Reply to comments by reviewer #1 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.

The authors here try to assess the role of bathymetric roughness in establishing the mean circulation in the Southern Ocean. They do so using a series of idealized, zonally-reentrant simulations of primitive-equations on a beta plane.

The experiments performed and their analysis consist interesting numerical observations for how roughness affect the dynamical balances. However, the authors' attempt to explain the dynamical processes that take place and, thus, assess the dynamical role the bottom roughness brings about, are lacking. I have pointed out specific points below.

Overall, the paper is not very well-written and therefore major revisions are in place. Presentation is often sloppy and figures could definitely be improved. I find the numerical experiments performed here, as well as the accompanied analysis the authors went through, interesting and worthy of publication. However *not* at the manuscript's current form. Regarding dynamical explanation, e.g., section 4, I would like to see the arguments cleared up a bit; I provide specific comments below.

We thank the Reviewer for his valuable input and refer to detailed responses to all of its comments below. We have tried to improve the text and figures in many places, in particular with the aim to make our dynamical interpretations as clear as possible.

Major points

These need to be addressed by the authors.

1. general: Please number all equations.

   All equations have been numbered.

2. line 50: Refrain from referring to a figure in a different paper! If the specific figure is crucial for the discussion then consider reproducing it here.

   We remove the reference to the figure, here and after, and now we only retain the reference to the paper Goff and Arbic 2010.

3. line 55, 59, ...: The authors use "*form stress*" and "*form drag*" interchangeably. Please choose one and stick to it throughout the manuscript. Personally I'd go with the former as this term does not always behave as drag (see Holloway's series of papers about the "Neptune effect").

   Thanks. Following your suggestion, we stick with form stress all along the manuscript.

4. line 88: This expression is completely different from that in Abernathey et al 2011. I believe (hope) this is a typo.

   Thanks, yes this was a typo. It has been corrected.

Line 137: I would like to see a time-series of PE since, usually, that's what takes longer to equilibrate. It is important to see whether PE is equilibrated before one talks about time-mean isopycnal slopes.

Indeed the reviewer is right about the fact that PE is not fully equilibrated after 150 years as illustrated in Figure R1 below. We have extended the simulations for another 100 years. Simulations are closer to PE equilibrations after 250 years of simulation although a slight adjustment is still visible for simulations without northern restoring (Figure R1). This being said, comparison of the density fields around t=150 y and t=250 y indicates that adjustments are minor and in particular that stratification differences between the sensitivity runs are large compared to stratification drifts diagnosed for each model run (see Figure R2). Therefore, we have stuck with our original analysis period year 140-150.

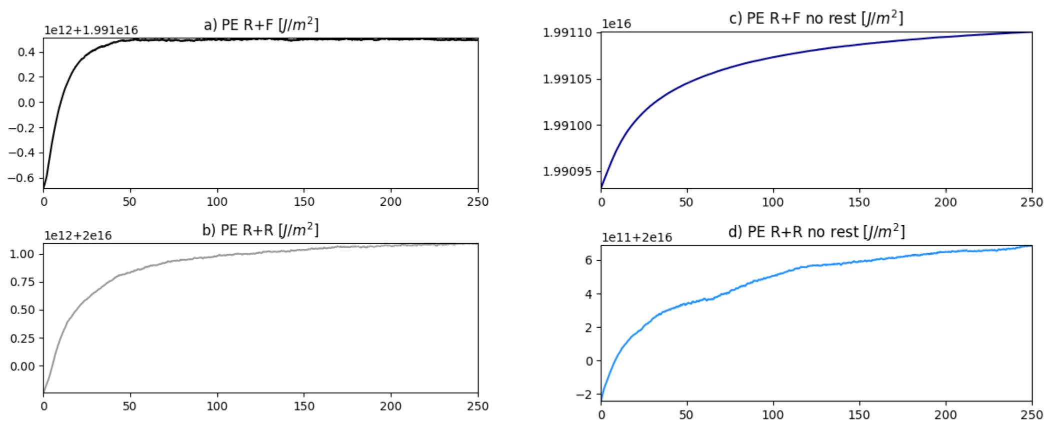

Figure R1. Time evolution of PE in the four simulations.

