Review of "**Technical Note: assessing predicted cirrus properties between two deterministic ice formation parameterizations**", submitted to *GMD*.

**General comments.** This technical note describes box model and GCM tests of two approaches for treating deterministic ice nucleation in climate models. The two methods are based on a differential activated fraction (AF) approach (KM21) and the current scheme in the ECHAM model that is a cumulative AF scheme (ML20). The authors find some substantial differences between the two approaches for some cases in the box model, but overall only a very small impact in the GCM. They conclude that the simpler ML20 scheme may be appropriate given the similarity in results compared to the more detailed KM21 scheme.

Overall, this is an important subject and I think the methodology is reasonable. My main criticism is that the writing is confusing in many places, particularly when describing the ice nucleation approaches. Several detailed comments and suggestions related to this are given below in major comments. I also have numerous minor comments and suggestions followed by a handful of editorial comments. Overall, my recommendation is minor revision. However, I want to emphasize that the description of the two ice nucleation approaches and the overall presentation quality need to be improved before I would recommend accepting the paper.

**Recommendation:** *Minor revision*

**Major comments.**

1. As mentioned above, the description of the approaches is rather confusing in several places. In particular, the top part of p. 5 is confusing when discussing the KM21 approach. Some specifics:

a. It's not clear on p. 5 (though it is explained later in the paper) if the implementation of KM21 here considers previously removed INP (e.g., by including N0 as a prognostic variable following the "explicit INP-budgeting"). This should be explained clearly up front on p. 5.

b. I would remove the quotes around "differential AL" on line 127, otherwise this makes it confusing what psi actually is.

c. The example applying Eq. 1 does not seem correct, or at the least it is very confusing. As I understand it here, AF (psi) = 0.05 for both steps. Since 5 L-1 is removed from the total IN in the first step (with "explicit INP-budgeting" in this approach), Eq. 1 should give the correct total of 9.5 L-1 ICNC after two steps (5 L-1 from the first step, plus 4.5 L-1 during the second step). It's stated on line 135 that "phi is based on N0" which then does not include the removal of INPs activated during the first step (that is, no INP-budgeting). Then Eq. 1 gives ~10 L^-1 which is incorrect and too large. But it's not stated here (only later) that for the implementation of this approach the INP are tracked and they are removed from the population when nucleated, that is, that N0 is tracked as a new prognostic variable (or "tracer" as the authors call it). Again, it should be clarified here that N0 is tracked when implementing this approach, so that it does account explicitly for previously nucleated INPs (related to comment a above). Overall, the example given here on p. 5 gives the impression that the KM21 approach will overestimate ICNC, but I don't think this is not the case when N0 is tracked as an additional tracer.

2. For implementation of the ML20 approach (Eq. 2), are all ice species counted as ICNC (including cloud ice, snow, etc). Or is only cloud ice included?

3. For implementation of both approaches in the box model, there are also many confusing aspects (especially for KM21). Again, some specific issues:

d. Near lines 181-182, it's stated that phi is set to the maximum AF in the cycle, and does not decrease in subsequent cycles. But this raises the question of how INP are recovered (i.e., how phi is relaxed to the background value phi = 0). This is clarified in the next paragraph on line 188 (phi is to 0 outside cloud), but I would state it in the paragraph around lines 182-183 as otherwise this is confusing.

e. For implementation in the GCM, presumably phi as well as INP are tracked as prognostic variables and advected and diffused? Of course, there is no transport in the box model, but it should be mentioned how the tracked phi and INP are treated in the full 3D GCM.

f. Relatedly, also on line 181, in the box model it seems that phi is not actually added as a tracer (prognostic variable), but rather ICNC_i-1,j=n (see line 203). Please be very clear about which exact variables are actually tracked in the implementation of these schemes.

g. p. 6, eq. 3. This equation is confusing. Why are i and j different indices? Are both time indices? How are they different? Also, line 192 is confusing. I think by "the previous AF" you mean the maximum AF from the previous cycle? If so, please clarify this and use more precise wording. It's also not clear why phi_i-1,j=n has 2 subscripts, when all previous instances of phi have a single subscript. Why? Again, what do i and j actually indicate, and how are they different?

4. To improve the presentation and flow, I suggest describing the default ML20 approach before the newer KM21 approach. Thus, switch sections 2.1.1 and 2.1.2?

5. A key limitation of the box model is lack of sedimentation. Around lines 320-322 you mention that some of the large-scale tendencies for Si and INP concentrations in the box model tests may not be realistic, but it seems the lack of ice transport and sedimentation may be a bigger issue when comparing the box model and GCM results. Also see lines 347-350 where it's implied that the biggest differences between the box model and GCM are the magnitude of changes in Si and INP concentrations, not mentioning the role of precipitation generation and sedimentation in the GCM at all. The sentences on lines 376-377 are also relevant here.

