Atmos. Chem. Phys. Discuss., 9, C2856–C2863, 2009
www.atmos-chem-phys-discuss.net/9/C2856/2009/

Atmospheric Chemistry and Physics Discussions

ACPD

9, C2856–C2863, 2009

Interactive Comment

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

L. Rotstayn (Referee)

leon.rotstayn@csiro.au

Received and published: 15 July 2009

General Comments

This is a good paper, which makes a significant contribution to the literature. The careful use of satellite retrievals in combination with consistently derived model output offers real hope of reducing the uncertainty in the aerosol direct and indirect effects. The specific points below generally concern incomplete arguments or suggestions for improving the clarity of the paper.

Specific Comments

1. P12733, lines 13-14: "It is shown that this is partly related to the representation

of the second aerosol indirect effect in terms of autoconversion." This sentence in the abstract is rather vague for a reader who hasn't read the paper. I suggest that a sentence similar to the one used in the main text would be better, e.g., "This suggests that the implementation of the second aerosol indirect effect solely in terms of an autoconversion parameterisation has to be revisited in the GCMs".

2. P12734, lines 3-4: The authors' use of "clear-sky" and "cloudy-sky" needs to be explained here (or they should be left out of the abstract altogether), because this use is slightly non-standard, and hence liable to be confusing. A possible explanation would be: "The radiative flux perturbation due to anthropogenic aerosols can be broken down into a component over the cloud-free portion of the globe (approximately the aerosol direct effect) and a component over the cloudy portion of the globe (approximately the aerosol indirect effect). An estimate obtained by scaling these simulated clear- and cloudy-sky forcings with estimates of anthropogenic τ_a and satellite-retrieved $N_d - \tau_a$ regression slopes, respectively, yields a global, annual-mean aerosol direct effect estimate of...".

3. P12736, lines 10-16: The argument supported by Andreae (2009) is noted, but are there any biases caused by the fact that the GCMs have a different definition of "clear-sky" from the satellites? For example, is there any evidence that the satellite retrievals reflect conditions that have lower RH than average, because they only process scenes that are "cloud-free"? If any such biases are known or suspected, it would be worth a mention.

4. P12740, lines 2 to 4: "The reasons for the reduction of the slope when averaging over cloud ensembles are the variability in liquid water path, updraft velocity, and aerosol concentrations (Feingold, 2003; McComiskey et al., 2009)." This sentence was mysterious, but then I looked at McComiskey et al. (2009) and their paragraph [54] was insightful. The authors could add another sentence or two to give the reader a clearer idea of the effect of averaging over larger scales,

ACPD

9, C2856–C2863, 2009

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

because it looks like an important point.

5. P12741, lines 27-28: According to Rotstayn and Liu (GRL, 2005), the slope is also expected to correlate with the autoconversion *rate* as well as the exponent. It is easy to see why if you consider the limiting case of an autoconversion rate that approaches zero — even with a large (negative) exponent, changing N_d in the autoconversion will then have only a small effect on the simulation. It is probably not feasible for the authors to duplicate Fig. 4 with "global-mean autoconversion rate" on the horizontal axis, but what about choosing a representative value of in-cloud liquid-water content (e.g., 0.1 g m^{-3}) for each parameterization, and plotting the autoconversion rate for that LWC on the x-axis? It would be interesting to see to what extent the autoconversion rate also explains the variability.

6. P12742, line 1: It *is* remarkable, but can the authors suggest why this result occurs in the LMDZ-INCA model? Here are two possible ideas: (1) Increased RH $\Rightarrow$ increased LWP and increased τ_a, or (2) aqueous sulfate production is increased in regions of high LWP. On the other hand, high LWP could also correlate with increased aerosol scavenging, which would have the opposite effect. It is hard to diagnose this with only one data point (global mean), but the author who uses this model should be able to provide more information.

7. P12742, line 16: Another possible mechanism in the tropics might be aerosols stabilising the lower atmosphere, thus reducing convection and LWP. (If the authors agree with this, then it should also be mentioned in the Summary.)

8. P12742, lines 17-19: I don't understand this point, even though two references are given. Isn't the expected first-order effect from the "cooling aerosol forcings" a *decrease* of LWP (due to the change in slope of the Clausius-Clapeyron equation)? Also, it should be noted that only the land-surface temperature can change, since SSTs are fixed.

9. P12743: “The GCMs do include some parameterisation of this effect, though relatively crudely as discussed above.” This is an important point, and the above discussion is too brief. To my knowledge, most of the GCMs parameterize cloud cover in a manner that is, to first order, related to relative humidity. The GFDL model uses a variation of the Tiedtke (1993) cloud scheme, which explicitly treats the sources and sinks of cloud water in the parameterization of cloud fraction, and I suspect that this may account for the fact that this model has the strongest variation of f_{cld} with τ_a. Can the authors relate this to any specific aspect of the Tiedtke cloud scheme? It’s less clear why CAM-OSLO and CAM-PNNL should also show a relatively strong correlation between f_{cld} with τ_a, although it is interesting that both have a very similar spatial signature (supplementary figure). Boville et al. (J. Climate, 2006) confirm that cloud fraction is essentially a function of RH in CAM3, so it isn’t obvious why these models also show a fairly strong correlation. Perhaps it will be too difficult to make definitive statements, but some further discussion seems warranted, in view of the importance of this question.

10. P12743, lines 10-12: Why would the nudged simulations show a stronger covariance due to large-scale dynamics? Is it due to the time-averaging that is done in analysis of the five-year climate runs?

