**Atmospheric
Chemistry
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Discussions**

Interactive
Comment

Interactive comment on **"First comparison of a global microphysical aerosol model with size-resolved observational aerosol statistics"** *by* **D. V. Spracklen et al.**

D. V. Spracklen et al.

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We appreciate the helpful comments of the referee. The issues raised are well taken and we have responded to every comment made by the referee.

General comments

1. Points 1-4. The referee comments on our approach of making a statistical comparison between observations from field campaigns and output from a chemical transport model (CTM), suggesting that a 'point by point' comparison would be better. We believe both statistical and point-by-point comparisons are important in the evaluation of a model and we are currently pursuing both. In this paper, we compare probability distributions from each field campaign with probability distri-

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butions from the model. The analysis has highlighted some important differences between the model and observations. We now intend to do a point-by-point comparison to find out the causes of the differences.

2. Point 7. We have tried plotting model and observed distributions side by side as suggested by the referee. Unfortunately, because the plots are on a log-log scale, plotting in this manner hides some key differences between model and observations. We believe that the current way of plotting, although maybe not ideal, is the best way to illustrate these differences.

   As the referee points out the Heintzenberg climatology is size but not composition resolved. In Spracklen et al. (2005) we show the contribution of sea salt and sulfate to modelled aerosol distributions. In this paper we mainly restrict our analysis to a comparison between model and the Heintzenberg climatology. As this dataset does not provide information on aerosol composition we do not comment on the fractional contribution of sea salt and sulfate to model aerosol.

3. Point 8. We agree with the referee that the modelled number concentration at the minimum between the Aitken and accumulation modes is too low, an issue that is explored later in the paper.

4. Point 9. Again we have not commented on the fractional contribution of sea salt and sulfate to the modelled aerosol distributions as the observations provide no information on this quantity.

5. Point 12. We include a reference to Lewis and Schwartz (2006) and the discussion of uncertainty in sea salt flux paramaterisations.

6. Point 13. The referee points out that we use the Kulmala et al. (1998) nucleation scheme rather than the updated Vehkamaki et al. (2002) scheme. We discussed this choice in et al. (2005b). We add the following text to P8877, l3:

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'The mechanisms by which particle formation occurs in the atmosphere are highly uncertain. Suggested mechanisms include binary (H_2SO_4-H_2O), ternary (H_2SO_4-H_2O-NH_3), ion-induced and kinetic activation schemes. Here we assume that binary nucleation is the only particle formation mechanism. Other nucleation schemes also take place in the atmosphere leading to observed BL particle formation events over many continental areas (Kulmala et al., 2004). The impact of these events on BL total particle number is studied in Spracklen et al. (2006). Binary homogeneous nucleation rates are uncertain to within several orders of magnitude. The most recent binary paramaterisation (Vehkamaki et al., 2002), whilst more physically realistic, do not agree better with laboratory measurements than earlier schemes (Kulmala et al., 1998). Here we use the nucleation scheme of Kulmala et al. (1998) which calculates nucleation rates as a function of temperature, relative humidity and gas phase concentration of sulfuric acid.'

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8871, 2006.

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