Atmos. Chem. Phys. Discuss., 14, C7161–C7164, 2014
www.atmos-chem-phys-discuss.net/14/C7161/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A pathway analysis of global aerosol processes" *by* N. A. J. Schutgens and P. Stier

N. A. J. Schutgens and P. Stier

schutgens@physics.ox.ac.uk

Received and published: 17 September 2014

We thank the anonymous reviewer and Pete Colarco for carefully reading our manuscript and providing us with useful comments. Below you can find our responses to their questions/comments.

Answers to referee's comments

Pg 15047, line 19-21. Include Adams and Seinfeld (2002) in JGR (TOMAS model) in list of global aerosol microphysics model references

Adams and Seinfeld 2002 should have been in there but somehow was dropped. Thanks for spotting this.

Pg 15051 final paragraph re: SOA. Based on this paragraph and not much mention of organic condensation elsewhere, is it only sulfuric acid condensation that is included in the pathway analysis? Organic condensation is known to dominate atmospheric nanoparticle growth (see for example, Riipinen et al. 2012, Nature Geoscience). Will this omission cause underestimation in the particle growth tendencies? I suggest discussing in more detail the impacts of organic condensation.

This is a very good point. The standard version of ECHAM-HAM uses a simplified treatment of SOA where all organic volatiles condense at the time of emission. Consequently, particle growth through organic condensation is modeled but in an abstract way. In our budget this "condensation" process is lumped with other emission processes. There exists another version of ECHAM-HAM that uses a more sophisticated treatment of SOA (O'Donnel et al. 2011). We hope to publish results for this version, together with a comparison against another model (HadGEM-UKCA) in the future.

We have extended the paragraph with more explanation about our choice and its consequences. Note also that our paper initially stated that explicit SOA treatment is the standard model version, but it is not. This has been corrected.

Pg 15052 line 7: "looses" should be "loses"

Agreed, corrected.

Pg 15052, line 25 and beyond. Might be worth mentioning what exactly the emitted sulfate size distribution is quantitatively (e.g. what fraction is distributed to what mode).

Agreed, done.

Pg 15056 and elsewhere. Make sure you are consistent with your spelling. "Ageing" is mostly used in the text but Fig. 10 uses "aging".

Agreed, we only use ageing now.

Pg 15059 line 13: I suggest giving the degree by degree resolution of T31L19 for those

who might not be familiar with the ECHAM naming conventions.

Agreed, done.

Sect. 4.1 and Fig. 2: Any particular reason 1/mËĘ3 is used as the unit instead of 1/cmËĘ3? The latter is more universal for CCN, and would lower the magnitude of your numbers a bit in the legend.

Other column densities and budgets in our paper use 1/mˆ3 or 1/mˆ2 so for consistency we decided to stay with meters instead of cm.

Sect. 4.3 and Fig 4: Is it possible to include percentages of the total budget for the mass and number tendencies? The thickness of the arrows already gives a rough idea, but something quantitative would be useful.

The figures are already quite crowded. Adding percentages might not be that interesting we think. The logarithm of the flux scales with the arrow's width, so most budgets would be close to zero anyway.

Sect 4.4: Some issues here with Figure numbering. On line 14 of page 15063, I believe the correct reference is to Fig. 5, not 6. Similarly, the references to that figure in Sect 4.4.1 should be changed to Fig 5a, 5b, etc. There are some things listed as "Fig ??" which I believe should read Fig. 6 as it is presented in the list of figures. After that the figure numbering seems correct.

Agreed, corrected.

Fig 5, 8, 11... : in each of the 6 panel figures, there is a number in between the two columns in the last row (in Fig. 5, "45"). What is this? Explain or delete if a typo.

Indeed, this seems to be caused by the ACPD latex style files. Running the same tex-file for ACP removes these numbers. We'll keep an eye on this.

Sect 4.5: Can the correlation coefficient values for Fig 24 (and perhaps for some of the other regions) be reported? Yes the correlations look good, but best to be quantitative.

This is a good idea. Correlations are now given in the figures and a new table lists them for different regions. The vast majority of these correlations is larger than or equal to 0.95 for any region and either sensitivity experiment. Text is added to Sect. 4.5 to explain the new information. The figures for East Asia have been replaced with those for North America which shows larger (PI-PD) deviations.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 15045, 2014.