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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A six year satellite-based assessment of the regional variations in aerosol indirect effects" *by* T. A. Jones et al.

Anonymous Referee #2

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General comments:

I found this to be a disappointing paper. The authors do not sufficiently consider whether the satellite retrievals of aerosol optical depth are reliable (e.g. in conditions of 95% cloud cover), nor whether satellite-retrieved aerosol optical depth really corresponds to the parameter of physical interest; that is, aerosol number concentration or CCN. Moreover, the authors come to no general conclusion about the value/sign of the first aerosol indirect effect, other than to state that it varies from one region to another and depends on atmospheric conditions (something we already could guess). Their treatment of the role of meteorology is very limited (essentially seasonal cycle and four one-day regional case studies), and they spend much time speculating on reasons why their estimated aerosol indirect effect has a particular value for a particular region

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Interactive Discussion
Discussion Paper

rather than rigorously testing hypotheses. I don't think I gained anything by reading this paper, nor do I think anyone else will much, so I recommend rejection.

I expect the authors will disagree with me about the validity of the satellite retrievals for assessing aerosol indirect effects, but otherwise I urge them to undertake a much better treatment of meteorology than what they have done in this paper. They should also engage in less speculation and instead actually directly and specifically investigate how atmospheric conditions impact the aerosol indirect effects. I think it would also be very preferable to cut down detailed descriptions of everything going on in various regions and instead focus on a simple message. I realize that aerosol indirect effects and the role of atmospheric conditions are difficult to characterize and understand, but for science to progress, we need to find some basic principles. If these things are done, it may be worth resubmitting a very modified study and paper.

Specific comments:

1) Abstract first sentence: I advise modifying "changes in aerosol concentration having significant impacts on the corresponding cloud properties" to *may have significant impacts* because aerosol indirect effects have not been well-established on a large-scale basis (e.g., besides ship tracks, etc.).

2) Abstract should be more concise.

3) The introductory section should be more concise. There are too many unneccesary details, especially in the latter half.

4) One shortcoming with using such a large footprint (20 km for CERES-SSF) is that clouds may only partially fill a footprint, thus resulting in biased retrievals of cloud properties. Although the authors state they are looking only at relative changes so biases shouldn't matter, I don't share their apparent confidence that the biases are independent of cloud and aerosol conditions.

5) In Section 2.2, it appears that the authors will use aerosol retrievals from pixels

ACPD

8, S10650–S10655, 2009

in a 10 km footprint even if the footprint is nearly cloud-filled. This is an extremely questionable practice for attempting to retrieve something corresponding to aerosol concentration. If the footprint is nearly cloud-filled, then clouds will side-scatter photons into the clear areas, thus enhancing reflected radiance and retrieved AOT. The humidity in the clear areas is also likely to be larger when clouds are nearby, thus creating larger/more haze droplets and enhancing the retrieved AOT even though there is no change in dry aerosol concentration. Also, there could be small subpixel clouds that are mischaracterized as aerosol. This opens the possibility of many potential biases and artificial relationships when correlations are calculated at the pixel level within a region, as stated at the beginning of Section 3.

6) Near the beginning of Section 3, the authors state that only monthly average atmospheric conditions are examined. Using such a long time scale is useless for investigating meteorological impacts on cloudiness and aerosol since meteorology, cloudiness, and aerosol all substantially vary on daily time scales.

7) Equation 7 indicates that the first indirect effect is calculated using aerosol optical thickness. This implicitly assumes that there is a direct relationship between aerosol optical thickness and aerosol number concentration (or more specifically, CCN). Considering the possibility of photon scattering by clouds, humidification, etc. mentioned above, I doubt that there is a sufficiently direct relationship between satellite-retrieved aerosol optical depth and aerosol number concentration (what we really want to know) for a satellite-based correlation study like this to produce valid results.

8) At the bottom of p. 20363 and top of p. 20364 the authors acknowledge the problems mentioned in comments 5 and 7 and argue that they are not important, but I don't think the authors provide sufficient evidence to support this assertion. They cite Yuan et al. (2008) for declaring that estimates of aerosol indirect effect are not sensitive to the problems mentioned above, but Yuan et al. actually find a positive correlation between aerosol optical thickness and droplet effective radius in their region of study. This finding by Yuan et al. contradicts the authors' hypothesis that greater aerosol

ACPD

8, S10650–S10655, 2009

Interactive Comment

optical thickness is associated with reduced cloud droplet radius. Moreover, Yuan et al. restricted their analysis to certain clouds meeting certain conditions in a particular region. There's no basis to generalize their finding that retrieval uncertainties are unimportant to the regions and cloud types investigated in this analysis. I'm not sure I agree with Yuan et al. results anyway.

9) Section 4 is very tedious because it provides many details on possible aerosol indirect effects in many regions around the globe, but there is little sense of any organizing principles or large-scale framework with which to understand aerosol effects. The authors instead spend considerable effort speculating on why various regions behave the way they do.

10) The figures are small and very difficult to read. The captions are also unclear, and I have a difficult time deciphering what the figures are meant to portray. E.g., the caption in Fig. 2 says correlation between AOT and Rc, but the text in the figure merely says "radius". Also, the caption in Fig. 3 says "Same as Fig. 2", but are any correlations plotted?

11) The figures mostly show seasonal cycle of cloud and meteorological parameters, which doesn't yield much insight into cloud-aerosol-meteorological relationships. The four case studies are too small a sample size to draw any general conclusions about cloud-aerosol-meteorological relationships.

12) When calculating statistical significance, did the authors take spatial and temporal autocorrelation into account? This could substantially reduce the sample size.

13) In the final paragraph of the paper, the authors state that aerosol indirect effects are highly dependent on the surrounding atmospheric conditions. Unfortunately, the authors hardly investigated this point. Monthly mean averages and four case studies are not sufficient to characterize atmospheric conditions associated with cloud-aerosol relationships! Moreover, the two-day history of meteorological conditions is likely to have more impact on cloud-aerosol correlations than the conditions at the time of the

ACPD

8, S10650–S10655, 2009

Interactive Comment

satellite retrieval.

ACP Questions:

1) Does the paper address relevant scientific questions within the scope of ACP?

Yes.

2) Does the paper present novel concepts, ideas, tools, or data?

Only minimally so.

3) Are substantial conclusions reached?

No.

4) Are the scientific methods and assumptions valid and clearly outlined?

I don't share the authors' trust in the validity of the satellite aerosol retrievals for the purpose they use them.

5) Are the results sufficient to support the interpretations and conclusions?

Much of the interpretation is speculative, and not much is concluded.

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Probably.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes, they extensively review prior work.

8) Does the title clearly reflect the contents of the paper?

Yes, except the paper is more of a "random description" than an "assessment".

ACPD

8, S10650–S10655, 2009

9) Does the abstract provide a concise and complete summary?

The abstract is not concise.

10) Is the overall presentation well structured and clear?

I found many sections to be tedious and overly detailed.

11) Is the language fluent and precise?

Generally yes.

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

I think so.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

I don't find the case studies to be useful.

14) Are the number and quality of references appropriate?

I think so.

15) Is the amount and quality of supplementary material appropriate?

N/A.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 20349, 2008.

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8, S10650–S10655, 2009

Interactive Comment