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Interactive comment on "The impact of the planetary β-effect on the tilting vertical structure of a mesoscale eddy" *by* Shengmu Yang et al.

Anonymous Referee #1

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The concept explored by the authors is interesting, and worthy of eventual publication. Despite my enthusiasm for the topic, I have some substantial concerns that I believe must be addressed prior to publication.

1 Substantial issues:

1) The authors present several simulations as evidence to support their assertion that beta affects the tilting of the vertical axis of mesoscale eddies, and indeed these simulations do show differences when beta is included or excluded. However, the impact is not quantified, either through analytical scaling arguments (which would greatly im-

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prove section 2) or a numerical sweep of the parameter space. I would ideally like to see the inclusion of both a scaling argument and the results from multiple simulations in which beta is varied in a systematic way and its effect is quantised.

2) The impact of stratification is mentioned several times in the manuscript, and yet it is not quantified anywhere. Including a thermocline appears to suppress the tilting of the axis in the upper ocean (figures 10 and 11 show that most of the tilt appears below the thermocline). The simulation with a linear stratification is not described in much detail, but it seems as though the linear stratification reduced the tilt of the eddy axis (10 km tilt compared with 35 km when a thermocline was present). I would once again like to see some rigorous analysis and/or a series of simulations in which the stratification is systematically varied. The authors address this very briefly in final sentence of section 4 "... it is evident that the ocean stratification is an important factor in controlling the vertical structure of a mesoscale eddy" but I would like to see a more rigorous analysis.

3) Interactions with the bathymetry are mentioned several times in the manuscript, but there is no evidence that the authors have attempted to systematically address the influence of the sea floor on the tilting of the vertical axis. They show that using a realistic bathymetry map alters the tilt of the axis, but there is no indication as to why. Is it the slope perpendicular to the direction of travel? Is it because the presence of a slope forces the eddy to cross lines of latitude? An ensemble of simulations in which the direction and steepness of the slope are varied would go a long way towards answering these questions.

4) The description of their numerical setup is incomplete. Someone reading this manuscript has almost no chance of reproducing the simulations. For example, what form does a(z) from equation 4 take? What equation of state does the model use? Which advection scheme do they use? These are important numerical decisions that can substantially affect the results of numerical experiments, and the authors do not provide enough details for their readers to assess the results.

To summarise these concerns: the topic of the manuscript is very interesting, but the analysis presented lacks sufficient rigour to be published in its current form.

2 Minor concerns:

1) I found the paragraph encompassing lines 13-20 on page 3 to be extremely vague. As mentioned above section 2 would be greatly improved by the inclusion of a rigorous scaling argument showing the expected impact of beta, stratification, and bathymetry on the tilting of the vertical axis.

2) Page 7 lines 17-18: Why are the authors correlating vertical velocity and axis displacement? This statistical test lacks context. Furthermore, the authors present a correlation between vertical velocity and axis tilt as evidence to support their argument, and yet examining figure 11 clearly shows that the vertical velocity is non-zero at depths where the axis is still vertical. Given their argument that non-zero vertical velocities cause the axis tilting, this discrepancy should be discussed.

3) I found many of the figures difficult to interpret. I would appreciate longer, more descriptive captions.

4) Figure 1 is meant to show the asymmetry in the pressure field and the vertical motion. The asymmetry is adequately conveyed, but there is no way for the reader to learn anything about the vertical motion from what is plotted.

5) Figures 5, 7, 9, and 10 would benefit from a divergent colourmap – zero is an important value in these figures, and it would be good to make it clear.

6) Figure 13 – the authors describe the eddy's propagation as following a contour of ocean depth, yet the figure does not show any contours in the region of the eddy. The contour interval should be adjusted to show whether the eddy does indeed follow contours of the ocean floor.

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7) Minor numerical issues:

a. MITgcm is a z-coordinate model. It has levels, not layers. The references to model layers within the manuscript should be changed to refer to levels.

b. The vertical resolution of the model is quite coarse, especially in comparison to the horizontal resolution (see Stewart et al. (2017) for a discussion of vertical resolution in ocean models). The authors should either use a more appropriate vertical resolution, or justify their choice to use only 28 levels with such a fine horizontal resolution.

c. The authors should cite at least one of the papers describing the development of MITgcm, e.g. Marshall et al. (1997).

d. Using a bottom-drag of some sort would likely damp away the bottom intensified counter-rotating eddies.

8) A brief description of why anticyclones are much more prevalent in the South China Sea would allow a wider range of readers to engage with the manuscript.

9) The simulation on an f plane is not particularly informative. It should be an end member of a series of simulations in which beta is systematically varied, not an entire section.

10) Page 6 lines 16-18: what happens to the vertical velocity at the bottom? The text indicates that it increases with depth, but given the flat bottom it must be zero at the bottom.

11) Page 7 line 1: what baroclinic instability are the authors referring to here?

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3 References:

Marshall, J. C., Adcroft, A., Hill, C., Perelman, L., Heisey, C. (1997). A finite-volume, incompressible Navier Stokes model for studies of the ocean on parallel computers. Journal of Geophysical Research: Oceans, 102(C3), 5753–5766. http://doi.org/10.1029/96JC02775

Stewart, K. D., Hogg, A. M., Griffies, S. M., Heerdegen, A. P., Ward, M. L., Spence, P., England, M. H. (2017). Vertical resolution of baroclinic modes in global ocean models. Ocean Modelling, 113, 50–65. http://doi.org/10.1016/j.ocemod.2017.03.012

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