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Interactive comment on "DCMIP2016: The Splitting Supercell Test Case" *by* Colin M. Zarzycki et al.

Anonymous Referee #2

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General comments:

The authors present an idealized test case (supercell storm) for global atmospheric models and briefly discuss the results from a large set of models that participated in a model comparison project (DCMIP2016). The premise of the study is good and useful. Demonstrating variability in the simulation of atmospheric phenomena due to differences in the dynamic core is an important topic and one that is often overlooked by the science community. The methods, analysis and even the figures are almost identical to a paper by Klemp et al. (2015), so the paper adds nothing new in this respect. The usefulness of the paper comes from the presentation of results from a large and diverse set of models. Despite this, the paper needs significant revisions to improve the presentation and additional analysis/simulations are needed to make the

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results clearer.

The introduction was disappointing. No literature review is done on the impacts of dynamic cores on atmospheric problems such as extreme weather events. There are recently published papers on this topic that should be cited. In addition, more discussion is needed on the motivation for the study. Why do we need to compare dynamic cores? Do we expect significant differences and if so, do these differences explain some of the uncertainty observed in forecasts of extreme weather?

The results of the resolution sensitivity study don't appear to be converged at 0.5 km for several models. For example, there are significant differences in one or more fields going from 1 km to 0.5 km resolution for the following models: CSU, NICAM, ICON, GEM, TEMPEST. The authors should show a 0.25 km resolution simulation for one or two of these models to demonstrate better convergence properties. In addition, shouldn't we expect that as the grid spacing becomes very small, the model solutions and bulk statistics should be fairly similar across models? This is not the case with the current results so they are likely far from converged. Some discussion and additional example tests (as described above) are needed.

Specific comments:

Page 1, line 8; What is meant by "physics-dynamics coupling"?

Page 1, line 11; What is the difference between convective-permitting and convective-allowing? These mean the same thing to me.

Page 2, sentence starting with "It is based on the work of…"; Sentence doesn't read well and needs a re-write.

Page 2, line 15; Need few sentences on what a "reduced-radius sphere" is and what it intends to represent. Is the use of a reduced radius sphere a good approximation to true radius, global dynamics? Why do you need to simulate a supercell to study the performance of a global model? Wouldn't a global phenomenon (with non-hydrostatic

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processes) be better suited for studying the performance of a global model? How many horizontal grid points are used in the reduced radius simulations?

Section 2.1; The notation in this section is confusing. What is Ueq a function of , z? If Ueq is the zonal velocity at the equator, then isn't u = Ueq? I also don't understand the transition to defining ubar after u. This whole section needs to be described better.

Page 3, line 1; This can't be gradient wind balance because no Coriolis force is shown in the equation.

Page 4, line 7; what is machine epsilon? Are you referring to machine precision?

Page 5, line 3; Why is the warm bubble hydrostatically balanced? The introduction and abstract highlight the importance of non-hydrostatic processes for testing global models, so this doesn't make sense. The vertical accelerations should be fairly small for this warm bubble relative to the supercell that is simulated with very large vertical velocities. The balance adjustment is a physical process that should be tested in the models.

Page 5, line 13; Should say "second-order diffusion operator with a constant sub-grid scale viscosity (value) is applied...". Are you using the same values in the horizontal and vertical dimensions? Is that appropriate for the grid cell aspect ratio?

Page 5, sentence starting with "ICON applied. . ."; This doesn't sound like a departure from the default described above. Also, is diffusion really applied to the mass continuity equation (rho) in ICON? This is not typically done.

Section 3.2; This whole section is very subjective with no analysis to back up any of the claims. In order to comment on the reasons for the differences in the model runs, some analysis is needed.

Page 7, line 11; How do you know that this is noise? This statement is subjective and no quantitative analysis is done to back up this claim.

Page 7, line 15; This is a very weak statement and should be removed. What is meant by "coupling between the dynamical core and subgrid parameterization" and why would this lead to these differences?

Page 7, line 21; This is not a bulk, integrated measure of supercell intensity. How about showing the domain integrated kinetic energy or total energy?

Technical corrections:

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