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Comment on egusphere-2022-1057

Anonymous Referee #1

Referee comment on "Assessing predicted cirrus ice properties between two deterministic ice formation parameterizations" by Colin Tully et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1057-RC1, 2022

In this manuscript, the authors demonstrate the effects of implementing a new heterogenous ice nucleation parameterization on cirrus cloud ice number concentrations (ICNC) in a cirrus cloud box model as well as cloud properties and ice processes in the global climate model ECHAM-HAM. Developed in Kärcher and Marcolli (2021) (KM21) for a single cirrus formation cycle, the differential AF approach estimates the number of INPs that activate in each timestep due to the incremental change in supersaturation with respect to ice (Si). The authors reformulated KM21 for GCMs such that it explicitly budgets the INPs available for ice formation at each timestep via deposition mode ice nucleation, accounting for INPs left over from the previous timestep. This differs from the cumulative AF and implicit INP budgeting approaches more commonly implemented in large scale models, such as the ML20 parameterization native to ECHAM-HAM. The authors first tested the impacts of KM21 in a box model based on the cirrus scheme in ECHAM-HAM, simulating different combinations of Si and INP concentrations and comparing the resulting ICNC between ML20 and KM21. At worst, ICNC was within a factor of 2X between the two INP representations. In a 5-year ECHAM-HAM simulation, the study shows that contrary to the box model results, KM21 decreased zonal mean ICNC compared to ML20 in some locations by 10 %. The results also show that KM21 increased cirrus cloud fraction and RH, yet did not significantly impact the TOA radiative budget.

Overall, this manuscript shows that the impacts of implementing explicit INP budgeting in GCMs can be inconsistent with their effects on cloud properties in cloud resolving models. However, this study is limited to deposition mode ice nucleation in cirrus clouds and it remains to be seen how explicit INP budgeting could impact cirrus cloud properties via immersion mode ice nucleation, and whether this could improve cirrus cloud representation. I believe this manuscript is of interest to the GMD community and it covers multiple themes relevant to the journal scope, including the development and assessment of numerical models of Earth system model components. I recommend publication after minor revisions and provide some suggestions below.

General comments:

There are a few contradictions in this manuscript that need to be addressed. Kärcher and Marcolli (2021) motivated the differential AF approach by arguing that the cumulative AF approach may overpredict heterogenous ice nucleation, yet the box model results featured here show that when ICNC does not agree, the cumulative AF approach underestimates ice nucleation compared to differential AF. The box model results also show that predicted ICNC between the two schemes frequently agree (and at worst show a discrepancy of < 2X in extreme conditions, looking at Fig. 2), yet an argument is made later in the manuscript when explaining impacts of KM21 on cirrus simulated in ECHAM-HAM that this ice nucleation parameterization often increased ICNC compared to ML20. Also, though KM21 sometimes increased ICNC in the box model results, it decreased zonal mean heterogeneously nucleated ICNC compared to ML20 over much of the SH and NH. Perhaps these issues could be resolved with more careful discussion of agreement and significance. Otherwise, I suggest addressing each of the contradictions explicitly in the discussion.

The author also argue in the abstract and conclusion that ML20 leads to increased interpretability of GCM results, but I think more discussion is needed to support this argument. The formulation of ML20 is indeed simpler, but explicit INP budgeting makes more sense intuitively than the implicit treatment of INP removal in ML20 (e.g., …"ICNC is updated only if the amount of new ice formation as a portion of N_0 exceeds ICNC from the previous sub-timestep"). In short, an argument for increased interpretability could be made for either scheme, so I don't see this as a clear benefit of ML20, though there are other obvious benefits that are described in the conclusion.

Please also check that all tables and figures appear in the section in which they are first referenced.

Abstract:

I think it is important to point out in the abstract that this study focuses on deposition mode ice nucleation only, possibly in the title, and to allow for the possibility that explicit budgeting could still impact cirrus properties through immersion mode nucleation. As you state on L64, immersion mode is likely the dominant mode of heterogenous ice nucleation in cirrus. It would also help the reader to briefly describe the difference between explicit and implicit INP budgeting.

L3: "There is more uncertainty surrounding heterogeneous nucleation processes due to the complex physio-chemical properties of ice nucleating particles."

