Atmos. Chem. Phys. Discuss., 9, C10443–C10445, 2010
www.atmos-chem-phys-discuss.net/9/C10443/2010/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 3 February 2010

The authors have calculated "radiative flux perturbations" (RFP) caused by the direct radiative effects as well as the albedo effects of aerosols using five GCMs. They then compared the results with the conventional estimations of the radiative forcing of aerosols. In addition, long-lived and relatively well-mixed gases of CO2 and CH4 have also been included in the analysis, clearly for the purpose to evaluate whether the RFP values corresponding to different forcing agents are consistent. There are growing concerns on the use of radiative forcing concept when dealing with both long-lived, well-mixed species such as CO2 and relatively short-lived forcing agents with significant geographical gradients such as aerosols. Further brought issues into this debate is the documented unique forcing profile of absorbing aerosols. The concept of radiative forcing, as mentioned by the authors in the paper, was proposed to estimate, preferably in a rather linear fashion, the equilibrium climate response without involving

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

long-term integrations of models or other sophisticated methods. The previous practice in deriving the radiative forcings of CO2 and tropospheric ozone, again as indicated by the authors, had hence adopted various “adjustments”. By including certain feedbacks and responses in “forcing”, the explored method in this paper actually linearizes the forcing and response relation to achieve the similar practical procedure of deriving the radiative forcings of CO2 and ozone. Therefore, for practical reason this attempt is quite useful and the result is informative to other efforts in searching alternative derivation of radiative forcing of aerosols, despite the fact that the concepts of forcing, feedback, and response under this method will no longer have their original or theoretical meanings.

In my opinion the paper is relatively well organized and written. The results are original and could serve as a good reference for future works or similar attempts. The authors should consider addressing the following concerns before publishing the paper.

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.

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.

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.

ACPD
9, C10443–C10445, 2010

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.

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 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.

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

ACPD

9, C10443–C10445, 2010

Interactive Comment