Reply to Anonymous Referee #2

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We thank the referee for his/her careful reading and helpful comments. Our replies are given below.

General comments

• The topic is of interest to the readers of ACP, but the conclusions which are drawn here are so far rather technical and mainly aimed at other users of the CAM5 model and/or the ice nucleation schemes which are discussed here. I suggest that the revised version should include a deeper discussion of the physical interpretations and the implications of the results presented here.

It is true that this paper focuses on the LP and BN ice nucleation schemes implemented in CAM5, and does not include results from other models. Nevertheless we believe our results are useful to a large number of readers of ACP. In response to the reviewer's suggestion, we add in the revised manuscript comments on the following points:

Our analysis of the SPARTICUS measurements shows that in this campaign, a marked increase of ice crystal number concentration with decreasing temperature was observed in synoptic cirrus clouds. Such a relationship was not clearly seen in datasets from earlier campaigns from the Northern Hemisphere mid-latitudes, possibly due to the very limited temporal coverage of the earlier research flights, and potential observational errors in the warmer temperature range (> 225K) caused by the shattering of large crystals on older instruments. This highlights the value of new instruments and long-term observations like SPARTICUS in helping to improve our understanding of ice clouds as well as our ability to simulate them in climate models.

When evaluating the model results against observations, we have concentrated on the parameter-induced sensitivity. Since the three parameters discussed in the manuscript are widely used in many models and parameterization schemes, our results provide information that will probably be useful for the developers of other models.

The sensitivity experiments performed in this study show that the simulated ice crystal size and number concentration are sensitive to the parameters discussed. We also identified which of those parameters have large impact on the simulated cloud forcing. Because the CAM5 model is widely used to investigate climate-related topics such as aerosol-cloud-precipitation interactions, the parameter-induced sensitivities we analyzed provide hints on the uncertainties associated with such applications.

• At the same time, the relation to the previous works by Liu et al. and Barahona et al. should become clearer, as this is not the first comparison between the LP and BN schemes.

Following the reviewer's suggestion, we explicitly point out the following in the revised manuscript:

Barahona et al. (2008) described in detail the BN scheme and compared it with LP and several other parameterization schemes in box model calculations. Liu et al. (2012) implemented the BN scheme in CAM5 and investigated the role of dust ice nuclei on climate using model simulations with the LP and BN schemes. In the present work, we look specifically into the SPARTICUS region and concentrate on parameter-induced sensitivities of the LP and BN schemes.

Detailed comments

• p. 1202, line 10 (and other occurrences in the text): what are "in-situ" ice nucleation schemes? (Where else should the ice nucleate if not in situ?)

We meant to use the word "in-situ" to refer specifically to ice nucleation processes in cirrus (pure ice-phase) clouds, in order to distinguish them from the detrainment of crystals from shallow/deep convective clouds. This is clarified in the revised manuscript.

• p. 1202, line 18: here, it is unclear what the "critical ice crystal size" means (it is explained later, but should already be understandable in the abstract).

We have replaced *"the critical ice crystal size"* by *"the critical diameter that distinguishes ice crystals from snow"* in the revised manuscript.

• p. 1203, line 15: "freezing fraction of the aerosol population": I believe it should read "freezing fraction of the potential IN population" (or dust population). Only a subset of all aerosols can nucleate ice heterogeneously. The same correction should be applied in Table 3, description of group B.

Corrections are made throughout the manuscript.

• p. 1203, line 25: Here, I am missing a discussion of the laboratory measurements which indicated a significantly higher value of alpha (e.g., Skrotzki et al 2012).

Following the reviewer's comment, the following sentences are added to the revised manuscript:

"As pointed out by Skrotzki et al. (2012), so far this parameter has not been well

constrained by laboratory experiments, with values obtained by different research groups spanning about three orders of magnitude (e.g., Table 1 in Skrotzki et al., 2012, and Table 4 in this paper). Magee et al. (2006) inferred from laboratory measurements a most-likely range of 0.0045 to 0.0075 for ice particle growing at −50◦C, while recent cloud chamber experiments of Skrotzki et al. (2012) suggested a value of 0.6±0.4 rather independent to temperature in the wide range between 190K and 235K. The reason for the discrepancies is not yet clear (Skrotzki et al., 2012)."

