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Discussions

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

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begindocument We thank the reviewer for his thorough review. Our specific responses are detailed below.

Response to main review points

- *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*

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

– We appreciate the comment and understand that not all the details of the algorithm are explained. The paper has a clear reference to Sarna and Russchenberg (2016) and is not meant as a repetition of the work already presented. The figures were redesigned in the revised version of the manuscript following the advice of both reviewers.

- *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, ACIr, 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.

- – As mentioned in the comment above, the manuscript was revised and figures redesigned following the advice given by both reviewers.

Response to detailed minor review comments

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

  - – This was a mistake. The correct citation here is Ramanathan(1989) . This was corrected in the revised version of the manuscript.

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

  - – That's correct. The paper and the cited equation relate to the cloud optical thickness which is defined as a relation of Liquid Water Path (LWP) and cloud droplet effective radius (re). It is not about the cloud activation.

- *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)*

  - – Ground based remote sensing instruments are operating at a high temporal resolution and at the same time at a high spatial resolution. It is possible to measure aerosol properties with a ground-based lidar with a high accuracy (Welton, 200) and the same is true for cloud droplet observation with the use of cloud radar and radiometer (Knist, 2014).

- *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 formula for the relation between aerosol concentration and cloud droplets size.

- – The correct reference for the cited formula here is Twomey (1977). This was corrected in the revised version of the manuscript.

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

- – Both equations are take from literature as cited in the manuscript.

- *L. 16: γ is not the 'proportionality factor'*

- – True. Gamma is a factor with which aerosol number concentration and cloud droplet number concentration depend on each other. This was corrected in the revised version of the manuscript.

- *I don't see how Eq. 4 directly relates to Eq. 3*

- – Eq. 4 is derived by Feingold (2003) to account for the gamma factor mentioned in the comment above.

- *please explain where the value of effective radius comes from – cloud top?*

- – The value of effective radius comes from the cloud base area. The location of the measurement of the effective radius was specified in the revised version of the manuscript.

- *please use different notation. The meaning of ln(cloud) and ln(aerosol) are awkward. The same mln(aerosol).*

- – The notation was adjusted in the revised version of the manuscript.

- *please explain 'well-mixed conditions'*

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– Well-mixed condition refer to a cloud at the top of the boundary layer, where the vertical mixing of the layer is strong. The definition was added to the revised version of the manuscript.

- *please explain explicitly which points are disregarded.*

  – All points where any form of precipitation or insects were identified by the Cloudnet classification scheme were detected are disregarded.

- *The sentence starting with 'However, to secure. . ..' Has nothing to do with information given just above.*

  – The paragraph was rephrased in the revised version of the manuscript.

- *the whole paragraph is a little bit hectic.*

  – The paragraph was rewritten in the revised version of the manuscript.

- *Are all data presented in Figure 1 used in the analysis?*

  – Yes. This was stated explicitly in the revised version of the manuscript.

- *first sentence is a repetition. We already know it.*

  – Paragraph was rewritten in the revised version of the manuscript.

- *How do you know where is the precipitation threshold?*

  – The threshold was chosen based on the values taken by Feingold in the first work about the ACI (Feingold, 2003). A citation was added in the revised version of the manuscript.

- *What is the meaning of negative ACIr????*

– The negative value of ACIr are caused by the error of the retrieval algo-
rithm or the sampling. Negatives values were reported in previous studies
(Feingold, 2006).

- *are you sure that there is a considerable increase in the value of ACIr? 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'.*

  – The paragraph was rephrased in the revised version of the manuscript to
better describe the results.

- *'values of ACIr are higher for the smaller values of LWP'. Smaller than what? I
see that ACIr 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.*

  – It is the meaning. Paragraph was rewritten in the revised version of the
manuscript.

- *only one value of ACIr in the updraft region for LWP>100 is high!!!! Not all values.*

  – Paragraph was rewritten in the revised version of the manuscript.

- *supersaturation (dependent on thermodynamical properties and the strength of
the updraft) plays a crucial role in droplet activation*

  – Yes, but the influence of the aerosol size was also indicated in some studies
(McComiskey, 2009).

- *explanation is not convincing.*

  – The difficulty of deriving the cloud droplet number concentration was de-
scribed in detail in Knist (2014).

- *I would say between 60 and 100.*

  – Values were adapted in the revised version of the manuscript.

- *L.26: probably up to 100.*

  – Values were adapted in the revised version of the manuscript.

- *L.28-33: this section presents on Figures what was already discussed before – see my major comments.*

  – The revised manuscript was adjusted in this section after redesigning figures based on the comments from both reviewers.

- *what about the negative values?*

  – A short explanation about the negative values was added in this paragraph in the revised version of the manuscript.

- *usually collision and coalescence produce drizzle. . . but you said that you discarded drizzle from your dataset.*

  – Drizzle was disregarded base on the Cloudnet target classification. It is possible that not all instances were disregarded. Some studies suggest that drizzle can occur at the values of liquid water path between 75 and 100 gm-2 (Remillard, 2012).

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