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**Geoscientific
Model Development
Discussions**

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

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Reply to Anonymous Referee #1 , referring to interactive comments on "The global middle-atmosphere aerosol model MAECHAM5-SAM2: comparison with satellite and in-situ observations" by R. Hommel et al. From 27 October 2010

We thank the referee for the review of our manuscript and the constructive comments. We hope the revised manuscript will be much improved. Below, each comment (in quotation marks) is followed by an individual response.

"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., "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.)"

The reviewer raised a very good point. However, comparable data in literature were derived from different approaches. Our comparison of mass densities is based on the simplest approach one can apply. The particle composition we derived may be seen as a respective mean composition of lower stratospheric aerosols. We think that any further analysis makes sense, e.g. by using simultaneously observed lower stratospheric water vapour concentrations, but one has to bear in mind that the two SAGE II aerosol size products also apply two somehow different methods to consider the composition of aerosol particles. Wurl et al. (2010) used observed temperature

and humidity, consistently measured by the SAGE II instrument. The surface area and volume density, derived from the look-up table algorithm of Bauman et al. (2003) used aerosol extinction coefficients from precomputed complex refractive indices at 215K and a fixed 3 ppmv stratospheric water vapour content, which corresponds to an H2SO4 weight percentage of 70.85 %.

"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."

This is an excellent proposal and this study is already on our agenda. However, our manuscript presents results from a study which has to be seen as work in progress. This paper is the first in a series of companion papers, each of which will highlight certain aspects of aerosols in the upper troposphere/lower stratosphere (UTLS) region. The aim of this paper is an exposition of the models potential to reproduce observed characteristics of the stratospheric aerosol layer. We wanted to write such a fundamental model evaluation paper first, to prove the models reliability, before addressing specific scientific questions with respect to UTLS aerosols, e.g. new particle formation events as observed in Antarctica [Khosrawi et al., 2010; Eija Asmi, FMI, personal communication 2010]. In agreement with the referees deliberations, historically all models used to study UTLS aerosol, were somehow different in complexity. As stated in our manuscript's introduction, all previous models developed to study UTLS aerosol dynamics had a significant lower complexity than ours. Reproducing and comparing known and somehow validated UTLS aerosol quantities completes the manuscript and leaves no place to illuminate more specific aspects. As stated above, with the help of this model we are planning further studies on the behaviour of UTLS aerosol and associated effects, i.e. new particle formation in polar regions, climate effects due to Mt. Pinatubo aerosol, modulations of LS aerosol quantities induced by the equatorial quasi-biennial oscillation (QBO) of the stratosphere, just to name a few.

At this point, let us emphasise scientific highlights we achieved in this study once more: This study is the first global model study which directly assesses and confirms the inconsistency between aerosol size products from SAGE II and the aerosol size inferred from in-situ measurements as highlighted e.g. in WMO/SPARC ASAP (2006). From the binary homogeneous nucleation theory, which still reflects our understanding of sulphate aerosol formation in the cold environments of the free and upper troposphere, as well in the lower stratosphere, this study confirms new particle formation in the LS, as shown e.g. by Brock et al. (1995) and Lee et al. (2003), which is either not reproduced in other model studies or at different heights and magnitudes (e.g. Timmreck, 2001) which are not explainable by BHN. Furthermore, we found that planetary wave mixing transports significant quantities of fine mode particles to regions where nucleation is unlikely. Those particles are to small to be detected by space-borne remote sensing instruments, but are not unlikely to play a role in the catalytic cycles of stratospheric ozone chemistry (e.g. Drdla et al., 1999) and are likely to serve as an important transport regime for particles in a geoengineered stratosphere. We also show for the first time that sulphate aerosol degradation above regions of peak mixing ratios is more a reversible process due to synoptic scale temperature fluctuations, than a steadily evaporation process as thought so far (Hamill et al., 1977, 1997). We also want to point out again that precursor abundances in the LS are one of the most crucial factors when examining the evolution of stratospheric aerosol. Be believe, with our study we made substantial progress on the the whole "story" because we are drawing a consistent picture of the life cycle of aerosols in conjugation with their precursors. Prescribed OCS concentrations might introduce an uncertainty at this point (stratospheric OCS turnover in our study is about the half of that shown in other studies), but several factors about OCS are consistent with the literature, namely the global mean boundary layer emission strength, its tropospheric abundance and the shaping and extent of the stratospheric gradient in the mixing ratio.

" 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 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!"

We agree. The three-dimensional characteristics of our model was not stated clearly for the reader from the beginning on. In the revised manuscript we will provide the reader with a better understanding of the model which is applied in the study. Regarding the model description, Chapter 2, we followed a "trend" in peer review articles about model studies, where model descriptions are often sparsely described and model subcomponents are preferably is referenced wherever it is possible. We think its not worth going into detail here, we absolutely agree to the referees comment and rewrite the model description.

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

Since we use a off-line chemistry scheme we think it does not make sense to blow this chapter up with to much details. The set of reactions is similar as in comparable model

studies (Turco et al., 1979, Weisenstein et al, 1997, Niemeier et al., 2009). Future versions of the aerosol microphysics code will be embedded in a fully-coupled chemistry-climate model and will allow sophisticated sensitivity studies in a more consistent way than an offline model allows, taking into account reactions and mechanisms, e.g. as proposed by Veida et al. (2003) or Sorokin (2010). However, we will revise the chapter and will provide more information about the chemical scheme of the model. We will also revise the results chapter to highlight specific differences between this and previous model studies which might be caused by different formulations of sulphur chemistry in the models.

"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."

Indeed, the paper is on comparison between model and observations and so we are reluctant adding more of the proposed information. But see also above comments!

"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. 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."

We will follow the recommendation of the referee and revise the language of the manuscript.

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