Atmos. Chem. Phys. Discuss., 9, C11226–C11228, 2010
www.atmos-chem-phys-discuss.net/9/C11226/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Total aerosol effect: radiative forcing or radiative flux perturbation?" *by* U. Lohmann et al.

U. Lohmann et al.

ulrike.lohmann@env.ethz.ch

Received and published: 2 March 2010

Response to reviewer 2:

We thank the referee for his/her valuable comments and suggestions. The responses to your comments are below each comment.

1. Fundamentally speaking, the explored method, though practical, did include certain responses or feedbacks in the forcing. This would mostly suitable for the case of estimating long-term EQUILIBRIUM climate response.

Your point is well taken.

2. The authors might want to provide certain detailed information of their experiment.

These include the actual lengths of integration and spin-up (not just 5-10 years or a few months), and the derivation of the quantities in analyses (5 year means or last year average in deriving RFPs and also conventional radiative forcings). These would help others in repeating the experiment and enable the reader to better understand the temporal scale the authors assumed in separating forcing from response.

We added more detail.

3. In particular in discussions of Figure 2 and 3, what would be the outcome if the aerosol effects were accounted separately from those of CO2 and CH4? Indicating how different they were would be an interesting result. Also, the insignificant difference between RFP and F in clear sky case should suggest that the feedback (should through precipitation rather than clouds) to aerosol quantities included in the models is rather small.

The aerosol effects were separately accounted from the effects of CO2 and CH4. We added that there are no significant changes in precipitation between the forcing and the RFP simulations as otherwise one would see differences in the direct and indirect aerosol effect.

4. Again, in Figure 2 and 3, the overlapped 1:1 lines do not serve the purpose in my opinion, they somewhat prohibit the reader to appreciate the fitted slopes of the correlations.

We prefer to keep the 1:1 lines as they serve as a visual guide as to where the points should be in the ideal case.

5. The discussion of Figure 4-6 is too brief. To what extent were the distributions of RFP correlated to those of clouds or precipitation? By stating in the last sentence of Section 3, “RFP ... are a noisy version of forcing distribution ... not fundamentally different”, did the authors imply that one should not expect a systematic difference between RFP and F of aerosols, or in other words that the slopes in Figure 2-3 should not be different than

ACPD

9, C11226–C11228, 2010

1:1? A same description appears in the second paragraph of the Conclusion. Perhaps these statements are more suitable for long-lived species than aerosols? Otherwise, the authors should have drawn a conclusion that the attempt described in this paper was not necessary.

We expanded the discussion of Figures 4-6. We looked at the changes in cloud cover, water vapor and precipitation but the noisy pattern for instance of RFP of CO2 bears no resemblance to either of them. When we started this project, we were not clear if one would see systematic differences. Thus not seeing any systematic difference is an outcome of this study. This implies that the deviations from the 1:1 line when regressing the TOA net radiation flux perturbation vs. the tropopause forcing should not be systematic. Looking at the net radiation in Figure 2, this is almost the case. We discuss that there are effects, as the semi-direct cloud response from CO2 that are present in all models and seem to be systematic. With the data that we diagnosed for our study, we cannot suggest how to eliminate this systematic effect. That is something for a follow-up study.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 25633, 2009.

ACPD

9, C11226–C11228, 2010