General comments:

I appreciate much of the authors work on the revision and the paper has improved. However, there are still a few items that were not addressed and the study still requires major revisions.

A simple google search of "impacts of dynamic cores on weather" reveals two studies on the impacts of dynamic cores on non-hydrostatic weather. These studies should be discussed in the paper as they are very relevant:

(1) Guimond et al. (2016), The impacts of dry dynamic cores on asymmetric hurricane intensification. This is a theoretical study that documents differences in hurricane intensification from inner-core asymmetries generated from heating (e.g. non-hydrostatic effects) due to dynamic cores. The authors analyze the physical and numerical reasons for these differences and is relevant to the present study.

Guimond, S.R., J.M. Reisner, S.M. Marras and F.X. Giraldo, 2016: The impacts of dry dynamic cores on asymmetric hurricane intensification. J. Atmos. Sci., 73, 4661 – 4684.

(2) Gallus and Bresch (2006), Comparison of impacts of WRF dynamic core, physics package, and initial conditions on warm season rainfall forecasts. This is an applied study that documents differences in the simulation of warm-season rainfall due to the choice of WRF dynamic core (either ARW or NMM) and physics package, among others. The interplay between the physics and dynamics is discussed and should be relevant to the present study although the grid spacing for this paper was coarse (~ 8 km).

Gallus, W.A., Jr., and J.F. Bresch, 2006: Comparison of impacts of WRF dynamic core, physics package, and initial conditions on warm season rainfall forecasts. Mon. Wea. Rev., 134, 2632 – 2641.

Regarding the convergence study, it would be too much to run 250 m simulations for all (or a group) of the models, but the request for a 250 m simulation for one or two of the identified models (CSU, NICAM, ICON, GEM, TEMPEST) shouldn't be asking too much given the large number of authors on the paper that are available for assistance. In addition, the simulations are for short times (120 min), which should help. I appreciate the authors work on this and I think it will help to address the spread observed in the models listed above.

Regarding the integrated measure of supercell intensity, I don't consider maximum vertical velocity a good choice because it is a point value that is highly sensitive to "noise" in the simulations and doesn't really reflect a bulk (or "storm-wide") measure of intensity. Figure 4 already showed snapshots of vertical velocity so we can already see the impacts on this field. Figure 6 shows area-integrated precipitation rate, which is a good global metric to show, but the authors need a global metric for kinetic or total energy in Figure 5. I don't see a problem calculating this: all models get the same initial conditions and supercell storms should be producing significant horizontal and vertical winds, you can focus in on the storm and integrate over a smaller domain or remove a filtered field from the total fields to focus just on the storm perturbation energy.

Specific comments:

“This result is likely due to the differences in explicit diffusion treatment as noted before, as well as differences in the numerical schemes’ implicit diffusion, particularly given the large impact of dissipation on kinetic energy near the grid scale (Skamarock, 2004; Jablonowski and Williamson, 2011).”

--Reference (1) noted above (Guimond et al. 2016) also discuss the role of explicit and implicit diffusion in structural differences in non-hydrostatic weather and is relevant to the statement above.

“It is also hypothesized that differences in the coupling between the dynamical core and subgrid parameterizations may lead to some of these behaviors (e.g., Staniforth et al. (2002); Malardel (2010); Thatcher and Jablonowski (2016);”

--Reference (2) noted above (Gallus and Bresch 2006) also discuss this issue and is relevant to the statement above.