Atmos. Chem. Phys. Discuss., 8, S9178–S9181, 2008
www.atmos-chem-phys-discuss.net/8/S9178/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global error maps of aerosol optical properties: an error propagation analysis" *by* K. Tsigaridis et al.

K. Tsigaridis et al.

Received and published: 16 November 2008

We would like to thank the anonymous referee for his comments. Detailed answers to the explicit points mentioned follow:

1) Probability: The authors, apparently, treat all input values as equally probable. This is far from the case. A Monte-Carlo type approach would be better to develop probability distributions for the parameters required for the radiative transfer model (g, SSA, etc). Then, these probability distributions could be applied to the aerosol maps. This may sound like an arduous task, but authors have already done most of the computationally-intensive work (Mie calculations). Lookup tables could be used based on the outputs presented in the figures.

In order to use a probability distribution for the cases studied requires that we assume

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

that some cases are more probable than another. We believe that such an assumption would give too much weight to our range of size and hygroscopicity assumptions. See also the valid questions from reviewers #1 and #2 on whether we indeed used a realistic range. For that reason, we decided not to give an extra weight to eg the central cases. The inclusion of the probability distribution would not be difficult, but it would require arguing for a given distribution, instead of keeping the simplest one, that all cases are equal. The partial error derived from this method is a conservative estimate at least. For this, we chose not to include it.

2) Ranges chosen: For size distribution, authors state that radius varied by only 20

This question appears to be incomplete. It looks like though that it was supposed to be the same with reviewer #1, point #15 and reviewer #2, point #3, to which we have tried to provide satisfactorily answers. Please see our responses there.

3) Detail level: The optical calculations are described with far too much detail, and not enough attention is given to the resulting optical property maps, which are the new part of this paper. Figures like 1, 2, 3 - even if not these exactly - have appeared in other papers and are not really new. This discussion should be condensed greatly. Then, more attention should be given to the figures 5-10. There is some discussion of where uncertainties are largest, but this should be expanded. This leads into my next comment.

We will follow the reviewer's suggestions.

4) Figures: I think that Figs 5-10 could be more informative. The figures look nice, but it is hard to extract useful information from them. Means and medians are not so different that they require two plots. Differences could be plotted spatially or in scatterplots. I realize that authors have generously made their data publicly available, for which I commend them. However, the authors should be doing the analysis - not other researchers who can access the data.

ACPD

8, S9178–S9181, 2008

Interactive Comment

We agree that the figures look very much the same and keep only one version of them. We will also enrich their discussion in the revised manuscript.

5) The goal of this uncertainty analysis, I think, is estimates of uncertainty in satellite retrievals (interpretation of radiance) and perhaps aerosol radiative forcing. These will depend on AOD, g, SSA as provided here. But those variables are not independent, so authors should explain how they will account for the interdependences when moving the work forward.

Our work is related to the direction from aerosol abundance and microphysics to optical properties, a way global models make calculations, and not the opposite, which is the way satellite retrievals do. We tried to make them as much independent as possible, knowing though that the change in aerosol size affects the dry components (all but water) while the change in aerosol water alters the wet radius but not the dry. We will put a comment for this in the revised manuscript.

We should also make clear that this is a purely model based partial uncertainty analysis. In an assimilation procedure one takes normally one measurement error into account. However, there are rather two errors to consider: measurement and model. The model error is seldomly computed. Our approach tries to quantify it partially.

6) Interpretation of uncertainty. Authors state that uncertainty in g and SSA not as large as AOD. This seems like a trivial conclusion. Of course g (which varies from -1 to 1) is more tightly constrained. The real question is how much that uncertainty alters predicted quantities of interest (radiance or radiative forcing). Also, I disagree with the statement that SSA has low uncertainty. The important quantity is co-albedo (1 minus SSA), and this will become obvious if authors examine the sensitivity of radiative forcing to the optical parameters calculated here.

This is also addressed by the other reviewers and we will include more results, including the co-SSA, in the revised manuscript (see also the answer to reviewer #1, points #1 and #2).

ACPD

8, S9178–S9181, 2008

7) The paper needs some additional detail for clarification. The aerosol model in the LMDz-INCA model should be described briefly. I realize that the readers could look up the citation, but the aerosol treatment in that model is critical to the paper, and so should be presented here. Does LMDz-INCA predict sizes or is it just a mass-based model? What emissions were used and what removal parameterizations?

A description of the aerosol module of the LMDz/INCA model will be included in the revised manuscript, also following the comment #14 of the anonumous reviewer #1.

8) Kahnert et al 2007 is not in the reference list

We apologize for the omission and will add the reference to the revised version.

9) p 16036 'water is not mixing', change to 'water does not mix'

We will make this change.

10) p 16036 lines 10-22 are very confusing. Text needs to be rewritten. I could not reproduce this calculation if I needed to. That should be the test of clarity.

We will try to clarify this part.

11) p 16037 'participated' should be 'participating'

We will make this change.

12) spread on aerosol size should be 'spread of'

We will make this change.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16027, 2008.

ACPD

8, S9178–S9181, 2008

Interactive Comment