Technical Note: assessing predicted cirrus ice properties between two deterministic ice formation parameterizations (egusphere-2022-1057)

Colin Tully, David Neubauer, and Ulrike Lohmann

Topical Editor Decision Author Response

Dear Po-Lun,

Thank you very much for taking time to serve as the topical editor for our submission to GMD.

I have quoted each of the reviewer's comments below with our responses and changes in the text where applicable.

Please be aware that some of the lines that are quoted in the comments do not align with the lines in the manuscript. We tried to match the comments with the relevant lines as much as possible.

Sincerely,
Colin Tully (on behalf of all co-authors)

Comments

1. **Comment:** Lines 12-13, abstract. "over-prediction of the importance of heterogeneous nucleation within cirrus" seems awkwardly worded. Would "over-prediction of heterogeneous nucleation within cirrus be better"?
    a. **Response:** We agree, and we changed the phrase in the text with your suggested wording.
    b. **Changes in the text:**

"They argued that this new approach with explicit INP-budgeting, which removes INPs from the total population after they nucleate ice, could help to correct a potential over-prediction of heterogeneous nucleation within cirrus when budgeting is not considered."

2. **Comment:** Line 26, abstract. Remove "and".
    a. **Response:** Thank you for finding that typo. This was removed.

3. **Comment:** Line 74. Grammar: the sentence starting here doesn't seem like a complete sentence.
    a. **Response:** Is this the line starting with "Generally, the factors discussed above"? We reworded this sentence in the revised manuscript.
    b. **Changes in the text:**

"Generally, the factors discussed above lead to an overall poor predictability of how INPs influence heterogeneous nucleation mechanisms in cirrus and they contribute to uncertainties when simulating these mechanisms in numerical models."

4. **Comment:** Line 92. Should "saturation" be "supersaturation"?
    a. **Response:** If this is referring to the "ice saturation ratio" then no, it should read as such as that is the established name of that variable. It's value > 1.0 implies supersaturation.

5. **Comment:** Line 121. I'd replace "for" with "to understand".
    a. **Response:** We agree, and we revised the text with your wording.
    b. **Changes in the text:**

"In this note, we utilize the box model to compare a GCM-compatible differential AF approach based on Kärcher and Marcolli (2021) to understand heterogeneous nucleation to the AF approach by Muench and Lohmann (2020), hereafter abbreviated as ML20."

6. **Comment:** Line 147. I don't understand where you say "Threshold mechanisms are based on the stochastic nature of nucleation rates", whereby all particles that can potentially nucleate under given conditions do so when the threshold is crossed. But wouldn't a stochastic process imply there is not a such a threshold, and instead there is a fraction of INPs that actually nucleate ice over some time interval? I would think the stochastic nature of nucleation rates would imply a continuous process.
    a. **Response:** This wording was incorrect on my behalf. We reworded this paragraph to make it clearer and to fix my mistake.
    b. **Changes in the text:**

"Muench and Lohmann (2020) distinguish between two approaches (a threshold approach and a continuous approach) for heterogeneous nucleation within cirrus. For the threshold approach, as soon as the S_i reaches a critical value (i.e. a threshold), the model assumes nucleation rates are efficient enough such that all of the available aerosols that can potentially serve as INPs nucleate ice during a single step of the cirrus sub-model. For immersion freezing of internally mixed mineral dust particles, it is assumed that only 5% of the background concentration can act as INPs (Gasparini and Lohmann, 2016; Muench and Lohmann, 2020). Heterogeneous deposition nucleation, on the other hand, is based on laboratory measurements of AF by Möhler et al. (2006), which are determined by temperature (T) and S_i and which increases continuously with decreasing T and increasing S_i. In the cirrus sub-model, this approach is applied to deposition nucleation onto externally mixed (insoluble) mineral dust particles only."

