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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A long-term study of aerosol–cloud interactions and their radiative effect at a mid latitude continental site using ground-based measurements" *by* E. T. Sena et al.

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Sena et al present a study relating temporal variability in cloud radiative effects to variability in various quantities including near-surface aerosol index, microwave-retrieved cloud liquid water path, as well as boundary-layer decoupling index and lower-tropospheric stability from 1-min resolution data with a conditioned sampling of low-level non-precipitating liquid-water clouds from 14 years of data at the ARM SGP site. The central result, presented in Fig. 6a, is that the surface aerosol index is uncorrelated with the cloud radiative effect, indicating a negligible aerosol influence on cloud radiative effect. This comes along with Fig. 6b demonstrating the dominant effect of LWP variability on the cloud radiative effect. The result is corroborated by

Fig. 13 showing that sensitivity of the cloud radiative effect to aerosol variability is driven by sensitivity of LWP to aerosol variability.

Very usefully, the authors proceed with a more detailed analysis. Eq. 8 and 9 set the theoretical framework of the discussion, namely the fact that the cloud radiative effect, rCRE, is influenced by three state variables: cloud fraction, LWP, and cloud droplet number concentration, N_d. The theoretical considerations (as well as experience and previous literature) clearly show that cloud fraction is the dominant influence, with LWP of slightly lesser but still very large importance, and that variability in N_d has a rather weak influence.

No measurements in N_d are used in the study by Sena et al.. Rather, near-surface nephelometer measurements of the aerosol index are used as a proxy for cloud condensation nuclei concentrations (CCN) and subsequently for N_d. The results of the decomposition analysis to identify impacts of the various parameters on rCRE are presented in Fig. 3 where all three effects are convolved. More instructive still is Fig. 5 where only overcast cases are selected. At the theoretical level (Eq. 8 and 9), for a given LWP bin, rCRE can only be a (strictly monotonically increasing) function of N_d. In light of this, the results are puzzling. There is no clear relationship of rCRE at given LWP with near-surface aerosol index (Fig. 5a). There is no influence of w' on rCRE (Fig. 5b). There is, however, a more systematic influence of the decoupling index and also of lower-tropospheric stability (LTS). The authors interpret that the aerosol impact is small. This is a straightforward interpretation of Figs. 5a and 6a. But how can this conclusion be true? Does this not imply the simple theoretical model in Eq. 8 and 9 is wrong? The other possible explanation is certainly that the nephelometer measurements near the surface are not a good proxy for in-cloud N_d. Perhaps one could test this by trying to relate remote sensing retrievals of N_d to the aerosol index?

A very interesting question is further, why is there the impact of decoupling index (Fig. 5c) and LTS (Fig. 5d), but not of w'?

My own experience would point to spurious variability in LWP and cloud fraction that the binning into bins of 5 g m^{-2} and the constraint of the retrieved cloud fraction at 100% is not able to completely inhibit. Given that despite the length of the data record not overly much data is available due to the conditioned sampling, an option would be to use coarser bins and see whether the effects are larger.

Else it is possible that the retrievals of either rCRE or LWP somehow depend on decoupling index or on LTS, but I am not enough of an expert on the retrievals to say whether this is possible. Another possibility is that the diurnal cycle in the decoupling index (Fig. 12) impacts rCRE more strongly than one might anticipate via the not-eliminated impact of the solar zenith angle (Eq. 8).
par

It would be very useful if the authors could discuss these questions in order to understand to which extent the “negative” or “null” result of no influence of aerosols on rCRE is a robust finding.

The authors also conclude that microphysical metrics in general are misleading. This conclusion mainly stems from the results in Table 2. While there is probably consensus that retrievals of cloud microphysical quantities are error-prone, such a broad conclusion should be corroborated better. If the authors decide this is a focus of the paper, then they should include a much more detailed discussion and ideally evaluation of the three different retrieval algorithms for the effective radius and explain why all three are (equally?) valid. In this regard, it should be noted that retrievals of microphysical quantities from the surface in general are more difficult

than from the top of the atmosphere (Brueckner et al., J. Geophys. Res., 2014 doi: 10.1002/2014JD021775)

Besides these two main comments, I have a few specific comments listed below.