• p. 1204, line 10: "meanwhile have good data quality in general": This statement is very universal, a more differentiated judgment would be in place.

Following the reviewer's comment, the sentence is changed into:

"In contrast, direct measurements are more straightforward to use and meanwhile can provide concurrent data for various quantities at high frequency."

• p. 1205, section 2: I am missing here some information about the sampling rate and its conversion to a spatial scale. How much horizontal distance has to be covered before a value of total Nice and a size distribution can be derived?

The observational data are available at the frequency of 1 measurement per second. Considering the speed of the aircraft, the sampling rate translates to a horizontal distance of 150 – 200 m. This is clarified in the revised manuscript.

• p. 1206, line 12: Not sure what a "sample" is, does this mean individual crystals?

By "sample" we mean an individual measurement record of ice crystal distribution, temperature, RHi, etc., obtained at 1 Hz frequency. This is clarified in the revised manuscript.

• p. 1207, line 3: some more information should be provided about how many modes and which aerosol species are treated in the model (in particular for the ice nucleating aerosols sulfate and dust).

In response to the reviewer's comment, the following information is added to the revised manuscript:

The aerosol species considered in the model include sulfate, sea salt, primary and secondary organic matter, black carbon and dust. Among these species, dust particles can act as ice nuclei. Ice particles can also form through the homogeneous freezing of aqueous sulfate solution droplet. The size distribution of aerosol particles is described by either three or seven lognormal modes. In this study we use the three-mode version MAM3, which consists of the Aitken, accumulation and coarse modes. Dust in the accumulation and coarse modes participate in heterogeneous ice nucleation; Sulfate in the Aitken mode is allowed to participate in the homogeneous nucleation.

• p. 1208, line 16: Please mention explicitly that deposition nucleation and freezing by soot are neglected (and why).

Deposition nucleation and immersion freezing on soot are neglected in the model due to the still poor understanding of these processes (Kärcher et al., 2007). This is stated in the revised manuscript.

Reference:

Kärcher, B., Möhler, O., DeMott, P. J., Pechtl, S., and Yu, F.: Insights into the role of soot aerosols in cirrus cloud formation, Atmos. Chem. Phys., 7, 4203-4227, doi:10.5194/acp-7-4203-2007, 2007.

• p. 1208, line 21: I'm surprised by a) there is a maximum value for the in-cloud vertical velocity, not a minimum value as e.g. in Morrison and Gettelman, 2008, for liquid droplet activation; and b), that this maximum seems to be quite low. Please provide more information.

We have confirmed that in the standard code release of CAM5.0 and CAM5.1 (http://www.cesm.ucar.edu/models/), there is indeed a maximum (not minimum) in-cloud updraft velocity of 0.2 m s^{-1} used for ice nucleation. In the literature the threshold has been documented differently. The Scientific Description of CAM5.0 (Neals et al., 2012, page 135) described a minimum value of 0.2 m s^{-1}. In the work of Gettelman et al. (2010), the reference simulation used a minimum value of 0.2 m s^{-1}, while the sensitivity experiment WSUB tested a maximum of 0.2 m s^{-1}. Liu et al. (2012b) also mentioned a maximum value of 0.2 m s^{-1}. In the code used in our study (CAM5.1), the updraft velocity is limited to 0.2 m s^{-1} maximum. This is clarified in the revised manuscript.

• p. 1208, line 25: "with a constant freezing rate": this sounds wrong. Heterogeneous ice nucleation in mixed-phase clouds should be strongly temperature-dependent.

The sentence was unintentionally truncated during editing. It should read: *"with a constant freezing rate below -20°C"*. Correction has been made in the revised manuscript.

• p. 1209, section 4: It is certainly a challenge to compare a climatological simulation to 6 months of observational data. It should be briefly discussed in how far the SPARTICUS data can be considered to represent a climatological average, i.e. are not influenced by peculiar weather conditions, impacted by unusual aerosols (e.g. volcanoes) or the like.

