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Interactive
comment

Interactive comment on "Statistics of vertical velocities in supercooled cloud layers over Leipzig and Praia measured with Doppler lidar" *by* Johannes Bühl et al.

Anonymous Referee #1

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In the manuscript by Bühl et al, two remote sensing datasets are used to derive and compare the statistics of cloud base vertical velocities in supercooled and mixed-phase stratiform clouds at two different locations: In Leipzig and Cape Verde. Their results show that, for the observed cases, the vertical velocity statistics associated with the clouds at the two sites are very similar and attempt to couple this with the subject of ice formation efficiencies at the two sites.

Overall, datasets comprising detailed vertical velocity measurements from different regimes and especially in mixed-phase clouds, as shown in this manuscript, are interesting for the aerosol-cloud community. However, I am quite concerned about the

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presentation of the analysis as well as the results and the conclusions, which I feel should be more substantial to be published in ACP and which would probably require changes beyond a major revision.

General comments

1. The Authors conclude that something else than cloud dynamics must affect the differences in ice formation, which clearly points towards the role of aerosols. In fact, the role of aerosols is acknowledged already in the Introduction, so I find it very limiting that this is not commented in the rest of the paper. I understand that the Authors would like to have this manuscript mainly focused on the vertical velocity statistics. However, given that the main conclusion is related to the cloud freezing efficiency and that the Authors limit their analysis to clouds where immersion freezing presumably dominates, I find this approach quite counter-intuitive and a serious concern for the scientific significance of the paper.

2. The Authors provide very little or no information about the differences in large-scale meteorological and environmental conditions, that can contribute to the cloud properties as well as the boundary layer mixing and thus vertical velocities. The analysis must go deeper and will need to include more details to provide the reader with more substantial information. It would be nice to also have explicit consideration of the boundary layer structure – whether the mixing is surface or cloud driven, or if the boundary-layer exhibits decoupled structure for example.

3. Since the analyzed data comprises relatively long periods of time especially for the Leipzig data, the previous point would probably call for analyzing also the seasonal differences – another interesting point which is not considered in the current manuscript. Do the same conclusions about the vertical velocity statistics hold throughout the year? Now the analysis comprises three kind of "random" months (Jan, Feb, June) at Praia vs. 3 full years from Leipzig lumped together.

Specific comments

1. Since the Authors focus on certain types of clouds and limit the temperature ranges etc in the sampling, should we actually expect to see large differences in the dynamics, i.e. vertical velocities? While the Authors state (Section 5, line 219) that they expected strong differences, it is not at all clear what sort of differences they are going after specifically. Please elaborate.

2. As stated in the manuscript, the ice formation can be expected to take place predominantly in the cloud top region, most likely through immersion freezing in this case, due to the screening of the cloud types. How would you rationalize the role of cloud base vertical velocities here, especially since immersion freezing essentially depends on the properties of the aerosol?

3. Section 5, line 207-209: Is this the case even when you limit the vertical velocity retrievals to -2...2 m/s?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-230, 2017.