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

Anonymous Referee #1

Received and published: 29 July 2014

Review of the manuscript “Effects of preexisting ice crystals on cirrus clouds and comparison between different ice nucleation parameterizations with the Community Atmosphere Model (CAM5)” by X. Shi, X. Liu and K. Zhang.

The manuscript investigates the effect of preexisting ice crystal on cirrus clouds and its climatic implications. Also the manuscript describes the updated cirrus scheme in CAM5, namely the consideration of in-cloud variability in ice saturation ratio as well as the removal of two unphysical limiters. The manuscript deals with current open

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issues in the field of cirrus cloud science, adds valuable knowledge and is of high scientific interest. So, I strongly recommend publishing the manuscript in ACP. However, I fear that the written language and the structure of the manuscript need some improvement before publishing. There are several spelling and grammatical errors all over the manuscript and I could not take the effort and mention them all here in my review. I encourage all authors to take a close look to that when revising the manuscript. Also, I think parts of the results (the comparison between different ice nucleation parameterizations) need some deeper investigation rather than speculations. Since the data/information is given in current literature this is possible to do. My more detailed comments and suggestions can be found below.

Major:

Page 17637, Line: 14: I think this needs to be rewritten, since it is not totally true that cirrus cloud knowledge is still in its infancy. I think in recent years big steps had been made forward in both, cirrus cloud measurements as well as cirrus cloud modeling. Please add this and give adequate citations.

Section 2: Section 2 is not well structured and thus repeats information and/or is difficult to understand. Thus I recommend to 1) change the order of the subsections, 2) use slightly different titles for the subsections and 3) delete some of the redundant repeating information. Detailed information on this issue can be found below (Page 17639-17646):

Page 17639-17640: Subsection 2.1. I suggest to rename this subsection and to put parts of this subsection to 2.2, since descriptions of the cirrus scheme in CAM5 and the ice nucleation parameterization in CAM5 are very close and don't need different subsections. In order to have 2 separate subsections I suggest to have a subsection 2.1 called "CAM5" and 2.2 called "cirrus cloud scheme in CAM5". I further suggest to put the text between page 17640 line 6 and line 16 to subsection 2.2.

Page 17644: Subsection 2.4: It is totally confusing to explain small modifications to

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the base model after introducing the concept of PREICE, which is the main topic of this study. So, I suggest swapping subsection 2.3 and 2.4. This means of course that the text needs to be changed accordingly. I further suggest to rewrite text on page 17641, line 15-17 as follows: “To account for the effect of preexisting ice we introduce preexisting ice into our model CAM5 based on the concept of Kärcher et al., 2006, which is based on the concept of an adiabatic rising air parcel.”

Page 17643: Description of Figure 2: I don’t understand how you derived these numbers? These numbers should be dependent on vertical velocity, temperature, supersaturation. . . Please add this info and explain in more detail.

Page 17653-17654: The differences in the effects of PREICE between different models (CAM5, GEOS5, ECHAM5) but using the same ice nucleation parameterization are very large. I think this needs some more detailed investigation and explanation. You speculate that the input parameters like w_{sub} , RH_i and aerosol number concentration used to drive the ice nucleation parameterization are different in the above mentioned models. In Kuebbeler et al., 2014 aerosol number concentrations are presented, thus a comparison with that data is possible. Also, how are processes influencing the ice number concentration like sedimentation, accretion, aggregation, etc. realized in the different models? Besides, what is about freezing details like critical supersaturation for heterogeneous freezing? Which freezing mechanisms (immersion, deposition) are used in the models? All these can influence your results and should be investigated/discussed in more detail.

Page 17655, line 26 to end of page: Numerous laboratory based studies have shown that not all dust particles can act as IN and thus an upper limiter for heterogeneously freezing particles does indeed make sense. As far as I know, only Hendricks et al., 2012, Kuebbeler et al., 2014 and some studies looking at the potential of soot as IN in cirrus clouds have considered this. I would be curious to see how your results change if you also put an upper limiter to dust particles in your model. Also I assume that this might indeed be the reason why the results differ so strongly between models. How-

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ever, as already mentioned above you could easily check this, when plotting aerosol data of your model using KL parameterization and comparing to Kuebbeler et al., 2014.

Page 17657-17658: Discussion and conclusions: Conclusions should shortly describe what was done in the manuscript and review the most important findings of this study. To me, this sections is again not well structured. I suggest rewriting this section in a way that each paragraph describes one important finding. (In more detail, I suggest to put text from page 17657, line 8-12 as well as from page 17568, line 8-13 to the last paragraph since it all deals with findings of the comparison between different ice nucleation schemes.)

