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Discussions

Interactive comment on "Understanding cirrus ice crystal number variability for different heterogeneous ice nucleation spectra" *by* S. C. Sullivan et al.

Anonymous Referee #2

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Review of "Understanding cirrus ice crystal number variability for different heterogeneous ice nucleation spectra" by S. C. Sullivan et al.

This paper evaluates the ice concentrations (and sensitivities to input parameters) produced by several ice nucleation parameterizations within the framework of global-model simulations. I have significant concerns about the modeling framework and discussion, particularly concerning the importance of vertical velocity that is given little attention in the paper.

Major comments:

A number of detailed modeling studies have shown that both the number of ice crystals produced by homogeneous freezing and the competition between homogeneous freezing and heterogeneous nucleation depend strongly on the assumed vertical wind speed (cooling rate) driving the nucleation events (DeMott et al., 1997, JGR; Jensen and Toon, 1997, GRL; Kärcher and Lohmann, 2002, JGR; Karcher et al., 2006, JGR; Jensen et al., 2013, JGR). This paper includes very little discussion of vertical wind speeds in the CAM model that are used as inputs to the nucleation parameterizations. Are the resolved vertical winds used? That would presumably result in very low ice concentrations produced by homogeneous freezing and result in unrealistic predominance of heterogeneous nucleation. Is a turbulent kinetic energy scheme used? Is output from the CAM wave parameterization used? The model apparently produces larger vertical winds over the high mountain ranges, which is reasonable, but I doubt these are resolved vertical winds given the coarse resolution of the simulations (2.5 x 1.88 degrees). A detailed discussion of the vertical wind speeds in the model should be included in a study of the performance of ice nucleation parameterizations and the competition between heterogeneous and homogeneous ice nucleation. Ideally, the statistics of vertical wind speeds in the model should be compared with observations (i.e., from aircraft campaigns). If it cannot be demonstrated that the vertical wind speeds in the model are realistic, then the paper should clearly state that the analysis demonstrates the performance of the nucleation parameterizations *as they are operating in CAM* rather than indicating the competition between nucleation modes in the real atmosphere.

In the discussion of the comparison between measured and simulated ice concentrations, the authors note that sedimentation and autoconversion will reduce ice concentrations. Are the ice concentrations shown simply what comes out of the nucleation parameterizations, or are they mean ice concentrations in the model including cloud aging processes such as sedimentation, mixing, and autoconversion? If the former case is true, then the comparison with measured ice concentrations is inappropriate. Recent studies have shown that sedimentation produces broad regions of relatively low ice concentrations (Spichtinger and Gierens, 2009, ACP; Jensen et al., 2012, JGR; Jensen et al., 2013, JGR, Murphy, 2014, ACP). Sedimentation can reduce mean ice concentrations by up to an order of magnitude compared to concentrations immediately after ice nucleation events. Small-scale dynamics and entrainment further reduce ice concentrations as cirrus age (e.g., Jensen et al., 2011, JGR; Dinh et al., 2014, ACP).

Specific comments:

1. Page 21672, Line 25: There are better citations than Gettelman (2002) for the points made here. Chen et al. (2000, J. Clim.) would be appropriate for the climatic effects of cirrus. For the general issue of dehydration of air entering the stratosphere, Brewer (1949, QJRMS) and Jensen et al. (1996, GRL) would be appropriate.

2. Somewhere in the paper, it would be appropriate to cite the Cziczo et al. (2013, Science) observational evidence for common occurrence of heterogeneous ice nucleation.

3. Page 21677, Line 17: TAPENADE is not defined.

4. Section 2.1: The authors should add some discussion about the errors associated with approximations made in the BN parameterization based on comparisons with detailed cloud models. Perhaps these comparisons were made in earlier papers that can be cited here.

4. Section 2.4: Are the parameters in the classical nucleation theory spectrum for dust and black carbon based on field or laboratory measurements? Or are they purely theoretically derived?

