Author Response to Reviewers for:
Sensitivity of precipitation formation to secondary ice production in winter orographic mixed-phase clouds

Zane Dedekind[1], Annika Lauber[1], Sylvaine Ferrachat[1], and Ulrike Lohmann[1]
[1]Institute of Atmospheric and Climate Science, ETH Zurich, Switzerland

We sincerely thank Sylvia Sullivan for the constructive feedback. Her suggestions considerably improved the quality of the manuscript.

Below we present a detailed response with the reviewer comments in black, our responses in blue and additions to the manuscript in blue italics.

1 Reviewer 1: Sylvia Sullivan

Technical Corrections

1. Line 82-84: Reword to avoid using "surface" so much? However, this parameterization is independent of the surface concentrations of hoar crystals and including temporal variations in these surface concentrations could improve the parameterization accuracy.

   AR: Thank you. We added the suggested sentence.

2. Line 157-163: I still have a bit of difficulty following this paragraph. Could lines 157-163 be rephrased as follows?
   "Although in-situ measurements of aerosol concentration were made at Weissfluhjoch (2670 m a.s.l), their temperature did not match the range of validity for the DeMott et al. 2010 INP parameterization. Hence, an aerosol profile was derived from retrievals of the upward-pointing LIDAR at Davos Wolfgang."
   Then I would start a new paragraph from "The freezing of all cloud droplets.." to clarify that the discussion changes from aerosol to other microphysical process descriptions.

   AR: Thank you for the suggestion. The suggestion is a summarized version of lines 157-165. However, the full description of capturing the aerosol concentration is necessary for validating the aerosol concentration. Observations at Weissfluhjoch were made two days before (March 5, 2019) the cold frontal passage that we simulated. Therefore, we feel a brief description on the process is required.

Yes, we started a new paragraph where you suggested.

3. Line 189: Being pedantic...
   "ICNC-INP discrepancies have also been observed at temperatures"

   That could be said, but it is the secondary ice production (SIP) that causes the ICNC-INP discrepancy and, therefore, it is more suitable to use SIP in this regard.

4. Line 194: This is unclear. The only existing temperature constraint for this process is the maximum probability of droplet shattering at -15° C?

   We have adapted the sentence to be more clear:
   So far, no temperature constraint is known for this process to be active below 0° C

5. Line 272: delete "simulation"
   We have removed "simulation".

6. Line 276: change "here" to "there"
   We made the correction.

7. Line 304: For readability, I would break to a new paragraph here.
   We agree and added a break in the paragraph.

8. Line 321-322: Is this just a spin-up effect since the simulation starts directly at 9:30?
   Yes, especially between 7:00 and 10:00 UTC. We started the simulations at 7:00 UTC and three hours of spin-up are used to reach equilibrium. We added the spin-up effect to the results and modified the methods with the following correction:
   Methods: For this study, we simulate the cold front passage between 7:00 and 14:45 UTC and analyze the results between 9:30 and 14:45 UTC on March 7, 2019.
   Results: Three hours of spin-up was allowed between 7:00 and 10:00 UTC, however, none of the simulations was able to correctly simulate the onset of the precipitation. All the simulations underestimated the amount of precipitation between 10:00 and 10:45 UTC (Fig. 6a).

9. Figure 6: Maybe restate the date here.. March 7, 2019
   We added the date to figure 6.

10. Line 342: "due to the"
    We modified the text accordingly.

11. Line 350-354: I did not understand this argument about the turbulence scheme (I don't think it was in the first version?) Does this 1D scheme mean you are underestimating subgrid scale variability in vertical velocity?
    This argument was already in the first version. Yes, your interpretation is correct. We added your interpretation to the text.

12. Would you say that collisional break-up may be relatively more important for regions with strong orography than without? Also in regard to the last point in the conclusions. Would be interesting to know if you think this is a takeaway of the study.
    Yes, this is a takeaway, and we will add it to the conclusions.
    During winter when the thermal heating is minimal, the orography plays a significant role in lifting air parcels to higher altitudes, enhancing large drop formation and primary ice nucleation. These conditions are favorable for graupel formation and can result in enhanced collisional breakup. Therefore, collisional breakup may be more important in regions of strong orography than without during winter.

13. Line 444: But also, as far as I know, there are very limited ICNC measurements with concentrations greater than 2000 L-1. So hitting this threshold often may indicate unphysical values of F_BR.
    Yes, the measured ICNC are limited to below 2000 L^{-1}. Typically the limit in COSMO two-moment cloud microphysics scheme is set to 500 L^{-1} to avoid larger unphysical ICNC (e.g. Henneberg et al., 2017; Sullivan et al., 2018; Eirund et al., 2019, 2020).

References

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