

Interactive comment on “The breakup of levitating water drops observed with a high speed camera” by C. Emersic and P. J. Connolly

Anonymous Referee #3

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This paper presents the results of water drop collision experiments carried out in a wind tunnel. The paper is in general well written, however a detailed description of the objectives is missing. The subject of the study is of crucial importance for the atmospheric research community, as the collision-coalescence and collision induced break-up processes play important roles in the drop size distribution evolution of cloud and rain drops. There are, however, some major aspects which should be taken into account; therefore I recommend a major review of the paper before publication.

The study revisits in some sense the experiments of Low and List (J. Atmos. Sci., 39, 1591-1606, 1982), who carried out similar measurements on drop-drop collisions and presented a parameterization on drop size distribution after break-up. Since the parameterization of Low and List (J. Atmos. Sci., 39, 1607-1618, 1982) was based on

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their own measurements only, it is desired to verify or complete their study using either theoretical (as McFarquhar, J. Atmos. Sci., 61, 777-794, 2004; for instance), numerical (Straub et al., J. Atmos. Sci., 67, 576-588, 2010), or experimental techniques. In experimental studies, however, it is of crucial importance to appropriately characterize the measurement conditions, and – especially in the laboratory based atmospheric simulation measurements – to correctly simulate the real-atmospheric physical processes. From these aspects I see a lot of deficiencies of the presented study.

The first very important characteristics which has to be accurately determined in wind tunnel experiments is the air flow aerodynamics. It has to properly simulate the real-atmospheric conditions, where the raindrops are falling at their terminal velocities in air. From this reason the turbulence level and the wind shear level in the wind tunnel has to be precisely characterized and described. A plot of the velocity profile at the level where the drops are levitated should be therefore presented. One can estimate, however, the velocity shear from the speeds at the centre and at the edge of the tunnel given by the Authors in the manuscript: Within the scales of the measured drop sizes a wind speed difference more than 10% can be estimated, which is definitely not realistic (see e.g. a summary of the turbulence levels in the atmosphere in Vohl et al., J. Atmos. Sci., 56, 4088-4099, 1999) and makes the drops probably instable. This effect can result in false experimental results and conclusions as, e.g., in the paper of Villermaux and Bossa (Nat. Phys., 5, 697-702, 2009), which is also cited by the Authors, who studied the spontaneous break-up of raindrops, and concluded that raindrops larger than 6 mm in diameter cannot be exist. (As can be actually seen in the presented experiments, too, drops larger than 6 mm can be levitated in a stable fashion in a wind tunnel.) Thus, the characterization of the air stream would be also desirable for the comparison of different experimental results.

As far as the proper simulation of raindrops concerns, I suggest to present the results of the drop oscillation, and compare it to former experimental and model studies (a summary on that can be found, e.g., in Szakáll et al., Atmos. Res., 97, 416-425, 2010).

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The shapes, oscillation frequencies and oscillation amplitudes could also help to verify the correct simulation of atmospheric conditions and to compare the results with other measurements. Furthermore, it has to also be clarified why the drop levitating speeds in the present study (~ 10.4 m/s) deviate so much from the literature value (~ 9 m/s). It might be a consequence of an unnatural distortion of the drop (more prolate) caused by an enormously high wind shear, which results in higher terminal velocities. (I note here that the publication year of the book Pruppacher and Klett: *Microphysics of Clouds and Precipitation*, has been given false, it should be 1998.)

The next critical aspect from the physical point of view is the simulation of the collision of two drops. As smaller drops fall generally slower than larger ones, at collision the small drop should be flow upstream relative to the large one. In the presented experiments the smaller drops always impact from above on the larger ones which can probably happen if the small drop moves into the wake of the large one, but I doubt it to be a significant process in the real atmosphere. The authors "felt this approach was appropriate to address the aims of this study"; it should be clarified why this approach was appropriate and what the aims of this study exactly were. It has to be also noted that right before the collision both drops should move at their real terminal velocities relative to the wind stream which was not the case in the present experiments. As far as the result discussions concern, the concluded remarks are questionable as they are based on only 25 measurements, which is too few to carry out proper statistics. The sizes of the colliding drops seem to be also too large. Does the collision of drops with 4 and 6 mm diameter happen in the atmosphere frequently enough to be a significant process? Some estimations on it should also be presented in the paper.

To conclude I suggest a major review of the paper and a statistics where more collision processes are included. Furthermore, it would be desirable to compare the results also to improved DSD parameterizations (e.g. McFarquhar, 2004, and Straub et al., 2010).

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