We thank the reviewer for the constructive comments and suggestions for improving this manuscript. The reviewer's comments are in italics and our responses in standard fond below.

General Assessment:

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 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.

Based on the reviewer's comments, we corrected the spelling and grammatical errors and improved the manuscript structure. We also made further analysis to understand the differences between different ice nucleation parameterizations.

Major comments:

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.

Reply: Following the reviewer's comment, the sentence was rewritten and related citations were added. The new sentence now reads as: "In recent years, significant progress has been made in both cirrus cloud measurements and cirrus cloud modeling (e.g., Heymsfield et al., 2005; Krämer et al., 2009; DeMott et al., 2011; Cziczo et al., 2013; Jensen et al., 2013; Diao et al., 2014; Barahona and Nenes, 2011; Jensen et al., 2012; Spichtinger and Krämer, 2013; Murphy, 2014)."

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 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."

Reply:

1) Based on the reviewer's comments, the structure of Section 2 was changed: Subsection 2.1 titled "CAM5" introduces cloud schemes, aerosol scheme, and

radiation scheme in CAM5. Subsection 2.2 titled “Cirrus cloud scheme in the standard CAM5” describes the cirrus scheme and ice nucleation parameterization in CAM5. Redundant repeating information was removed.

2) We swapped subsection 2.3 and 2.4. The text was modified accordingly. For example, the first paragraph in the new subsection 2.3 was rewritten as follows: “In this study, several modifications have been made in the ice nucleation scheme in CAM5. First, the effect of PREICE is taken into account (subsection 2.4). Second, …”. In the new subsection 2.4, the text on page 17641 line 15-17 was replaced by “To account for the effect of preexisting ice (PREICE) we introduce PREICE into CAM5 based on the concept of Kärcher et al. (2006)”.

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.

Reply: In the discussion paper, the numbers in Figure 2 are used for illustration purpose only. In the revised manuscript, we modified Figure 2 with ice crystal numbers derived from CAM5 model results that describe cirrus cloud evolution within one model grid cell (3°N, 75°W, ~198 hPa, ~217 K). In this experiment, the updraft velocity is set to 0.2 m s^{-1}, and the sulfate number concentration is set to 100 cm^{-3}. Heterogeneous nucleation is not taken into account. The simulation is run 3 months. Just one cirrus cloud evolution process is shown here.

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 Wsub, RHi 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.*

Reply: Following the reviewer's comment, we made additional analysis and add more discussions on why the simulated PREICE effects between CAM5, GEOS5, and ECHAM5 are different.

With the same BN ice nucleation scheme, the PREICE effect in CAM5 is stronger than that in GEOS5. In GEOS5, the heterogeneous nucleation mechanisms include immersion and deposition ice nucleation on dust, black carbon, and soluble organics (Barahona et al., 2014). The global mean ice number concentration (N_i) produced from the heterogeneous nucleation and its percentage contribution to the total N_i are ~22 L^{-1} and ~30%, respectively (see Fig. 7 in Barahona et al., 2014). In the CAM5.3 simulation with BN, N_i from the heterogeneous immersion nucleation on the coarse mode dust and its percentage contribution to the total N_i are 5.1 L^{-1} and 9.4%, respectively, which are significant lower than that from GEOS5. As a result, in CAM5.3 there are less IN competing with the homogeneous ice nucleation and PREICE has a relatively larger impact. This might be the main reason why the PREICE effect from CAM5.3 with BN is stronger than GEOS5.