6. Line 162. Where does the downdraft part fit it? Does the model include an updraft-downdraft cycle? Or is it only updraft. More description is needed here – this was very confusing.

7. Lines 220-225. It seems that each cirrus cycle is assumed to have a timescale equal to the GCM time step. This seems like a major assumption and difficult to justify on physical grounds. What is the sensitivity to this? That is, what if there was more than 1 cycle per GCM time step, or less than 1?

8. You use term “error” when describing differences between the two approaches. Since there is no benchmark or truth, I feel “difference” would be a better term to use than “error”.

**Minor comments.**

1. Abstract. Could you explain the differential activated fraction (AF) approach in a simple way while staying within the abstract length limit (e.g., simplify the description on lines 72-75 for here)? This would be useful for readers who aren’t familiar with this approach so they can more fully understand the abstract.

2. Line 72. Would it be clearer to say that the differential AF approach describes the number of INPs that are active *per temperature interval*, rather than “within a certain temperature interval” as currently written. Taken literally, the latter does not necessarily imply a differential quantity (i.e., units of IN number per temperature).

3. Lines 80-82. I think the term “INP-budgeting” should be “explicit INP-budgeting”, in contrast to the “implicit INP-budgeting” in the ML20 approach as mentioned on line in the abstract. Also, not clear why explicit INP budgeting might over-predict ICNC. Can this be explained better?

4. Line 90-91. Can this be explained more? What is the differential ICNC approach, and what is the issue stated by Karcher and Marcolli (2021)?

5. Lines 98-99. But according to line 14 in the abstract, GCM simulations were also run and analyzed as part of this study. I’d mention that here.

6. Lines 115-119. These two sentences seem to contradict one another. Perhaps in the first sentence, reword to “such that all of the available aerosols that can potentially serve as INPs nucleate ice during a single substep of the cirrus sub-model”.

7. Line 135. I would replace “should be” with “will be”, as “should be” could be mistaken to imply what the concentration would be if there were no error.

8. Line 140. It's not the values per se that are relevant here, but the magnitude of error. Thus, I'd suggest replacing "values" with "error" and then "are not large" with "is not large" for subject-verb agreement.

9. Line 186. The "tracers" are tracked in time (and time and space in the GCM), so I think it's confusing to say you added a tracer for the "previous INP concentration (N_0,i-1)". Better to say you added a tracer for the INP concentration (N0).

10. Lines 230-234. The description in these sentences is very confusing. The way this is written makes it seem like most cases have small error (< 1%), but this is true for only 9 of the 16 cases. Please rewrite this to make it clearer.

11. Line 237. It's confusing to say that ML20 only considers the starting INP concentration, because the INP concentration changes over time in most runs (e.g., Table 1). Or do you mean it only considers the initial IN concentration at the start of each cycle? If so, please clarify this.

12. Figure 3. Perhaps label which cases a) and b) are at the top of the plots, rather than only mentioning this in the figure caption.

13. Line 298. Not clear what you mean by "with exceptions for non-zero errors". Can you reword this?

14. Line 300. I don't think you mean "error between these cases", but rather the "error for these cases".

15. Same comment for Fig. 4 as Fig. 3 above.

16. Lines 336, 341, and elsewhere. What do you mean by the HOM and HET anomalies? Does this just mean the difference in HOM and HET between KM21_GCM and ML20? In which case, I'd replace "anomaly" with "difference" and "anomalies" with "differences" throughout.

17. Appendix. Similar comment for all the figures here as my previous comments for Fig. 3-4.

**Editorial/technical comments.**

Line 24. I'm not sure what the convention for GMD is, but should acronyms be defined again in the main text even if they're defined first in the abstract? I would lean toward yes.

Line 29. Remove the second comma.

Line 44. "that is sparsely populated" seems somewhat awkward wording. Perhaps something like "that has low concentrations"? Similarly, perhaps on line 51 replace "are also sparsely populated" with "are sparse".

Line 69. Again, I would define AF here (first time it's used in the main text).

Line 77. Suggest replacing "that can occur" with "occurring".

Line 84. Add "approach" after "AF"?

Line 85. "This method demonstrated that it is able to counteract" is confusing. I think you mean "Karcher and Marcolli (2021) demonstrated that this method is able to counteract…"

Line 125. Add comma before "when".

Line 128. Change the second "of" to "to" or "in".

Line 269. I think "produce" should be "produces"?

Line 308. Remove comma right before "approach".

Figure A2 caption. Period is missing at the end of the last sentence.