

Interactive comment on “Sensitivity of cirrus and mixed-phase clouds to the ice nuclei spectra in McRAS-AC: single column model simulations” by R. Morales Betancourt et al.

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Responses to Anonymous Reviewer #1:

We thank the reviewer for the positive and very helpful feedback. Responses to specific points raised follow below in italics.

Page 14931: “. . . I suggest that you include (for citation purposes) several of the new parameterizations developed over the last few years. . . .”

Thank you for suggesting this. References to it, and to some of the work described there were added to the manuscript.

C7721

Page 14931, line 28: Since the term “homogeneous freezing of deliquesced aerosols. . . .”

This suggestion was included in the manuscript.

Page 14932, first paragraph: I have some problem with how the logic is built in this paragraph: “Below T_{hom} , where ice-only clouds form, the supersaturation with respect to ice is the result of the competition between the rate of cooling of the cloud parcel and the condensation on the nucleated ice crystals. Therefore, it varies dynamically given the amount of IN present and the dynamical forcing available. Furthermore, since homogeneous and heterogeneous ice nucleation may occur simultaneously, the competition from both mechanisms and their impact on supersaturation further complicate the calculations.”

This suggestion was adopted and included in the manuscript.

Page 14933, line 28: Please indicate the location or region for where the TWP-ICE observations were made.

The TWP-ICE campaign was held in Darwin, Australia. This is now stated in the text.

Page 14935, line 24: How is Meyers et al. (1992) used in the BN parameterization? Do you use it as described in equation (1) or as it is original in Meyers et al. (1992)?

In the BN it is used in its original form. This is now stated in the manuscript.

Page 14937, line 7: It is stated that ice crystal concentration N_c is determined by ice nucleation, contact freezing and by melting of cloud ice. What about processes such as homogeneous freezing of deliquescent aerosols and homogeneous freezing of droplets, sedimentation of ice and accretion of cloud ice by snow

This sentence was rephrased in the manuscript to make it clearer. In the original manuscript we used the term “ice nucleation” to include the different processes by which it occurs. This is now made more explicit.

C7722

Page 14937: I am surprised to see that you do not have dust as aerosol input for heterogeneous ice nucleation, or from the text it seems this way. From line 11, you describe the soot and sulfate aerosol distributions but have left out dust. The PDA08 scheme has nucleation from dust included in the parameterization, so I would think that the dust should be included.

Dust aerosol input was taken into account for the contact freezing calculations, but not for the ice nucleation calculations. We performed new simulations in which dust is included also in the ice nucleation parameterization of BN-PDA08, and they are now incorporated in the manuscript.

Page 14939, Ice fraction: I question the steep curve for ice fraction, showing that the fraction of liquid is almost zero at as warm temperatures as $-5\text{ }^{\circ}\text{C}$. For example other studies from the same time period seem to indicate liquid at much lower temperatures. Would you care to comment on the different findings?

Thank you for pointing out this excellent reference. The ice fractions in our simulations show this steep transition most likely because the contribution of contact freezing might be overestimated. A more detailed discussion of this behavior is now included in the manuscript.

With the above said, in-situ observations summarized in several studies (e.g., Rotstayn 2000) rarely show supercooled water at temperatures below $-20\text{ }^{\circ}\text{C}$, contrary to what is often observed using radiometric and polarization measurements (for example, from satellite retrievals; Didenhoven et al. 2012). This suggests that considerable uncertainty (and observational variability) still remains in the principal characteristics of the freezing fraction – temperature relationship.

Page 14940, line 14: It is not given explicitly in Sect2.1 that the LP numerical correlations is immersion freezing. Please make this clearer in Sect2.1. Also the use of parenthesis is wrong, it should be after “2005”, not “Eq. 1”.

C7723

We apologize for this oversight. Section 2.1 (page 14934, line 16) states that “immersion freezing on soot particles was included in the parcel model simulations. . .” This is now rephrased to make explicitly clear that the numerical simulations that served as the basis for the LP parameterization included immersion freezing, as well as deposition freezing (represented by the Meyers spectrum), and freezing of deliquesced sulfate aerosol.

Page 14940, line 23: It seems that contact freezing is effective at temperatures warmer than 260K in this mode. In the temperature region of 270-265K, the Hallet-Mossop process can be an important source of ice crystals. Is this process included in the McRAS-AC model?

Ice multiplication processes are not included in McRAS. This is now explicitly stated in the text.

Page 14942, line 21: “rage” should be “range”

This is now corrected.

The authors use the term IN spectra and heterogeneous ice nucleation parameterizations. Are these terms the same or are there any differences between them? If they are the same, please be consistent and use only one of the terms. If they are different, can you explain how?

Thank you for pointing this out. Both terms were used interchangeably in the text; this issue is now addressed in the revised text.

Summary and Conclusions: I think you should include a statement in the final remarks on the specific need for more observations of contact freezing

Good point! Done.

A new paper in ACPD discusses the implementation of BN in CAM: “Sensitivity studies of dust ice nuclei effect on cirrus clouds with the Community Atmosphere Model

C7724

CAM5", Liu et al 2012. This study seems to be along the same line as the work presented in this paper. If the Liu et al. 2012 paper is published before this paper, I would like to see a reference to this paper, and perhaps a short discussion where the result from these two papers of compared.

A reference will be included in the revised text.

Figures:

Figure 4: Correct the title on the y-axis in figure a.

Figure 6: Correct the unit on the y-axis.

The typos in the figures were corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 14927, 2012.