Review of “Exploring Sources of Ice Crystals in Cirrus Clouds: Comparative Analysis of Two Ice Nucleation Schemes in CAM6” by Lyu et al. [Research Article, egusphere-2024-4144]

This study coupled a novel ice nucleation parameterization scheme based on Karcher (2022) into CAM6 and compared its representation of cirrus ice cloud microphysics against the default Liu and Penner (2005) scheme, with a particular focus on ice sources in cirrus cloud formation. The authors conducted a thorough assessment using both long-term simulations and case studies from the SPARTICUS and ORCAS campaigns. Their findings revealed several similarities between the two schemes, such as the climatological location of orographic gravity wave (OGW)-induced ice crystals and the same dominant source (OGW-induced) for orographic cirrus. Notable differences were also identified, primarily attributed to the distinct nucleation/competition mechanisms within the two schemes. Overall, the manuscript is well written, but its structure could be improved for better readability. For example, the excessive use of short paragraphs disrupts the flow, and combining some of them could enhance clarity. This work holds significant potential for advancing ice cloud simulations, particularly in refining parameter tuning and improving the representation of competition mechanisms. However, my major concern is the lack of sufficient physical explanations and robust evidence for the model biases and the differences found between the two schemes. If these issues can be addressed, I believe this paper will be well-suited for publication in ACP.

Major comments:

1. A key concept in this study is the competition between homogeneous and heterogeneous freezing in ice cloud formation. The authors argued that the competition is stronger in the LP05 scheme than in K22 due to differences in their parameterization of homogeneous nucleation occurrence. However, this claim appears to be more of an assumption than a rigorously validated conclusion, as it is not directly substantiated from the parameterization formulas (not shown by the authors). The authors have used this assumption multiple times (e.g., Lines 246-248 and 268-270) to explain discrepancies in simulated ice cloud microphysics between the two schemes. I think a more appropriate way would be to first make this assumption explicitly and then examine it using supporting evidence from simulation results.

The authors found that fewer new ice crystals form in LP05 with the presence of pre-existing ice crystals (Line 247), which aligns with the assumption of stronger competition in LP05. However, a critical underlying assumption is that both schemes should have a similar or comparable number concentration of pre-existing ice crystals. If the LP05 experiments contain a higher concentration of pre-existing ice crystals than K22, it becomes difficult to determine whether the reduction in new ice formation is genuinely due to stronger competition in LP05 or a result of differing initial conditions. To address this issue, the authors should ensure that the number concentration of pre-existing ice crystals is close across experiments or, at the very least, discuss the potential influence of variations in pre-existing ice concentrations on their results.

Additionally, the proposed indirect explanation for the increase in ice number concentration in K22 is not sufficiently substantiated. For example, the authors did not show evidence on how the changed circulation dynamics impact the sub-grid turbulence, making this explanation remain speculative rather than a well-supported conclusion.

2. Since one purpose of this paper is to evaluate the K22 scheme, incorporating climatological (6-year) observational data is important for assessing the performance of both schemes. If obtaining global vertical profiles is challenging, bulk or regional observational data would still be valuable in determining whether K22 improves ice cloud simulations compared to LP05 from a climatological perspective.

3. To deepen the insights of this study, the authors could discuss the potential impact of incorporating K22 into CAM6 on high cloud feedback. For example, if the proposed indirect mechanism for the higher ice number concentrations in K22 is true, large-scale circulation changes induced by global warming could modify sub-grid turbulence, subsequently affecting ice nucleation, cloud frequency, and longwave radiative effects.

4. One structural issue in the manuscript is the overuse of short paragraphs, which disrupts the flow of the text. I recommend revisiting the paragraph structure and merging shorter paragraphs with logically related content to enhance readability and coherence. I will provide some specific suggestions in the minor comments, though they are not exhaustive.

Minor comments:

L7: I'd suggest reorganizing the abstract into two paragraphs or three at most.

L88: Please give a brief reason why both field campaigns are used for validation. Any differences between these two or just for increasing the sample size?

L98: No definition for "DET" upon its first appearance.

L127: "thermaldynamic" to "thermodynamic"

L150: Suggest moving this paragraph up.

L152: "compared" to "compared to"

L211: Since OGW-induced cloud nucleation is a very important source for ice cloud formation, I'd suggest comparing the climatology simulation results between land and oceans. The results over the land might be more contrasting between the two schemes.

L215: Why more cirrus due to OGWs in high latitudes, particularly near the Poles (Figures 2e and 2f)?

L216: Please clarify the physical mechanisms for turbulence-induced ice nucleation.

L232: "results" to "resulting", "resemble" to "resembles"

L279: Any physical explanations for changes in sub-grid turbulence?

L303: "Together with … (Fig. S7)" to "Together with simulated IWC and D_{num} (Fig. S7)"

L316: What does “Temperature in X-axis” represent? Pressure-level mean temperature?

L330: “ΔN_i” to “ΔN_i due to OGWs”

L336-337: Be specific. It looks dependent on the source types.

L351: As in K22 the detrained ice crystals do not have a significant competition, I’d expect that D_{num} is slightly lower in K22_no_DET-SP (red lines in Fig. S9) than in K22_OGW-SP. However, why is it slightly higher in K22_no_DET-SP when T is less than 227 K?

L388: Suggest moving this paragraph up

L391: Why is its magnitude so large between 220 and 230 K?

L402: It seems like no_TKE experiment shows the highest peak. Please double check.

L413: Please rephrase this sentence.

L456: “OGW-induce” to “OGW-induced”

L456: Are these dates selected when the simulations of both schemes align with the observations?

L481: “considering” to “with a focus on”

L490-495: Are there any physical reasons, or formula-related proofs? If not, the first reason is more like an assumption.

L496-500: Have you examined the changes in large-scale circulations and their association with sub-grid turbulence variations? If not, you would have to soften your tone when proposing the indirect reason.

L524: Please move it to the last paragraph.

L532: Be specific for these critical INPs.