7. **Comment:** Lines 165-166. You say that the activation of INPs during the lifetime of a cirrus is implicitly included by requiring ICNCj > ICNCj-1. Does this mean there are no sink processes for ICNC considered during the cirrus sub-steps (from, e.g. sedimentation). Also, I think there should be a greater than or equal to sign here, rather than greater than, since it's possible that no further nucleation occurs during a cirrus sub-timestep.

a. **Response:** Correct. there are no sink terms for ice within the cirrus sub-model. It only calculates new ice formation. You are right about the greater than or equal to sign. We changed this in the text.

8. **Comment:** Line 169. I don't think you need to redefine "phi" here since it's already used in Eq. 1 and defined as the cumulative AF.
    a. **Response:** Is this referring to this sentence: "In the first instance of ice formation in the current cirrus cycle (current GCM timestep), the leftover INPs nucleate ice according to the differential AF (ψ_j) and the newly available INPs nucleate ice according to the cumulative AF (ϕ_j) as denoted in the numerator of Equation 3"? If so, we agree and we used the symbols to describe each AF approach in the revised text.

9. **Comment:** Lines 176-185. The example shown here to illustrate over-prediction of ICNC for the cumulative AF approach is much clearer than the previous version of the manuscript. However, it would still be good to clarify why phi = 0.05 in the first step and 0.1 in the second step. Is this simply taking the total AF (0.1) and dividing half of it into the first cirrus substep?
    a. **Response:** No, it is simply an explanation of how it works. The exact values in this case do not make a difference as we are explaining KM21's argument. We revised the text to make this clearer that these values are simply assumptions.
    b. **Changes in the text:**

"As a short conceptual example of their argument (see also KM21 Figure 1), let's assume two cirrus model timesteps starting from $\phi_{j=0}$ = 0. In the first cirrus model timestep $\phi_{j=1}$ = 0.05 and in the second cirrus model timestep $\phi_{j=2}$ = 0.1 under ambient temperature and S_i."

10. **Comment:** Line 185. Why not be more precise in this example for "psi", where it's equal to ~0.0526? When this is multiplied by 95 L^-1, it gives very close to 5 L^-1. I don't think you need to give psi approximately as 0.05.
    a. **Response:** We somewhat agree that this should be clearer, so we added one more digit to the text. It now reads as "0.053".

11. **Comment:** Lines 208-212. The "fictitious downdraft" in the box cirrus model still does not make sense to me. In the authors' reply to my previous comment about this, they state that such a downdraft "acts to reduce the updraft and slow down the increase in saturation if enough deposition has taken place". It's not clear what's meant by "enough deposition". Enough relative to what? It seems the vertical velocity (updraft and downdraft) should only be an input to the cirrus model, and it's not clear why the vertical velocity should be modified somehow (by adding downdraft, thus effectively reducing updraft) to limit deposition.
    a. **Response:** The vertical velocity is an input variable to the cirrus sub-model, which is used to determine the cooling rate of the adiabatic ascent of an air parcel, which in turn determines the degree of ice supersaturation. Therefore, we must have a way to counteract Si

increasing if water vapor has been consumed by deposition onto an INP or ice crystals. We quantify this consumption as a "fictitious downdraft" that is only ever used to update the vertical velocity at every time step in the cirrus sub-model in order to simulate the effect of a "deceleration" of Si increasing. If a sufficient amount of new ice has formed or if there is a large concentration of pre-existing ice crystals, then deposition will consume all available water vapor and the fictitious downdraft will outweigh the updraft and prevent further ice formation from occurring within the cirrus sub-model. The modified vertical velocity is only used to compute new ice formation in the cirrus model. For the deposition of water vapor onto INPs or ice crystals the unmodified vertical velocity is used. We added a short statement in the revised text to make this clear.

b. **Changes in the text:**

"The resulting net updraft velocity is termed the "effective updraft velocity" and is used to calculate S_i (note that the original updraft velocity is used to compute vapor deposition onto newly formed or pre-existing ice crystals)."

12. **Comment:** Line 243. Perhaps to be clearer, you could say the tracer is not "advected or diffused" in the GCM implementation, rather than simply "not transported".
    a. **Response:** We agree and your wording was used in the revised text.
    b. **Changes in the text:**

"Note as well that in the ECHAM-HAM GCM both $N_{0,i-1}$ and ϕ_{max} are not advected or diffused."