In general I think this paper is a very useful contribution to the important discussion on how statistical analysis of remote sensing data may be used to quantify aerosol-cloud interactions. The paper is very well written and the figures are excellently prepared. The topic is highly pertinent to Atmos. Chem. Phys.

Specific comments

Title: Why not name the ARM SGP site?

Abstract p1 l18: I think that in the abstract “weak” needs quantification. One would expect the aerosol to be second order anyway.

P1 l29: Most publications currently would suggest additional effects due to microphysical adjustments, not so much a compensation (e.g. Lohmann and Feichter, Atmos. Chem. Phys. 2006)

p2 l6: I think we should in general aim to be more specific about what we mean by “meteorology”.

P2 l19: It would be useful to discuss Bender et al. (J Climate 2015 doi 10.1175/JCLI-D-15-0095.1)

P2 l27: It would be appropriate to cite Li et al. (Nature Geosci 2011, doi 10.1038/NGEO1313) here.

P2 l29: Again it would be good to specify what is meant by “meteorology”

p3 l13: abbreviate second as “s”

p3 l20: overcast at which scale (1 min$\approx$ 600 m for 10 m/s wind speed?)

P2 l8: “s” instead of “second”

p4 l31: this is only true on climatological (monthly-mean) time scales (e.g. Nam and Quaas, Geophys. Res. Lettt., doi:10.1002/grl.50945)

p5 l2, l4: minute $\rightarrow$ min

p5 l12: the stricter criterion was second in the earlier sentence

p5 l18: This is an interesting result. Is the conclusion that clouds are independent of the turbulence and other boundary-layer properties, at least with regard to the LWP?

P5 l24: It would be useful to also list the other numbers: what is the fraction of data-points with f=1, what with f $>$ 0.99?

P5 l27: To which extent are these two quantities independent at all? Is not actually one derived from the other one (Table 1)?

p8 l11: Why this choice? Why not choosing bins such that each contains the same amount of data?

P8 l17: But Fig. 3e still shows considerable f changes that dominate rCRE variability at low LWP.

P9 l1: Discussion perhaps on negative relation between τ and rCRE for lower LWP?

P9 l12: can one tease out the result more clearly e.g. by averaging over the LWP bins?

P9 l12: “this result suggests” $\rightarrow$ “this result confirms” (as this important point was discussed and explained earlier)

p9 l17: again, only on monthly timescales

p9 l25: as before “These results indicate” $\rightarrow$ “These results confirm”

p9 l31: good idea! How many points remain? Maybe show a joint histogram?

P9 l32: interesting result. What could be the interpretation?

P10 l11: for LWP bins? Or all data?

P10 l18: How can a conclusion about cloud-level aerosol conditions be drawn from these results?

P11 l12: RH measured where?

P11 l23: to have a physically more consistent equation, use the density of liquid water in the denominator (and then all quantities in SI units, or whatever consistent units)

p11 l26: typical for this type of scale and measurements, one should probably add and cite MdComiskey and Feingold (2012)

p14 l6: An obvious way to overcome the problem of variable LWP is to use droplet number concentration retrievals instead.

P15 l1: This conclusion seems only to stem from the examination of the three different effective radius retrievals. If this is intended as a main conclusion, more information about the retrievals is necessary, including some discussion on how reliable each one of these is.

p15 l4: “meteorological conditions” should better be explained. What actually is meant here is co-variability of the aerosol with cloud macroscopic quantities (LWP in particular), I believe.

P15 l7: “meteorological drivers”, or rather liquid water path and cloud fraction?

p16 References Abbreviate journal names appropriately (many instances)
Barnard et al. (2008): correct journal name

p26 Figure 3: add a joint histogram perhaps?

p32 Figure 9d: one more tick mark on the x-axis to properly define it.

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