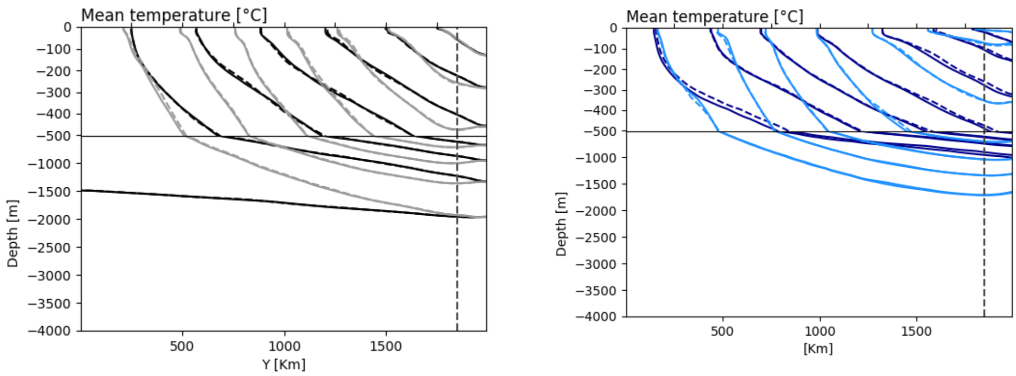

Figure R2. Time mean isotherms averaged over the years 140 to 150 (continuous line) and year 240 to 250 (dashed line). Color code is the same as in Figures 5c,f of the manuscript.

5. line 185: The author's at this point try to explain why bottom roughness diminishes the gyres that can be found in the configuration with just the high-ridge. They compute the dominant terms in the Sverdrup balance (see figure 9 & 10). They do find that with and without roughness different terms dominate the Sverdrup balance. However, the paragraph here explains nothing! It's more like a chicken-egg argument. What the authors effectively say is that with roughness gyres turn off and the term βV is not important. But of course, with no gyres term βV can't be large. Do the authors try to argue here that roughness somehow implies that the vorticity balance must change from that in figure 10a to that in figure 10b and, therefore, the gyre turns off? If this is what they are trying to argue they need to back up the claim.

   Although we agree with the reviewer that the chicken and egg trap shall be avoided, we believe the text clearly sticks to a descriptive objective at this place and does not try to propose any interpretations. Interpretations on the BV balance are presented in section 4.1 and have been carefully rewritten to make sure there is no chicken and no egg there either. Precisely, we will only be making the point that R+F has no choice but to balance the wind curl input with βV outside the ridge area while R+R does have more freedom and in fact balances wind curl input with bottom pressure torque.

6. line 200: Regarding comparing the experiments with and without restoration at the northern boundary, the authors say: "*Most of our previous results are not qualitatively dependent on the choice of restoring the northern stratification.*" However, from figure 4b,c I conclude the opposite. I see that experiments with 'nr' show opposite dependence on bottom roughness compared to the restoring experiments, especially in the upper 500m. Right?

   You are right and this was discussed in same paragraph, but somehow embedded in the discussion of total KE sensitivity. We now made this discussion on EKE sensitivity in Figures 4b,c more explicit :

   "*Second, bottom roughness strongly decreases total KE when restoring is applied while total KE is very weakly affected when no restoring is applied (Figure 5a,d). We attribute this to the fact that the more efficient release of available potential energy in* *the absence of rough bathymetry (Figure 4c), that lead to larger EKE in the upper 500 m (Figure 4b), can significantly modify the ACC thermohaline structure in the simulations without restoring whereas it cannot when tightly constrained by the restoring (compare the departures between isotherms in Figs.5c and f). Further elaboration is provided in Section 4.*"

And in Section 4 :

"The reduced baroclinicity and zonal transport in R+F and R+Fnr can thus be seen as the manifestation of the boundary current effect on local baroclinic instability in the lee of the ridge. In the simulations without restoring this manifestation on baroclinic instability is less evident because the mean thermohaline structure of the ACC has significantly more freedom to adjust in response to the strength of baroclinic instability processes. In turn, this response of the mean state lead to a negative feedback by modulating the intensity of baroclinic processes which ends up being quite similar with and without rough bathymetry in the absence of northern restoring (compare EKE and APE release rate for R+Fnr and R+Rnr in Figures 4c and 8f)."

