

Interactive comment on “Sensitivity of mixed-phase moderately deep convective clouds to parameterisations of ice formation – An ensemble perspective” by Annette K. Miltenberger and Paul R. Field

Anonymous Referee #1

Received and published: 6 July 2020

Recommendation: Major Revision

This work addresses the topic of sensitivity of fine-scale model predictions of convective clouds to different representations of ice formation and to initial conditions uncertainty. Mean hydrometeor profiles as well as bulk cloud field properties are examined for one convective case. The manuscript addresses a very interesting and up-to date topic. However, it suffers from the fact that it is in a way a “Part 3” of other manuscripts (Miltenberger et al., 2018a and b). The readability can be increased by adding information about the meteorological situation, the microphysical parameterisations and

C1

their prediction on the tested case. Some explanations are missing. A damaging point is that we do not withdraw physical information on the validity of microphysical tests. The discussion could also be strengthened a bit on the possible generalization to other meteorological cases from this case study. Therefore, the manuscript requires major revision prior to acceptance for publication.

Major comments:

1. In general, this manuscript heavily depends on Miltenberger et al., 2018a and b of aerosol-cloud interactions in mixed-phase convective clouds. However, it should also be readable without the knowledge of the contents of Part 1 and 2. Therefore, I suggest that the authors give more information about: - the case study and the measurements used to validate the control simulation - the CASIM microphysical scheme : 2-moment scheme for all hydrometeor species, method of aerosol initialization - the physical configuration of the simulation: turbulent scheme, subgrid condensation scheme or not, radiation scheme I understand that it is not necessary to give all the information presented in the 1st paper but the reader should not be forced to seek all the information in the previous articles.
2. The seven heterogeneous freezing parameterisations introduced in this paper are not sufficiently presented. The main differences between DM10 (used as the control simulation) and M92, A13, DM15, N12 and T13 need to be explained.
3. It is said that DM10 successfully captures many features of the observed cloud and precipitation, but what about the other FSENS experiments? A figure like Fig.2 of Miltenberger et al. (2018b) applied to FSENS is necessary. It would be interesting for instance to see if NoHM modifies significantly the spatial pattern of clouds and surface precipitation.
4. Explanations about the cloud condensate budget used in Khain (2009) and Miltenberger et al. (2018a) presented in Fig.4 are missing.

C2

5. In the discussion, the authors have anticipated the question whether this dominance of initial condition uncertainty is a special feature of the chosen case. We understand that it is difficult to answer on the basis of this study. But they may try to discuss about the extension of the analysis to other cloud types and meteorological scenarios.

No minor comment : The text is well written and figures are clear.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-253>, 2020.