This manuscript addresses the fate of energy (mostly near-inertial energy) imparted by Southern Ocean storms and its effect on the Southern Ocean overturning circulation using a suite of semi-idealized high-resolution ocean models. The numerical experiments designed by the authors are suitable to address this important topic. Although the results on near-inertial energy generation, propagation and dissipation are somewhat expected, its effect on Southern Ocean overturning circulation is new. Furthermore, I haven't seen sensitivity studies of near-inertial energy budget to model numerics at such high resolutions before. Therefore I think this manuscript will make a useful and necessary contribution to this topic. I have some comments for the authors to consider, but most of my comments are minor in nature.

1. The manuscript talks about "energy imparted by mid-latitude storms". Well, it is not strictly correct. The synoptic winds associated with mid-latitude storms not only generate near-inertial waves via time-varying wind stress, but also contribute significantly to the time-mean wind stress at mid-latitudes via the nonlinear dependence of wind stress on the wind. For example, Zhai et al. (JPO, 2012) showed that when the synoptic winds are included in the stress calculation, the wind power input to the ocean general circulation can increase by as much as 70%.

Indeed, our model setup does not allow to evaluate the response of the ocean to modification of the time-mean wind stress by the storms : by construction the 10 year average wind stress is the same in the storms and no-storms simulations. This was a deliberate choice so we were sure that the sensitivity of the solution was not due at first order to changes in the time-mean wind stress. One another side, our focus is not only on the near-inertial energy input (as you raised/questioned below, a substantial part of the storm energy input also feed the balanced circulation). For this reason, we choose to maintain the initial version of the title. Nevertheless we add the following sentence in the abstract in order to clarify the scope of our study : ¨The forcing strategy ensures that the time mean wind stress is the same between the different simulations so the effect of the storms on the mean wind stress and resulting impacts on the Southern Ocean dynamics are not considered in this study.¨

2. Recently, Rath et al. (JGR, 2013) found that accounting for the ocean-surface velocity dependence of the wind stress leads to a large reduction of wind-induced near inertial energy of approximately 40% in their 1/10 degree Southern Ocean model. This relative wind damping effect seems to be a very important way of dissipating near inertial energy in the ocean. When you force your model directly with wind stress that knows nothing about the surface ocean currents, this relative wind damping effect is absent. I think this issue should at least be discussed, given that this manuscript is about energy dissipation.

The following paragraph has been added to the discussion section (7.2 Energy pathways) : ¨Using a 1/10° model of the Southern Ocean, Rath et al. (2013) found that accounting for the ocean-surface velocity dependence of the wind-stress decreases the near inertial wind power input by about 20% but also damps the ML near-inertial motions leading to an overall ~40% decrease of the ML near inertial energy. Overall, this damping effect is found to be proportional to the inverse of the ocean-surface-mixed-layer depth. In our set of simulation, we do not include any wind stress dependence on ocean-surface velocity which remains a debated subject (Renault et al. 2016). Our main motivation for doing *so was to ensure that the mean wind stress remains the same between the different model experiments that have been performed in this study. Nevertheless, we should keep in mind that we miss a potentially important dissipative process for the NIWs.¨*

3. line 19-20 on page 3. Should be "2000 km long" and "3000 km wide".

Corrected.

4. line 26 on page 3 and in many other places in the manuscript. It should be "3x104".

It has been corrected here and elsewhere.

5. The last sentence on page 3 needs to be rephrased. It is not clear to me what you want to say here.

This sentence has been rephrased as follows : "Our horizontal resolution ≥ 1km and the hydrostatic approximation used to derive the model primitive equations do not permit the proper representation of upward radiation and breaking of internal lee waves (Nikurashin et al., 2011). Nevertheless, the deep flows impinging on bottom irregularities generate fine-scale shear which enhances dissipation and mixing close to the bottom, as generally observed in the Southern Ocean (Waterman et al. 2013). "

6. In the model configuration section on page 4, there is no information about the existence of the background wind forcing without storms at all. It should not be left completely in the appendix.

The description of the background windstress has been removed from the appendix and included in Section 2.1.

7. line 21 on page 4. Brackets are needed for "2012".

Corrected.

8. page 6. (2) also includes diffusive energy transport in the vertical direction, not just energy dissipation?

The energy transport by diffusion is removed by the vertical integration.