This is a bit of a general, catch-all statement. The authors could consider a different approach to motivating this study. For example, why is it important to improve representations of ice formation in cirrus? Or, if you want to discuss the complexities of INP properties in the abstract, you should elaborate on how their complex properties (by which I assume you mean the myriad types, ice activation mechanisms/chemistry) relate to poor predictive understanding of INP, and how the poor predictive understanding relates to the challenges of representing ice formation processes in cirrus. In other words, you need to hold the reader's hand a bit more to help them understand the motivation for studying two different deterministic INP parameterizations.

L4: "…follows a time-dependent (stochastic) or time-independent (deterministic) approach…"

This sounds like a point that would be more appropriate in the introduction. I don't think this is a necessary distinction to describe in the abstract because the two parameterizations you are comparing are both deterministic.

L6: Please define "differential activated fraction"

L12: "…as it does not account for INP fluctuations across GCM timesteps."

After reading the full draft, I return to this line and understand what you mean here, but I

think this phrasing is misleading. Looking at Fig. 1 for example, there are “fluctuations” in INP concentrations at each timestep in both schemes. Perhaps “…as it does not account for interstitial INPs remaining from the previous time step”?

L15: “…small and insignificant”

Please clarify what is meant quantitatively by small and insignificant.

Introduction:

L29: typo at “Climate Change”

L31: “..are mostly well established.”

“mostly” is unnecessary.

L47: “…due to the presence of an INP surface.”

Please delete “surface”.

L59: “…e.g., on mineral dust (Murray et al. 2021) or on black carbon particles…”

Please delete these two instances of “on”.

L65: “In general, as there are more factors that govern the complexities of heterogenous nucleation than homogenous nucleation…”

This is another catch-all statement. Again here, please elaborate on how the poor predictive understanding of INP relates to the challenges of representing ice formation processes in cirrus. Also, please define “ice nucleation competition.”

L69: Please define “AF of available INPs.”

L68: “…that are based on laboratory measurements of ice formation.”

There are several parameterizations of heterogenous ice nucleation that are derived from field measurements.

L75: “As Vali (1971) explains, on the one hand, the former approach is useful to describe the freezing behavior of a single particle.”

Single particle type? Single INP species? Please rephrase or explain how differential AF approach can also be useful for simulating an INP population.

L80: “For example, if a model explicitly removes INPs from the total available population after each ice formation event and adds them to nucleating ice number concentrations…”

Please rephrase. Do you just mean that INPs are effectively removed from the population when they trigger ice formation?

L82: Please elaborate. It is not immediately clear to the reader how the cumulative AF approach would lead to the overprediction of heterogeneously nucleated ice.

L99: Please specify somewhere in this paragraph that this study is limited to deposition mode nucleation.

Methods

L114: In my opinion, this paragraph should begin with the prior line beginning “Muench and Lohmann (2020)…”

L124: What is meant by “freezing processes”?

L125: What is AF(Ð¤)? Is AF a function of (Ð¤)? I see Ð¤ is partially described in L126. Please more explicitly define.

Figure 1: The flow and organization of this figure needs to be improved. Please consider putting the resulting ICNC in a single column and creating a single column for the other features of the blocks, such as “total available INP”. You could do the same with subheadings for “Previous INP” and “New INP” for KM21_GCM blocks. The comparable features of each block are not in consistent locations which makes it hard for the reader to follow this figure. Please also specify in the caption whether the INPs in each scenario are interstitial or “total available” or if they are inclusive of previously activated INPs. For KM21_GCM, consider at “+” to indicate that INP from the previous time step are added to the new INP according to AF. Why is the budgeted “leftover” INP in timestep 1 not included in the bottom right blue box for KM21_GCM (i.e., why is $N^*_{i=1,j=n}$ =0?). Please also add the information about AF on L196 to the Fig. 1 caption. A brief explanation about the different INP treatments at i=2 in the caption would also help make this figure read more easily.

L201: “In order to account for leftover INPs as well as INPs that are still included in ice crystals…”

That are removed? Activated and thus removed?

L204: “This allows us to properly consider changes…”

Remove “properly”. Track changes?

Table 1: Please move to the following section where it is first referenced. I noticed this issue with another figure or two. Please check.

L215: What does “non-exhaustive of the changing conditions…” mean? Please rephrase.

Results

L229: What is “agreement”? Please explain how you consider agreement quantitatively.

L233: Please remove “Nevertheless”

L235: Here, and throughout the paper, there is a contradiction that needs to be addressed, whether here or further down in the discussion (but please reference where that discussion begins here). Given that cumulative AF approaches such as ML20 can overpredict ice nucleation compared to explicit budgeting approaches as you described in the Methods, how is it that KM21_GCM is predicting higher ICNC compared to ML20?

