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Comment on gmd-2021-417

Anonymous Referee #2

Referee comment on "Simulated microphysical properties of winter storms from bulk-type microphysics schemes and their evaluation in the Weather Research and Forecasting (v4.1.3) model during the ICE-POP 2018 field campaign" by Jeong-Su Ko et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-417-RC2>, 2022

Review of "Simulated microphysical properties of winter storms from bulk-type microphysics schemes and their evaluation in the WRF (v4.1.3) model during the ICE-POP 2018 field campaign" by Ko et al. 2022

Recommendation: Could be acceptable for publication following mandatory major revision

This study evaluates the performance of four bulk microphysics schemes for the simulation of snowstorms during the Pyeongchang 2018 Olympics and Paralympic Games. The analysis compares the amount of ice, snow, precipitation and cloud water predicted by the various schemes, and attributes the processes giving the production of the relevant hydrometeor categories. They conclude that melting is key for generating rain, and the bias in precipitation for war-low and cold-low biases can be mitigated through the use of inefficient melting in all schemes. Although I think the paper makes a contribution by testing these microphysical schemes in new meteorological situations, there are aspects of the presentation that should be improved before the paper is accepted for publication.

MAJOR COMMENTS

- I found that the introduction was not overly focused. Although the stated goal of this study is to evaluate the performance of the microphysical schemes in the simulation of wintertime precipitation, much of the introduction compared how these schemes have previously been used to simulate convection. There should have been more focus on how the use of these schemes has been evaluated in simulations of wintertime storms, and also the understanding that has been gained from past observational and modeling studies of winter storms should be highlighted. There were many past studies of winter storms that were not referenced.

- I am concerned about the resolution of the model that is being used as the highest horizontal grid spacing is 1 km. A lot of the convection and generating cells that commonly occur in winter time storms are on scales of much less than 1 km. Thus, the model is not able to represent well the spatial scales on which the evolution of these storms is occurring. This limitation should be clearly acknowledged (or run at finer resolution) and explain the caveats with the interpretation of this study because of this resolution difficulty.

- The authors use four different microphysical schemes in their investigations, but do not use some of the most state-of-art microphysics schemes in their simulations. Why is this? For example, the P3 schemes (Morrison and Milbrandt 2015), Predicted Particle Properties scheme is the next generation parameterization scheme that uses a very different approach for representing ice, and would offer an interesting complement to the schemes that are presented here. It predicts bulk properties rather than predicting separate species, which eliminates unphysical conversion processes between traditional ice categories, and hence can be used for giving a better comparison against observations.

- For the most part, the writing in the manuscript where the results of the different

simulations and comparisons against observations is performed is overly qualitative. Terms such as "overestimate", "matches well", "substantial amount", "abundant", "insignificant", "similar" and others are used excessively, with little information on what this actually means. The paper would read more clearly if these descriptions were made more quantitative with appropriate reference to the figures. There are a few places where this is done (e.g., approximately 10 times larger, magnitude is 5.5 g/kg), but this could be done much more effectively.