**REVIEW FOR:**

**Multi-thermals and high concentrations of secondary ice: A modelling study of convective clouds during the ICE-D campaign**
*by Cui et al*.

This paper examines the mechanisms responsible for enhanced ice crystal number concentrations (ICNCs) in convective clouds observed during ICE-D campaign. Sensitivity tests are performed through variations in the primary ice nucleation scheme and the dynamic conditions to investigate their impact on secondary ice production through rime-splintering. Changes in the onset freezing temperature or the freezing efficiency of the nucleation scheme result in weak ICNC enhancement, not sufficient to explain observations. The highest concentrations are achieved by inducing a second instability in the domain to generate an additional thermal. The authors conclude that multi-thermals play a critical role in generating large ICNCs in tropical clouds.

**Major comments:**

The subject of this study is very interesting as the mechanisms responsible for the large ICNCs in convective clouds remain poorly understood, and especially those that concern secondary ice production. However, due to lack of detailed description of the sensitivity simulations it is not very easy to derive any robust conclusions. For example, the authors mention that Cooper parameterization is used in the Morrison scheme to treat primary ice production. However, as far as I know, this scheme utilizes different expressions for different freezing mechanisms (condensation, immersion, contact) and Cooper represents only one of them. Is this indeed the case with the particular model? If yes, then it doesn't make much sense to modify the freezing efficiency or onset temperature for only a single freezing parameterization (e.g. while the onset temperature for immersion freezing remains constant at -4$^{o}$C as indicated in Morrison et al. 2005). Also it wouldn't make much sense to replace the Cooper description with the immersion freezing parameterization by Paukert and Hoose (2014), if the immersion mechanism is represented by Bigg (1953) formula in the scheme. This means that the immersion freezing process would be described twice in the respective simulations.

Finally, since the main conclusion of the study is based on the THERMAL simulation, it is important to understand the realism of this experimental set-up. While injecting instabilities at the beginning of Large-Eddy or Cloud-Resolving Simulations is necessary, I would expect that the model would eventually be able to develop turbulent motions in a relatively realistic way. So basically I don't understand why a high-resolution model cannot produce multi-thermals in a 15 km x 15 km domain. I would appreciate a discussion on the ability of cloud models and weather prediction models to represent such turbulent structures. This would highlight the importance of this study's findings for atmospheric modeling.

**<u>Minor comments:</u>**

**Line 26:** please specify… enhancement of **the freezing efficiency**… Relaxation of the **onset temperature**

**Lines 29-30:** ***in a similar way to other convective clouds observed elsewhere in the world'*** This is a general statement that is not suitable for the abstract section, which aims to summarize main results. You have not provided references in the text for studies that have shown that multi thermals are important for cloud ice generation in 'other convective clouds elsewhere in the world'

**Line 52:** are you sure about the -14$^{o}$C threshold? I think Wilson et al (2015) showed that marine organics in sea-spray aerosols can nucleate at temperatures up to -10$^{o}$C in the immersion mode

**Line 63:** ***The INP concentrations are highly variable at a temperature.*** This is a very general statement. You mean variable at a given temperature depending on aerosol, dynamic and humidity conditions?

**Line 73:** Lawson et al (2015) developed a parameterization for secondary ice. Lawson et al. (2017) developed an expression that predicts the level in the updraft core, where liquid water gets depleted.

**Line 78-79:** you might want to rephrase this. There are actually models that are fully coupled with a chemistry component. Maybe say that this is not the case for most models?

**Lines 91-95: *'It occurred before…recommendation'.*** This sentence is redundant. It is common to analyze older campaign data to address different scientific questions.

**Line 95: *'previous field campaigns relevant to ice nucleation'.*** Maybe say 'campaigns investigating or focusing on ice nucleation'? Anyway, this needs to be rephrased

**Line 105: *'Secondly, the first ice particles appeared at temperatures greater than -8 °C.'*** Why is this mentioned here as a surprising finding? As you state in the introduction, there are several dust types that nucleate at temperatures higher than -10$^{o}$C. Is this because such aerosol types are not expected in the studied region? Please explain.

**Line 125: *'to measure… retrieval of aerosol optical properties'*** Please rephrase, it does not make any sense. You mean 'to retrieve aerosol optical properties'?

**Line 145: *'Only the outside-cloud temperatures were used to avoid the bias caused by wetting'…*** were used for what?

**Lines 152-153: *'Firstly, the ice particles appeared at temperatures greater than -10 $^{o}$C'.*** Please expand the discussion why this is listed as a main point of the study. Ice presence at such temperatures has been observed many times in dust-dominated environments. Even if we exclude nucleation, seeding from higher levels is also a possibility

**Line 155:** ***'Cloud … designed for idealised simulation of convective clouds using relatively less memory'*** Could you provide information on the adapted simplifications to understand the level of idealization in the simulations? This would help assess the realism of the results, especially in terms of the simulated dynamics

**Line 158:** maybe say 'which allows for quick simulations'

**Section 3.2:** Please provide a detailed description of the individual parameterizations and sensitivity set-ups. The DeMott expression and simulation description is only stated in the Table. Also please provide details about the Paukert and Hoose (2014) parameterization. For example, it is not mentioned until the Results section that a colder onset temperature is used in this description. Also later you mention that you modified the INP number to drop number ratio in this scheme, without giving any information on the default values or the implemented modifications. Describing the set-up of each sensitivity test in detail is critical for the interpretation and reproduction of the results.

