Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-345-RC1, 2019

Atmospheric Chemistry and Physics Discussions
Open Access
EGU

Interactive comment on "Small Ice Particles at Slightly Supercooled Temperatures in Tropical Maritime Convection" *by* Gary Lloyd et al.

Anonymous Referee #2

Received and published: 12 July 2019

General comment. Understanding the formation of ice in mixed phase cumulus clouds is hampered by our ability to measure small ice particles. Studies reported in this paper utilize a relatively recent technique for airborne ice measurements, using holography, which offers an improved method to sample small ice and to discriminate the signal from large cloud droplets. This paper represents an early example of how this technology can be applied to studying ice formation in cumulus clouds and will be of interest to the readers of this journal. However, the paper should be expanded and improved in some areas before it is published as discussed below.

In cloud temperature. A significant piece of this report involves in situ temperature measurements that appear to be as much as -7 °C lower than computed based upon the assumption of water saturation and measured water vapor concentrations. The

Printer-friendly version

Discussion paper

authors attribute this to evaporative cooling of the temperature sensor, a problem that has been reported before. However, the magnitude of this cooling appears to be higher than reported before and the analysis of this phenomenon is much less thorough than it needs to be to explain why this case should be different from previous studies. For example, evaporative cooling should not be constant across the cloud pass and temperatures and liquid water contents together should be compared with adiabatic values. Why was the wetting/cooling so much worse for the first three passes? The authors should consider, for example, some of the analysis techniques used by Lawson and Cooper (1990) to better explore and document their results. It would also be useful to compare their results with other studies on cloud buoyancy (e.g. some examples are given in Lawson and Cooper).

Secondary ice. The analysis of primary versus secondary ice formation is not thorough enough to be of much use. For example, is there sufficient evidence in their data, in terms of satisfying the criteria for rime-splintering (Hallett-Mossop) secondary ice mechanism, to support that as explaining (or not) the observed increases in ice concentration? The authors claim "in this case.... there is strong evidence for this mechanism being involved.." What strong evidence are they referring to? Is there enough information here to suggest the other mechanisms are active? How does the holographic data allow an improvement over previous studies? Etc.

Potential seeding from above. The paper claims "seeding of the cloud from above was looked for through analysis of instrumentation before and after the penetration, with no evidence of this process taking place." What analysis was done?

Dust and CCN. This section seems to be added on to the paper, without much tie-into the previous sections. The fact that there was dust present seems unremarkable given the location.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-345, 2019.