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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Adjoint sensitivity of global cloud droplet number to aerosol and dynamical parameters" *by* V. A. Karydis et al.

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General comments

The manuscript "Adjoint sensitivity of global cloud droplet number to aerosol and dynamical parameters" presents the development and evaluation of the adjoint of an aerosol activation parameterization. This is further used in 2 global models to analyze sensitivities with respect to the different input parameters. The paper is very well written with proper English, all figures are pertinent and well chosen and the study is in the scope of the journal. This manuscript is very interesting and should be published after some revisions. My main comment is that the simulations for the two models are not consistent: they are from different years and use different emissions. I would like to see the authors to bring one of these simulations as close as possible to the other

one which would help unravel the reasons of the discrepancies in a better way.

We would like to thank the reviewer for the positive response and for raising important issues. An intercomparison between models is ideally carried out with identical meteorological fields and emissions inventories; we felt however that such a comparison remains useful even if the meteorological fields are not identical. In response to the reviewers concerns, we have removed Figure 5 and reduced the comparison discussion, emphasizing now the qualitative similarities between model frameworks.

Specific comments

The choice of running both models for different years and with different emissions makes the comparison between models difficult. Several times along section 4.1 the argument that different emissions are the main reason of the discrepancies is used. More insight into the differences between models could be made if emission and year of run were chosen the same.

We understand the concerns raised; the original comparison is now reduced in scope and focuses on the similarities between the simulations.

It's not clear for how long and what dates does the two models run. The results section says "Given that the years simulated by GEOS-Chem and GMI are 2008 and 1999" so both models run for 1 year? This information should be provided in the methods section. Also, in the GMI description it should be mentioned what base year are the emissions.

GMI used the meteorological field derived from the NASA Goddard Institute for Space Studies (GISS) and represents the period from January 1997 to February 1998 with the first two months used to spin-up the model. GMI emissions are based on the draft IPCC-specified 2000 scenario. GEOS-Chem was driven by the NASA GMAO GEOS-5 GCM and was executed for December 2006 to December 2008, with the first year considered to be model spin-up. The above information is included in the revised

manuscript.

In section 4.2, for updraft velocity and uptake coefficient it's mentioned that the main reason for the differences between both models is the higher activated fraction in GEOS-Chem. What is the reason of this higher activated fraction in GEOS-Chem?

The lower activation fractions predicted from GEOS-Chem are attributed to the higher initial aerosol loadings, especially over oceans. Higher aerosol loadings decrease S_{max}, hence the activation fraction. This results in higher sensitivities than GMI. The revised manuscript includes this discussion.

Page 12097, lines 19-21: "This discrepancy can be attributed to the higher anthropogenic aerosol concentrations predicted by GEOS-Chem over marine environments compared to GMI." This is because the transport and processes are different? Or because emissions are different? This could be better answered if emissions and year of run were the same.

The higher anthropogenic aerosol concentrations predicted by GEOS-Chem over marine environments originates from the different meteorological fields used by the two models, leading to differences in the transport and diffusion of pollutants. Over land, the predictions of the two models agree relatively well, indicating that differences in emissions do not play significant role in this case. Moreover, emissions over Europe are slightly higher in GMI, compared to GEOS-Chem; nevertheless, GEOS-Chem predicts higher aerosol loadings over Atlantic Ocean.

Page 12098, lines 10-13: "Where GEOS-Chem suggests negative sensitivity, GMI predicts positive sensitivity; the difference is explained by GEOS-Chem predicting higher anthropogenic aerosol concentrations than GMI model over extended regions in the mid-latitudes and Southern Oceans." Again, why does GEOS-Chem predicts higher anthropogenic aerosol? Another reason could be that sea-salt is different in both models and is creating these differences, can you explore this?

The long-range transport of pollutants is more efficient in GEOS-Chem, resulting in higher anthropogenic aerosol loadings in relatively remote areas, such as the Atlantic Ocean.

Page 12100, lines 2-3 "Over the other arid areas of the world, GMI predicts much higher sensitivity than GEOS-Chem." Why? Is it due to different dust concentrations in the models? Or other reasons?

The sensitivity of droplet number to dust hydrophylicity is related to the relative contribution of dust and anthropogenic aerosols to total aerosol number concentration. Higher fractions of anthropogenic aerosols will reduce the sensitivity of total droplet number to dust hydrophilicity. The more efficient transport of pollutants in GEOS-Chem yields higher anthropogenic aerosol concentrations over deserts, and hence the simulations exhibit lower sensitivity to dust hydrophilicity as compared to GMI. This information has been added to the revised text.

Fig 6. The +- 50% lines look odd. This is more clearly seen in the same figure in the Karydis et al. (2011) paper as here the minor tick marks are shown: +50% for Nd=10 would be 15 while the +50% line intersects the Y axes at almost 30. Please correct or clarify this. I'm attaching a figure showing how a +/- 50% line in a loglog plot should look like. Some conclusions about model performance are based on this 50% threshold (page 12116) so this must be revised and clarified.

We apologize for this oversight. The figure is redrawn correctly now.

Fig 6. For Continental locations, a big % of model estimates (for both models) seems to remain fairly constant while observations vary over one order of magnitude. Can you elaborate on possible reasons of this?

Most of the observations are based on *in situ* data which span over a decade resulting in a wide range of CDNC. Model calculations are based on monthly mean aerosol distributions in an average climate state resulting in fairly constant CDNC predictions.

Page 12082, Lines 25-26. Add references (e.g. Twomey, Albrech) and also consider macro-physical effects (e.g. Pincus and Baker, 1994)

Good point. We have revised the sentence accordingly.

Fig 7c is not mentioned in the text.

A reference to Fig 7c has been added to the text.

Technical corrections

Page 12086, Line 7. Not clear what "probed" means

We have used "thoroughly investigated" instead.

Table 1, first aerosol type should be anthropogenic fossil fuel, right? (as described in page 12090, lines 14-16)

It is anthropogenic (which also includes fossil fuels) and biomass burning (but only for GEOS-Chem as it is denoted in the footnote). To avoid confusion we have add a comma after "anthropogenic".

Fig 5, (a) might be more clear in log scale

Following the first comment of the reviewer we have removed Figure 5 from the revised manuscript.

Fig 7. Put labels (a,b,c,d) in the figures

This change has been made in the revised text.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 12081, 2012.