7. line 206: If this is the total KE how come is smaller than EKE? I expect the total to be greater than any of its constituents.

   Here we referred to Figure 5 where we show total KE. For clarity, we add reference to the figure. Moreover, we add in Figure 4 caption more details on how we compute the "MKE" (kinetic energy of the mean flow) so there should be no more ambiguity: "The kinetic energy of the mean flow (MKE, a) is computed using 10-years averaged velocities." On the other hand, if you remark concerned Fig. 8 (which we did not refer to at line 206), please note that there was an error in the legends/captions of that figure and that panels a) and d) show the kinetic energy of the mean flow.

8. line 228: I don't understand what are the "*general expectations drawn from eddy saturation theories*" the authors refer to at this point. Could they elaborate a bit? Also, citations should be relevant, potentially to the work by Straub *JPO* 1993, Marshall et al. *GRL* 2017, and Constantinou & Hogg *GRL* 2019.

   We agree the sentence was vague. We preferred to remove it since discussion on the eddy saturation process in given in section 4.2.

9. line 250: "*As a consequence, only in the flat bottom configuration can the Sverdrup balance emerge.*": I don't understand what the authors want to say. In both R+F and R+R configurations the Sverdrup balance balances (see figure 10)! I guess they mean to write that when roughness is present, the balance is different and diverges from the textbook picture that crucially involves the role barotropic Rossby waves? In either case, they should rephrase to make the text clearer.

   Sverdrup balance specifically refers to the dominant balance between the wind stress curl and planetary vorticity term (βV) as classically understood. Such balance is a good approximation of the vorticity balance in the "R+F" case (Figure 10b) but not in R+R. We're not trying to say more than that. The text has been rewritten in such a way that, we think, no confusion can happen.

Line 255: "... *geophysical flows*": a citation to Rick Salmon is relevant here, e.g., "Baroclinic instability and geostrophic turbulence. *Geophys. Astrophys. Fluid Dyn.* 15, 167-211 (1980)."

We agree and choose to refer to this study: Salmon, R., Holloway, G., & Hendershott, M. C. (1976). The equilibrium statistical mechanics of simple quasi-geostrophic models. Journal of Fluid Mechanics, 75(4), 691-703.

10. line 295-297: The authors here present baroclinic instability as the explanation for eddy saturation. But it has been established by a series of studies that bathymetry plays dominant role in eddy saturation (Thompson & Naveira Garabato *JPO* 2014, Katsumata *JPO* 2017, Barthel et al. *JPO* 2017, Youngs et al. *JPO* 2017, Constantinou and Hogg *GRL* 2019). The authors should update their explanation of eddy saturation.

    Thanks for these references we missed. We complete this section as follows:

    "Baroclinic instability, which is the main source of energy for the mesoscale eddy field in the SO consumes the APE imparted by wind-driven upwelling. It occurs in such a way that additional energy input by the wind enhances EKE but leaves APE and ACC transport nearly unchanged. This contributes to the so-called eddy saturation effect which limits the sensitivity of the circumpolar transport to changes in the wind forcing magnitude (Morrison and Hogg 2012, Munday et al. 2013, Marshall et al, 2017). Processes involving the barotropic circulation and its interaction with the bathymetry may also participate to reduce the sensitivity of the ACC's baroclinicity. Specifically, the standing meanders that forms through the interaction of the barotropic flow with the topography contribute to the bottom form stress and may also participate to the saturation process (Thompson & Naveira Garabato, 2014, Katsumata, 2017). Constantinou and Hogg (2019) recently highlight the role played by the eddy production through lateral shear instabilities of the barotropic circulation or interaction of the barotropic current with the topography, in establishing the eddy saturated state of the Southern Ocean. Overall, our findings confirm the robustness of the saturation process with respect to major changes in model configuration, which translate into varied baroclinic instability regimes/efficiency (as previously noted in Nadeau et al. 2013), but also flow with varied barotropic dynamics and a wide range of ACC transports."

11. figure 3: Add the same panels for the R+F experiment. Use the same colorscale.

    Same panels for R+R have been added in Figure 3 and the same colorbar is used.