Following the reviewer's suggestion, the following discussion is added to Section 4 of the revised manuscript:

Strictly speaking, it may not be ideal to carry out model evaluation by comparing a 5 yr climatological run with measurements from a single campaign. However, considering that (i) most field observations of cirrus clouds in the past lasted several hours and days, while the SPARTICUS data used here have a relatively long temporal coverage (200 hours spanning 6 months), (ii) there were no peculiar weather conditions, extreme events or unusual aerosol concentrations (e.g., after volcanic eruptions) encountered during the flights, and (iii) the model data used in the analysis are taken from the same months of year as the measurements, the SPARTICUS data is valuable for evaluating models and constraining uncertain model parameters. In the future, better evaluation strategies can be employed by using, e.g., the nudging capability that recently became available in the CAM5 model (Kooperman et al., 2012) that can constrain the model towards the reanalysis of weather conditions during the observational period.

Reference:

Kooperman, G. J., Pritchard, M. S., Ghan, S. J., Wang, M., Somerville, R. C. J., and Russell, L. M.: Constraining the influence of natural variability to improve estimates of global aerosol indirect effects in a nudged version of the Community Atmosphere Model 5, Journal of Geophysical Research: Atmospheres, 117, 2156–2202, doi:10.1029/2012JD018588, 2012.

• p. 1210, line 20 onwards: this paragraph is more or less a repetition of p. 1203, please avoid.

Following the reviewer's suggestion, the corresponding paragraph is revised so as to avoid repeating information that has already been mentioned in the introduction.

• p. 1213, line 27: To my eyes, it looks like much more of the data lie above 100%. But this might be due to the representation. I'm not sure where the bin boundaries are (is there a boundary at exactly 100% or lower)? It would be helpful to plot a horizontal line at 100% in Fig. 6.

In response to the reviewer's comment, horizontal lines at 100% are added to Fig. 6. The RHi shown in this figure is the input value to the ice nucleation scheme (i.e., the model state before ice nucleation happens). Therefore values higher than 100% appear frequently.

• p. 1214, line 5: I'm not sure I understand what is said here. Does this mean that heterogeneous ice nucleation is triggered already at RHi=100%? This clearly would explain the low RHi bias in Fig. 6.

In the current CAM5, heterogeneous nucleation can be triggered at RHi=120% (local value) when ice nuclei are available. The model considers the sub-grid RHi variability for ice nucleation by multiplying grid mean RHi by 1.2. This means to trigger the heterogeneous ice nucleation, the *grid-box mean* RHi needs to be higher than 100%. (Gettelman et al., 2010). This is clarified in the revised manuscript.

• p. 1215, line 9: "insignificant" - on what level?

The sentence is change into:

"Differences smaller than the standard deviation of the monthly mean values have been masked out".

• p. 1215, section 5.2: This is an interesting result and seems to apply that the 5% activation are reached very often. In that case, the time dependence included in the CNT-scheme becomes irrelevant. It could mean that the time dependence is too strong from the beginning. Please discuss.

Results of the sensitivity experiments indicate that, at least in the SGP region, the 5% freezing fraction produces results that agree best with observation. This small fraction suggests that the stochastic component of CNT is small, and the resulting nucleation spectra are practically time-independent.

• p. 1216, section 5.4: The differences between these sensitivity experiments occur mainly at temperatures above -35C, i.e. at mixed-phase cloud conditions. In this range, the model has not been evaluated with respect to ice number concentrations and freezing mechanism, therefore the best value for Dcs found here could be impacted by compensating errors in the mixed-phase cloud microphysics.

For Fig. 12, the measurement records and model results that include cloud droplets have been excluded from the data, in order to minimize the impact of mix-phase clouds on the analysis. This was not mentioned in the discussion paper, but is clarified in the revised manuscript. In addition, the results indicate that the Dcs value of 250 μm leads to the best agreement between the simulated and measured effective diameter in the whole temperature range shown in the figure. Although the mixed-phase cloud microphysics may have an impact, we do not think the performance at the temperatures relevant for pure-ice clouds (lower than -35°C, which is the focus of this study) is strongly affected.

• p. 1216, section 5.4: It should also be discussed in how far a global and temperature-independent value of Dcs can be a good assumption. Depending on temperature and humidity, ice crystals grow to different shapes and are e.g. affected by riming to different degrees, and all of this is expected to influence the cloud ice to snow conversion.

