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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol indirect forcing in a global model with particle nucleation" *by* M. Wang and J. E. Penner

Anonymous Referee #1

Received and published: 20 August 2008

Wang and Penner present a GCM-based estimate of the contribution of atmospheric nucleation and primary sulphate particles on the CCN concentration and on the 1st indirect aerosol radiative effect.

The manuscript is well written and highlights some current and important issues related to CCN sources, and the methodology used is scientifically sound. Some of the results presented are novel and should be interesting to a range of atmospheric scientists. I therefore recommend the manuscript to be published after the following comments have been addressed.

Specific comments:

1) The same authors conclude in the abstract of Wang et al. (2008, submitted to JGR)

that a 3-mode representation of sulphate in their model improves the simulation results against observations and that this indicates the importance to represent the freshly nucleated particles separately. Why was a 2-mode version then chosen for this study that specifically focuses on nucleation and the growth of these particles to CCN sizes? The authors should include a discussion of how the 2-mode only version might affect their results and conclusions.

2) As also pointed out by J. Kazil in his comment, this study assumes BL nucleation to occur via activation of sulphuric acid containing clusters and uses globally the prefactor reported for one boreal forest site. However, Riipinen et al. (2007) have shown that this prefactor can differ at least by one to two orders of magnitude between different sites - and even between two campaigns for the same site! Furthermore, Spracklen et al. (2008) showed that changing this prefactor by two orders of magnitude has significant effects on the predicted CCN from nucleation. The authors do mention the uncertainty in the nucleation mechanism at the very end of the manuscript; however, I would like to see much more discussion on this topic.

Technical comments:

- p. 13945, line 8 and 9: change , condensation to , condensation and wet removal, and e.g. to i.e.

- p. 13945, line 20: Laaksonen et al. (2005) measurements are from Po Valley, a highly industrialized and polluted region in Italy - not from a sub-Arctic boreal forest

- I suggest the use of term global aerosol models to refer simultaneously to both GCM and CTM based models. The currently used term global transport models may give the impression that models discussed are all CTM based. (E.g. top of page 13947)

- p. 13948, lines 2 and 7: change Shito to Sihto and Hyytiäliä to Hyytiälä

- p. 13948, line 18 (and again on p. 13960, lines 12-19): Spracklen et al. (2008) does include primary sulphate although this is not explicitly mentioned in their paper

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(personal communication with Dom Spracklen).

- p. 13954, line 5 and Figure 1: change nucleation parameterization to cloud activation parameterization or to cloud nucleation parameterization

- p. 13955, line 6: Are both equation numbers wrong? If not, explain in more detail as I cannot follow the logic here.

- p. 13958, line 3: Use consistently either Wang08 or Wang et al. (2008) throughout the manuscript

- p. 13962, line 27: change decrease of 6% to decrease of 6 percentage points (also p. 13963, line 14; p. 13965, lines 22 and 23)

- p. 13966, line 14 onwards: It is quite optimistic to say that BHN_PAR agrees well with the satellite data. In addition to 20-60 degrees S, there are large discrepancies over desert areas, over northern Eurasia etc.

- p. 13966, lines 26-27: In my opinion even a more important point is whether including BL nucleation improves the comparison compared to BHN_PAR case.

- p. 13970, line 5: change Lohmman to Lohmann

- p. 13973, line 1: what does smaller increase and larger decrease refer to?

- Table 1: give units for Ni. Give Ni for the largest dust mode as 7.3e-5 to be consistent.

- Table 2: what do the hygroscopicity values refer to? Please explain in more detail.

- Table 3: superscript f should be one line lower (BHN_PAR, not BLBHN). I also agree with Referee #2 that the simulation names are hard to follow.

- Figures 2 and 6: Y axis label is incomprehensible

- Figure 3 and 4 captions, and in text: refering to this model level as 930 hPa is much more informative than calling it the third model level

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- Figure 9: explain in caption that the values given in the title are global averages. Also in caption write reference to panels (a) and (b) before the explanations of figure content (the same comment applies to Fig. 11)

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