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Comment on acp-2021-781

Minghui Diao (Referee)

Referee comment on "Microphysical processes producing high ice water contents (HIWCs) in tropical convective clouds during the HAIC-HIWC field campaign: dominant role of secondary ice production" by Yongjie Huang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-781-RC1, 2021

Reviewed by Minghui Diao

Review of Huang et al. Microphysical processes producing high ice water contents (HIWCs) in tropical convective clouds during the HAIC-HIWC field campaign: dominant role of secondary ice production.

In this work, the authors use observations from the High Altitude Ice Crystals (HAIC)-HIWC experiment to evaluate WRF model simulations based on the P3 cloud microphysics scheme. Specifically, model performance for representing high ice crystal number concentrations (Nice) observed during convective activity is examined. Several model setups are investigated, including the default P3 microphysics scheme, P3 scheme with various horizontal grid spacings, and P3 scheme that includes several secondary ice production (SIP) processes, such as (1) the rime-splintering or Hallett–Mossop (HM) process, (2) ice–ice collision fragmentation (IICB), (3) raindrop freezing breakup (RFZB). The results show that the P3 scheme that includes all three SIP mechanisms (HM, RFZB, and IICB) provides the most comparable results to the observed Nice / ice water content (IWC) ratio and radar reflectivity. The dominant SIP mechanism at various temperature ranges is also evaluated.

Overall, the manuscript is well written. The model experimental design is straightforward and easy to follow. The reviewer recommends the manuscript being considered for publication after making the following revisions.

Major comments:

(1) The sensitivity tests conducted for WRF mainly focus on the inclusion of various SIP processes. There are other factors that potentially affect the distribution of IWC and Nice significantly, such as the threshold of relative humidity with respect to ice (RHice) used to initiate ice nucleation. As far as the reviewer knows, P3 scheme still uses the ice nucleation formulation of Cooper (1986), similar to Morrison double moment scheme. If so, the reviewer thinks that the P3 scheme may be subject to similar problems seen in previous WRF simulations of convective systems.

Previously, D'Alessandro et al. (2017) evaluated several double moment schemes (i.e., Morrison 2-moment, Thompson 2-moment, and Thompson-Eidhammer aerosol aware scheme) in WRF model, and Diao et al. (2017) evaluated the Morrison 2-moment scheme in the NCAR CM1 cloud-resolving model (similar to WRF model). Both studies showed that the Cooper parameterization used to initiate ice nucleation has a RHice threshold that is too low (D'Alessandro et al., 2017, doi:10.1002/2016JD025994; Diao et al., 2017, doi:10.1175/JAS-D-16-0356.1). When the Cooper parameterization is used in Morrison 2-moment, it does not allow clear-sky ice supersaturation to exceed 108%. When comparing WRF simulations against aircraft observations of anvil clouds during the NSF DC3 field campaign, such activation of ice nucleation at 108% leads to an underestimation of the occurrence frequency of ice supersaturation in both clear-sky and in-cloud conditions. Consequently, it allows ice nucleation to happen too early when RHice is still relatively low and leads to higher ice water content and ice crystal number concentrations than those seen in the observations.

The reviewer suggests testing the activation of Cooper 1986 parameterization at a higher ice supersaturation threshold (such as RHice = 125% or 130%). This would allow more clear-sky ice supersaturation to exist without turning into ice crystals too early. It would be interesting to see the impacts of such revision compared with the inclusion of SIP processes.

(2) Following the first comment, evaluations of thermodynamic conditions (such as RHice) would be helpful besides the evaluation of Nice and IWC in relation to vertical velocity. It would be valuable to show the distributions of ice microphysical properties in relation to RHice with or without SIP processes included as well as how these simulated distributions compare with observations.

(3) The current study includes three SIP mechanisms, while previous literature pointed out other existing SIP mechanisms as well. If the current three SIP mechanisms already provide a similar amount of Nice as the observed value, wouldn't adding other SIP mechanisms in the future lead to Nice that is too high compared with observations? The reviewer suggests adding some discussions on this potential problem when more SIP processes are included in the WRF model.

(4) Since Nice is a key observed variable used to evaluate WRF simulations, what is the range of uncertainty associated with Nice, as it is affected by various potential problems such as shattering and poorly defined depth of field? Can the authors give a range of observed Nice at various temperatures by using a more rigorous versus a less rigorous quality control procedure?

(5) Can the authors elaborate on how liquid droplets or raindrops are separated from ice crystals in 2DS and PIP measurements? The paper of Huang et al. (2021) briefly mentioned that "The two optical array probes, 2D-S and PIP, recorded 2D images of ice crystals nominally in the size range of 10–1280 and 100–6400 μm, respectively." But this statement is not entirely true because 2D-S and PIP can capture liquid phase as well.

(6) The analysis uses the size range (Dmax) of 0.1 to 12.845 mm, which is slightly higher than the range of 0.05 – 12.845 mm used in the companion paper (Huang et al., 2021). Is there a reason that a higher minimum threshold of Dmax (0.1 mm) is used in this work?

In addition, is the model output of ice microphysical properties (e.g., IWC, Nice, mass-mean diameter, etc.) re-calculated based on this partial size range in order to match the size range of the observations? A partial size selection procedure has been applied to model output as shown in previous studies, e.g., Fridlind et al. (2007), Eidhammer et al. (2014), Patnaude et al. (2021) https://doi.org/10.5194/acp-21-1835-2021, Yang et al. (2020) DOI: 10.1002/essoar.10504450.1.

Since the smaller ice particles dominate the Nice value, if the authors only evaluate the range of 0-0.05 mm (given that the observations at this range have large uncertainties), do the simulations show too many or too few ice particles at this size range? This size range (0-0.05 mm) is quite important because if the simulations already provide several orders of magnitude of higher Nice than observed Nice at 0-0.05 mm, getting a more similar Nice for > 0.05 mm by adding SIP processes can potentially lead to worse results when considering the entire size range of ice crystals. Alternatively, if the default P3 simulations provide smaller Nice at 0-0.05 mm compared with observations, it can be interpreted as either the observed Nice is overestimated due to shattering, or the default P3 simulations really underestimate Nice even at small ice crystal sizes.

Minor comments:

Line 19 – 22, some of the temperature ranges do not have the complete range. See suggestions in brackets: "… shattering of freezing droplets dominates ice particle production in HIWC regions at temperatures > −15C [temperatures between -15C and 0C??] during the early stage of convection, and fragmentation during ice–ice collisions dominates at temperatures > −15C [temperatures between â□□15C and 0C??] during the later stage of convection and at temperatures < −20C [temperatures between â□□40C and -20C??] over the whole convection period."

Line 218, "... interpolated to temperatures of -10, -30, …", can the author explain what kind of interpolation was done?

Figures 6 and 7 have two colors that are very similar in the legend, that is, HM in red and Others in fuchsia. Even though the colors are readable from a computer screen, when printed out on paper these two colors look almost the same. The reviewer suggests changing "Others" to some other colors, such as green, yellow or light gray.