L236: “On the one hand, while ML20 considers…” I like this paragraph. The differences between the two treatments are clearly stated.

L251: “Non-zero error between the predicted ICNC for KM21_GCM and ML20 occurs from the start of the second cirrus cycle in the first case (Fig. 3a), and from the start of the third cirrus cycle in the second case (Fig. 3b), where KM21_GCM initially predicts a higher ICNC than ML20.”

Please remove “initially”.

L262: “For ML20, despite a larger AF at the start of the second cirrus cycle, the number of newly formed ice crystals that could nucleate onto the fewer number of available INPs does not exceed the pre-existing ICNC.”

The authors present this condition as the main feature of ML20 that causes the unexpectedly lower ICNC compared to KM21. Please elaborate on the broader implications. Among cumulative AF approaches, is this condition unique to ECHAM-HAM (where ice does not form unless INPs > pre-existing ICNC)? If not, would you expect other cumulative AF schemes to result in increased ICNC as expected according to KM21 and your explanation in the Methods?

L279: What is meant by “To emulate the procedure in the GCM...”? Please rephrase.

L291: What is meant by “arguably”? Please elaborate.

L313: “Based on our box model results, it is likely that ML20 underpredicts the number of heterogeneously formed ice crystals under cirrus conditions compared to our KM21_GCM approach as it neglects the different ice nucleation behaviors of available INPs.”

Again, please discuss the discrepancy between the result, your expectations for overprediction described in Methods and the motivation for KM21.

Also, please elaborate on or rephrase “…it neglects the different ice nucleation behaviors of available INPs.” I am not sure what this means. Please specify what is “different” and what entities are being compared. Are you arguing that KM21 better emulates the variability in IN-activity between dust populations, or between individual dust particles? If so, you will need to supplement this section with supporting evidence.

L322: “…this may not be the case using a GCM.” Do you mean it remains to be seen how frequently these errors occur?

L336: Please briefly describe the false discovery rate method by Wilks (2016) to help the reader understand the significance testing applied. If this is the only significance test applied, please state that Wilks (2016) is what is referred to throughout the rest of the results and discussion.

L353: “This is only partially reflected in zonal profiles of cloud fraction and relative humidity (RH) anomalies in Fig. 6, where there are only small positive anomalies in the southern hemisphere (SH) tropics of up to 1 % that are insignificant (as denoted by the stippling).”

What is meant by “partially reflected” if the anomalies are insignificant? Also, please be explicit here and elsewhere throughout the manuscript on what is meant by significant.

L357: “There are significant, positive cloud fraction and RH anomalies between 1 and 10 % towards the mid-latitudes and the poles in both hemispheres.”

Please add a reference to Figure 6.

L358: “However, the HOM signal is not consistent throughout the SH and is insignificant.”

What is meant by HOM signal?

L363: See previous comment on “different ice nucleation behavior of available INPs.”

L363: “…we found that it often allows for higher rates of ice formation in cirrus.”

Did you track ice formation rates in ECHAM-HAM? Or do you mean KM21 results in higher ICNC? This point about frequency is another contradiction with your previous box model results, in which the results showed that the resulting ICNC between the two schemes frequently agree. The authors further argued that the conditions for which the resulting ICNC differed would occur infrequently in a GCM (L320). Please address this contradiction.

L370: “While there are relatively large, but insignificant changes…”

Please define quantitatively what you consider “relatively large”.

L373: “Nevertheless, these changes correspond to only a small positive top-of-atmosphere (TOA) warming effect by around 0.02 ± 0.35 Wm−2 that is driven predominately by a weaker shortwave (SW) cloud radiative effect (CRE).”

Is there a reference for the cirrus contribution to CRE in ECHAM-HAM? This would be helpful context.

Conclusions:

L400: “…nor does it consider the different ice nucleation behaviors of available INPs.”

Please clarify.

L404: “The large-scale Si conditions and the changes in INP concentrations between cirrus cycles that we tested with our box model were rather extreme and may not occur frequently in a GCM.”

Would it be possible to calculate the frequency of these conditions from the ECHAM-HAM output?

L408: “However, the signal is mostly insignificant for the five years that we tested (2008-2012), and is inconsistent with the findings from our box model simulations, except in the NH.”

Please define “signal” and “mostly insignificant.”

L414: “Not only does this require additional memory allocation, but it also introduces more room for potential error.”

Please elaborate on “room for potential error."