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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Structure of Turbulence and mixed-phase Cloud Microphysics in a Highly Supercooled Altocumulus Cloud" *by* Paul A. Barrett et al.

Anonymous Referee #1

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Review of 'The structure of turbulence and mixed-phase cloud microphysics in a highly supercooled altocumulus cloud" Barrett et al

The authors present a case study of a mixed-phase altocumulus cloud observed over the N. Atlantic. In situ observations collected from an instrumented aircraft provide details of the cloud thermodynamic, microphysical, and turbulent structure. The roughly 600 m thick cloud consisted of a relatively thin liquid top with mixed phase below, giving way to all ice in the lowest ∼half of the cloud. Observations were obtained through a combination of straight/level legs and sawtooth legs over ∼10 km horizontal distance. The authors carefully designed a coordinate system referencing the inversion

that topped the cloud layer, and by letting the cloud advect in time in relation to this coordinate system, reference all measurements to a single vertical coordinate.

The manuscript is well written and presents a comprehensive set of observations from a single case. The authors' analyses are thorough and their conceptual model presented in the discussion is well supported by the measurements. The authors also do a good job of placing their observations in the context of previous work. I recommend publishing after they address a few minor comments.

Minor Comments I don't believe the authors make a strong connection between the increasing negative skewness of w' with increasing distance beneath cloud top and what they assert is driving this, specifically longwave radiative cooling at cloud top. This assertion is stated both in the abstract and in the conclusions and while I agree their conceptual model fits well with that presented in other papers, such as Schmidt et al (2014) for instance, they really don't provide any evidence or even an explanation why radiative cooling should result in w' profiles as those observed.

Last sentence in the abstract: "...used to evaluate numerical simulations of altocumulus clouds at all scales from eddy resolving to climate." The authors are mixing up spatial scales (eddy-resolving) with temporal (climate). I understand the point they are trying to make, but this should be reworded in my opinion.

Line 93: I'm pretty sure the thermometer did not respond at 32 Hz, even though measurements may have been reported at this high rate. Authors should provide actual response rate of the instrument. Also, many of the measurements were conducted in cloud. Were corrections made for wetting of the element? Or how were in-cloud (liquid) temperatures determined?

Page 4 and variety of locations in manuscript regarding discussion of using the CIP15 (and CIP100) to determine NI (ice crystal concentrations). Some number of 30-60 (or maybe even 90) micron particles measured by the CIP15 are likely liquid droplets. First, some liquid in these larger size ranges might exist and the CIP15 does not provide suf-

ficient resolution to distinguish between liquid and ice for such small particles. Second, smaller, out-of-focus droplets will appear larger. This could lead to an over-estimation of the ice crystal concentration nearer cloud top where liquid droplets are largest. How do the results change if the analysis were to ignore all measurements from the CIP15 with diameters smaller than 90 microns?

Bottom of page 6, line 187: the impact of wind-shear across the inversion and resultant contamination of the filter should only occur near the inversion, correct? If that is the case, then why substitute w' in place of u' and v' throughout the depth of the profiles. I think you should only do that near cloud top.

Around Line 280, discussing estimate of INC based on aerosol larger than 0.5 um. The authors provide an estimate based on DeMott et al (2015) and then appear to 'adjust' (increase) that based on a correction factor of 3. But DeMott et al's equation (2) includes CF (calibration factor as they call it). Thus to be consistent, authors should only report the larger value; the lower value of 0.3 L-1 doesn't make any sense, or at least is NOT CONSISTENT with DeMott et al (2015).

Technical Comments Line 4: change 'Turbulence data' to 'Turbulence measurements'

Line 16: '. . .mixed-phase conditions began within a few metres. . .' began is probably not the right work, it implies time. I sugges something like '. . .mixed phase conditions were observed within a few metres of cloud top extending downward through the cloud'

Line 89: abbreviation of 'knots' should it be 'kts' ??

Line 275: 'Fig. 12@(b)' ??

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