

Reviewer comments on ‘Using a holographic imager on a tethered balloon system for microphysical observations of boundary layer clouds’ by Fabiola Ramelli, Alexander Beck, Jan Henneberger, Ulrike Lohmann

Response to Reviewer #2

We would like to thank the anonymous referee for his/her valuable feedback and suggestions on the manuscript. We incorporated the suggestions within the revised manuscript, which substantially improved the quality of the manuscript. In the following, we will address the comments.

General comments

The paper describes balloon-borne measurements of microphysics inside supercooled boundary layer stratus clouds collected with use of a modern holographic imager HOLIMO. The paper consists of two very distinct parts. From the beginning to section 4.3 the paper is clear, very well written and there are no major drawbacks in the text. The description of the measurements, calibration is sound. The results are interesting, show unexpected behavior of cloud microphysics, hard to document with different, than HOLIMO instruments. This, with some additional discussion and maybe selected examples of local samples of droplet spatial and size distributions would be enough to justify the publication. However, instead of focusing on microphysics, in the last sections of chapter 4 and in the discussion author speculate on mixing and dynamical effects which are aimed at explanation the unexpected results of microphysical measurements, in particular large variations in droplet number concentration. These speculations should be backed with the data, but are not. As shown in Fig.1 the HoloBallon is, together with the HOLIMO, equipped with a sonic anemometer, which should provide in-situ high-resolution data on turbulence (velocity fluctuations) and virtual temperature. The authors, instead of using data from the device, speculate on turbulence and waves, Kelvin-Helmholz Instabilities, downdrafts. I strongly believe that insight into sonic data could be used to verify which speculations are justified and which are not. In particular virtual temperature fluctuations might help to understand mixing, velocity records should allow to document turbulence, waves and K-H instabilities.

In my opinion the paper in the present form is hardly acceptable. I suggest the major revision of the text. Two options is possible: 1) to make the paper shorter, remove the speculative part of the chapters 4 and 5 and to write that the explanation requires additional, highly demanding analysis of turbulence data recorded; 2) to use sonic data and do the analysis in a simplified form, to show some dynamical properties of the flow to support speculations presented in the text.

If the authors chose the second option, I suggest more detailed insight into the cited Mellado’s paper about stratocumulus top and into references therein. Such insight, in my opinion, could help very much in the analysis.

Thank you very much for your valuable comments and suggestions. In the substantially revised manuscript (especially Sect. 4.4 and Sect. 5), we focus more on the technical aspects of the HoloBalloon platform, rather than on the scientific outcome of Sect. 4.4. We agree that further data and analysis are required to back the hypotheses presented in Sect. 4.4. As the aim of the paper is (1) to introduce and characterize the newly developed HoloBalloon platform, (2) to provide a proof of concept for the HoloBalloon platform (case study) and (3) to show the potential and limitations of the platform in studying boundary layer clouds, we shortened the section 4.4 (especially the speculative part) as well as the discussion of it. Furthermore, we clearly indicate that further analysis of our data as well as auxiliary data (e.g. three-dimensional wind field, turbulence) are required to study the proposed mechanisms/ test the hypotheses, which lies beyond the scope of this paper.

We agree that the data of the 3D sonic anemometer could provide useful information about the dynamical properties of the flow and help to support the proposed hypotheses. However, we decided to not analyze the turbulence data of the 3D sonic anemometer, as it is installed on the keel below the balloon. Several experts in the field advised us to install the instrument package in future 20-30 m below the balloon in order to reduce influences of the balloon on the turbulence measurements. Thus, in the present study we cannot exclude influences of the balloon on the turbulence measurements (described in Sect. 5.1). For future field campaigns, we will follow the advices and install the instrument package 20-30 m below the balloon to be able to analyze turbulence data of the 3D sonic anemometer. The feasibility of a hanging mount was already successfully tested in the field in autumn 2019.

In the revised manuscript, we changed the order of subsections 5.1 and 5.2 in the discussion section. Moreover, we completely rewrote Sect. 5.2 (previous 5.1). In Sect. 5.2, we focus more on the technical aspect of HoloBalloon, rather than on the scientific outcome of Sect. 4.4 (in contrast to the previous version). The observations of the presented case study are used as an example to discuss the potentials and limitations of the HoloBalloon in studying boundary layer clouds.