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Ocean Science
Discussions

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 #1

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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 idealised, 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.

Major points

These need to be addressed by the authors.

1. general: Please number **all** equations.
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.
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").
4. line 88: This expression is completely different from that in Abernathey et al 2011. I believe (hope) this is a typo.
5. 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.

6. paragraph starting 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.

7. 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 200 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?

8. 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.

9. 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.

10. 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.

11. 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)."

12. 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.

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

14. 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.

15. 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?

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

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.
2. line 41: "*Further*" → "*In their setup, further*"
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.
4. line 57: "*periodic*" → "*zonally reentrant*"
5. line 66: Use subscripts in math, e.g., L_x, L_y.
6. line 74: Don't write, e.g., "1.10^{-4}"..., just write "10^{-4}". (Btw, why didn't you take $f_0 < 0$?)
7. line 83: $u_{10} = ...$ is erroneously repeated at the beginning of the line. Also I presume $u_0 = 10m\,s^{-1}$ should be $U_0 =$...
8. line 84: Delete repeated "*formulation*". Also, why not writing the formulation for wind stress; it's just a single line equation?
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.
10. line 119: "*The **time-mean** BV equation...*"?
11. line 120: (a) p_b needs a subscript; (b) use "$\cdot$" and not "." for inner products; (c) refrain from putting parentheses around a single variable.

12. line 121: "β the derivative of planetary vorticity" → I suggest defining this when it first appears further up.
13. line 121: "V *the integrated **time-mean** meridional vorticity*"?
14. line 147: "*steady and turbulent*" → "*time-mean and transient*"?
15. line 154: "$1/(2 \cdot 10^{-4} m^{-1})$" is a pretty convoluted way to say "*5km*".
16. line 310: Nadeau & Ferrari (**2015**).
17. figure 5: The figure's quality is very poor. It only consist 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.

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