

Interactive comment on “Aerosol indirect effects – general circulation model intercomparison and evaluation with satellite data” by J. Quaas et al.

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1. This is an ambitious and important paper. It aims to assess the parameterizations of processes used to represent aerosol indirect effects on stratiform liquid water clouds, in a collection of 10 leading chemical transport models. Aerosol optical depth (AOD) relationships to six cloud-related factors are considered: (1) cloud droplet number concentration; (2) liquid water path (LWP); (3) cloud fraction; (4) cloud top temperature; (5) cloud albedo; and (6) OLR. 2. This is primarily a correlation study, identifying the diversity in the way the aggregate of mechanisms operating in each model produces a net effect on clouds, given specific changes in AOD. Comparisons are made with satellite observations of AOD vs. each of the six cloud parameters.

We thank the reviewer for her or his kind summary of our work.

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3. *Introduction, P12735, Lines 21-22. You might also want to include Twohy, Coakley, Tahnk, JGR 114, 2009, regarding the interpretation of satellite retrievals of AOD in the presence of clouds.*

Thank you very much for pointing us to this interesting and important reference.

4. *Methods. P12736, Lines 8-10. Are the models sampled at 2.5 degrees daily average or instantaneously, coincident with the satellite overpasses? – This is ambiguous here, though I think it is clarified on P12738, lines 2-3.*

We added a couple of words in the revised version hopefully clarifying this.

5. *Methods. P12736, Lines 15-16. There are several subtleties associated with the CCN-AOD relationship given in Andi's paper. For example, humidification can introduce large variations in the CCN-AOD relationship (e.g., Kapustin et al., JGR 2006), and there can be enormous variations in RH in the vicinity of clouds. I don't have any great suggestions about how to get around these issues, but the statement in the paper seems overly optimistic.*

Indeed, our formulation might imply that this correlation is generally true, and is thus misleading. We tried to put it into more careful wording in the revised version.

Line 18. The Minnis et al. reference is for CERES, not MODIS.

Well, it is the reference for the CERES SSF version of the MODIS retrieval. We tried to clarify this in the revised version by rearranging the references.

Line 22. The 10:30 and 1:30 local times apply near the Equator.

Thank you, we added this clarification.

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Also, did you use MODIS Collection 4 or Collection 5 aerosol products?

It is collection 4, as provided in the CERES SSF dataset.

6. Methods. P12736, Line 26. You use the diversity among MODIS Terra, MODIS Aqua, and AATSR as an indicator of uncertainty in the satellite-derived quantities. For the data shown in the Supplemental Material, they are all quite similar. But some mention of the actual uncertainties in the satellite-derived parameters, to the extent this is available, would be helpful here, especially as the three sources entail similar limitations in many respects, and you are depending upon slopes derived from the retrieved quantities, which are yet more sensitive to measurement errors than the individual retrieved values.

Thank you, we added a statement to remind the reader about this limitation.

7. Methods. P12737, Line 9. Does the difference between 10:30 AM and 1:30 PM equator-crossing time provide convincing information about diurnal variability, given other uncertainties in the measurements, combined with the lack of late afternoon sampling?

Indeed, we do not find much of a systematic difference, and we agree that only quite limited information can be obtained from the two times. We re-formulate the phrase more cautiously in the revised manuscript.

8. Methods. P12738, Lines 11-13. Is Feingold's log-log relationship valid over the entire range of interest? It might be; the process does plateau out for high enough aerosol concentrations, but such high concentrations might never occur at the averaging spatial scales considered here.

In earlier studies, we showed (e.g., Quaas et al., 2006; consistent with other studies showing similar results) that a linear fit in log-log is a relatively good approximation even for the coarsely-resolved satellite data. We think this is a relatively good choice

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(certainly much better than a regression linear in AOD).

9. *Section 3.1, P12739, Line 16. Africa as a whole is dominated by dust during many but not all seasons; same with Oceania. In November-December, for example, smoke often dominates Africa. Your seasonal stratification might reflect this difference.*

This is true, and we modify our statement in the revised version.