9. line 12 on page 7. The average KE exceeds......, but the caption of Fig. 6a says "EKE"?

This has been corrected in the text.

10. line 1 on page 9. What is WKB stretched CW and CCW? Need to explain it or at least point the readers to the appendix.

We now refer to appendix B.

11. line 21-22 on page 9. Why does the storm lead to strengthening of the eastward current?

This strengthening is due to the position of the mean eastward current which is not symmetric with respect to y=1500 km but intensified on the northern portion of the domain (as in Fig. 2 of Abernathey et al. 2011), so the domain average additional zonal wind work imparted by the storm is nonzero and positive. This is now mentioned in the manuscript in Section 4.2.

12. line 25 on page 9. The Coriolis force is perpendicular to the current, and therefore should not do any work?

Indeed, the contribution of the Coriolis force to energy budget should be zero. But errors due to the staggering of the Arakawa C grid turn its contribution to the kinetic energy balance nonzero (but very weak compared to the other term of energy equation). This is now mentioned in the manuscript (Section 4.2).

13. line 17 on page 10. The authors should check what these papers say before citing them here.

We retained the references to Garrett 2001, Blaker et al. 2012, Komori et al. 2008, but indeed, we found the references to Zhai et al. 2005 and Zhai et al. 2009 not adequate and we remove them. We also add a reference to Zhai et al. 2004 (GRL) on advective spreading of NIW that is the one we should have cited first.

14. line 21 on page 10. You need to explain how the strength of the storm activity is varied seasonally here, or at least point the readers to the appendix.

We add the following sentence : "The seasonality of the storms is included by seasonally varying the maximum wind stress of the storms from 0.75 N m^{-2} in austral summer to 1.5 N m^{-2} in austral winter (see details in Appendix A)."

15. line 31 on page 10. Again, why does the mean KE increase in response to the storms?

Again, this is because the mean ACC is not centered at y=1500km and not symmetric.

16. line 7 on page 11. How do you define the mixed layer ?

The mixed-layer depth is computed with a fixed threshold criterion (0.2°C) relative to the temperature at 10 meters. This is indicated in the caption of Figure 2.

17. line 23 on page 13. The effective frequency should be "f+/2", according to Kunze (JPO, 1985)?

We used the effective frequency as defined in Kunze et al. 1995 that was describing inertial oscillations from a reference frame rotating with a geostrophic flow with relative vorticity ζ . Since our reference *frame is only rotating with earth you are right : the correct definition of the effective frequency is* $f+\zeta/2$ *(Kunze, JPO, 1985). This has been corrected in the text and the figure has been updated.*

18. line 12 on page 17. Should be “easy comparison”.

This has been corrected.

19. line 22 on page 17. I assume that changes in air-sea fluxes are due to the feedback term in surface heat flux forcing? If so, need to say it.

Yes, the changes in air-sea fluxes are due to the feedback term. This is now mentioned in the manuscript as follows : “The change in the air-sea fluxes is due to the feedback term that act to restore model SST toward a SST climatology.”

20. lines 13-14 on page 19. It is not clear to me why you conclude that your results are noticeably different from the previous two studies, since your results also show that the majority of energy imparted by the storms is dissipated within the top 200 m.

We agree that the overall conclusion is coherent with previous studies. This sentence has been rephrased as follows : “Our results are in good agreement with these studies: ...”

21. line 18 on page 19. What is that part of wind work that is not near-inertial wind work? Wind energy input to the surface Ekman currents?

The part of the wind work which is not near-inertial feed the subinertial and mean circulation.

22.Figure 5. Three different color maps are used. Is this necessary? Why is 17 days after the passage of the storm is chosen? Is it to give enough time for the near-inertial waves to reach the base of the anticyclonic eddy?

We choose three different colormaps to help distinguish the different fields : horizontal velocities, vertical velocities, shear. Yes we choose 17 days so it gives enough time to the waves to reach the base of the anticylonic eddy (as suggested by Figure 8b). This is now mentioned in the figure caption.

23. Figure 8. a) and b) are plotted differently (one on log scale and one not), which makes it hard to compare them quantitatively. In the caption for c), the symbol “< >” appears messed up on my printed-out version.

Figure 8a and 8b are now both plotted in log scale. The <> symbols have been corrected.