Atmos. Chem. Phys. Discuss., 12, C12480–C12485, 2013
www.atmos-chem-phys-discuss.net/12/C12480/2013/

Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Host model uncertainties in aerosol radiative forcing estimates: results from the AeroCom prescribed intercomparison study" *by* P. Stier et al.

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Received and published: 10 February 2013

Response to comments of referee 2

We would like to thank the reviewer for the helpful comments that substantially improved the manuscript. We very much appreciated the detailed remarks and hope to have addressed all raised issues.

General comments

In the manuscript the purpose and logic are good, but I have one major comment. I think the authors need to explain the differences in spatial distribution of

surface albedo and cloud fraction between models (corresponding to Fig. 2-15) in terms of the difference in the modules.

While we agree that it would be desirable to attribute the shown surface albedo and cloud fraction differences to model specific representations of these processes, this is far from trivial. For the case of cloud properties this is part of extended intercomparison studies conducted in other frameworks, such as the Cloud Feedback Model Intercomparison Project (CFMIP). The actual cloud distribution does in addition to the selected cloud parameterisations also crucially depend on the tuning for the specific application and used boundary conditions, such as sea surface temperatures. For example, the cloud distribution in coupled atmosphere-ocean simulations, such as performed in CMIP, can differ from the cloud fraction in the same model run in nudged mode with prescribed sea surface temperatures. Similarly, a significant fraction of the variability in surface albedos comes from the snow cover, which in many models is varying dynamically and will depend on the exact setup of the simulations.

Taking these issues into consideration we decided that it is not feasible within this study to aim to attribute the documented differences in surface albedos and cloud fractions to specific causes. The objective of this study is to identify and quantify the effect of *given host model differences* on current aerosol forcing calculations, realising that more work is needed (and ongoing) to attribute and minimise some of the host model differences.

Specific comments

1. P25943, L25: Please add a module description for surface albedo and cloud fraction in Table 2.

While we would very much like to do this, and have explored this option, the description these representations in sufficient detail for attribution of model differences will not fit in a table or indeed the paper (taking ECHAM as example, the description of the surface albedo and cloud schemes require more than 20 pages in the original model description report and only the details e.g. of the interactive snow cover scheme would allow sufficient insight to draw conclusions about high-latitude surface albedos).

We have therefore decided to guide the interested reader in Section 2.2 to the primary model references:

Table 2 lists the participating models and details about their radiation schemes, to be complemented for information about other used model parameterisations, including cloud and surface albedo schemes, provided in the listed references.

2. P25494, L5: How many periods and/or which years did you calculate? How about the difference in meteorological fields among models? Especially water vapor can be critical to determine cloud fraction. Please clarify or discuss them somewhere.

Thanks for pointing out this omission. We have extended the sections 2.1 on the Inter-comparison Protocol and 2.2 on the Description of Participating models as described in the response to Specific Comment 1 by the first referee.

As described above, the aim of this study is not to explain differences in host model parameterisations itself, which is discussed in detail elsewhere, but to assess the impact of (given) host model differences on the uncertainty in aerosol radiative forcing estimates. See also next response.

3. P25495, L3: Could you explain why the simulations differ over these areas? Is it possible to explain them in terms of a difference in the cloud module?

The simulation of low-level stratocumulus cloud has remained a persistent problem in climate models for decades. While many processes contribute to the model differences, including vertical resolution, drizzle formation or aerosol effects, it has not been possible to attribute the differences to a single difference among cloud modules. This issue is at the core of many activities in the Cloud Feedback Model Intercomparison Project and is discussed widely in the literature. For the reasons stated above, we consider the discussion of those differences out of scope of this specific study.

However, we have added the following references for the interested reader:

Significant regional differences are evident in the model cloud fractions, a known issue for global general circulation models documented in the literature (e.g. Pincus et al., 2008; Probst et al., 2012) .

4. P25494, L22: The models without aerosols simulate the cloud fraction in Fig.2. Originally, how much is the cloud fraction changed if the models with aerosols? It depends on how strong interaction between aerosols and clouds each model takes into account.

To facilitate an unambiguous attribution, the setup of the simulations in this study is purely diagnostic, i.e. aerosols are not feeding back to the model meteorology via radiation or cloud microphysics. Thus, there is no interaction between aerosols and clouds in the simulations and the cloud fraction for each model is identical among the simulations.

This is now explicitly explained in the revised Section 2.2:

All models report diagnostic instantaneous radiative forcing, i.e. aerosol radiative effects do not feed back to the model meteorology, which remains identical for the radiative transfer calculations of the different simulations.

5. P25496, L4: Could you show figures for the mean and standard deviation among the models? The mean and standard deviation may be enough for your statement in the manuscript. This comment is common in Fig.2 to Fig. 15. These figures are absolutely helpful for reader to understand the model performance and variability.

This is a good suggestion. We have added those figures to Fig. 2-15 and referred to those in the manuscript. Please note that this required to re-grid all submission to a common grid (we chose a Gaussian grid with 1.875° x 1.875° resolution).

6. P25501, L14: The shading in Fig. 19 is rather difficult to see it. First please

enlarge these figures. Second please use proper color ranges. I think narrower range is better here. I cannot judge which is positive or negative in these figures.

We have revised the individual figures in Fig. 19. The fine hatching may require to enlarge the plots on some screens with lower resolution. We will ensure to discuss this with the ACP production office.

7. P25501, L21: I suggest that the authors also discuss it in terms of shading regions and other regions.

We refer to the shadings throughout Section 3.3. However, given the - by construction - small sample size of this inter-model perturbation analysis (and generally, e.g. Ambaum et al. 2010) we caution of over-interpretation of such significance tests and decided to make our discussion not too dependent on this.

8. P25503, L28: In Figs. 21 and 22, did you take into account the interaction between aerosol and cloud? In this case, how did you treat the interaction in each model? In addition, could you show new figures of the mean and standard deviation or change the range to easily find the difference between models.

As also discussed in response to comment 4 above, all simulations were conducted with diagnostic aerosol radiative forcing, i.e. aerosols do not feed back to the model meteorology via direct or indirect effects.

Technical comments:

1. P25490, L8: references are required.

We have added the following primary references: Ångström (1962), Grassl (1975, Hansen et al. (1997)

2. P25490, L9-10: Is the paper "Mann et al. (2012)" still in preparation? I suggest published or accepted papers are preferred here.

We agree in principle. However, this study (to be submitted shortly) will be the first

intercomparison of global microphysical aerosol models so we think it justifies the reference to a paper, which has not yet been published (but should be shortly accessible via ACPD for the interested reader).

3. P25495, L9: Is a global mean 0.21 or 0.203 in Fig 3? Which is correct?

Thanks for spotting this: 0.203 is the correct value, which has been corrected in the text.

4. P25499, L21: Which (2.65 in text or 2.62 in Fig, 17) is correct?

The updated value is 2.64 Wm^{-2}, which has been inserted in the text.

5. P25499, L26: Which (2.81 in text or 2.90 in Fig, 18) is correct?

The updated value is 2.67 Wm^{-2}, which has been inserted in the text.

6. P25507, L13&14: W-2 $->$ Wm-2

Corrected.

7. P25540: The caption is almost same as that in Fig. 21. I think it is easy to understand these figures when the authors shorten it like "Same as Fig, 21 but for all-sky". I also recommend the authors to shorten the caption in Figs. 6 – 15.

A matter of personal style. We prefer self explaining captions for each figure over repeated "Same as" captions and have retained this in the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25487, 2012.