With the same KL scheme, we see a stronger PREICE effect in CAM5 than in ECHAM5-HAM. In the model used in Kuebbeler et al. (2014), the ice nucleation process requires that the model grid is supersaturated with respect to ice (*RHi* > 100%). Furthermore, the depositional growth of ice crystals is treated based on the model grid *RHi*. If a model grid is supersaturated and a large number of PREICE is present, the depositional growth of these PREICE will remove the supersaturation in the grid, and thus prevents the following ice nucleation. In other words, the ice deposition growth (the cirrus clouds scheme condenses mass on PREICE) in ECHAM5 also takes into account the effect of PREICE by removing the supersaturation. However, in CAM5 the treatment of deposition growth of ice crystals only relaxes the grid *RHi* towards 100%, and does not remove the supersaturation well below 100% (Morrison and Gettelman, 2008). In other words, the ice deposition growth process cannot prevent the ice nucleation process from happening. As compared to ECHAM5, ice nucleation processes frequently occur in CAM5. Thus, the effect of PREICE during the ice nucleation process, which is represented by reducing the updraft velocity driving ice nucleation parameterization, is weaker in ECHAM5.

We have also compared the number concentrations of heterogeneous IN, sulfate particles and ice crystals presented in Kuebbeler et al. (2014) with those from CAM5. Freezing mechanisms (immersion, deposition) in GEOS5 and ECHAM5 are also discussed. The corresponding text can be found in the third paragraph of section 4.

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. However, 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.

Reply: The default CAM5 uses dust in the coarse mode as potential heterogeneous IN and there is no upper limiter applied. In the model it is assumed that dust is internally mixed with sulfate, and thus only immersion freezing of dust particles is considered. Furthermore, soot contribution to the heterogeneous IN is turned off. Therefore, it is assumed that all dust in the coarse mode can act as IN (immersion freezing) in the default CAM5. In CAM5, the global averaged heterogeneous IN (i.e., dust in the coarse mode) number concentration in cirrus clouds is 10.4 L^{-1}. 71% of IN number occurs in the range of 1-10 L^{-1}, 25% of IN number in the range of 10-100 L^{-1}, and 3% of IN number in the range of >100 L^{-1}. In this study, we do not focus on dust or soot effect on cirrus clouds. So we didn't modify the default dust number concentration used in the heterogeneous nucleation parameterization. To compare different ice nucleation parameterizations under the same aerosol condition, for BN and KL parameterizations, we also used all coarse mode dust (no upper limit) as the potential heterogeneous IN.

We note that, our earlier study (Zhang et al., 2013) using the standard CAM5 model with LP and BN parameterizations has studied the influence of the upper limiter (f_a) to dust IN. Results from the BN parameterization with f_a=100% are similar to those from the LP parameterization (f_a=100%). With a larger f_a (100% versus 5%), not only are more crystals produced by heterogeneous nucleation but also the homogeneous nucleation is suppressed and contributes considerably less to the total ice crystal production. In the Northern Hemisphere, where the main sources of dust aerosols are located, a larger f_a (100% versus 5%) leads to considerably less ice crystals. That study used the standard CAM5 without considering the preexisting ice effect.

Following the reviewer's comment, we performed a simulation using the updated CAM5 model with the BN parameterization and applied an upper limiter (5%) to the coarse mode dust particles. Model results show that the change in total ice number is moderate because of the low number concentration of coarse mode dust (<10 L^{-1}) used to drive the ice nucleation parameterization. This suggests the limiter to the coarse mode dust might not be the main reason why the results differ so strongly between the models (CAM5 with BN versus GEOS5 with BN). We note that the contribution from heterogeneous ice nucleation to total ice crystal number concentration is ~30% in GEOS5, while it is only 9.4% in CAM5.3. Based on our sensitivity tests, the effect of PREICE increases with increasing contribution from the homogeneous nucleation. This might be the main reason why the PREICE effect in CAM5.3 (with a higher homogeneous nucleation contribution) is stronger than GEOS5.

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.)

Reply: Following the reviewer's comment, we reorganized this section. The model modifications (for PREICE and f_{hom}) and evaluation are discussed in the first paragraph. The PREICE effect on the relative contribution of homogeneous nucleation versus heterogeneous nucleation is discussed in the second paragraph. The comparison between three ice nucleation parameterizations is discussed in the last paragraph. We moved the text from page 17657, line 8-12 as well as from page 17658, line 8-13 to the last paragraph.

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,RHi) 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.

