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Geoscientific Model Development Discussions

Interactive comment on "Improved tropospheric and stratospheric sulfur cycle in the aerosol-chemistry-climate model SOCOL-AERv2" *by* Aryeh Feinberg et al.

Anonymous Referee #2

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This paper describes a new version of the aerosol-chemistry-climate model SOCOL-AERv2a, in particular very significant improvements to the tropospheric/stratospheric sulfur cycle. It is a bit of a textbook example for a GMD paper. The interesting results of the new version of the models are analyzed in detail but they are also systematically and thoroughly compared to the results obtained with the old version of the model including various sensitivity simulations. It is well written, clear and very helpful to the numerical modelling community working on atmospheric sulphate aerosols. I recommend publication. Nonetheless, the authors may wish to take on board the minor comments provided thereafter.

Abstract: There is something confusing in the abstract. It starts by recalling that there is a very good agreement between the stratospheric aerosol burden calculated by the old model version and a satellite-derived estimate (about 110 Gg S); I guess it is an old satellite estimate and it is for a volcanically quiescent aerosol burden. Later on, it is stated that that there is a good agreement between the stratospheric aerosol burden calculated by the new model version and a new satellite-derived estimate (about 160 Gg S). It is a bit confusing. Have the authors more confidence in the new satellite-derived estimate than in the old one? If that is the case (I have more confidence), why mention the old estimate in the abstract? I suggest to be selective and mention only the change in stratospheric aerosol burden from the old to the new model version and compare it to the new satellite-derived estimate.

P4, l16: I was wondering whether it would have been possible to run SOCOL by relaxing wind and T fields towards meteorological analyses. This would have partly removed sources of biases in global aerosol calculations, i.e. biases in transport or temperature-dependent processes such as condensation/evaporation as the top of the aerosol layer etc. . . I think that a more realistic transport and temperatures would help to confine the origins of biases to aerosol emissions, microphysical processes, and deposition.

P4, l27: I have some doubts about the realism of nucleation calculations (Vehkamäki et al., 2002) based on grid-box temperature and humidity in a global model. One would expect nucleation, a highly non-linear process, to occur in environmental extremes at sub-grid scales. Having said that, all models use this approach without accounting for sub-grid scale variability.

p20 ,l13-16: I agree that a too large cross-tropopause flux is a common bias in global models. On the top of convective transport and aerosol-specific heterogeneous processes, I think a big problem is the model resolution, in particular vertical. The vertical gradients in chemical fields are very steep at the tropopause, difficult to avoid artificial diffusion when the vertical resolution is of the order of a km, typical in global models. For instance, cross-tropopause ozone fluxes tend to be overestimated in global

models.

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