

## ***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 Prof. Trude Strovelmo:**

Overall comment: This paper presents a single column modeling study of mixed-phase and cirrus clouds observed during the TWP-ICE campaign. Sensitivity simulations are presented exploring the sensitivity to the choice of ice nucleation schemes and spectra. The paper addresses a key issue within the field of aerosol-cloud-climate interactions currently, namely the topic of aerosol effects on cold clouds. The paper is well written and logically presented. However, I do have some concerns and questions that I would like to see addressed before this paper is published in ACP:

C7726

*We truly appreciate the constructive and thoughtful comments on the manuscript. Below is a detailed set of responses in italics.*

Comments Questions:

- Typically the term “cold clouds” refers to all clouds with temperatures colder than 0 degrees C, whereas here the term seems to refer to cirrus clouds. Please clarify.

*This is a good point. The terminology is now clarified in the manuscript.*

- In the Model description (Section 2), a thorough description of the treatment of the Bergeron-Findeisen process is missing. The BF process is referred to frequently in the discussion section, yet the reader has practically no information on how this process is represented in the model. Furthermore, it was not clear whether ice multiplication processes are included in this modeling framework. Both processes are crucial for phase transitions in mixed-phase clouds.

*Ice multiplication processes are not included in this modeling framework. This is now stated explicitly in the manuscript. The reader was deferred to the Rotstajn et al (2000) paper for the details of the Bergeron-Findeisen process, but the reviewed manuscript now includes some of those details explicitly.*

- Section 2.2: there is no mention of aerosol species other than BC and sulfate, yet later on contact freezing on dust particles is discussed. Furthermore, as the PDA08 ice nucleation parameterization also represents freezing on organics, were organic particles contributing to ice nucleation in this study? Please clarify.

*Thank you for pointing this out. Now the manuscript includes a description of the dust input for the contact freezing calculations. Contribution from organics was not included in the manuscript since no organic aerosol input was available. We will note this in the manuscript and leave it for future work.*

- Section 2.2: While heterogeneous freezing on BC seems to be the focus of this study, most laboratory studies find BC particles to be very poor IN. Please discuss.

C7727

*This study focuses on testing different ice nucleation parameterizations within a single-column version GCM. Several IN spectra were tested, and the differences between them were discussed. Black Carbon is included in the study since the numerical simulations for the LP parameterization already include the contribution of this aerosol species. BC was not intended to be a major focus of the study; new simulations which include the effect of dust aerosol is (besides its contribution to contact freezing) are now included in the revised manuscript. Finally, the IN efficiency of each aerosol species is taken into account by the nucleation scheme used here, which includes the (poor) IN efficiency of BC. This discussion is now included in the text.*

- Section 2.2: Is the vertical velocity distribution centered on 0m/s? Also, the choice of a standard deviation of 25cm/s seems arbitrary. The INCA campaign is mentioned, but surely the standard deviation varied through the campaign? As also mentioned below, I would like to see further sensitivity studies testing the robustness of your results to different choices of the standard deviation.

*The velocity distribution was indeed centered about 0 m/s; the 0.25 m/s standard deviation was prescribed because it is not explicitly resolved in the model and reflects the mean standard deviation for the INCA campaign. Clearly this quantity can vary, so we examine the sensitivity of our results to the quantity. The simulations show that the transition temperature from heterogeneous to homogeneous freezing varies only slightly with updraft velocity. Given the low IN concentrations in our simulations the PDA08 spectrum, the freezing will be dominated by homogeneous freezing except for very weak dynamic forcing.*

- Section 3: In general, some of the figures are not really (or only very briefly) discussed.

*Thank you for pointing this out. A more extended discussion of the figures is now included.*

- Section 3: It is very difficult to spot any differences between the different plots in Fig. 1

C7728

and Fig. 2. I suggest using difference plots (Sensitivity test – Control) for the sensitivity simulations.

*This suggestion is well taken. We have adopted the suggested and included anomaly plots.*

Section 4: The Choi et al. (2010) paper studied the relationship between dust aerosol concentrations and ice fraction, not aerosol concentration in general.

*Good point. This is clarified in the text now.*

- I am pretty sure the Bergeron-Findeisen process is more crucial in determining the ice fraction than is contact freezing. In fact, neglecting contact freezing only has a significant impact on the BN-PDA08 simulation, for which the nucleation rate is low enough that contact freezing makes a contribution. The fact that all simulations have very similar ice fractions, despite spanning a wide range of ice crystal concentrations, suggests that the BF process is very efficient. Are the simulated ice fractions consistent with what was observed during TWP-ICE?

*Indeed, the Bergeron-Findeisen process is the primary driver of cloud glaciation, and this is reflected in all of our simulations. The parameterized rate of conversion of liquid to ice in the Rotstajn et al (2000) formulation exhibits an important increase (5 orders of magnitude) from 273K to 268K, which further increases as supercooling is promoted. In that regard, the impact of crystal concentration is of secondary importance (see for example figures 6 and 7 of Rotstajn et al 2000).*

*Direct comparison of the simulated ice fractions to observations during TWP-ICE is difficult to conduct since the reconstructed 3D IWC fields (assimilated from radar and satellite retrievals) available from the ARM website, are not collocated with corresponding measurements of LWC, IWC and temperature. We therefore compare modeled LWP and IWP with the available assimilated observations.*

Section 4: In discussing the changes in cloud optical properties, it would be nice if you

C7729

could compute clouds optical depths for each of the cases.

*This is a good suggestion. A new discussion on the impact of the changes in cloud optical properties is now included in terms of the SW fluxes.*

- Section 4: I am pretty sure the dominating ice nucleation mechanism in cirrus clouds will be crucially dependent on your assumptions on the vertical velocity distributions. Several studies have reported that the relative importance of heterogeneous vs. homogeneous freezing is controlled by the vertical velocity. I strongly recommend sensitivity tests with shifted, wider or narrower distributions

*Done. The BN framework is capable of simulating the impact of temperature, IN concentration and updraft velocity on the competition between homogeneous and heterogeneous freezing. We now included sensitivity tests to the assumed PDF of updrafts, and a discussion of the impact of vertical velocity on the ice nucleation mechanism. In the case of our simulations, given the relatively low IN concentrations (e.g, with the PDA08), a considerably low updraft velocity ( $w < 0.1$  m/s) is required to favor heterogeneous freezing over homogeneous freezing.*

Minor/specific comments:

- Add "by" before "modifying cloud optical: : .." on line 23, Page 14929.

*Thank you. This is now done.*

- Page 14936, line 12: "its" should be "it is".

*Done.*

- Caption, Figure 1: The abbreviation "BN09" is used here but nowhere else.

*This is now corrected.*

- Figure 4a) I believe the Y-axis label should be  $N_{c,nuc}$ .

*Thank you! This typo is now corrected.*

C7730

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

C7731