Reply: Following the reviewer's comment, this finding is stressed more in the conclusion section, such as "The differences among this study, Barahona et al. (2014) and Kuebbeler et al. (2014) can be more driven by differences in meteorological input parameters (W, T, RHi), the assumptions of aerosol distribution (immersion versus deposition freezing, aerosol characteristics, etc.), and the methodology of parameterization implementation, than ice nucleation parameterizations themselves."

Minor comments:

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

Done.

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

Done.

Page 17638, Line 3: "reduces" rather than "reduce"

Done.

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 number of aerosols, supersaturation, temperature, vertical updraft.

Following the reviewer's comment, we added some new text regarding this issue: "If homogenous nucleation is prevented totally from occurring or how the rate of homogeneously nucleated ice crystals is reduced depends on several parameters, such as number of heterogeneous IN, temperature, vertical updraft (Liu and Penner, 2005; Kärcher et al., 2006; Barahona and Nenes, 2009)".

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.*

Done. We added “Hendricks et al., 2011”.

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

Done. We added “IPCC, 2013”.

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

Done.

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.

A detailed explanation about these two references (Kuebbeler et al., 2014; Barahona et al., 2014) will be given in section 2.5 and section 4. Here in this paragraph, we just focus on the effect of PREICE. We note that Barahona et al. (2013) has been published in GMD this year. So “Barahona et al., 2013” was changed to “Barahona et al., 2014”.

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

Done. We added the according reference “Barahona et al., 2014”.

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

This sentence was removed here. The detailed explanation about “occurrence probability of homogeneous freezing events in cirrus clouds” was added in the previous paragraph:

“Analysis of in situ data sets obtained in cirrus clouds found that ice saturation ratio, Si, is highly variable both spatially (Jensen et al., 2013) and temporally (Barahona and Nenes, 2011), and that ice nucleation takes place only in a portion of cirrus cloud rather than in the whole area of cirrus cloud (Diao et al., 2013; 2014). However, most GCMs assume that cirrus cloud is homogeneously mixed, and ice nucleation event occurs in the whole area of cirrus cloud (Gettelman et al., 2010; Salzmann et al., 2010; Hendricks et al., 2011; Kuebbeler et al., 2014). Only until recently have GCMs attempted to account for the fraction of cirrus cloud where homogeneous freezing occurs (f_{hom}) (Wang and Penner, 2010; Barahona et al., 2014; Wang et al., 2014).”

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

Done.

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

We agree with the reviewer that deposition nucleation on pure dust particles can be an efficient ice nucleation mechanism. In CAM5, dust is internally mixed with sulfate in the aerosol treatment. Thus, there is only immersion freezing of dust particles considered in the standard CAM5. In this study, we use the default heterogeneous nucleation mechanism in CAM5.

Page 17641 Line 3-7: I don’t understand this sentence. Please explain. Page 17641 Line 8: What does aai stand for in Naai?

Cloud droplet activation usually happens at the cloud base or for the newly-formed clouds at every cloud level, as represented in CAM5. In comparison, ice nucleation can happen inside preexisting cirrus clouds, because relative humidity with respect to ice (*RHi*) up to or even more than 120% are frequently observed inside cirrus clouds (Krämer et al., 2009). To make the sentences (Page 17641 Line 3-7) clearer, we modified them as "The cloud droplet activation in warm liquid-phase clouds only occurs at the cloud base of preexisting clouds or in all levels of newly-formed clouds, as represented in CAM5. In contrast, ice nucleation is allowed to happen in all levels of preexisting cirrus clouds in CAM5 if the nucleation thresholds are met because *RHi* up to or even more than 120% are frequently observed inside cirrus clouds (Krämer et al., 2009)."

The "aai" in "N_{aai}" stands for the aerosol activation to form ice crystals (i.e., ice nucleation) in the cirrus microphysics scheme. We note that ice crystals can also come from the convective detrainment in CAM5.

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

The "current" indicates ice crystals from previous time step. The corresponding sentence was changed to "New ice crystals will be produced if the in-cloud ice number concentration, N_i, from the previous time step falls below N_{aai}".

