Atmos. Chem. Phys. Discuss., 11, C10055–C10057, 2011
www.atmos-chem-phys-discuss.net/11/C10055/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The scale problem in quantifying aerosol indirect effects" *by* A. McComiskey and G. Feingold

Anonymous Referee #1

Received and published: 10 October 2011

This is a timely, well written and relevant paper. The issue of scale in determining ACI has been largely ignored and a paper addressing the issue will be helpful in bringing renewed focus to this key aspect of the aerosol-cloud problem.

I have some concerns with the authors proposed 'observationally-based' method to calculate ACI, which I believe is largely sensitive to the prescribed physics of a cloud parcel model. It is not that this would not be a useful metric but rather that it is only loosely tied to observations. I think that the more fundamental issue here is that relationships between aerosol and cloud observed from space are not directly comparable to those from ground based observations (or theory) for a host of reasons, two of which are emphasized here. This point was lost by me (although I'm a bit slow) to a large degree in trying to figure out how this pseudo-observational method works and how

one might apply it to real data.

Nonetheless, I have no major concerns with the methods or findings herein and would recommend acceptance for publication conditioned on minor revisions.

171: mean (mu)

283: What MODIS collection is used?

313: Figure 4 should be Figure 5.

330: It seems that the cloud parcel model is largely determining the ACI here. What role do the observations or model output really play here?

333: Figure 5 should be Figure 4.

359: Two mechanisms are proposed to explain the apparent contradiction. Later (lines 547-556) you mention that the unconstrained satellite observations may be more representative of the full system of aerosol cloud interaction and therefore should have a different interpretation. I think that this thought deserves a specific mention here and more emphasis throughout the manuscript.

386: I appreciate the authors point but I don't like the use the word 'actual'. These are actual relationships, they may, however, not be causal in the sense that they should be interpreted as Na causes a change in re.

399-407: The point may be valid here but the illustration is not convincing to me. Figure 9b does not have the same color scale as Figure 8b, making any visual comparison impossible. Also, visually integrating 8a I do not see how you can get the inset in figure 9a without perhaps averaging all of the clear sky (tau=0 pixels). This isn't really a fair or even useful comparison. At the very least the averaging method should be described. Alternatively the L2 data could be averaged by the authors and the effects of the averaging could be quantified for this scene much like in Figure 4.

459: Isn't this a physical effect as opposed to a mathematical one? The cloud and

aerosol observations are physically separated in time/space.

547-556: This point deserves more emphasis throughout the manuscript.

569: The method (Lines 573-584) seems to be largely driven by the model physics instead of the observations so it is difficult for me to see it as 'observationally-based'. Take the case of MODIS. The authors propose to use some measure of aerosol and a measure of L. Fundamentally MODIS provides two pieces of cloud information one of which (re) is being thrown away by this method. Level 3 MODIS data for example includes joint distributions of L and re for each one-degree grid box. How might this information be used?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26741, 2011.