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Geoscientific Model Development Discussions

Interactive comment on "Description and evaluation of GLOMAP-mode: a modal global aerosol microphysics model for the UKCA composition-climate model" *by* G. W. Mann et al.

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The paper describes in detail an updated version of the GLOMAP global size-resolved aerosol microphysical model and provides a comprehensive qualitative evaluation of this model against a wide ranging variety of observational data sets. Given the interest and uncertainty in the effects on climate of aerosols, the subject matter is appropriate for GMD. The aerosol microphysical model as described follows the M7 model described in Vignati et al. (2004) very closely. The descriptions of the size distribution; hygroscopic growth; nucleation; coagulation; condensation and ageing, are as the authors acknowledge, effectively the same as those in Vignati et al. (2004). The authors

should therefore seriously consider defining GLOMAP-mode as GLOMAP+ "a version of M7", rather than presenting it as a new modal version of GLOMAP. The only substantive difference that I found was the use of 10 mono-layers of H2SO4 rather 1 in M7 as the ageing threshold. In any case, the authors should note that the M7 model on which this work relies is not a modal model per se. As the authors note, in a 3 moment modal algorithm (e.g. Whitby and McMurry, 1997), the mode standard deviation is a prognostic variable. In half-modal models with fixed sigma parameters (e.g. Binkowski and Shankar, 1995) the coagulation and condensation rates are calculated by integrating over the modes. In M7 and GLOMAP-MODE, as in half-modal models the sigma is fixed, but the coagulation and condensation rates are based on a single particle diameter (the geometric mean diameter). This is not dissimilar to mono-disperse models such as the original GLOMAP, The only difference is that the calculated mass and number are, after the fact, spread out across a lognormal distribution with an assumed sigma. Vignati et al. (2004) called this "pseudomodal" and the authors should consider doing so as well.

Given the degree of reliance on the approach used in M7 in GLOMAP-MODE it would be appropriate for the authors to include the development and principal applications of the 'pseudomodal" approach, as background in order to put their work in a proper context. They refer to its' use in a GCM (Stier et al, 2005), but do not include CTM examples, either of the precursor to M7 (Wilson et al. 2001) or that of Vignati et al. (2010), which uses a very similar set-up to GLOMAP mode. This paper reports BC burdens and lifetimes (6.2 days & 0.14 Tg) that are very similar to those reported by the authors (6.6 days, 0.14 Tg), but uses a single H2SO4 monolayer aging threshold. One would expect the requirement of having 10 times more H2SO4 condensation for each aged particle would increase the BC lifetime and burden. That it does not implies that the condensation pathway is not as important as coagualation in ageing BC. Perhaps the authors would like to comment on this.

Regarding the use of aerosol observations from the Global Atmosphere Watch, I note

that the WDCA and the scientists providing the data are acknowledged. Could the authors also confirm that they have contacted the scientists concerned and obtained their approval for the use and citation of the data in this manner, in accordance with the WDCA data citation conditions specified in each dataset.

The paper's novelty is the coupling of the particular components TOMCAT+ GLOMAP + a version of M7, rather than presenting new concepts tools or ideas. Nevertheless such an approach is valid and contributes to advancing modelling science by demonstrating the reliability and robustness of the components. The methods and assumptions made are valid and clearly outlined, subject to the comments above, and the results support the interpretations and conclusions made. The description is complete and precise. Again, subject to the comments above, work is properly credited. The title reflects the contents, the model number is not however reported. The abstract is sufficient and the overall presentation and language is well structured, clear and easy to read. The formulae and abbreviations are understandable & I did not find any errors with units. The paper is clear and none of the material is surplus. Subject to the comments above, the references are appropriate. No supplementary material is provided.

Specific Comments.

1. I did not find the year or years of the met data used reported in the description of the model set-up. From some of the results it appears to be 2000 is this correct? 2. If a single year is used, would it be of interest to compare results with observations from that year where available?

Typos: P701 line 27 ofr – of P701 line 31 Wilson, G. – Wilson, J.

References: Binkowski, F. S. and Shankar, U.: The Regional Particulate Matter Model: 1. Model description 30 and preliminary results, J. Geophys. Res., 100(D12), 26191–26209, 1995.

Stier, P., Feichter, J., Kinne, S., Kloster, S., Vignati, E., Wilson, J., Ganzeveld, L., Tegen,

I., Werner, M., Balkanski, Y., Schulz, M., Boucher, O., Minikin, A., and Petzold, A.: The aerosol climate model ECHAM5-HAM, Atmos. Chem. Phys., 5, 1125–1156, 2005, http://www.atmos-chem-phys.net/5/1125/2005/.

Vignati, E., Wilson, J., and Stier, P.: M7: An efficient size-resolved aerosol microphysics module for large-scale aerosol transport models, J. Geophys. Res.-Atmos., 109, D22202, doi:10.1029/2003JD004485, 2004.

Whitby, E. and McMurry, P.: Modal aerosol dynamics modeling, Aerosol. Sci. Tech., 27, 673– 688, 1997.

Wilson, J., Cuvelier, C., and Raes, F.: A modeling study of global mixed aerosol fields, J. Geophys. Res.-Atmos., 106, 34081–34108, 2001.

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