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.

All equations are double-checked. The equation editor in Microsoft Word 2013 (for Mac) is used for this manuscript. Using Preview (Mac PDF viewer), formulas look normal.

Page 17646, Line 7: what is fhom? It wasn't introduced yet.

The "f_{hom}" has been introduced in previous subsection 2.4 "Thus, we can find out the fraction of cirrus cloud, f_{hom}, where …". In the revised manuscript, the "f_{hom}" was mentioned several times in previous section 2.3. For clarity, we changed this sentence to "The f_{hom} used for the LP parameterization, as discussed in subsection 2.3, is also used for BN and KL parameterizations".

Page 17647, Line 17: I suggest to rewrite the part ", there are no Wsub 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 s^{-1}."

Done. The sentence was changed as suggested.

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?

Compared to the Preice and Nofhom experiments, the occurrence frequency of higher N_i (>100 L^{-1}) from the NoPreice experiment is increased significantly. It seems that NoPreice fits observations best. We note, however, that the observed N_i is from in situ measurements, while the modeled N_i represents the averages over the whole area of cirrus clouds within a model grid cell (~100 km). In addition, although measurements during the SPARTICUS campaign have significantly reduced the shattering of ice crystals, it is unclear whether the very high *Ni* (>1000 L^{-1}) is affected by the shattering artifact. When considering these issues, it is difficult to judge that NoPreice fits observations best. We have added these notes in the revised manuscript.

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.

Following the reviewer's comment, we more clearly pointed out that the modeled trend of N_i 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.

Nofhom agreed better with observations as compared to Preice. The sentence (Page 17650, Line 9-11) just focused on the comparison between updated CAM5.3 and Default CAM5.3. Following the reviewer's comment, this sentence was replaced by "the Preice and Nofhom experiments show better agreement with observations …".

Page 17652, Line 18-19: I suggest to rewrite "equals to Wsub minus Wi.pre" as follows: " (Weff=Wsub-Wi,pre)"

Done.

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.

Done. The sentence was rewritten based on the reviewer's comment.

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.

We rewrote the sentence as follows: "One distinct feature of N_i distribution patterns from these experiments is that N_i reduces towards lower altitudes".

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.

We rewrote the sentence as follows: "ΔCDNUMI from the PreiceKL experiment is smaller between 30°-60° N as compared to other experiments. In regions larger than 60° N or smaller than 30° N, all experiments are rather similar".

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.

This was mentioned and explained as follows: "ΔCDNUMI from the Preice experiment between 60°-80° N (negative) has opposite sign than the other experiments (positive). However, these changes are generally within the ranges of two standard deviations".

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

We added more comments on this as follows: "Compared to ΔCDNUMI, the fluctuation of ΔIWP is more complicated because many other microphysical processes (especially in mixed phase clouds) can impact ΔIWP. Furthermore, the changes in cloud properties caused by aerosol indirect effects may modulate atmospheric circulation and water vapour transport, and then impact IWP in other regions. Thus, ΔIWP from all experiments are not statistically significant".

Page 17657, Line 8-9: This sentence is confusing. Make clear, what is same and what is different between the 3 parameterizations.

The method used for calculating $W_{i,pre}$ in the LP parameterization is exactly same as the KL parameterization. Also, the PREICE effect in the BN parameterization is considered by the same concept of KL parameterization. Our box model calculations show that $W_{i,pre}$ calculated from BN and KL parameterizations are very similar under the same condition.
This sentence (now in the third paragraph of section 6) was rewritten as follows: "Both LP and BN parameterizations consider PREICE effects based on the concept of KL parameterization".

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

We added the explanation why there are large differences in the PREICE effect between CAM5 and GEOS5 (now in the third paragraph of section 6).

Figures:

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

Done. We changed that. Cirrus clouds are now indicated by ovals.

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.

Done. The color of NoPreice was changed to orange.

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.

Done. A delta sign was added in front of the variable.