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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol microphysics simulations of the Mt. Pinatubo eruption with the UKCA composition-climate model" *by* S. S. Dhomse et al.

Anonymous Referee #1

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This paper describes the implementation of the GLOMAP modal aerosol scheme in the UKMA model. Subsequently, the stratospheric aerosol simulation is compared to observations after the eruption of Pinatubo and in quiescent volcanic periods.

Major comments:

The paper is well written and presents results in quite a large number of figures, which are described in detail. This makes the paper rather tedious to read. Moreover, the aim of the paper and the associated advancement of scientific knowledge are difficult to extract from the paper. As such, the paper would fit better in a journal like GMD,

because that is exactly what it describes. I my understanding, the scientific novelty of this paper is in the comparison of simulated size distributions with balloon soundings. However, the authors fail to explore these results further and do not work towards structural model improvements. With the model at hand they could perform interesting sensitivity simulations that would greatly enhance the science presented in the paper. Below, I give some suggestions that would make the paper suitable for publication in ACP. Without exploring one or more of these suggestions, I would advice the authors to consider GMD as journal to publish these results.

1. The definition of the aerosol modes. The size distribution the authors come up with differs substantially from the study by Niemeier et al. (2009). As I understand the description, the current study keeps the coarse soluble mode (mainly to simulate tropospheric sea salt aerosol), but with a sigma of 1.4 and uses the same sigma for the accumulation mode. The also state that the coarse mode is mainly populated in the troposphere and that similar sigma values are desirable (page 2812): "which is desirable numerically when the mode-merging algorithm transfers particles between the two modes in strong particle growth conditions". In the results section (figures 9 and 10) the authors note strong discrepancies between model and observations, especially on the separation of the N5 and N150 sizes. In the discussion the results are further analysed (figure 12) but no attempts are made to investigate the role of the mode definition. Further sensitivity studies exploring different definition would be a valuable addition to the paper, e.g. given the different definition in Niemeier et al. (2009). In sensitivity studies, the authors could define and test different mode bounds and explore the impact on the results.

2. Nucleation process. A recurring theme of the paper is nucleation. The authors could try reduced or enhanced nucleation rates, along the lines of what is written on page 2811: "Note that we also use the expression of Kerminen and Kulmala (2002) to convert the "real" nucleation rate from Vehkamäki et al. (2002) into an "apparent nucleation rate" at 3 nm." Another suggestion is already given by the authors themselves:

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emit part of the SO2 as sulfate, to influence the strongly H2SO4 dependent nucleation process.

3. Dynamical coupling. At various locations in the manuscript the authors suggest that the heating by the aerosol layer may be the reason for discrepancies between model and observations, because this effect has been excluded. Although I read that this is the subject of forthcoming studies, this paper would become less incremental if some results would be presented here, e.g. as sensitivity study.

4. Evaporation of H2SO4. In the methods the authors discuss the possible evaporation of H2SO4 from the aerosols. However, they do not include this process, while at several places in the result section they blame model deviations in the upper stratosphere to this process. ("In reality the rapidly evaporating particles will release their H2SO4 to the gas phase causing a sharp reduction around 40 km. .....As well as biasing gas phase sulphur species, our neglect of particle evaporation will overestimate surface area density at the top of the aerosol layer." Or page 2825: "which is almost certainly due to the particle evaporation not being resolved in the model". Why not include a sensitivity study that quantifies the model improvement?

Other major comments

In the abstract, the authors state: "Our comparison suggests that new particle formation in the initial phase of large eruptions, and subsequent particle growth to optically-active sizes, might be playing an important role in determining the magnitude of the climate impacts from volcanoes like Pinatubo" Since the climate impact from volcanoes is not studied in these uncoupled simulations, this claim cannot be made here. I think the authors want to say that their model has problems simulating the correct size distribution after the eruption, and that this might have implications for the calculated climate forcing. They should rephrase this statement to reflect the work described in this paper.

The authors only analyse monthly output. Most of the time this is OK, but in some

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places in the manuscript it is not appropriate. For instance, on page 2816-2817 the SO2 lifetime is analysed from monthly burdens. This seems inaccurate to me. Better use daily burdens or budget terms on the SO2 + OH reaction. Figure 2 shows the global burdens on monthly timescales. So, a large fraction of the SO2 is already moved to aerosol sulfate within one month. At first sight it looks like something is wrong with the units (TgS), but later I realized that monthly burdens are plotted. I suggest to use daily output to avoid confusion. Also in figure 9 and 10 monthly model output is compared to individual measurement profiles. I understand that the model is run in climate mode, so sampling the model at a specific output time is meaningless. But showing the variability within a month (e.g. as error bars for modeling) would be quite relevant.

Minor comments:

Page 2803, line 25: Guo et al. (2004) re-evaluated these numbers (18.5 Tg SO2, and 24 days. I think this should at least be mentioned.

Page 2804, line 13: “use” should be “used”

Page 2806, line 16: “the” before stratospheric should be omitted

Page 2807, line 14: pptm. Is this ppt by mass? Not commonly used, so clarify. Also in figure 3 I see that mass mixing rations (ppt) are plotted, while commonly ppt is interpreted as pptv. Line 26: lead & leading in one line. Please rephrase.

Page 2808, line 4: “at a fraction off”. Unclear. Please explain. Line 14: “faster”. Please specify “computationally faster”

Page 2809, line 1-4. Please state somewhere that these issues will be described in the sections that follow.

P2809, section 2.2.2: Unclear why a different density definition is needed in the stratosphere.

Page 2810, line 4. You mean: “vapour pressure p over a flat surface” ? Line 17: 10

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156 has a unit. Please specify

Page 2812, line 14: 2x also in the same line

Page 2813, line 12: unclear: “SO2 injection spread across eight model grid boxes between”

P2814: Last two paragraphs in 2.3 would be more suitable in the results or discussion sections.

Page 2816, line 21. 15 Tg S. Should this be 0.15 Tg S? Line 22: format Textor reference.

P2817, line 1-14: I suggest using several months after the eruption for making a better estimate of the SO2 lifetime.

P2817, line 18: 36 Tg is not slightly larger than 21.5 Tg. I would call this substantial.

P2817, line 20: Unclear how the aerosol lifetime was calculated. If only stratospheric burden was used, than it should be clarified.

P2817, line 21: Replace ’model aerosol’ by ’modeled Pinatubo aerosol’.

Page 2819, line 2: P-H2SO4 ? Please clarify

P2821, line 1 and P2822, line 8: Saturation values given are different. One of them should be corrected.

Page 2821, line 17: unit?

Page 2823, line 10: format reference.

Page 2827, line 6: “lower” should be “smaller”

P2830, line 8-10: Replace ’84 km’ by ’40 km’, since results presented here show aerosols only up to 40 km. Problems above 30 km due to missing evaporation should also be mentioned in this section.

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Page 2830, line 22. Remove “tropical” before AOD

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