Reply to comments by reviewer #2 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, July 2020

We thank the reviewers for their thoughtful comments and have done our best to address them. Before we proceed to the specific responses, we wish to highlight a general aspect of our review work. Both reviewers seem to have had problems with the sequential structure of the manuscript (broadly section 3 is a description of results that do not seem particularly connected; sections 4 provides interpretations/discussions to attempt to connect/unify the pieces together). We understand that this organization is less common in our field than in other ones. We have attempted to reorganize the manuscript differently but we ended up not doing so, mainly for the reason that the chains of processes we propose in section 4 is complex and is more clearly explained once all supporting material it needs has been presented. This being said, we have carefully rewritten some key parts of the manuscript (including the final paragraph of the introduction) to make sure the reader is well aware that the key interpretations will come in section 4. Therefore, and with the improvements in the lay out of the discussion in 4.1 (following the suggestion of reviewer 2), a reader like reviewer 2 who would feel "stuck" in section 3, could more naturally jump to section 4 for a scanning of our interpretations.

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.

We are very pleased the Reviewer enjoyed reading the paper and found it relevant. We thank her/him for the time taken to review our manuscript. We discuss each of the comments sequentially below. We have seriously contemplated the possibility to narrow down the scope of the paper but we remained, in the end, quite convinced that all the pieces of the story are important. We have tried to improve several bits of text with the intent to make this more apparent. The only part that could possibly be removed, we think, is the final comment on local versus global instability because it does not seem relevant to rationalize the relative behaviours of our various ACCs. On the other hand, we feel that it can be important to keep it because it is such a key element of Abernathey and Cessi (2014) and may otherwise seem strangely absent.

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).

Thanks for this suggestion and for the references. The section has been rephrased as follows:

"Another potentially important aspect of the dynamics through which ridges affect the SO circulation is the formation of closed recirculating gyres driven by Sverdrup like dynamics that co-exist with the circumpolar flow (Tansley & Marshall 2001, Jackson et al. 2006). From idealized numerical simulations of the ACC, it was recently highlighted by Nadeau and Ferrari (2015) that increasing wind intensity leads to increasing gyre circulation without modification of the circumpolar transport, suggesting that the saturation of the circumpolar transport with increasing winds may be connected with gyre dynamics. Patmore et al. (2019) further highlight that ridge geometry is important for determining gyre strength and the net zonal volume transport."

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)?

Yes the horizontal advection scheme has some implicit diffusion. Its contribution to the energy balance in a similar channel configuration has been described with details in Jouanno et al. (2016). The model also includes some vertical diffusion : "*The vertical diffusion coefficients are given by a Generic Length Scale (GLS) scheme with a k-ε turbulent closure (Reffray et al. 2015).*". But as shown below, the residual overturning computed with the last ten years of the R+R simulations is quasi adiabatic in the interior. We have added a parenthesis saying "(implicit diffusion can be diagnosed whenever necessary; see for instance Jouanno et al, 2016)".

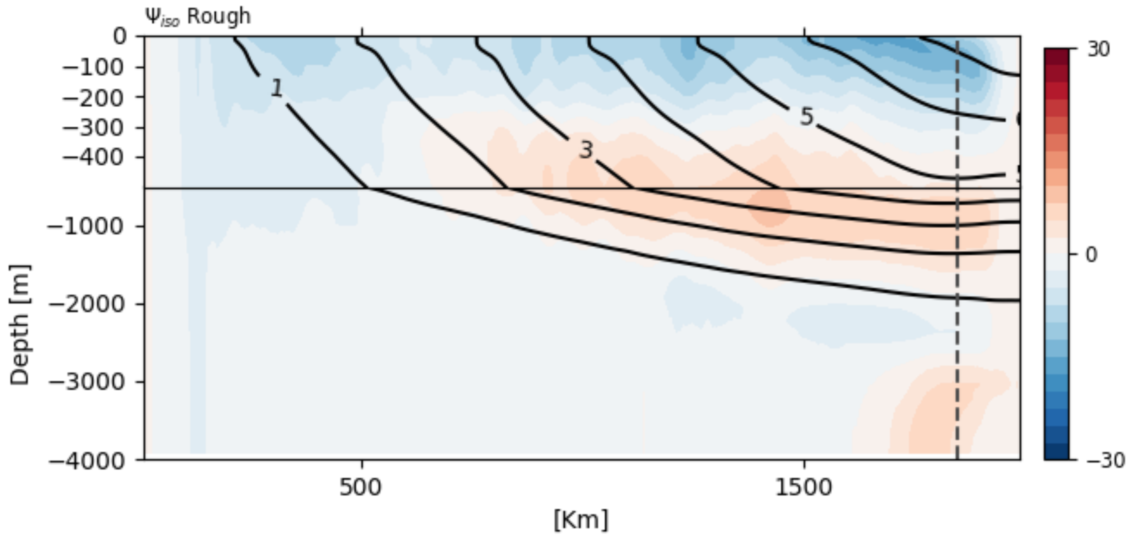

Figure R2: residual overturning in R+R. Diapycnal transformations occurs at the northern boundary where restoring is active, in the upper ocean, and at the southern boundary where the mixed-layer can reach the bottom.

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.

