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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Impact of heterogeneous ice nucleation by natural dust and soot based on a probability density function of contact angle model with the Community Atmospheric Model version 5" *by* Y. Wang et al.

Anonymous Referee #2

Received and published: 22 May 2014

General comments: In this study, the authors improved heterogeneous ice nucleation parameterization for mix-phase clouds in CAM5 by implementing a classical-nucleation-theory-based parameterization (CNT), and further improved it by extending it from a single contact angle model to a PDF of contact angle model. The paper represents a significant advancement in parameterizing heterogeneous ice nucleation in global climate models and the authors have done a careful job on implementing the parameterization in CAM5. In particular, I applaud the authors efforts on a) refitting the CNT in single alfa mode and PDF of alfa mode to constrain the key uncertain parameters in these parameters from observations; b) utilizing the cloud-bore aerosol capability in CAM5 modal aerosol treatment and treating the cloud-born and interstitial aerosols separately in their heterogeneous ice nucleation parameterization; c) evaluating IN concentrations using available observations around global. The implementation in CAM5 makes it possible to examine how natural and anthropogenic aerosols affect mixed-phase clouds and further climate. Though I agree with the reviewer #1 on the challenge in representing the time-dependent behavior in climate models with long time step, this challenge is generally true for any time-dependent processes treated in climate models with long time step, especially those related to cloud microphysics, such as for droplet activation for liquid clouds and homogeneous freezing/heterogeneous freezing in cirrus clouds. I would suggest the authors to add some further discussion/review on how climate models treat time-dependent processes, which will help to put this study in context (for example, how Hoose et al. (2010) implemented CNT in their climate model). The paper is also well written and is a great addition to the literature dealing with heterogeneous nucleation and their parameterizations in climate models. I would strongly recommend the publication of this paper after some further clarifications are made:

Specific comments:

1. lines 15 to 23, page 7143: This part reads awkward, and needs rewording. In particular, "On the other hand" does not fit well.

2. Line 11, page 7145: "weak time dependence". It will be helpful for readers to further elaborate what "weak time dependence" mean here, as this concept are mentioned several times later in the paper.

3. Page 7149, line 25: dt is the model time step. This may warrant further discussions here, as also mentioned by reviewer #1. I noted that the authors have some discussion on this in the last paragraph of Section 5. I agreed with the authors that one way to handle this is to add ice-borne aerosol particles, though this is clearly beyond the

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scope of the current manuscript. Some further discussions on this will be helpful here. For example, dt may be thought as time scale to replenish IN population in a grid point. Also, how do other climate models handle similar situation.

4. Page 7150, line 18: so 2000 bins are used for calculating activation fraciotn from Eq. (4) for refitting some of uncertain parameters. How about online in CAM5? How many bins are used for calculating activation fraction online in CAM5?

5. Page 7151, line 9: why is the same activation energy as that in the single alfa model used for the PDF alfa model?

6. Section 3: How are aerosol number concentrations (dust and soot) used in the CNT single-alfa and PDF-alfa model caclucated? For 3-mode treatment, dust and soot are internally mixed with other aerosol species in the accumulation mode.

7. Page 7153, lines 1-15: there are some discussions here regarding coated vs. uncoated. However, how soot or dust particles are counted as coated particles are defined in next paragraph (lines 16-21). Suggest to move the latter before lines 1-15.

8. Page 7153, line 20: here one monolayer is used to define a coated particle. Any uncertainty on this definition and how this might affect your results? How does lab experiment define coated vs uncoated dust particles?

9. Page 7154, line 9: fine dust is separately as well in MAM7, which may affect coated vs. uncoated dust number concentrations.

10. Page 7153, line 12: dNi: how is this calculated? Does this Ni change include all changes in the Ni prognostic equation, such as sources/sinks from ice nucleation, advection, convective detrainment, conversion from ice to snow?

11. Fig. 7: so each data point sampled in Fig. 7 represent one annual mean value at a particular grid point? This needs clarification.

12. Page 7157, line 14-25: I agreed with reviewer 1 that the comparison between

observations and model can be challenging here, as aerosol number concentrations can be different, though the same location is chosen.

13. Page 7159, line 29: what is the prescribed size distribution for transported dust?

14. Figure 11: I applaud the authors effort for collecting these IN observations around global for evaluating their model. For those measurements in 1980s and 1970s, what are the measurement techniques used and how would that affect the comparison here with the model results. For example, 10s residence time is used for comparing with DeMott et al. results. Is that still used for comparing to these old results as well?

15. Page 7160, line 4: the size of sea salt particle is not small.

16. Section 4.6. I found section 4.6 is very interesting, and it is worth to add some further discussions. For example, while IWP in present day simulation is generally smaller with the new parameterizations compared to CTL, changes in IWP are generally larger. So what might cause this? Is this due to increased dust concentrations (partly due to less efficient wet scavenging) and increased soot concentrations in the PD simulations? As for changes in LWP and LCC, why are they generally larger than in CTL? How column-integrated droplet number concentration changes, and how LWP from stratiform clouds changes? It may be beneficial for readers to add some of these results into the abstract.

17. Page 7162, lines 5-7: This “On the other hand” does not fit well here, and suggest to reword this sentence here.

Technical corrections:

1. page 7144, line 10: “which includes” → “which include”?

2. page 7149, line 5: “we can” → “we”?

3. page 7153, line 7: remove “both”.

4. Page 7157, line 9: suggest to replace “it results” with something like “increasing the

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standard deviation results”.

5. Page 7159, line 10: “diagnosed” $\rightarrow$ “diagnose”?

6. Page 7162, line 26: “their behaviors explored in global models”. This sounds not like a complete sentence.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 7141, 2014.