Second review of “**Technical note: Assessing predicted cirrus ice properties between two deterministic ice formation parameterizations**”, by Tully et al., submitted *GMD*.

Note: line numbers referred to below correspond to the track changed version.

General comments. The paper is substantially improved from the previous version, particularly with regard to the description of the KM21 and ML20 approaches and their implementation in the box model and GCM. I still have a handful of minor comments and suggestions below, and also point out a few places where the descriptions are still confusing (e.g., the “fictitious downdraft”). My recommendation is to accept the paper pending these minor revisions.

I will also note the authors said in their replies to previous comments that they will provide a video supplement replacing Fig. 1, which further describes the approaches. This was not available at the time of my review, so I can’t comment on it. That said, I think the description of the approaches in the current revised manuscript is sufficient for understanding by readers.

Overall recommendation: *Minor revision*

Minor and editorial comments.

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”?

Line 26, abstract. Remove “and”.

Line 74. Grammar → the sentence starting here doesn’t seem like a complete sentence.

Line 92. Should “saturation” be “supersaturation”?

Line 121. I’d replace “for” with “to understand”.

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.

Lines 165-166. You say that the activation of INPs during the lifetime of a cirrus is implicitly included by requiring $ICNC_j > ICNC_{j-1}$. 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.

Line 169. I don't think you need to redefine ϕ here since it's already used in Eq. 1 and defined as the cumulative AF.

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?

Line 185. Why not be more precise in this example for ψ_j, 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 ψ_j approximately as 0.05.

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.

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".

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.

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.

Line 431. Perhaps "just north of the equator" rather than "just above the equator"?

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.

Line 441. I don't think "SH" is defined yet as Southern Hemisphere.

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.

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.”

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”.