5. Figure 2e: This Figure is missing recent field campaign observations made with improved instrumentation (e.g., SPartICus, MACPEX, and ATTREX; see Jensen et al., 2013, JGR, and Jensen et al., 2013, PNAS). These datasets provide much more extensive data (better statistics) than those included in Krämer et al. (2009) and are less susceptible to shattering artifacts that can inflate the measured ice concentrations.

The data is publicly available, and it is relatively easy to generate ice concentration statistics from the data. The temperature variability in ice concentrations apparent in the Krämer et al. (2009) dataset is very likely simply a result of limited sampling statistics. The ATTREX data shows that larger ice concentrations do occur in cold TTL cirrus on some occasions (Jensen et al., 2013, PNAS).

6. Page 21682, Line 15: There are more appropriate citations for the issue of surprisingly low ice concentrations at low temperatures. Specifically, Krämer et al. (2009) and Jensen et al. (2010, ACP) were the first to note this issue.

7. Figure 2: It looks like the ice concentrations are relatively high poleward of about 40° in the PDA13 simulations and in the northern extratropics in the CNT and AIDA simulations. Are these results simply caused by a lack of available IN resulting in predominance of homogeneous freezing? It is noteworthy that the ice concentration geographic variability is quite large, and I am not aware of any observational evidence for such large variability. Perhaps this goes back to the issue raised above of whether the ice concentrations shown are just after nucleation events or mean values at all cloud ages. Cloud aging processes would tend to wash out the large variability just after ice nucleation.

8. Figures 2–4: In each of these multi-panel figures, it would make comparison much easier if same color scale were used for all four panels.

9. Section 3.1: What are the relative contributions from homogeneous and heterogeneous ice nucleation in these simulations? Later in the paper, ice nucleation tendencies are used to infer relative contributions from different modes of nucleation, but would it be possible to simply show side-by side figures with the ice concentrations from homogeneous and heterogeneous ice nucleation?

10. Page 21684, Line 2: Presumably, the authors mean 1 cm^{-3} here rather than 1 L^{-1}.

11. Section 3.2: It is apparent from this section that there are considerable discrepancies between laboratory results of ice nucleation efficacy, particularly for BC. There are also a number of parameters in the INP parameterizations that are difficult to constrain. BC and dust concentrations in global model simulations are highly uncertain. It might be worth adding a general note that all of these factors lead to large uncertainties in the ice concentrations predicted by the parameterizations.

12. Page 21687, lines 11-12 and Figure 4b: The color scale on Figure 4b gives the impression that all of the sensitivities are negative. It would be helpful to indicate where the positive values occur. Perhaps a zero sensitivity contour could be added to the panels b and c of Figure 4.

13. Figure 5: It would help if the axes were labeled.

14. Page 21688, Line 23: The text refers to peaks in N_i in Figure 5d, but the figure just shows updraft speed and dust number sensitivities. This sentence should be rewritten to clarify the point.

15. Section 3.4: It is not clear what the meaning of mean efficiency is for spectra that produce both positive and negative tendencies, depending on geographic location, temperature, and vertical wind speed. Are the means calculated exclusively in regions where heterogeneous nucleation on dust dominates? The efficiencies would seem to make little sense when both homogeneous and heterogeneous nucleation are occurring or when BC and dust are competing. Wouldn't it be clearer just to show the contributions to ice concentration from homogeneous freezing, heterogeneous nucleation on soot, and heterogeneous nucleation on BC for each of the four parameterizations?

16. Page 21692, Lines 13-14: So, how large is the sulfate sensitivity in the model, and how does it compare to detailed cloud models? Kärcher and Lohmann (2002) showed that the sensitivity of ice concentration from homogeneous freezing to aerosol concentration is relatively weak (at least compared to the sensitivities to cooling rate

and temperature).

17. References: It would seem that there are errors in the reference list. For example, the Hoose and Möhler reference has four numbers following the year. Are these all page numbers? Other reference also have more than one number following the year. The authors should carefully proofread the reference list.

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