**Line 174:** ***'noted that the factor was 4'.*** Please explain what is meant by this phrase. Do you mean that this is the factor that provides the best agreement with observations?

**Line 180:** ***'DeMott et al. (2010) based on all available data'*** . This statement is wrong. This parameterization takes into account several datasets from different regions, but definitely not all available INP data.

**Line 186:** Explain PAUKERTD set-up. How ice concentration is enhanced in the dust layer? (actually all tests should be described in detail).

**Line 194-197:** Please add a figure that shows the profiles used to initiate the simulations. Documenting the initial thermodynamic conditions is important for comparison with other modeling studies or for other modellers to simulate the same case

**Line 198:** ***'where another bubble was added 20 min from the starting time'.*** I am familiar with the fact that a bubble is needed to initiate instability in cloud resolving simulations, but after this I thought that the models can develop turbulence and simulate multi thermals within a 15x 15 km region. What is the physical explanation for the second bubble? What are the model limitations in representing boundary-layer dynamics? Does grid spacing has any impact on this? Also is choice of injection time random or does it have any physical basis (e.g. updraft lifetime). And do results show sensitivity to this choice?

**Line 216:** ***'The cloud top continued to descend was about 8.5 km (T ~ - 18 $^{o}$C) by 40 min'*** Please rephrase, it does not make any sense

**Line 217:** you refer to the ice crystal concentrations? (graupel is also ice)

**Line 221**: please fill the empty brackets

**Line 251:** What do you mean 'without the dust layer being considered'? Is the parameterization applied only up to a certain level?

**Line 261:** replace 'next' and 'later' with 'in Section…'

**Line 263:** ***' due to different microphysical schemes' ,*** you use the same microphysical scheme (Morrison). Please rephrase

**Line 295:** maybe specify that you refer to the ice crystal concentration

**Line 306:** '***enhancing the efficiency'*** of what?

**Line 314:** ***'increased approximately above 8 km and decrease' , ' There was less riming',*** sometimes past tense is used to describe results, and some other times present tense. Please be consistent throughout the whole manuscript

**Line 319-320:** further clarifications on the implemented modifications are needed

**Line 324*****: 'the different microphysical schemes affect*** ', you use Morrison scheme in all runs.

**Line 326**: ***' It would be incorrect not …'.*** Remove this sentence or rephrase (e.g. 'it is important to consider… since both play a critical role in ice production', sth like this)

**Line 328:** maximum values of what parameter

**Line 335: …**lasted for a longer time, approximately 30 min (rephrase)

**Line 340:** ***'but there left some'*** , please rephrase

**Lines 259-360*****: ' in the zone to 121*** $L^{-1}$***',*** please rephrase

**Lines 363-371:** I suggest to add a third panel with temperature timeseries in Figure 12 and include it in the discussion here.

**Line 384:** '***The shattering mechanism may be most efficient between -10 C and -15 C',*** yes but ice fragments generated by this mechanism at higher altitudes can be transferred at the lower cloud levels ,examined here, right?

**Line 385:** ***'Although the Knight mechanism operates at the similar temperature range, there is no parameterization of the process'*** Why the Phillips et al. (2017) parameterization does not account for this mechanism? They have developed a scheme that takes into account different ice types and habits. The one referred as 'planar' ice category in their study includes needles.

**Line 368-369:** ***'Fragments of frozen drops were found, but not in a great amount (figures not shown)'.*** This is important evidence and should be mentioned above, where the possible contribution from other SIP mechanisms is discussed. You cannot assess the contribution of the drop-shattering process only from images, so your conclusion that this mechanism is insignificant is not necessarily accurate.

**Lines 370-371:** ***'However, there is no causal evidence and we cannot rule out other mechanisms of secondary ice production'.*** Maybe refer to Qu et al. (2020) who showed that several SIP mechanisms can operate within a convective cloud.

**Line 394:** '***The Morrison microphysics scheme was applied for the control run*** ' Morrison scheme was applied to all runs. Maybe just clarify 'the default Morrison scheme'

**Technical corrections:**

**Line 117:** involved **an** (not in)
**Line 124:** to measure (not measures)
**Line 125:** remove 'so that'
**Line 128:** to measure instead of to measurement
**Lines 134-141:** it is Figure 1 you are referring to, not Figure 2
**Line 139:** I don't understand to which panel of Figure 1 you are referring to
**Line 287:** correct 'increasei7'
**Line 312:** PAUKERT **and** CTL
**Line 321:** to produce secondary ice concentrations similar…
**Line 325:** observation**s**
**Line 344:** concentration**s**
**Line 406:** **'**resulted in even lower' instead of 'had'

**References:**

Qu, Y., Khain, A., Phillips, V., Ilotoviz, E., Shpund, J., Patade, S., and Chen, B. : The role of ice splintering on microphysics of deep convective clouds forming under different aerosol conditions: Simulations using the model with spectral bin microphysics. J. Geophys. Res. Atmos., 125, e2019JD031312. https://doi.org/10.1029/2019JD031312, 2020

Lawson, R. P., Woods, S., & Morrison, H. (2015). The Microphysics of Ice and Precipitation Development in Tropical Cumulus Clouds, *Journal of the Atmospheric Sciences*, *72*(6), 2429-2445.