We fully agree with the reviewer that ice cloud microphysics in the real atmosphere is much more complex than currently represented in CAM5 (and many other models). The classification of ice-phase hydrometeors into crystal and snow is, by itself, artificial. Recent studies have proposed new methods to describe the gradual transition from small to large ice particles (Morrison and Grabowski, 2008; Lin and Colle, 2011). In this paper, however, we restrict our investigations to the value of Dcs under the current set-up of the CAM5 model. A value of 250 μm produces best agreements between our simulations and the SPARTICUS campaign, although more observational data are needed to find out whether this value is widely applicable to many regions over the globe. How a dynamically determined Dcs would change the behavior of a highly nonlinear model is hard to predict a priori, but needs to be investigated by future modeling studies.

References:

Morrison, H., and Grabowski W. W.: A Novel Approach for Representing Ice Microphysics in Models: Description and Tests Using a Kinematic Framework. J. Atmos. Sci., 65, 1528–1548, 2008

Lin, Y., and Colle B. A.: A New Bulk Microphysical Scheme That Includes Riming Intensity and Temperature-Dependent Ice Characteristics. Mon. Wea. Rev., 139, 1013–1035, 2011

• p. 1217, line 1: I'm not sure whether this refers to the different model versions in the sensitivity experiments of this study or model versions employed by other authors. Please clarify. In any way, it would be good to include TOA net radiation into Table 6 (even if the simulations are not retuned).

Here (Page 1217 Line 1) we are referring to different CAM5 code versions released by NCAR. In order to tune the cloud forcing and radiative balance at the model top, Dcs was set to different values in CAM5_0 (325μm) and CAM5.1 (400μm), which are different from the originally proposed values in Morrison and Gettelman (2008) for CAM3 (Dcs=200μm) and the value documented in Gettelman et al. (2010, 250μm). This is clarified in the revised manuscript.

In addition, the TOA net radiation is added to table 6.

• p. 1218, line 9: Would it be of interest to show the Krämer et al (2009) data also here?

The Kraemer et al (2009) dataset was not used in the discussion paper because (1) it contains only 4 flights in mid-latitudes, and (2) the ice crystal number concentrations were obtained with the Forward Scattering Spectrometer Probe (FSSP) which is known to have the potential issue of overestimating the ice crystal number concentration due to shattering of large crystals (typically happening at warmer temperatures). In the revised manuscript we have included discussions on this issue at various places. For further details, please see our reply to referee #1.

• p. 1218, line 12: Please comment on how it is assured that this "particular type of cirrus clouds" is also sampled from the model.

To select only the synoptic cirrus clouds, we excluded the model grid boxes in which the ice crystal number tendencies due to convective detrainment or cloud droplet freezing were non-zero. This is clarified in the revised manuscript.

• p. 1219, line 28: I don't think the negative bias in the updraft velocity was mentioned earlier, please give more details.

Our recent work has revealed that the sub-grid updraft velocity used by the ice nucleation schemes in CAM5 features negative biases in comparison with observations from multiple campaigns in the mid-latitudes, partly due to an artificial upper bound of 0.2 m s^{-1} used in the model for cirrus clouds, which leads to a factor of 1.5-2 difference in the average updraft velocity. This is explained in the revised manuscript.

• p. 1219, line 4: "will report the results in a separate paper": It would actually be nice to have these here, because these further analyses are so closely connected to this work (and the present paper is not overly long yet).

Our work on the updraft velocity is carried out in a broader scope that includes comparison with multiple observational campaigns and simulations with a different GCM. It also includes investigations on different ways to represent sub-grid variability of the updraft velocity. We think a separate manuscript would provide a better context for the discussion hinted at the end of the present paper.

• Table 3: Please provide also the contact angle for the group C experiments, as the parameters given here don't define the ice nucleation efficiency yet.

The contact angle for dust, which is used in the CNT-based heterogeneous ice nucleation scheme, is 16° (Chen et al., 2008). This is clarified in the revised manuscript.

