



## ***Interactive comment on “The global middle-atmosphere aerosol model MAECHAM5-SAM2: comparison with satellite and in-situ observations” by R. Hommel et al.***

**Anonymous Referee #2**

Received and published: 4 February 2011

Manuscript evaluation criteria

Scientific Significance: Does the manuscript represent a substantial contribution to modelling science within the scope of Geoscientific Model Development (substantial new concepts, ideas, or methods)?

2 - The paper describes an improved version of a model in a good amount of detail, and this paper will likely be the citation source for those wishing to perform simulations with this model.

Scientific Quality: Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work,

C768

including appropriate references)? Do the models, technical advances and/or experiments described have the potential to perform calculations leading to significant scientific results?

2 - The results of the experiments are discussed well, and this model will undoubtedly be useful in generating future scientific results.

Scientific Reproducibility: To what extent is the modelling science reproducible? Is the description sufficiently complete and precise to allow reproduction of the science by fellow scientists (traceability of results)?

2 - The description is good, and with a few possible details, this paper serves as a great description of the model's capabilities, with a specific focus on stratospheric sulfate aerosols. However, there is the issue of reproducibility which could be improved with a more detailed discussion of the specific processes involved that are new to this version.

Presentation Quality: Are the methods, results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)?

2 - The paper is nicely done. Some of the language/word choice could be improved, but I'm far from a competent copy editor, so I'll refrain from commenting further on this.

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Detailed comments

The journal suggests the model code and user manual be made available in a publicly accessible way. There should be a mention of this somewhere in the paper.

Page 1362, Line 1 - This sentence is a very blanket statement that needs to be qualified. I can think of any number of papers that discuss the climate response to stratospheric aerosols, so you really need to say what you mean here.

Page 1363, line 6 - "distribution median radius  $<0.2\mu\text{m}$ ": I've seen quite a few volcanic

C769

eruptions that fit this criterion. It's right to say that background aerosols are generally different, but you should be a bit more careful here.

Page 1363, line 7 - "distribution median radius  $>0.4\mu\text{m}$ ": This is for large (mostly tropical) eruptions, so you may want to say that.

Page 1363, line 15 - "measurements": Measurements of what, specifically? Radius? AOD? Everything?

Page 1363, line 19 - "formation and global dispersion": Do these also characterize the background aerosol layer? Up until this point, you've only talked about radius, so some discussion of this (if relevant) would be useful.

Page 1363, line 21 - "soluble aerosol above the tropopause": Do you mean the tropical tropopause? Or is this true for mid and high latitudes as well? Maybe some of the mechanisms could be briefly described.

Page 1363, line 28-29 - "systematically affects model predictions of aerosol and precursor transport and mixing": How much is the effect?

Page 1364, line 1 - "aerosol direct and indirect radiative forcing": I've seen plenty of treatments using bulk forcing where they did the direct forcing pretty well. Are you referring to anything in particular?

Page 1364, lines 2-4 - I'd be very careful with this statement, especially when using a reference as old as this one. IPCC (2007) says clouds are the greatest source of uncertainty. Please clarify what you mean here.

Section 1 (general comment): At some point, you might want to mention a shortcoming of the approach you've chosen. GCMs use the bulk treatment because it allows them to include other climate-relevant processes (including a dynamic ocean) which these more specialized models can't do due to the large computational time required. So what I'd like to see mentioned in this paper is what questions you plan to answer with your model and what the boundaries are for what questions you can ask of it.

C770

Section 2.1 - first paragraph: Very nice description.

Section 2.2 (general comment): In this paper, you only talk about sulfur. If SAM2 is restricted to sulfur parameterizations/calculations only, you might want to mention that.

Page 1367, line 9 - "time scales of interest": What are the scales of interest?

Page 1367, lines 24-25: Doing the simulations this way sounds very expensive. It would be interesting to give an estimate of the increase in required computer time.

Page 1368, first full paragraph: Excellent discussion of methods.

Page 1369, lines 4-6: This needs a bit more description, such as actual values of stratospheric injections you include. Do you also include quiescent emission rates?

Page 1370, lines 3-5: This will certainly affect your results. Do you have estimates of how much/what kind of error is introduced into your calculations by doing this offline?

Page 1371, line 1 - "data from km": Is there a number missing here?

Page 1373, line 23 - "last year of integration": Is this stable, or is there weather noise? Would it not be better to take averages of the last few years?

Page 1374, lines 9-10 - "where aerosol processes are more or less constrained to the troposphere": Does this imply stratospheric aerosol processes do not impact the global aerosol burden?

Page 1375, line 14: Whose treatment would you say is in better agreement with observations?

Page 1376, line 4 - "benchmark model": This benchmark model needs a lot more description. How does it work? References? Description? Why is this the benchmark? Is it more accurate than your current model, in which case, why are you using your current one? Also, why are you using a benchmark model? Aren't observations the benchmark?

C771

Page 1376, lines 4-9: If you have to reserve some of the sulfuric acid vapor for nucleation, doesn't this suggest there's something wrong with the rate of condensation?

Page 1378, line 9: I'm not sure what this line means.

Page 1380, lines 10-11: Is this due to conversion into aerosols? Or is it due to transport?

Page 1380, line 11 - "missing links": Please say a bit more about these.

Page 1380, line 13: I'm not sure what this sentence means. Does the H<sub>2</sub>SO<sub>4</sub> spend less time in the gas phase?

Section 3.3 (general comment): In talking about total atmospheric sulfate or tropospheric sulfate, there is the implication that the tropospheric treatment in MAECHAM5-SAM2 is different from ECHAM5. This is counterintuitive, because MAECHAM5 is a middle atmosphere model, so presumably most of the changes were made there. A bit of discussion on this would be helpful.

Page 1384, line 19 - "reproduced": Is that because these specific mechanisms were added to the model?

Page 1385, line 28: Is there any particular reason you chose this range?

Page 1386, line 23: This assumes SAGE II is accurate. Do you have another source of data or at least some estimates on the error involved in SAGE II measurements?

Page 1387, lines 8-27: Very nice discussion.

Page 1390, lines 6-7: Does this grid cell include the nearby Rockies and thus have the possible influence of orography? If so, it might be better to shift your grid cell downwind, since that's where the balloon will travel anyway.

Page 1390, line 8: You have to be careful with using the years 1998-2006, because the background aerosol layer has increased pretty dramatically over this time period

C772

(Hoffmann et al., 2009, GRL). It might be better to pick a few years that match your other simulation parameters (1999-2000, for example) rather than the whole range.

Page 1391, line 4 - "relative large uncertainties": If  $\pm 30\%$  is a lot, you need to say why.

Page 1391, lines 9-10: Is this an indication of a systematic bias in the model or the observations?

Page 1391, line 15: Well, sure, but consider the mass or radiative effectiveness of the aerosols in the two modes. In that context, it's effectively unimodal, which is more likely what the "typical" discussion has been.

Page 1391, lines 21-26: These particles are probably way too small to have a significant impact on radiation, surface area, and volume density. So you need to be a bit more clear about what you mean here. Also, back some of this up with calculations. For example, calculate surface area, volume, and radiative forcing with and without including this small mode.

Table 2 would be improved if there were a line or two of observations.

Figure 2 - "CSO" should be "OCS" for consistency with the rest of the paper.

Figure 8 - Parts of this graph don't make a lot of sense. How can a concentration be negative? Perhaps some more description is needed.

Figure 13, last two lines: Should  $\leq$  be  $\geq$  instead?

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Interactive comment on Geosci. Model Dev. Discuss., 3, 1359, 2010.

C773