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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Role of updrafts in aerosol-cloud interactions: lidar observations of layered warm clouds over central Europe" *by* J. Schmidt et al.

Anonymous Referee #2

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This manuscript reports susceptibility estimates of cloud droplet number concentration and effective radius to ambient aerosol, using retrievals from Doppler lidar and a dual field-of-view Raman lidar. Measurements from the dual field-of-view Raman lidar allow to infer cloud properties a bit more than traditional lidars can offer. Since the novelty of the retrieval algorithm has been reported in a separate journal paper, reviews on the manuscript are based on the following:

1) Has data analysis been done properly and are results statistically significant for the authors to address the question shown in the title "Role of updrafts in aerosol-cloud interactions"?

Although the authors analysed 2-year long night time data, the final sample size for this manuscript is 29 cases. For some reason, only 26 cases were analysed/plotted, and the authors didn't explain why the other 3 cases were not suitable for the analysis. I was hoping to see some statements indicating that each case actually contained quite a few profiles (data points) so the data analysis here was based on a considerable number of data points. Since I don't see such statements in the manuscript, I assume that the authors use 26 data points for their analysis. When these 26 data points are further stratified by vertical velocity, as shown in the manuscript, "the role of updrafts" is then discussed based on very limited samples. I believe if the authors perform proper statistical tests, all (e.g., Figure 6) would fail to pass the 95% confidence level, except the red bar at 30–70 m. Additionally, it is unclear if seasonal variability is properly taken care of in the data analysis. In summary, I am afraid that I fail to see how such a small dataset and a lack of rigorous statistical analysis presented in the manuscript can be scientifically appropriate to draw meaningful conclusions.

2) Has the manuscript provided any new insights for quantifying aerosol-cloud interactions?

I am afraid that the intercomparison and discussions in Section 4 are not sufficiently critical to provide any new understanding of aerosol-cloud interactions. I also feel that some conclusions really lack supporting evidence and rigorous justification. For example, Case A is used to indicate that the low ACI_N is in the right direction and is consistent with the past estimates over the continents. What is Case A? Is Case A in Figure 5 at all? Why is it OK to assume a simplified cloud droplet number concentration profile and conveniently choose an integrated number concentration, rather than certain "penetration heights" like results shown in Section 3? If Case A is consistent with the past estimates over the continents, estimates presented in Section 3 are ALL for the continents, so why are they so different from 0.1?

Specific comments:

1) Concern about statements/conclusions about mixing near cloud top: While dual (or multiple) field-of-view lidar measurements allow cloud optical depth retrievals, my understanding is that information on cloud geometric thickness will be still missing. I wonder how the authors can be so confident about the locations of cloud tops based on these lidar measurements. Is any additional instrumentation used?

2) Title: I am not sure that the title is appropriate. First, as explained, the sample is too small to conduct a meaningful analysis to investigate the role of updrafts. Second, I am not sure what "layered" really means here. Can the dual FOV Raman lidar provide information on the number of cloud layers? It looks like most of cases are probably single layered. If that is the case, "layered" in the title may confuse readers.

3) Section 2: Could the authors please explain how ice clouds are excluded? Additionally, please provide brief information on vertical and temporal resolution here (rather than referring back to Schmidt et al. (2014a)). This information is important for readers to understand how many data points have been used for calculating error bars shown in figures.

4) Section 3: Figure 1 doesn't bring in any additional information that the text hasn't provided; I don't feel it serves any purpose. The text and captions are full of arbitrary thresholds – justifications of these thresholds are needed. It is important to comment on how sensitive results are to the choice of these thresholds? Also, in Section 3.2, do the authors really mean 10–90 min for signal averaging? Is it for wind retrievals only?

5) Very minor – Page 31418, Line 12: I understand what the authors mean by "cloud penetration-depth effect", but clouds don't have such an effect. I would suggest writing this sentence in more precise words.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 31409, 2014.