

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## **Comment on acp-2021-404**

Anonymous Referee #2

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Referee comment on "Evaluation of modelled summertime convective storms using polarimetric radar observations" by Prabhakar Shrestha et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-404-RC2>, 2021

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### **General comments:**

This study utilizes ensemble Terrestrial Systems Modeling Platform (TSMP) simulations with forward dual-pol radar operator to evaluate the performance of simulated cloud microphysical processes for three summertime convective storms over northern Germany using bias corrected X-band radar observations. The paper presents some interesting results and contributes to scientific community in this field. However, the methodologies (section 2 and 3) needs to be more clearly written and reorganized to be published. In addition, there are many grammatical errors and typos that require corrections. Thus, the reviewer suggests the manuscript to be reconsidered after major revisions are made with the following conditions.

### **Major comments (scientific questions/issues):**

- 1) The abstract is too general and does not provide a solid conclusion based on the study results. Please revise the abstract.
- 2) The authors introduce many notations in sections 1 and 2 without proper explanation or actual use of the equations. e.g. backscatter differential phase ( $\Delta Z$ ), aerosol size distribution ( $R_2$ ), the logarithm of its geometric standard deviation  $\log(\sigma)$ , solubility ( $\epsilon$ ), aspect ratio (AR), width of canting angle distributions ( $\sigma$ ), etc.

Please provided proper equations to these notations.

3) In sections 1 and 2, authors cite too many online references or unpublished (not peer-reviewed) articles. The reviewer is skeptical with some of the research results mentioned in the paper. Please correct them or update them to more recent peer-reviewed papers.

4) This study uses TSMP to study the evaluation of modeled summertime convective storms using polarimetric radar observations. However, in sections 3.1 and 3.3 the paper discusses about land cover types and root zone soil texture without any context. Based on the three cases which are only for short convective periods, the reviewer finds little purpose of the coupling nor its relation to dual-pol study.

In addition, lines 201-205, evaluation of streamflow and discharge in the model has little to do with the research purpose. Again, section 5.2 river discharge serve little purpose in the paper. The locations of streams are not even shown in Figure 3, which makes it impossible to understand the results.

The reviewer suggests either to change the title and research purpose or delete the above mentioned descriptions.

5) Section 4.1 discuss synoptic situations of three cases but the reviewer finds it difficult to follow the descriptions. Some figures depicting synoptic conditions are needed.

6) The description in lines 260-262 is incorrect. Observations do not gradually increase as modeled CAF. In figure 4, there is no explanation of what (a), (b), (c) are. Further, the snapshot time periods (boxed area) between observations and model runs are different. The paper does not mention this issue or provide reasons of different time period selection. "Optimal" is not enough.

7) In sections 5.3.1, 5.3.2, and 5.3.3, there are many leaps in steps and logic in terms of how locations, elevations, and time is determined for analyses (figures 5 – 10). How can you compare the radar signatures with model simulations when the locations, elevations, and time periods are different? The range plots in Fig. 5 and 6 is also different and very difficult to compare. In Fig. 9 why is 8.2 degree elevation used?

8) Captions in figures 2, 4, 5-10 need to be improved. For instance, box plots of ensemble members in Fig. 2 is not explained well.

9) The first sentence in section 6 (discussion) is improperly placed. Further, in order to address the change in IC/BC and its influence on the simulated cases, the experiment needs to be conducted on the same cases. But, this paper does not. Thus, lines 424-429 should be deleted.

**Minor comments (technical corrections):**

1) There are many acronyms used in this study without spelling them out properly (e.g. EMVORADO, DE, GME, JUWELS, a.g.l., etc.

2) There are many typos and unnecessary use of "e.g." used in this paper. Please correct them.

3) L59: There are many more recent X-band study results that are published. Please include them as references.

4) L75: Runge-Kutta is not a "dynamical core" but "numerics" to solve PDE equations.

5) L90: No à No\_x

6) L175: The two X-band radar used in this study is calibrated based on GPM DPR (Ka band). However, there are many literatures that show ground based and airborne radar signatures are much different in characteristics and have lots of biases. Please provide some evidence of how DPR can be used as reference and not the other way around.

7) L224: How does anticyclonic rotation of the warm front produce the necessary lifting mechanism?

8) L231: Between "ensemble average." and "For the first case," there needs to be a sentence explaining Figure 2.

9) L237: northwestern domain à northeastern domain

10) L278: How can you tell where the melting layer is based on Fig. 5a. Explain in more detail.