**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Evaluating and constraining ice cloud parameterizations in CAM5 using aircraft measurements from the SPARTICUS campaign" *by* K. Zhang et al.

Anonymous Referee #2

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General comments

Zhang et al. analyse CAM5 simulations with two different ice nucleation schemes and with systematic variation of a couple of "tuning" parameters related to ice clouds. Results are extracted for a region centering around the ARM SGP site and are compared to aircraft observations from the SPARTICUS campaign. Such a comparison between global climate model output and in-situ measurements is a difficult task, which the authors accomplish quite well by focussing on statistical features in the observations versus model results. 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. 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.

From a formal point of view, the paper is well written (some language corrections are provided below), the figures are clear, the article is well structured and the length is appropriate. I thus recommend acceptence subject to minor revisions.

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?)

- 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).

- 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.

- 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).

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

- 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?

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

- 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).

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

- 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.

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

- 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.

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

- 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.

- 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.

- p. 1215, line 9: 'ìnsignificant" - on what level?

- 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.

- 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.

- 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.

- 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).

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

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

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

- 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).

- 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.

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

- 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.

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

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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".

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 1201, 2013.