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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 #1

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In the late 1970's, Turco and Toon developed a one-dimensional model of the stratospheric aerosol layer that contained the microphysical and chemical processes that affect the development of sulfuric acid particles in the lower stratosphere. This seminal work has been followed over the years by a number of other models, both two-dimensional and three-dimensional. All of them use slightly different techniques and obtain somewhat different results, but all of them agree to some extent with measurements of the aerosol made by satellite and balloon-borne instruments.

The paper under consideration is a discussion of the results of a model called MAECHAM5-SAM2 that simulates the stratospheric aerosol during non-volcanic (i.e.,

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"background") periods. The model results are compared to measured and derived values from the SAGE II satellite system and the balloon-borne optical particle counters from the University of Wyoming.

In general, I have no major scientific objections to the paper. The model agrees reasonably well with observations, although in some cases the agreement is not as good as one might expect. For example, in Figure 10 the authors compare the model derived mass densities with those obtained from SAGE II satellite data by a group at Oxford and a group at NASA Ames. The mass densities attributed to the SAGE data were obtained by Hommel et al. by using the volume densities obtained by the Oxford group and the Ames group and assuming all particles are 75% sulfuric acid, irrespective of the temperature (and water vapor content). This does not seem to be a reasonable comparison, particularly since it would be easy to calculate the SAGE derived mass densities using a more realistic composition as could be obtained simply using a climatology of stratospheric temperatures and water vapor. (By the way, the reference should be to NASA Ames Research Center, not to NASA Ames Laboratory.)

I think the authors missed an opportunity to consider some other interesting aspects of the stratospheric aerosol that could be treated with their model, such as the connection between the nucleation of sulfate particles in the subsiding air in the polar vortices and the formation of polar stratospheric clouds.

So, although I find the paper scientifically acceptable, I have two problems with the paper.

The first problem is that I found it difficult to read as a stand-along scientific article because there is insufficient information given about the models being used. For example, Section 2.2 on the aerosol module is too short to allow the reader to understand what is incorporated into the model or what assumptions have been made. The authors simply state the following: The microphysics model is based on the model of Timmreck and Graf, the model includes homogenous nucleation, condensation, evaporation and

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coagulation, the sulfuric acid particles are assumed spherical and in thermodynamic equilibrium with the environment, there are 35 bins, which are divided into four modes each advected individually, Brownian coagulation uses the scheme of Timmreck and Graf, sedimentation is as described in Stier et al. and wet and dry deposition are included. And that's about it! Any one of these points is worth a minor dissertation (which would not be appropriate), but I think a brief list of model properties and references to other papers is insufficient. Similarly, the transport model is only given a single paragraph. The description is so sparse, that I did not even realize it was a 3-D model, and read half the paper thinking this was a 2-D model! It was not until the sixth page of the paper that I read, "In this paper we evaluate a 3-D model that has been developed to study the dynamics of stratospheric aerosols..." This should have been the first line of the paper!

And finally, the chemistry module is also dealt with in a single paragraph.

I realize that models make many assumptions, and I might agree or disagree with some choices that were made in developing the model, but I feel that the present paper does not give enough information for the reader to be able to disagree with those choices. On the other hand, I also realize that the paper has the subtitle, "Comparison with satellite and in-situ observations" so the authors could argue that my complaint is not germane, that this paper is about the comparisons and not about the model. Therefore, I am merely expressing an opinion on something I would have liked to see in the paper, but will not insist upon.

Thus, I will overlook the fact that the models used are not fully described, and go on to a more serious problem with the paper. The article (as written) is not in acceptable English. (This was somewhat unexpected because two of the authors have affiliations at a reasonably well-known British university, and I know for a fact that they have an excellent command of English.) To illustrate my complaint, I will quote a few sentences from the paper.

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p. 1361 line 1: Far above the tropopause where nucleation is inhibited due to with height increasing stratospheric temperatures...

p. 1370 line 25: The instrument operated from October 1984 to August 2005 providing the so far longest record of...

p. 1371 line 4: ...Our analysis is build upon 1998 data...

p. 1373 line 12: Initializing aerosols rather than synthesise adequate abundances in the stratosphere solely from surface emission fluxes requires an assessment of the model's prognostic aerosol parameters in respect of their potential drift.

p. 1373 line 17: Shown in detail in Hommel (2008), we found that all diagnosed parameters are balanced...

p. 1374 line 17: It is widely approved that aqueous phase chemistry converts...

etc.

I could go on, but nearly every page has a grammatical or syntax problem and I don't feel that it is my responsibility to find every error. Consequently, I would suggest that the paper undergo a serious re-writing with an eye out for clarity, syntax and English usage.

In conclusion, if the paper is revised and formulated in better English, it will be acceptable. If the paper were revised and also included a reasonably complete description of the models being used, it would be more than acceptable.

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