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Atmospheric Measurement Techniques Discussions

Interactive comment on "Ground-based remote sensing scheme for monitoring aerosol–cloud interactions" *by* K. Sarna and H. W. J. Russchenberg

Anonymous Referee #2

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Sarna and Russchenberg present an analysis of ground-based active remote sensing, combining several instruments to infer below-cloud integral attenuated backscatter (ATB) as a measure for boundary-layer aerosol concentration and cloud-base cloud droplet number concentration, N_d, cloud droplet effective radius, r_e, and cloud liquid water path, LWP. The relationship of r_e and ATB for LWP bins, or the relationship of N_d and ATB can be interpreted as a metric for aerosol-cloud interactions.

The paper is rather well written, and the measurement technique interesting and useful, in particular as a monitoring tool for aerosol-cloud interactions.

My main comment on the paper is that the authors write at several instances that in

their opinion the aerosol-cloud interaction (ACI) metric (Eq. 5) is not ideal, but rather use the correlation coefficient. In my opinion, the paper does not justify this statement. No argument is explicitly given why the ACI metric should be inferior to the correlation coefficient. I believe, on the contrary, that the ACI metric yields relevant information that the correlation coefficient does not contain, namely the parameter \gamma in Eq. 1 that is highly relevant to determine the strength of the aerosol-cloud interaction and thus ultimately the forcing. Compared to the ACI metric, or the regression slope of the droplet concentration vs. ATB, the correlation coefficient is thus of lesser usefulness. The authors also do not explain why both the correlation coefficient and the coefficient of determination are useful. As far as I understand, the latter is just the square of the former and thus simply looses the information about the sign of the correlation, but does not bring any new information.

I thus request that either the point why ACI is not good be clearly explained, or that the authors move to determine the ACI metric.

Else I only have some minor remarks.

p11954 l10 Correlation coefficient for which quantities? l14 The abstract should explain what else is the best way

p11955 l19 This is true for convective clouds

p11956 l19 This is for the two studies cited, but – as e.g. discussed in the study by McComiskey and Feingold, a very large range of parameters is inferred from different methods. The theoretical bounds are 0 and 1.

p11957 l17/Eq. 5: The sign of the metric is different when considering r_e or \tau_d

p11958 l5 Twomey did not use aerosol optical depth, this came later with the arrival of satellite retrievals.

l25 This statement is unclear. Eq. 1 and Eq. 5 are the same if r_e $\sim$ N^-1/3 which is highly plausible, and it N_a $\sim$ \alpha, which is more debatable if a vertical integral

metric as aerosol optical depth is used.

P11963 l16: This is of course only true at constant or decreasing LWP.

P11983 Why not a linear scale for the effective radius?

P11970 l9 Although I believe I diligently read the paper, I missed the argument why the ACI metric is not the best way to analyse the data.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 11953, 2015.