Review of Effects of preexisting ice crystals on cirrus clouds and comparison between different ice nucleation parameterizations with the Community Atmosphere Model (CAM5) by Shi et al.

General Assessment

In this work the authors improve the representation of ice cloud formation within the CAM model but including previously neglected factors accounting for preexisting ice crystals and subgrid variability in supersaturation. In line with previous works they find that including preexisting ice crystals decreases the ice crystal concentration therefore improving (however not completely fixing) a current bias of the CAM model. They also find that the contribution of heterogeneous ice nucleation to the total ice crystal production increases when accounting for preexisting ice crystals. Additionally the authors test the sensitivity of the anthropogenic indirect forcing to the choice of the ice nucleation parameterization. The study is relevant to the atmospheric community. However the discussion is confusing and the work is difficult to follow. The authors need to clearly state their objectives and motivation behind the new implementation. The paper should be structured in a way that is easier to follow before it can be published in ACP.

General Comments

There are many grammatical and stylistic errors throughout the manuscript. I have tried to point out some but I'd recommend the authors to careful review the manuscript to make it suitable for ACP.

The approach used in the work needs more justification. It is not clear why the authors implemented the PREICE parameterization. The motivation for inclusion of the subgrid probability distribution and the comparison of several ice nucleation parameterizations is lacking.

Section 2.3: This approach to include preexisting ice crystals seems simplistic and may contradict what is said in Section 2.4. What are the assumptions behind this approach? Do the authors use in-cloud or grid scale ice crystal concentrations for their calculation? Do ice crystals sediment out of the cloud during a nucleation event? Since the cloud is not homogeneously mixed, one may argue that homogeneous nucleation occurs in cloud pockets devoid of ice crystals that will have high supersaturation (as assumed in Section 2.4). Maybe only the small ice crystals are transported with the cloud parcel Is there a cut-off size for the crystals that affects homogeneous ice nucleation? (See for example Spichtinger and Kramer 2012 and Barahona and Nenes 2011).

Figure 2 is confusing and can be done much better in a quantitative way showing the evolution of Ni in time.

Section 2.4: How does the variability in water vapor play a role defining the supersatutation distribution? The model proposed assumes that only temperature fluctuations are important, however several studies have pointed out that their role is secondary. Furthermore, it is not clear whether the PDF is prescribed or changes with the grid cell conditions. The authors should explain exactly and very specifically how Eqs. (8) and (9) are used.

Is the PDF connected at all to cloud condensate and cloud fraction? Otherwise it seems that the proposed approach may run into inconsistencies. The effect of defining different pdfs for different processes should be assessed.

Specific Comments

Page 17637 Line 1. Change import by important

Page 17637 Line 4. Remove “on other hand”

Page 17637 Line 11. Remove “then”

Page 17637 Line 13. Cirrus clouds is plural. Please correct.

Page 17637 Line 18. Please be more specific about what species are likely to be IN in the atmosphere.

Page 17637 Line 22. Enigmatic refers to mysterious. Maybe just say “difficult to understand”.

Page 17638 Lines 6-10. This statement is confusing. Please rewrite.

Page 17638 Lines 20-22. This is a badly formulated statement. Please rewrite.

Page 17639 Lines 12-15. This sentence literally comes from nowhere. The authors must justify why they think including a probability distribution of in-cloud supersaturation is required.

Page 17639 Lines 12-15.Is this actually in-cloud or it applies to the whole grid cell? What is done for the cloud-free part of the cell?

Page 17639 Lines 15-16. Again, why is it important to compare different parameterizations?

Page 17639 Line 23. Say: the version 5.3 of the Community Atmospheric Model.

Page 17639 Line 24. What does highly parameterized mean?

Page 17642 Lines 5-10. Neither S_i nor $q_{i,pre}$ are constant during the parcel ascent. What are the assumptions behind Eq. (6)? This particular approach is not new and has been proposed before (Numerically by Karcher et al. 2006 and analytically by others). The authors should cite previous works.

Page 17642, Lines 20-25. This paragraph is confusing. Please rewrite.

Page 17643, Line 2. Using the effective radius as defined in Morrison and Gentleman (2008) is incorrect (i.e., The third over the second moment of the distribution). To be consistent with Eq. (6) $R_{i,pre}$ must represent the mean volumetric radius (i.e., the first moment of the size distribution).

Page 17644, line 17. It is not clear what this means. How is the PDF used exactly (also how does it look like?)? Do the authors multiply Ni by $P_T(S>Shom)$ as done by Kärcher and Burkhardt (2008)? Please be more specific.

Page 17645, Line 13. It must be nucleation spectra.

Page 17646, Line 1. It is not clear what this means. As expressed in Eq. (5), Wpre is independent of the ice nucleation parameterization. What value of Si has been used to calculate Wpre?

Page 17646, Line 5. It should be: the parameter that sets.

Page 17646, Line 18-20. Remove the sentence starting with The Default…

Page 17647, Line 20. It should be upper limit.

Page 17647, Line 5-20. The aircraft measurements correspond to a scale much smaller than the GCM resolution. Averaging over a 50 Km grid would be incorrect and contradict the assumption that homogeneous ice nucleation occurs only in a fraction of the grid cell. According to that assumption, the grid scale average vertical velocity cannot be representative of the conditions where nucleation is actually occurring.

Page 17649, line 22. Would fhom decrease at low temperature (i.e., high altitude) since total water is also decreasing? Is this accounted for?

Page 17650, line 7-10. Please explain what the reason for the better agreement is.

Page 17655, line 3-10. This is a misrepresentation of previous work. The KL and BN parameterizations include transition regimes where heterogeneous freezing is active but not enough to prevent homogeneous ice nucleation. Thus newly formed crystals come from both homogeneous and heterogeneous ice nucleation. This paragraph implies that other parameterizations go from complete heterogeneous to complete homogeneous and that only the LP parameterization has such feature, which is not true. The paragraph must be removed.

Page 17657, lines 15-17. It must be mentioned that only variations in supersaturation from temperature fluctuations are taken into account whereas water vapor variability is neglected. Including the latter may lead to a much stronger effect and coupling between different nucleation events.