

Interactive comment on “Monitoring Aerosol–Cloud Interactions at CESAR Observatory in the Netherlands” by K. Sarna and H. W. J. Russchenberg

Anonymous Referee #2

Received and published: 29 September 2016

The paper discusses the problem of aerosol-cloud interactions using remote-sensing data collected at Cabauw observatory. The method is based on the approach presented in Sarna and Russchenberg (2016).

Sections 2-4 repeat what is presented in Sarna and Russchenberg (2016) although in less exhaustive way, therefore very often difficult to follow. In my opinion one must study first the former paper to understand the algorithm with all its restrictions. The dataset is briefly presented already in section 4.3. It refers to Figures 1-3, which are not really discussed in the text. In general results shown in figures support information that data fits imposed criteria. This can be simply stated in the text without overloading the paper with figures. The only interesting figure is Liquid Water Path in Figure 2,

C1

because it shows the number of samples in each LWP bin; this information is however repeated in Tables 2-3.

In fact only Section 5 is original, because results from selected measurements collected during 2 month long observation period in Cabauw are presented. Two metrics are calculated, supplemented by correlation coefficient. Data set is divided into bins of different LWP values. Data collected in updraft regions is analyzed separately.

Presentation of results in Figures 4, 5 and 6 is in my opinion useless. Values of metrics, correlation coefficient and number of measurements in each LWP bin are reported in Tables 2-3. I don't see which information can be inferred from the color clouds of points shown in 12 panels presented in Figures 4,5, and 6. If there is a reason for it, please discuss it in the text. Results put in Table 2 are presented in Figures 8-9 and it is the only way that allows understanding the discussion. I miss the same presentation of results from Table 3 summarizing the ACIN metrics. Without presentation of all results (metrics ACIN, ACI_r, and respective correlation coefficients) in a form like in Figures 8-9 all discussion in sections 5.1.1, 5.1.2, 5.2, and 5.3 is difficult to follow. Figures that I quickly prepared using data from Tables 2-3 don't really support author's conclusions, or don't show that the conclusions are robust enough.

I strongly recommend the authors to: revise the first (theoretical) part of the paper, and to reconsider the way results are presented in figures. Specific and minor comments are listed below.

Specific and minor comments

1. Introduction

Page 2

– L. 3: Ramaswamy et al., 2001 is not a good reference here. This paper doesn't discuss the impact of clouds on climate.

– L. 9: Stephens, 1978, there is nothing about activation in this paper

C2

– L 18: please explain how is it possible that the ground-based remote sensing instruments are able to examine effects at the scale of the cloud droplet formation (less than centimeter scale)

2. Theoretical basis. . .

Page 3

– L 2: Twomey (1974) – the reference should be probably Twomey and Warner (1967). In Twomey (1974) there is nothing about airborne measurements. It gives only the direct form ula for the relation between aerosol concentration and cloud droplets size.

– Eq. 1 and 2. I find awkward to state that the optical thickness is proportional to BOTH – cloud concentration and effective radius.

– Is not the ‘proportionality factor’

– L. 16: γ is not the ‘proportionality factor’

– L. 18: I don’t see how Eq. 4 directly relates to Eq. 3

– L 23: please explain where the value of effective radius comes from – cloud top?

– L 30: please use different notation. The meaning of $\ln(\text{cloud})$ and $\ln(\text{aerosol})$ are awkward. The same $\ln(\text{aerosol})$.

3. Methodology. . .

Page 4

– L 24: please explain ‘well-mixed conditions’ 4. Observations. . .

Page 5

– L. 1: please explain explicitly which points are disregarded.

– L 22-23: please explain the physical reason of this additional selection criterium

C3

Page 6

– L. 22-23: The sentence starting with ‘However, to secure. . .’ Has nothing to do with information given just above.

– L 25-28: the whole paragraph is a little bit hectic.

– Are all data presented in Figure 1 used in the analysis?

5. Results and discussion

Page 7

– L. 3: first sentence is a repetition. We already know it.

– L.7: How do you know where is the precipitation threshold?

– What is the meaning of negative ACI_r????

– L 22: are you sure that there is a considerable increase in the value of ACI_r? I don’t see that the increase is ‘considerable’ when I plot data from Table 2. As for the correlation coefficient it is rather a ‘decrease’.

– L23-24: ‘values of ACI_r are higher for the smaller values of LWP’. Smaller than what? I see that ACI_r increases with increasing LWP, and becomes smaller for LWP>100. Is it the meaning of your sentence? If it is so, the sentence should state it clearly.

– L. 25-26: only one value of ACI_r in the updraft region for LWP>100 is high!!!! Not all values.

Page 8

– L. 1-2: supersaturation (dependent on thermodynamical properties and the strength of the updraft) plays a crucial role in droplet activation

– L. 5-6: explanation is not convincing.

– L.13: I would say between 60 and 100.

C4

- L. 15-17: that is a very weak argumentation
- L.21: ‘Both parameters . . .’ I don’t agree with that sentence; based on data shown in Tables 2-3.
- L.26: probably up to 100.
- L.28-33: this section presents on Figures what was already discussed before – see my major comments.

6. Summary and conclusions

Page 10

- L 9: what about the negative values?
- L 18: usually collision and coalescence produce drizzle. . . but you said that you discarded drizzle from your dataset.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-262, 2016.