Page 17657-17658: Discussion and conclusions: I think one very interesting finding of our study is the fact that the 3 different parameterizations agree surprisingly well in the representation of Ni and the contribution from heterogeneous ice nucleation to the total ice nucleation. However, BN and KL parameterizations used in CAM5 give strongly different results than BN and KL in GEOS5 and ECHAM5. So, probably these results are more driven by input parameters (w, T, RH_i) and the assumptions of aerosol distribution (immersion vs deposition freezing, size of aerosols, etc.). I think this is an interesting results and should be stressed more here.

Minor:

Page 17636, Line: 24: Please include “the”: As a result, the experiment. . .

Page 17637, Line: 19: Please include the following reference: Hoose and Möhler, Heterogeneous ice nucleation on atmospheric aerosols: a review of results from laboratory experiments, Atmos. Chem. Phys., 12, 9817-9854, 2012

Page 17638, Line 3: “reduces” rather than “reduce”

Page 17638, Line 4: you could add here some more detailed information, such as: If homogenous nucleation is prevented totally from occurring or if the rate of homogeneously nucleated ice crystals is reduced depends on several parameters, such as

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number of aerosols, supersaturation, temperature, vertical updraft.

Page 17638, Line 13: I think the proper reference here is rather Hendricks et al 2011 than Lohmann et al., 2008. Please replace or add that.

Page 17638, Line 15: Please add the latest IPCC report here.

Page 17638, Line 29: Please use “may hinder” instead of “hinders”

Page 17639, Line 2-8: Since you are using the KL and BN parameterizations in your study I think these two references (Kuebbeler et al., 2014 and Barahona et al., 2013) can be explained in a little more detail here.

Page 17639, Line 8: Please give the according reference here.

Page 17639, Line 11-13: I don't understand this sentence. Please explain in a little more detail.

Page 17640, Line 3: Please remove “also”.

Page 17640, Line 20: Why do you neglect deposition freezing? Several studies have shown that this is the more efficient ice nucleation mechanism.

Page 17641 Line 3-7: I don't understand this sentence. Please explain.

Page 17641 Line 8: What does aai stand for in N_{aai} ?

Page 17641 Line 8: What does “current” mean? Ice crystals from previous timestep? Ice crystals from other sources?

Page 17641 Line 25: There seem to be problems with many of the equations. There are weird symbols in equation 1, 3, 7,8,9.

Page 17646, Line 7: what is f_{hom} ? It wasn't introduced yet.

Page 17647, Line 17: I suggest to rewrite the part “, there are no w_{sub} data larger than 0.24 m s^{-1} ” as follows: “the cut-off in Default is not exactly 0.2 m s^{-1} but 0.24 m

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s-1.”

Page 17648, 1st paragraph: To me, NoPreice fits observations best with only a small shift towards too high ice number concentrations. Can you comment why?

Page 17649, Line 18: So far as I know, most models have problems getting the correct trend of Ni with T suggested by Krämer et al., 2009. I think here it should be mentioned more clearly that the modeled trend of Ni tending to increase with decreasing temperature is the contrary of what is observed.

Page 17650, Line 9-11: I think here you are a bit too optimistic about the performance of Preice. To me, Nofhom is as good as Preice.

Page 17652, Line 18-19: I suggest to rewrite “equals to $w_{\text{sub}} - w_{i,\text{pre}}$ ” as follows: “ $(w_{\text{eff}} = w_{\text{sub}} - w_{i,\text{pre}})$ ”

Page 17653, Line 5-8: Please rewrite as follows:” NoPreiceBN, PreiceBN, NoPreiceKL and PreiceKL experiments are also analyzed, but not shown here, because the effects of PREICE from experiments using BN and KL parameterization are similar.

Page 17654, Line 23-25: You write “. . .Ni is reduced in low-level cirrus. . .”. I don't fully understand what you mean; reduced compared to what? Please be more specific.

Page 17656, Line 6-8: To me, changes in CDNUMI of KL are only smaller than LP and BN between 30-60N. In regions larger than 60N or smaller than 30N all parameterization are rather similar. Please be more specific here.

Page 17656, line 11-13: Why are changes in CDNUMI in Preice between 60-80N of opposite sign than the other experiments? This should be mentioned and explained here.

Page 17656, line 20: It looks like changes in IWP are not statistically significant. Can you comment on this?

Page 17657, Line 8-9: This sentence is confusing. Make clear, what is same and what

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is different between the 3 parameterizations.

Page 17568, Line 11-13: please explain why.

Figures:

Figure 2: The clouds look rather like cumulus clouds than like cirrus clouds. I strongly suggest to change that.

Figures 3,4,5,9: The colors of the two experiments Default and NoPreice are not distinguishable on printed paper. Please use a different color for one of both.

Figure 9: You are presenting changes (!) of the variables LWCF, SWCF, CDNUMI and IWP. You are missing a delta sign in the upper left corner of each of the four plots in front of the variable.

References:

Hoose and Möhler, Heterogeneous ice nucleation on atmospheric aerosols: a review of results from laboratory experiments, *Atmos. Chem. Phys.*, 12, 9817-9854, 2012

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 14, 17635, 2014.

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