11. P12743, lines 16-18: It is true that humidity swelling is treated in the GCMs, but I don’t believe most (or any) of them adequately treat the strong non-linearity, which causes a strong increase of τ_a as RH $\rightarrow$ 100%. A typical GCM treatment might use the mean RH in the cloud-free part of the grid box (or even the grid-box-mean RH) to calculate this effect, so the areas near cloud edges, where RH is close to 100%, are not well captured. This problem was shown years ago by Haywood et al. (GRL, 1997). So it is incorrect to say that “the GCMs would consistently show a relationship equally strong as the satellites”.

12. P12743, lines 28-29: The statement that “our results indicate that none of these

ACPD

9, C2856–C2863, 2009

Interactive Comment

four hypotheses can entirely explain the model-satellite differences in relationships between τ_a and f_{cld}" does not seem to be adequately supported. For example, the previous point could possibly explain the model-satellite differences, as could the fact that most of the GCMs (other than GFDL) treat cloud fraction as essentially a function of RH. A slightly different statement is made in the abstract and summary, namely "In a discussion of the hypotheses proposed in the literature to explain the satellite-derived strong f_{cld} – τ_a relationship, our results indicate that none can be identified as (a) unique explanation." I'm not sure that even this statement is justified by the results. I'd agree that the results *suggest* a number of possible explanations, but more effort would be required to justify a stronger statement.

13. P12746, lines 1-3: This is a nice point, but it is perhaps worth noting that a bias in α at high latitudes in winter would not have much effect on the radiative forcing. Note that if the global-mean planetary albedo is computed correctly as (global-mean solar out)/(global-mean solar in), the effect of biases at high latitudes in winter would almost disappear.
14. P12745, lines 4-5: Does Fig, 2 show that GFDL has a negative correlation of α against τ_a? This seems to contradict the text, and also there are missing values for these numbers for GFDL in Table 2.
15. P12745, lines 21-22: Again, I feel that there is a strong need for something to be said about the cloud parameterization in the GFDL model.
16. P12746, line 5: It should be stated up front (as it is on P12748) that this breakdown of the forcing into clear- and cloudy-sky components is only an approximation, especially when absorbing aerosol is present.
17. P12747, line 1-2: The point about detrainment of convective cloud water is interesting, but it merits another sentence or two of explanation. How is this equivalent

ACPD

9, C2856–C2863, 2009

Interactive Comment

to assuming a lower bound?

18. P12748, line 9: Is the estimate of anthropogenic τ_a from Bellouin et al. the only one that is worth mentioning? For example, Kaufmann et al. (GRL, 2005) also estimated this quantity from MODIS retrievals, giving a range of 0.030 to 0.036.

19. P12749, line 1: It seems surprising that the global-mean value does not lie in between the land and ocean values. Why is this the case?

20. Tables 2 and 3: The quantities below the solid line in Table 2 are essentially the same as those in Table 3, except that they are broken down into land/ocean, so it would be more logical to put them in Table 3. (I am unsure whether it is really necessary to show both land and ocean values: Perhaps it is just too many numbers?) Making this change would also avoid the confusing change of terminology between Table 2 (where “clear-sky forcing” refers to the traditional definition) and Table 3 (where “clear-sky forcing” is weighted by the clear-sky fraction).

21. Appendix A: The model descriptions should say something about the “cloud macrophysics”, i.e., treatment of cloud fraction. It is very relevant to the second indirect effect.

Technical Comments

1. P12733, line 19: Insert “a” before “unique”.
2. P12736, line 26: Is there a reference for the CERES data set including the revisions?
3. P12740, line 14: Suggest “similar” instead of “close”.
4. P12747, line 27: Not sure what is meant by “consistent” (with what?)

ACPD

9, C2856–C2863, 2009

5. P12751, line 15: Which observed relationships?
6. Table 2: Quantities should preferably be defined in the caption, because the reader might scan the table before reading (e.g.) Section 3.5, which explains that α is planetary albedo, not cloud albedo. Further, it would be good to make it obvious that the slopes are for the relative change in each quantity w.r.t. τ_a (and hence unitless). This was clear to me after I looked at Fig. 2. (Also, I trust that the fonts will be larger in the final version.)
7. Table 3, last line: Units have a typo.
8. Fig. 2: If the error bars show the standard deviations, then what do the solid boxes represent for each model? (Also, I trust that the figure will be larger in the final version of the paper, because it is hard to read in this version.)
9. Fig. 3: A shorter label on the first bar would fit better, e.g. “McComiskey” or “Surface obs.”. Or maybe line them all up so that the end of the label aligns with the tic mark.
10. Figures 8a and 8b: The difference between red/magenta and (to a lesser extent) blue/turquoise is difficult to see on the copy I printed. The figure will presumably be larger in the final version, but the authors could try orange instead of magenta, and green instead of turquoise. Also, the caption could be clearer: The sentence following “(a')” is very long, and the meaning of the vertical dashed lines in Fig. 8d is not explained. (I assume these lines are the Terra-derived slope estimates for land and ocean.)
11. Supplementary Figure: This file causes Adobe Reader 9.1 to crash repeatedly on my laptop (Windows XP Pro), though it was OK on another PC (running XP Home). Also, no caption seems to be provided.
12. Reference GAMDT (2004) does not appear in the reference list.

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

ACPD
9, C2856–C2863, 2009