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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interpreting the cloud cover – aerosol optical depth relationship found in satellite data using a general circulation model" *by* J. Quaas et al.

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First, we would like to thank the reviewer for her or his review of our manuscript. Important issues about the method of using a general circulation model to investigate the questions we pose are raised.
We indeed share to a large extent the reviewer's doubts about the usefulness of GCMs to quantify second aerosol indirect forcings (see e.g. the discussion of this in the recent paper by the second author of our study in Stevens and Feingold, Nature, 2009). However, we do not agree with the reviewer's conclusions about the validity of our approach for our present study. Nevertheless, we think the critics have helped a lot to improve

the formulation and explanation of our main findings.
Before answering in detail to the points the reviewer raised, we would like to point out the main intent of our study, which is to highlight that large-scale phenomena, which are to a large extent adequately simulated by a general circulation model with the complexity of the one we use here, are able to explain most of the positive relationship found in satellite retrievals between total cloud cover and aerosol optical depth. Our results show that the main contributor to this relationship is the swelling of aerosol in humid air, which simultaneously leads to large cloud cover and large aerosol optical depth, a fact that cannot be independently assessed using satellite date.
In the revision of the manuscript, we put a large effort into conveying this result in a much clearer way.

The study tests various hypotheses that relate the observed aerosol optical depth (AOD) to the total cloud cover (TCC). It does so by direct satellite measurements of AOD and TCC and by global circulation model (GCM) simulations with and without the aerosol impacts on cloud microstructure, and with and without aerosol expansion by absorbing water in high relative humidity. The subject is potentially of great importance to understanding the climate sensitivity to radiative forcing.
We thank the reviewer for her or his supporting statement about the importance of the study's subject.

The GCM does not resolve clouds, and therefore its parameterization cannot possibly be expected to represent realistically the impact of aerosols on cloud cover. The authors admit to that effect, but still keep using the generated model results, in line with the rest of the GCM community. This process feeds directly into the IPCC and makes the estimates of the aerosol indirect effect (AIE) look much less uncertain than they truly are. This, in turn, makes the uncertainty of the climate sensitivity to greenhouse gasses look much less uncertain than it truly is. The fact that this practice is shared by many colleagues and having passed most other reviewers does not make it right.

While the reviewer might be right in criticising past publications of some of us, we feel that critics is not really applicable to the present study. Our aim is to understand the relationship between TCC and AOD found in satellite *observations*. This strong positive relationship has previously been interpreted as a cause-effect relationship, which would imply a very large anthropogenic aerosol indirect forcing through a postulated cloud lifetime effect. We acknowledge that obviously our manuscript did not succeed in making the point clear enough. In the revised version, we underline the question we pose more clearly in the abstract and in the introductory paragraph.
In this respect, *not* publishing this kind of studies allowing to understand the reasons behind the TCC-AOD relationship would make "the estimates of the aerosol indirect effect (AIE) look much less uncertain than they truly are", since IPCC would rely on (Science-)publications postulating a very strong anthropogenic second aerosol indirect effect based on satellite observations.

Here, I would expect the authors to put a well justified uncertainly range on their estimates of the various components of the model calculations. This is very different than merely calculating the standard deviations of the results, which are given as the error bars in Figure 1. If well justified quantification of the uncertainty of the model cannot be given INDEPENDENTLY of satellite observations, the model results cannot be used for estimating the AIE.
It remains somewhat unclear to us what is meant by this critics. In this study, no estimate of AIE is aimed at, and no quantification is given. For the statistical relationships we analyse, additional statistical uncertainties could be provided, but due to the very large amount of data going into the regressions, the statistical uncertainty of the regression slopes is negligible compared to the differences between regions and seasons. If the intention of the reviewer was to request a full-blown analysis of parametric and structural model uncertainties, e.g. through perturbed physics/structural ensembles, we would like to point out that this: i) would be highly desirable; ii) has never been done in the context of a microphysical aerosol-cloud climate model; iii) would

require significant man-power and computational resources that would be appropriate for a collaborative research program but is certainly beyond the scope of this paper.

It seems to me that the main value of the simulations is in quantifying the effects of the aerosol humidification on the AOD and its contribution to the AOD-TCC relations. Here resolving the clouds is not necessary. The substantiation of additional claims with respect to the AIE requires rigorous quantification of the model uncertainty in calculating TCC and its dependence on aerosols, as already stated above.
We concur with the reviewer that the main result of this study is that aerosol humidification and subsequent increase in AOD in more cloudy scenes is the main contributor to the positive AOD-TCC relationship. In our revised manuscript version, we emphasise this more carefully, and underline that aerosol microphysical effects are of second order.

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