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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Microphysical simulations of new particle formation in the upper troposphere and lower stratosphere" *by* J. M. English et al.

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Review of "Microphysical simulations of new particle formation in the upper troposphere and lower stratosphere" by English et al.

This paper describes the coupling of the CARMA microphysics model to the WACCM GCM. Since WACCM extends to higher altitudes than typical GCMs, this combination makes for an ideal model for UT/LS aerosol studies. The authors compare predictions from this model with a large and diverse set of observations. The extent of the comparison is impressive and commendable, and it proved useful for finding biases in the model that could be corrected (e.g. with VDW coagulation correction, and the

meteorites). Finally the authors show the sensitivity of the model results to different nucleation schemes. They find that particle number concentrations and size distributions above 10 nm are not sensitive to the nucleation scheme.

The paper is well in the scope of ACP, provides good background info on the model, has the detailed evaluation and gives several new scientific findings (insensitivity of size dist to nucleation in the UT/LS, need for VDW coagulation fix etc.). I suggest this paper be published in ACP once the following minor corrections (mostly for clarification) have been addressed.

P 12442, L 6: I was confused when I first read that IMN is 25% higher than BHN, but the two BHN schemes were 2-orders of magnitude apart. I was thinking "How did they arbitrarily choose which BHN scheme to compare IMN to?". It made more sense when I realized that Yu's BHN scheme is related to Yu's IMN scheme when reading the paper. Maybe rewrite the sentence in the abstract.

P 12443, L 24: Should be changed to (changes in allcaps): "Pierce and Adams (2009) AND SNOW-KROPLA ET AL. (2011) calculated CHANGES IN IMN from solar cycle using a SECTIONAL model..."

Snow-Kropla, E. J., Pierce, J. R., Westervelt, D. M., and Trivitayanurak, W.: Cosmic rays, aerosol formation and cloud-condensation nuclei: sensitivities to model uncertainties, Atmos. Chem. Phys., 11, 4001-4013, doi:10.5194/acp-11-4001-2011, 2011.

P 12443, L 26: Yu did compare to BHN (and several other mechanisms) in the following paper:

Yu, F., G. Luo , T. Bates , B. Anderson , A. Clarke , V. Kapustin , R. Yantosca , Y. Wang , S. Wu, Spatial distributions of particle number concentrations in the global troposphere: Simulations, observations, and implications for nucleation mechanisms, J. Geophys. Res., 115, D17205, doi:10.1029/2009JD013473, 2010.

P 12443, L 29: In Kazil et al. (2010) they used activation nucleation in the CONTINEN-

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TAL boundary layer (not the entire boundary layer). Also, they said its likely because of the roll of organics, but activation nucleation is does not necessarily involve organics.

P 12445, L 17: Can you give more details on size-dependent wet deposition? Are all particles removed with the same efficiency (even particles too small to activate)?

P 12445, L 24: Is achieving stability good enough to achieve accuracy. I'd worry that if my timestep was long enough to have H2SO4 go negative, that only halving my timestep would still result in biased results. Which process is calculated first, nucleation or condensation? You could check the accuracy of your scheme by switching the order and running one more simulation to see if the results change.

P12446, L7 : Do you think the roll of organics in condensation/coagulation sink would affect your sulfate results?

P12446, L14: What do you mean by brownian diffusion of aerosols is important above 100 km? Is it important for diffusion between grid boxes there? Brownian diffusion is important everywhere for coagulation. Were you referring to diffusion between grid-boxes, or diffusion on the microscale.

P12449, L6: Why not use the ion formation rates from Usoskin, I. G. and Kovaltsov, G. A.: Cosmic ray induced ionization in the atmosphere: Full modeling and practical applications, J. Geophys. Res., 111, doi:10.1029/2006JD007150, 2006. However, I don't think it will change your results much (it will be a smaller change than the change between nucleation schemes), so probably not a big deal.

P12450, L9 and Figure 3: What is causing the 3rd SO2 max at the top of 3b?

P12451, L7: Figure 5c shows H2SO4 concentrations, not mixing ratios.

P12452, L9: I don't think this is an ironic situation. Maybe say "interesting" instead.

P12454, L18: Please add Snow-Kropla et al. 2011 to this list.

Section 4.2 and Figures 13 and 15. Did you pick individual grid boxes that corre-

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sponded to where flights were (at the same time of day as the flights) or did you average over all time and space in the regions? The averaging could lead to differences from the observations.

Section 4.2: Did you compare the size distributions when VDW corrections were turned on?

Page 12459, L10: Pierce and Adams (2009) would be a better citation here than (2007).

Page 12459, L17: VDW improves number concentrations. I'd like to see its affects on the size distributions too.

Figure 2b: What are all the different green lines?

Figures 7, 8, 9, 10, 11, 16: Can you make the blue and green colors lighter. They are hard to differentiate from the black lines in some instances.

Figure 11: VDW appears to have a large affect on aerosol surface area than number. This is not what I would have expected. Do you know why this is?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 12441, 2011.