Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-4-RC2, 2020

Interactive comment on "Connecting flow-topography interactions, vorticity balance, baroclinic instability and transport in the Southern Ocean: the case of an idealized storm track" *by* Julien Jouanno and Xavier Capet

Anonymous Referee #2

Received and published: 31 March 2020

This is an interesting paper that I enjoyed reading. The subject is of interest and relevant to an on-going discussion regarding Southern Ocean dynamics and circulation. There is a lot of material here covering, for example, energetics, modal decompositions, and spectral analysis, etc. I felt that this obfuscates the authors' message and means that they are left either trying to explain too much, or not explaining enough. The paper would be better served with a narrative that concentrates on the physical argument and only uses enough figures and analysis types to reinforce this. At the moment the paper's central argument is disguised by the extra material.

My main concerns are highlighted below, with additional minor comments appropriately titled.

1) The presence of gyres in a Southern Ocean model with f/h contours blocked by the northern boundary was not uncovered by Nadeau & Ferrari (2015). 'Highlighted' would be a better choice of word. There are numerous papers prior to, and contemporary with, Nadeau & Ferrari that show this same flow feature. Examples include Tansley & Marshall (2001) and Jackson et al. (2006), although there are plenty of others. A recent example that looks in detail at the formation of these gyres is Patmore et al. (2019).

2) At line 75 it is stated that 'no explicit diffusion' is used in the model. Later on diffusive terms are included in some of the figures, e.g. Figure 10. Does the model have diffusion? Or is it the case that vertical diffusive terms are included with no explicit horizontal diffusion? If this is the case, this means that the model is relying upon implicit diffusion in its advection scheme, which may have implications for the form of its overturning. Is the model's residual overturning quasi-adiabatic, as achieved in Abernathey et al. (2011)?

3) At lines 167-169 the authors write that 'rough topography limits the eddy energy at the location of the stationary meanders but also favors the persistence of the eddy energy far from the ridge.' On my first read through this felt like an important point. However, I don't think it's particularly born out by Figure 8. Rather, Figure 8 shows the more local confinement to the ridge due to the rough bottom. It's the flat bottom experiments that truly favour downstream persistence of EKE.

4) At lines 185-188 the authors briefly discuss the PV budget in experiment R+F. I expected something to be said about the much larger contribution of nonlinear vorticity advection over the ridge for this experiment. The import/export of vorticity into/out of this area could be an important reflection of the change in dynamics.

5) Section 3.3 discusses the experiments in which no restoring takes place at the north-

ern boundary. Something that gets overlooked here, but is apparent in Figure 5, is that the isotherms in both experiments with bottom roughness are at similar depths on the northern boundary. This suggests to me that the rough bathymetry may be constraining the transport by allowing geostrophic return flow at greater depths than the ridge alone. This would allow for deeper isotherms and higher circumpolar transport. Calculating the average depth of an isotherm, instead of zonally averaging across temperature classes, would make this clearer.

6) Section 4.1 is where I found myself becoming unstuck. This discussion is very important to the message of the paper and starts well by laying out a series of 5 points that the authors aim to connect. I think the progression of the discussion would be well-served by using these points as headers to the relevant paragraphs. This would help signpost the path for the reader. It might also be useful to introduce some of this material earlier in order to ease the cognitive burden at this point in the paper. Currently, this section is confusingly written and I found myself becoming confused by the authors' argument. Section 4.2 makes some very interesting and important points. It connects the authors' discussion with other relevant literature. However, I think this could also do with being revised in order to ensure it is as clear as possible.

Minor Comments

line 24 : 'The real SO. . .seems to lie in the "rough bottom/no wind-driven gyre" regime.' The SO clearly does have gyres and ending the abstract on something that causes less confusion would be better.

line 30 : Scotian -> Scotia

line 33 :' referred to' at end of line.

line 45 : 'thought to dissipate'

lines 44-56 : These two paragraphs feel disconnected from the rest of the introduction; the switch from discussing gyre dynamics to bathymetry is very abrupt.

line 87 : Equation immediately following. I think there's a typo in the form of the vertical restoring. This is very similar to the cited example of Abernathey et al. (2011), but not quite the same.

line 90 : A restoring of 7 /day would be very strong, is it 1/(7 days)?

line 140 : It would be very helpful for the reader to also specify the mean transports of the currents.

line 182 : Strictly speaking it isn't that the 'pressure torque is no more effective', its that the pressure torque is zero.

line 214 : 'In the presence'

References

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