiments (Figure 2 and Figure 3), it is expected that the perturbations related to the frontal instabilities should be advected not uniformly, thus generated the tilted pattern observed in Figure 2. Tilted patterns of frontal instabilities have been observed for a similar bathymetric setup in Durski & Allen (2005) and in relation to a vertically sheared alongshore flow.

5. Why is the model domain so big? The model domain shown in Figure 1b should have been more than sufficient for this investigation I would also recommend the use of cyclic boundaries as in Brink's studies. A bottom roughness of 2 cm seems large. What happens for a bottom roughness of 2 mm?

   The model domain is the same as Saldías & Allen (2020), and thus, their explanation applies here as well. Considering that the objective is to generate a wind-driven circulation with impose perturbations on the forcing to generate instabilities, the use of periodic boundaries could allow the propagation of waves and noise through the domain in a constant loop. Previous works have also noticed that in domains with submarine canyons, the propagation and reflection of waves make open boundaries a better choice versus periodic boundaries (e.g. Dinniman & Klinck, 2002; Zhang & Yankovsky, 2016). The study of Saldías et al. (2021) shows that the interaction of Coastal Trapped Waves with a canyon can promote the generation of new modes which also continue propagating along the coast. Thus, we opt for the configuration having a large domain with open boundaries, similar to other studies (e.g. Zhang & Lentz, 2017). The bottom roughness was computed following previous studies of flow over topography using ROMS (e.g. Whitney & Allen, 2009; Li et al., 2021).

6. A key finding of this study is the trapping of particles inside the canyon. However, rather than distributing particles uniformly throughout the domain, particles were released along a few selected transects. Why were particles not distributed throughout the domain?

   Thank you for the comment. The goal of the particle tracking experiment is to give some extra information on the circulation patterns inside the canyon from a Lagrangian perspective. With this in mind, we selected upstream positions to evaluate if particles outside of the canyon can enter and become trapped. The transects over the canyon look for particles that start in the canyon and remains in there. Overall, this approach of discrete transects allows us to show that particles near the canyon can become trapped, and use this in the future for further development. It also allows for a better observation compared to releasing particles along the whole domain.

7. It seems the particle module was run standalone afterwards using the results of the ocean circulation model, true? Were the results stored after each simulation step (which requires massive amounts of storage), or was the output deemed stationary (which is not applicable given the instabilities and the progression of the downwelling front)? Or was the particle module run simultaneously with the ocean model? How many particles were released? I cannot find this information in the text.

   The particles were run standalone using the velocities of the model as input for the lagrangian trajectories. For each time, the velocities of the model were interpolated on z-level coordinates with a resolution of 2.5 meters, and then this interpolated fields were used as input for Parcels (Delandmeter et al., 2019). The particles were released from surface to bottom at the indicated transects (locations indicated with white dots in Figure 9). These details of particle trajectories and the use of Parcels is included in the revised version of the manuscript. Thank you.

8. While most particles are topographically steered across the canyon, the particles becoming trapped can hardly be seen in Figure 9. It would be better, in my view, to include a figure only displaying the trajectories of trapped particles. With arrows indicating flow directions. Other evidence should also be provided showing the anticyclonic eddy inside the canyon, e.g. as averaged horizontal current fields.

We have added a new figure highlighting the initial position of the particles trapped in the canyon at the end of the simulation. Thank you for pointing this out. We believe this new figure clarifies this trapping effect.

9. The percentage of trapped particles doesn't provide much useful information. The authors need to focus more on the reasons as to why the particles becomes trapped. Where do these particles exactly come from? At what depths were these particles released? Sometimes particles cam becomes trapped once there are too close to the seafloor (that's why diffusive effects sometimes help). Did this happen in the simulations? Does the inclusion of diffusive effects increase the trapping?

   As mentioned before, the panels of Figure 9 show the initial position of the particles (white points). The depth ranges indicated in the title of each panels indicate the range of release of the particles. In our case, any particle that become trapped in the seafloor or in the walls of the canyon were discarded from the analysis. We did not analyze the processes that trap the particles in detail, as it would extent the paper way beyond the scope of the manuscript. Nonetheless, it is a good point to continue with further experiments in a future study.

   We included new information in the manuscript regarding the particle tracking experiments:

   "Velocity fields from ROMS were interpolated at each time step to z-levels from 5 m to 400 m with a 2.5 m resolution. These velocity fields were used as input to the Parcels environment. Particles that were trapped by the seafloor or stopped by the topography were discarded from our analyses, and thus, we only maintain particles that presented displacements through the entire period of tracking."

10. The discussion section refers to particles being released at mid-depths (e.g. 100-170 m). The use of the tern "mid depth" is incorrect and confusing. Instead of referring to the middle of water column, I think, the authors rather refer to a region where the total water depth ranges between 100 and 170 m. Please clarify this confusion.

    To clarify any potential confusion, the term Mid-depth was changed along the text for the corresponding depth interval, or a more general conditions such as "depths below the continental shelf". We want to thank the reviewer, Dr. Jochen Kämpf, for his comments.

    Please note that all the references we have specified here are included in the revised version of the manuscript.