10. *Section 3.1, P12739, Lines 20-22. How confident are you that the satellite land/ocean AOD differences are accurate? MODIS uses different algorithms over land and water. In Collection 4 and earlier, MODIS over-land AOD was much higher than AERONET [e.g., Kinne et al., 2006]. For Collection 5, the over-land algorithm allows negative AOD, which improves MODIS-AERONET agreement in an average sense, but not event-by-event, and there is still a high bias to the MODIS over-land values.*

We agree that the land-sea contrast might be biased in the satellite data, and added a sentence in the revised manuscript to inform the reader about this possible deficiency in the observations.

11. *Section 3.1, P12740, Line 13. Minor copy-edits I happened to catch. “. . . for the slope of N_d vs. total aerosol light scattering. . .”*

Thank you, modified in the revised text.

Line 14. “)” missing.

A perhaps lengthy parenthesis, the “)” is only in line 15.

12. *Figure 2. I think the full-page figure you have in the Supplemental Material is so much more revealing than the abbreviated version given in Figure 2, that I'm tempted to suggest you include the full figure in the article itself.*

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We agree that there is quite a lot of important information in this more detailed analysis. However, we think that this paper is just a first glance at the interesting data from the model intercomparison project, and hope to publish more results on this in the future, including a better exploitation of more detailed informations.

13. Section 3.2, P12742, Line 1-2. A bit more perspective would be helpful on how meaningful you think the specific, detailed agreements (and disagreements) between the models and measurements.

We added the following sentence to the revised manuscript at the end of the paragraph: “More process-oriented research is needed (e.g., following the approach by Suzuki and Stephens, 2008) to investigate the implementation of the second aerosol indirect effect in more detail.”

*14. Section 3.3, P12742, Lines 20-24. Right. The implication of these apparent correlations is controversial. You might find the following paper interesting in this respect, as it gives an example of how different remote sensing techniques can come to *opposite* conclusions about the aerosol/cloud fraction relationship: Tian et al., JGR 2008, doi:10.1029/2007JD009372.*

Thank you for pointing us to this interesting reference, which we included in our discussion.

15. Section 3.3, P12743, Lines 27-29. You might add something about specific, coincident measurements of multiple parameters needed to strengthen the observational base for model validation of these effects. It would be worth making a point here, and possibly also in the Conclusions section, about the needs implied by the current work.

We added the following sentence: “More detailed sensitivity studies, and/or detailed evaluation of satellite-derived relationships with ground-based remote sensing or aircraft observations are needed for a clearer distinction of the processes relevant for

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the relationship between τ_a and f_{cld} .”

16. *Section 3.4, P12744. As you know, T_{top} derived from TOA radiances actually samples a profile within the cloud, and to some extent, the atmosphere above it, depending on conditions and wavelengths used. Some mention of the impact this might have on the interpretation of these comparisons might be included in this section.*

This is true, but mainly for quite thin clouds. We believe that in our study, this effect is of minor importance.

17. *Section 3.6, P12745. Having a qualitative list of likely mechanisms involved in the OLR/AOD relationship is important, but in addition, providing at least a broad perspective on how well constrained the net relationship is from actual observations, would be helpful.*

Other than the fact that the relationships are consistently with only a few exceptions negative, we can't provide further corroborating evidence. However, OLR retrievals from CERES are quite robust, so the observations seem to be reliable.

18. *Section 3.7, P12748, Line 9. Why not work with a summary of different satellite inferred anthropogenic AOD estimates, such as Yu et al. that you reference in the Introduction?*

Yu et al. (ACP 2006) summarise total-aerosol radiative perturbations. Among the estimates of the anthropogenic fraction of AOD, we believe that the Bellouin et al. estimate is the most accurate one (over oceans only), which is why we use their estimate.

19. *Section 4. P12750, Line 6. You make the point here about measurement uncertainties. The distinction between diversity and uncertainty is blurred for measurements*

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(and models) in this paper, as it is in nearly all global-scale studies of aerosol effects, due to limitations in the data and complexity of the models. Addressing this directly is beyond the scope of the work presented here (and nearly everywhere else). But I think it would enhance the value of this effort if you could say a little more about key measurements and accuracies required, at the appropriate spatial and temporal scales, to take a significant next step in model validation of indirect effects on global scales, with the understanding that suborbital as well as satellite measurements might be needed. You have already done all the hard work required to extract these insights. We agree that it would be very valuable indeed to get a more reliable and more physically based error estimate from the observations. Thank you very much for advising us to add a statement on this, which we do at the end of the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 12731, 2009.

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