Reference

Chen, J.-P., Hazra, A., and Levin, Z.: Parameterizing ice nucleation rates using contact angle and activation energy derived from laboratory data, Atmos. Chem. Phys., 8, 7431-7449, doi:10.5194/acp-8-7431-2008, 2008.

• Fig. 1: What are the black lines? It would be good to show the topography on this plot.

The black frame indicates a 6°× 6° (about 600 km x 600 km) area centered at the ARM SGP site (36°N, 97°W), within which the ice crystal number measurements are used for model evaluation in the paper. This is explained both in the caption of the figure, and in Section 2 (SPARTICUS aircraft measurements) of the revised manuscript. The topography is included in Figure 1.

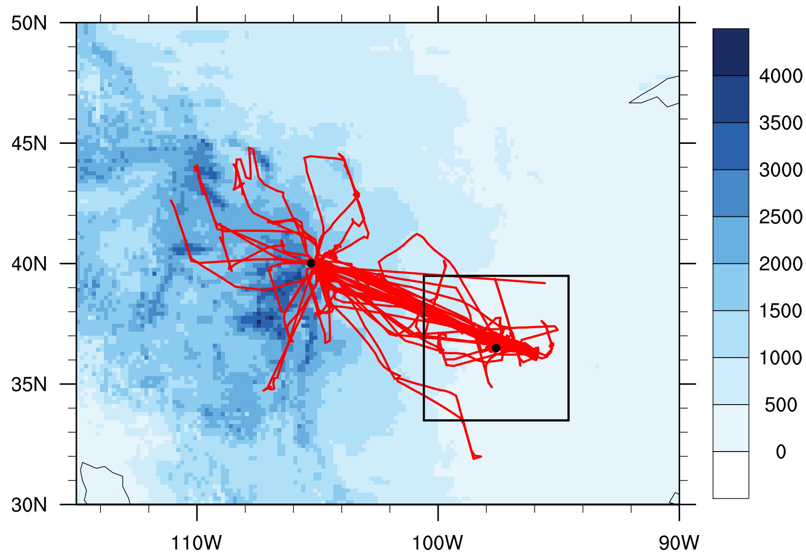

Fig. 1. Aircraft trajectories during the SPARTICUS field campaign. The color shading shows the surface elevation (unit: m). The black dot at 40°N indicates the location of Boulder, CO. The black frame indicates a 6°× 6° (about 600 km × 600 km) area centered at the ARM SGP site (36°N, 97°W), within which the ice crystal number measurements are used for model evaluation in this paper.

• Fig. 4 and Fig. 8: In the left figure, Het./Hom. is the legend and LP/BN are the columns, and these are swapped for the right figure. If there is no strong reason for this, I would find it less confusing to use Het./Hom. as the legend and LP/BN as the columns also in the right plot.

In the left panel we use stacked boxes to make the point that the partitioning between homogeneous and heterogeneous nucleation rates is different in the LP and BN schemes. In the right panel we are still putting the LP and BN results side by side in order to contrast the HOM and HET contribution in each scheme. The swap of legend and column labels happens because the two panels are of different chart styles (stacked versus clustered). We understand the referee's concern of confusion, but prefer to leave the panels as they are, because the current set-up helps to contrast two simulations (LP and BN in Fig. 4, fmax = 5% and 100% in Fig. 8).

• Fig. 6, caption: Koop et al (2000) doesn't provide a freezing threshold, so this line probably refers to a fixed freezing rate?

The (dashed) line in each panel indicates the threshold RHi value required for homogeneous nucleation of liquid solution droplets with 0.5µm radius, which implies a fixed freezing rate. This is clarified in the revised manuscript.

Technical comments

- ***p. 1205, line 16: double brackets***
- ***p. 1213, line 21: extra blank***
- ***p. 1214, line 19: is → are***
- ***p. 1216, line 12: crystal → crystals***
- ***p. 1217, line 15: "becomes too far from the observation": colloquial, please reformulate.***
- ***p. 1217, line 22: a weaker sedimentation sink***
- ***p. 1217, line 26: metics → metrics***
- ***p. 1217, line 27: Insert semicolon or full stop before "thus".***

Corrections have been made following all the technical comments above.