General comments

This paper applies the ROMS ocean model to study the impact of downwelling favourable winds on the interaction of wind-driven current with a shelf-incising canyon for different configurations. After studying this work, I recommend revisions based on the following specific comments. In other journals this would be a resubmission with major revisions.

Specific comments

1. Missing literature

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

- Feliks and Ghil (1993) Downwelling-front instability and eddy formation in the Eastern Mediterranean. JPO, 23, 61-78.
- Brink, K,H. (2016) Continental Shelf Baroclinic Instability. Part I: Relaxation from Upwelling or Downwelling, JPO, 46, 551-568.
- Brink, K.H. (2024) The effect of alongshore wind stress on a buoyancy current's stability, CSR 272, 105149
- Kämpf, J. (2010) Extreme bed shear stress during coastal downwelling. Ocean Dynamics. https://doi.org/10.1007/s10236-019-01256-4

2. Missing discussion on timescales & type of model forcing

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.

3. Frontal instabilities

3.1. 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.

3.2. Another question: Why are the instability patterns horizontally tilted (see Figure 2)?

4. Methodology

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?

5. Particle tracking

5.1. 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?

5.2. 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.

5.3. 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 horizonal current fields.

5.4. 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?

5.5. 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.