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Geoscientific Model Development Discussions

# *Interactive comment on* "Influence of Bulk Microphysics Schemes upon Weather Research and Forecasting (WRF) Version 3.6.1 Nor'easter Simulations" *by* Stephen D. Nicholls et al.

**Anonymous Referee #1**

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General Comments Seven Nor'easter cases were simulated with five microphysics schemes in the WRF model. The simulations were nested with four levels from 45 km down to 1.667 km. The verification focuses on Stage IV precipitation and GFS (GMA) operational analyses. The schemes are also intercompared with metrics that include the microphysical species, tracks, intensities of the central low pressure, and an energy norm. The paper has some results of interest within what is clearly a large dataset of model output. They have found that the modeled nor-easters have a stronger tendency to look more similar to each other than to the analysis, and perhaps this is not surprising because microphysics changes alone are not likely to change the motions of these systems. However, this makes comparisons with analyses harder to do, and



difficult to gain much from if the purpose is to intercompare microphysics schemes.

This brings up my main issue with the paper which is whether we can evaluate microphysics schemes against analyses such as these in a useful way. The value of the paper is more in the intercomparison of microphysics than in how they compare with analyses. Errors in the forecast are dominated by other causes, such as the initial analysis error, considering that these are initialized 72 hours ahead of the precipitation events. Perhaps initializing closer to the event would have given more accurate representations that could be compared with analyses.

I especially am not convinced that the energy norm metric has been demonstrated to be useful. The results with that show no obvious signal and are surely dominated by position errors even though the norms were centered on the model and analysis storms. The wrong track gives different sea-surface temperatures, for example, making it expected that storm properties would not be comparable.

There are also aspects of the model set-up that I would criticize. It seems that the central 1.67 km domain is at the same position for all storms, and this means that some storms pass through it while other would miss it and only be resolved in the 5 km domain. This makes for a complication in the comparisons too, especially between storms, or depending where on their track that they are verified.

Specific Comments

1. line 141. What are the perturbations relative to, the GMA analysis? This is not stated.

2. Section 3.2. It is not clear what area these results and Table 4 are for. It also seems that much of this would be in the 12 km domain where there is a cumulus scheme, and part is in domains 3 and 4 where there isn't.

3. line 208. WRF's common heritage with GFS is implied. I don't think there is much common physics heritage except for some relationship in the land-surface scheme.

What is meant here?

4. Abstract does not mention that there are seven cases and five microphysics schemes and has nothing on the energy norm. It is not adequately describing the work carried out.

5. line 234. What is meant by saturation heights?

6. line 236. cloud water? This should probably be cloud droplet number concentration?

7. line 241-246. Without knowing where the freezing level is, it is difficult to follow this discussion. How much of the cloud water is supercooled?

8. line 279. How does lack of a sedimentation term lead to low cloud ice? I thought sedimentation should reduce cloud ice extent and lifetime.

9. line 282. 'assumed water saturation'. What assumption is made about water saturation in a purely ice process?

10. Figure 7 (vapor) would have been better presented as a difference from analysis. Nothing can be seen with this plot as it is.

11. Section 3.4. It is hard to interpret what is meant by lowest energy norms and the metrics in Table 5 in general. Also make clearer what is meant by model-relative and GMA-relative norms.

12. As mentioned in the general comments, I do not think the energy norm statistics are adding anything useful to the paper. It would be better and more focused without this. There are so many factors that could make one simulation look temporarily better than another, related to timing and structure developments, that using such a high-level bulk measure as this conflates too many things to be useful in such an intercomparison.

13. line 334. Regarding the low-level jet which case is being referred to? Can it really be inferred from the v component of the energy norm that this jet is the cause? This looks highly speculative.



14. Generally I think the microphysical comparisons are the useful part of the paper and some effort has gone into explaining the results in terms of differences between the schemes. The caveat I have here is that the nesting locations make it unclear whether we are looking at the behavior at 1.67 km or 5 km, and it looks like it must be a mixture. This is a drawback of the methodology.

Technical Corrections

1. line 217. It is not stated that this is Case 4.

2. line 175 and 178. It seems that Figure 1 is not the correct one to refer to here?

3. line 200. BPMs

4. line 256. Extra 'and'.

5. line 320. Should it say WSM6?

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