Review of

"Role of eyewall and rainband eddy forcing in tropical cyclone intensification." (acp-2018-610)

by Ping Zhu, Bryce Tyner, Jun A. Zhang, Eric Aligo, Sundararaman Gopalakrishnan, Frank D. Marks, Avichal Mehra, and Vijay Tallapragada

This study introduces an in-cloud turbulent mixing parameterization for the HWRF model. The rationale for this parameterization is that the classic HWRF PBL parameterization scheme does not account for intense mixing in eyewall/rainband clouds. The authors admit their scheme is a rather crude approximation of mixing, but it seems to help with producing better hurricane intensity predictions.

This is a promising study. Under the premise that the results are not cherry-picked, the improvements are quite astonishing. However, there are a number of issues that should be addressed to improve the manuscript. One of them is some amount of carelessness when describing the results and the figures. This and other issues are detailed below.

General Comments:

1. One of the weaknesses of this study is that the authors do not discuss why the eddy forcing would be responsible for TC spin-up. There are some hand-wavy arguments about interactions between the turbulence and microphysics but the reader is left in the dark with what's actually going on.

2. How does this work relate to the LES hurricane studies by George Bryan (or the LES work of the first author)? My recommendation is to relate this work to previous TC studies that employ an LES approach.

3. Show aggregate statistics of how much improvement the turbulence parameterization yielded. Even though the authors present more than just a case study, there is no mention of the results from all their simulations. If these aggregate results were included, there would be less suspicion about "cherry picking".

4. There is no discussion of how large the eddy exchange coefficient of momentum, K_m, should be (see also comment XX below). Can't you compute K_m from your prior LES work and compare it to the values you get from the parameterization?

Specific Comments:

1. The title is misleading. Given this title, I'd expect a more quantitative study on the turbulent processes and their roles, but the actual manuscript is more about describing and applying the turbulence parameterization.

2. Page 6: "But cumulus schemes are not designed to account for the eddy forcing to the momentum, heat, and moisture budgets but rather serve as a means to remove the convective instability generated by the large-scale flow and alter the thermodynamic structure of the environment based on the parameterized convective fluxes and precipitation (Arakawa and Schubert 1974; Wu and Arakawa 2014)."
   —> This is not true for the CLUBB scheme (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015GL063672).

3. Page 12: "In contrast, "TL-HWRF" produces a well-defined closed ring around the storm center that is clearly shown in both dynamic (vertical velocity, Fig. 8b) and thermodynamic (hydrometeor mixing ratio, Fig. 8d) fields."
   —> Actually, none of the panels in Fig. 8 show a closed ring (although the inner core is much more defined in the TL-HWRF runs). Furthermore, the comparison between observations and model (Figs. 7 and 8) is subjective, hand waving and and does not add anything of substance.

4. Page 13: "Comparing Fig. 11b with Fig. 10b, it is easy to see that the model-resolved eyewall eddy forcing above the PBL in the "TL-HWRF" experiment has a magnitude about 5 times larger than the corresponding SGS eddy forcing, suggesting that the resolved eddy processes provide a major forcing that drives the primary circulation of the TC vortex in this case."
   —> At first look this contradicts the overall statement that SGS turbulence is important. The authors should comment on this apparent contradiction.

5. Page 14: "other 4 major hurricanes" —> four other major hurricanes

6. Page 14: As another example, Figure 13 compares the satellite observed vortex inner-core structure of Harvey (2017) with the simulated ones by the two HWRFs during the early and middle stages of Harvey's RI. The asymmetric rainband structure, the closed ring feature around the storm center, and the size of the convective ring shown in satellite observations are reasonably reproduced by TL-HWRF."
   —>Subjective and hand wavy. For a better comparison, the panels should at least be plotted on the same lat/lon domain.

7. Page 14: "one may concern about" —> one may be concerned about

8. Fig. 3: Why is there no sign of surface friction?

9. Fig. 4e,f: I'm curious, why is there no indication of a melting layer in the reflectivity plots?

10. Fig. 5: Why are the K_m values 2 and 5 km so much larger than at the surface? (this observation is based off the colorbar range, which goes from 0-80 in Fig. 5a, but from 0-300 or more in Figs. 5b, c).