Indeed, this is somehow hidden by the meridional averaging in Figure 8. We now show spatial map of EKE in Figure 3 that illustrate how "rough topography limits the eddy energy at the location of the stationary meanders but also slightly increases EKE downward over a restricted latitude band. This being said, this is not something that we make sense of later on or is used in our interpretations. Therefore, we have removed this bit.

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.

Indeed, on the ridge there is a change of sign of the non-linear vorticity advection between R+F and R+R we're having difficulty interpreting. Since the contribution of this term to the barotropic vorticity balance is second-order and we attempt to keep the focus of the study on first-order sensitivities we decided not to mention this in the revision.

5) Section 3.3 discusses the experiments in which no restoring takes place at the northern 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.

We provide Fig. R3 to answer this remark. In Fig. R3 we show the mean depth of isotherm in all four simulations. It helps figure out the similarities between simulations in terms of stratification. In fact, temperature fields at the northern boundary do not appear to be more similar in the 2 simulations with rough bottom than they are in the 2 simulations with smooth bottom. The reviewer may be right that a complex interplay between the stratification that gets established in the simulations and the associated transverse circulation also contributes to explaining some of the transport variations. In fact we allude to this in relation to the long or secondary adjustments in transport that are seen in the rough simulations (Fig. 5 and 12). This being said and despite some efforts (prior to submission and during the review) we have not been able to identify processes at play that would help the reader make sense of these second-order aspects. Given the relative complexity of the message on dominant processes at play we would prefer not add details on the stratification/transverse flow subject.

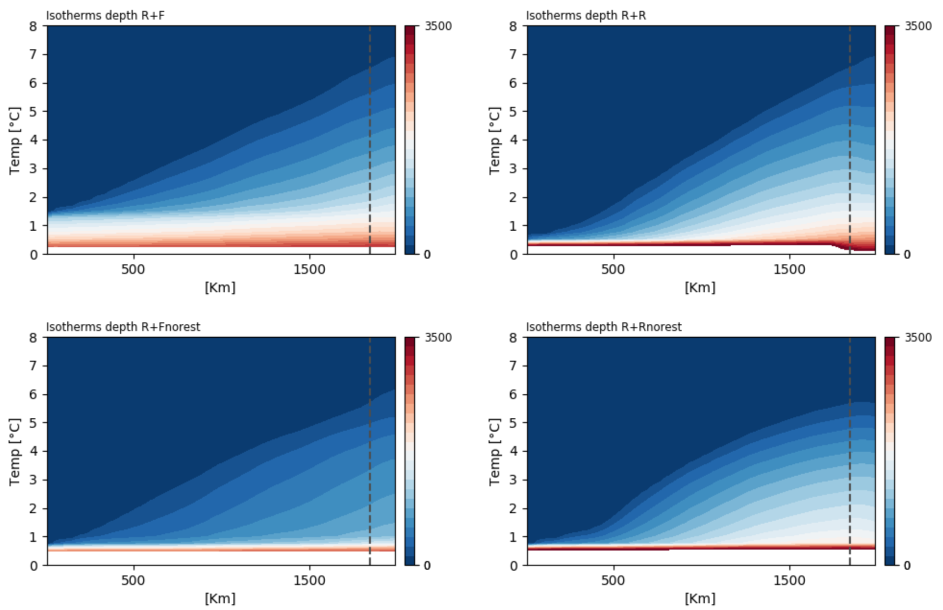

Figure R3. Field of zonal and time averaged isothermal depth for all 4 simulations.

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.

We agree, this discussion is quite dense. The different processes involved in the ACC are so interconnected that we think they need to be presented together. We have reworded some parts of this discussion and, most importantly, we have introduced headers as suggested by reviewer 2. We like it a lot and we think this will be helpful to readers.

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.

We have reworded that last sentence so as to cause less confusion: "The real SO having both gyres and ACC saturation time scales typical of our "no gyre" simulations may be in an intermediate regime in which mesoscale topography away from major ridges provides partial and localized support for bottom form stress/pressure torque."

line 30 : Scotian -> Scotia

Thanks. Corrected.

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

Thanks. Corrected.

line 45 : 'thought to dissipate'

Corrected here and one another location.

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.

We find the transition rather natural since "ridges" are bathymetric features. In the previous paragraph we discussed the known impact of the ridge on the circulation, and in these two paragraphs we discuss the known impact of smaller topographic features. In fact, to make sure the connection is clear to the reader, we have reworded the second paragraph which has been shortened and merged with the first one. We hope this will convince reviewer 2.

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.

Thanks, there was a typo that has been corrected.

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

We use a restoring time scale of 7 days. To avoid confusion this is described as follows : “The relaxation coefficient varies linearly from 0 at y=1900 km to 7 days at Ly.”

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

Transport have been added.

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

Thanks. We agree and corrected.

line 214 : ‘In the presence’

Corrected

References

Abernathey, R., J. Marshall, and D. Ferreira, 2011: The dependence of Southern Ocean meridional overturning on wind stress. J. Phys. Oceanogr., 41, 2261–2278.

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Tansley, C. E. and D. P. Marshall, 2001: On the dynamics of wind-driven circumpolar currents. J. Phys. Oceanogr., 31, 3258–3273.