13. **Comment:** Figure 1 and lines 296-305. It's still not exactly clear how this error is calculated. In the figure caption, "maximum relative error between KM21_GCM and ML20" isn't clear. Is this error relative to ML20, or to KM21_GCM. In other words, is this calculated as (KM21_GCM – ML20)/ML20? Or (ML20 – KM21_GCM)/KM21_GCM? Or something else? Giving the exact equation used to calculate the error would clear up any confusion.
    a. **Response:** Good point. An equation was added to the text to explicitly define this.
    b. **Changes in the text:**

"We assess each case by the relative error between the KM21_GCM approach and the ML20 approach, according to Equation 4. Finally, we conducted two simulations with the ECHAM-HAM GCM to compare ICNC fields and cloud properties between ML20 and KM21_GCM."

$$Error = \frac{ICNC_{KM21\_GCM} - ICNC_{ML20}}{ICNC_{ML20}} x100$$

14. **Comment:** Line 422. I don't think you want to call ML20 the "reference" case, since that implies this is a ground truth. Thus, I feel it's better to call this the "control" case. Also, throughout this section you refer to "differences", are these relative to ML20 or KM21_GCM? I think the former, but it would be good to make this very clear.
    a. **Response:** This is a good point. Thank you. We found two instances of this and changed both in the revised text. Regarding the differences, as this is related to your **Comment 13**, we feel the new equation 4 addresses this issue.

15. **Comment:** Line 431. Perhaps "just north of the equator" rather than "just above the equator"?
    a. **Response:** We agree and this was revised in the text.
    b. **Changes in the text:**

"In the tropics, just north of the equator, we find that KM21_GCM produces less HET than ML20, which corresponds to an increase in HOM around the same region."

16. **Comment:** Figure 4. In this section you've changed all usage of "anomaly" to "difference" in the text, which is an improvement (I had suggested it in the previous review). However, the plot titles in b) and d) still say "Anomaly", so I suggest changing this to "Difference" for consistency with the text and figure caption.
    a. **Response:** Thank you for finding that. We changed this in the revised manuscript.

17. **Comment:** Line 441. I don't think "SH" is defined yet as Southern Hemisphere.
    a. **Response:** Thank you for pointing this out. We defined the acronym in the revised text.

18. **Comment:** While it's clear that the fmax and N0 tracers for the KM21_GCM approach are not advected or diffused when implemented in the GCM, are the HET and HOM ICNC tracers advected/diffused in the GCM tests? I would assume so, since ICNC itself is advected/diffused in the model (I think). This should be clarified in section 3.2.
    a. **Response:** We agree, and we added a short statement to clarify.
    b. **Changes in the text:**

"Like Tully et al. (2022c, these ICNC tracers are advected and diffused every GCM timestep. Similarly, we calculate a 5% significance based on the inter-annual variability of the five-year simulations. For the remainder of this section, we base significance on this method."

19. **Comment:** Lines 518-519. I think this sentence is a bit confusing, suggest rewording it to: "In our model, it is assumed that each ice particle, including snow crystals, includes a single INP, where in reality there may be numerous

INPs associated with snow crystal aggregates composed of several ice crystals."

a. **Response:** We agree and we implemented slightly revised wording in the text.
b. **Changes in the text:**

"In our model, it is assumed that each ice crystal, including snow crystals, includes a single INP, whereas in reality there may be numerous INPs associated with snow crystal aggregates composed of several ice crystals."

20. **Comment:** Line 519. For ice crystals, the more common term for this process is aggregation rather than collision-coalescence. Thus, I suggest replacing "collision and coalescence process" with "collision and aggregation process".
    a. **Response:** We agree completely. This was our oversight. Thank you for point that out. We changed this in the revised text.
    b. **Changes in the text:**

"Therefore, by not considering collision and aggregation processes, our model may underestimate the number of previously formed ice crystals."