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Interactive comment on "Initial fate of fine ash and sulfur from large volcanic eruptions" *by* U. Niemeier et al.

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I have read the manuscript with interest, as I am currently working on modelling volcanic ash plumes as well. I have summarised some comments on the manuscript below to initiate discussion about several assumptions and statements of the manuscript which need, in my opinion, revisions or clarifications.

One major issue is the amount of fine ash assumed to be released from Mt. Pinatubo. On page 17537 it is written, that 100 Mt of fine ash mass corresponding to 1 % of the total erupted mass is assumed to be injected into the stratosphere. The amount of fine ash fits very well with measurements of Guo et al (2004). However, these measurements might miss a substantial amount of ash, as the BTD methodology cannot always distinguish very well between meteorological clouds, ice particles and ash clouds. In

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Dartevelle et al. (2002) and Rose and Durant (2009) the amount of fine ash released from different volcanic eruptions (including Pinatubo) is reanalysed, pointing to a much higher amount of fine ash released from silicic volcanic eruptions (e.g. Mt. St. Helens: 44.6-51.5 % very fine ash, El Chichon: 29.0-48.4 % very fine ash). For the Pinatubo eruption it is shown by Dartevelle et al. (2002) that the deposits show a considerable amount of PM10 particles. Also in Wiesner et al. (2004) a considerable contribution of fine ash particles to the measured deposits is shown. The comparison of modelled deposition with sediment trap measurements by Wiesner et al. (2004) in Fig. 2 of the manuscript however reveals a considerable underestimation. This is mentioned by the authors and explained by the missing larger particles not included in the simulation. This should be discussed in more detail in comparison to the results of Dartevelle et al. (2002) and Wiesner et al. (2004) who found a considerable contribution by fine ash particles. Also the amount of fine ash injected into the troposphere should be discussed. Fero et al. (2009) present evidence that the majority of ash after the Pinatubo eruption concentrated between 16 and 18 km height. In addition Fig. 2 conceals the difference in measured and modelled deposits as different units are used for the model results and measurements. The model results in Fig. 2 should be shown in [mm] as well. Considering the publications of Dartevelle et al. (2002), Wiesner et al. (2004), Rose and Durant (2009) and Fero et al. (2009), I have serious doubts, that the release of only 100 Mt of fine ash into the atmosphere is sufficient to reproduce the Pinatubo ash cloud realistically.

Another issue related to the release of volcanic ash and SO2 in the model, is the assumption that SO2 and ash are released into only one model layer (30 hPa for SO2, 50 hPa for volcanic ash). I expect a non-negligible fraction of ash vertically distributed in the eruption column below the maximum height, in particular as coignimbrite clouds occur during the eruption of Mt. Pinatubo (Dartevelle et al., 2002). According to Fero et al (2009) the majority of ash after the Pinatubo eruption concentrated between 16 and 18 km height. I wonder if model simulations which initialise volcanic ash with a certain vertical distribution represent a better reproduction of the initial volcanic emission

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distribution as well as the dispersion in the atmosphere and distribution and amount of ash deposits. Also the radiative effects of volcanic ash on the evolution of the volcanic cloud discussed in the manuscript might not be realistically reproduced when assuming volcanic ash released only at 50 hPa and only 1 % of the total erupted mass.

A few other comments:

- page 17540, second paragraph: As it is known that the poleward transport in the model is overestimated, it is not very reliable to write that the model suggests a dominance of Pinatubo aerosol in the southern hemisphere and not of aerosol originating from Mt. Hudson.

- page 17541, third paragraph, last sentence: With a horizontal model resolution of 2.8°, about 300 km, a rotation of the volcanic plume during its eruption cannot at all be resolved. As model results are mainly 'tuned' to fit with the measurements of Guo et al (2004), I think there is not enough evidence to conclude that there is a good representation of transport by the model nor to go further, that rotation is not necessarily valid for the Mt. Pinatubo eruption.

- page 17548, last sentence: Can it really be concluded from the model simulations that the longer polar day and therefore longer active OH chemistry do not have an effect on the lifetime of SO2? On page 17536, first paragraph it is written, that climatological concentrations of background OH values are taken from Timmreck er al.(2003), I wonder if these are monthly mean values and if these really have a diurnal cycle included?

- figures: I think the manuscript contains much too much figures

References:

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