12. figure 6: Caption mentions: "*Normal mode analysis has been performed for profiles located at* y = 1000km *and spaced by* 100km *all along the zonal direction, and using monthly instantaneous outputs from the last ten years of simulations (R+F and R+R)*." I must admit that I don't understand what the authors are saying here. Please explain clearly or remove; I'd suggest the former.

The caption has been edited as follows : “The kinetic energy given in (b) results from a combination of : spatial averaging over 40 profiles taken at the central latitude (y=1000 km) and regularly spaced in longitude all along the channel; and temporal averaging 120 snapshots obtained at monthly frequency over the last ten years of simulations R+F and R+R.”

13. figure 7a: This figure is puzzling since it shows that flow in R+F goes beyond 3500m in contrast with figure 2b. Also, what’s the dashed region below 3000m? Either remove or explain?

    This was to indicate the depths for which the spectrum was “polluted” by the rough topography. We modified the figure so we now only consider the depths entirely filled by the ocean outside the ridge (i.e. 3000 m in R+R and 3500 in R+F). The message to be taken from the figure remains unchanged.

14. figure 9e+f: Please use different linestyles. The lines are barely distinguishable at the moment and it would be impossible for a colorblind reader.

    We now show model runs with increased wind stress with dashed lines. Lines are much more distinguishable. Thanks for reminding us about this.

Minor comments/typos

What follows is a list of suggestions. The authors can take them or leave them.

1. line 29: Hughes’ name has a typo.

   Corrected.

2. line 41: “*Further*” → “*In their setup, further*”

   Thanks, we add this sentence.

3. line 50: Refrain from referring to a figure in a different paper! If the specific figure is crucial for the discussion then consider reproducing it here.

   Reference to the figure has been removed.

4. line 57: “*periodic*” → “*zonally reentrant*”

   Corrected

5. line 66: Use subscripts in math, e.g., L_x, L_y.

Corrected here and elsewhere.

6. line 74: Don't write, e.g., "1.10^{-4}"..., just write "10^{-4}". (Btw, why didn't you take $f_0 < 0$?)

   Corrected. An indeed, f0 is negative, so we have corrected the value

7. line 83: u_{10} = ... is erroneously repeated at the beginning of the line. Also I presume $u_0 = 10 \text{m s}^{-1}$ should be U_0 =...

   Thanks, corrected.

8. line 84: Delete repeated "*formulation*". Also, why not writing the formulation for wind stress; it's just a single line equation?

   Corrected. The Large and Yeager equation is not a single line expression, since it includes polynomial expressions for the drag coefficient, so we prefer not to write the wind stress formulation.

9. line 117: Section 2.3 reads a bit weird at this point. Perhaps I'd suggest you discuss the vorticity balance further down when you are about to show the results of figure 10.

   We are not comfortable in introducing the vorticity balance in Section 3.2C so we would prefer to let it in the methodology section. We have modified the text and hope it reads less weird now.

10. line 119: "*The* ***time-mean*** *BV equation...*"?

    Correct. We rephrased.

11. line 120: (a) p_b needs a subscript; (b) use "·" and not "." For inner products; (c) refrain from putting parentheses around a single variable. C5

    This has been corrected.

12. line 121: "β the derivative of planetary vorticity" → I suggest defining this when it first appears further up.

    This has modified. Thanks.

13. line 121: "V *the integrated* ***time-mean*** *meridional vorticity*"?

    Corrected

14. line 147: “*steady and turbulent*” → “*time-mean and transient*”?

Corrected.

15. line 154: “$1/(2 \cdot 10^{-4}\ m^{-1})$” is a pretty convoluted way to say “*5km*”.

Corrected

16. line 310: Nadeau & Ferrari (2015).

Corrected thanks.

17. figure 5: The figure’s quality is very poor. It only consists of lines, so the authors should be able to export it as a pdf/eps. Or, if they insist on using png/jpg, then I suggest they use higher dpi. Furthermore, please add a remark in the caption that the z-scale is not uniform. Also, consider reducing the y-limits of panels c) and f) down to only −2500m; there is nothing to be shown below that depth.

As mentioned above, figures were saved in pdf, with high quality. But their inclusion in the word documents degraded their quality. We will take good care, if the manuscript is accepted, that published version will respect the high quality of our figures.