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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.

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We thank the reviewer for their helpful comments and suggestions.

1 Minor Comments

I dont 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 dont provide any evidence or even an explanation why radiative cooling should result in w profiles as those observed.

Long-wave Radiative Cooling and skewness. We have added comment in the discussion relating to the Hogan 2009 study which observed the skewness profile of nocturnal stratocumulus to be similar to the profile we observed. This is the basis for our assertion that the source of the turbulence is at cloud top.

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.

Abstract text clarified to account for spatial and temporal scale confusion.

Line 93: Im 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?

Some of the temperature measurements were in fact collected in cloud. We expect that the low liquid water contents (less than 0.05 g m^{-3}) in the cloud result in a negligible impact on the cooling of the non-de-iced sensor housing. A review by Sinkevich and Lawson (2005) of temperature measurements in convective clouds would seem to confirm this. Figure 4 of that paper shows the difference a radiometric measurement of temperature and an immersion sensor as a function of liquid water content. For the liquid water contents that we observed the temperature difference or 0.05 ℃ is an order of magnitude lower than the quoted uncertainty of our temperature sensors.

The reviewer notes that the temperature sensor is unlikely to respond as fast as 32 Hz even though it is recorded at the rate. We agree and in-fact here we are reporting data at 1 Hz and have amended the text to reflect this.

We do however use the turbulent wind data at recorded at 32 Hz. We computed power spectra from one of the geometrically level flight legs and found that all three wind components follow a -5/3 power law out to at least 10 Hz, possibly closer to the Nyquist frequency of 16 Hz. We reprocessed the wind components after applying a smoothing window (to give data at a frequency of less than 10 Hz) and found that the TKE was not impacted by more than 5 percent, with skewness values not impacted.

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 sufficient 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

We refer to the CIP15 (which responds to particles larger than 15 microns) number concentrations for ice in multiple places throughout the text. Whilst we agree that some artefacts may be present in the CIP15 ice data at sizes between 60 and 90 microns, when we only consider the CIP15 data for particles larger than 90 microns we find only minimal difference in the statistics of the vertical profiles of ice number concentrations, that do not alter the conclusions. As part of the quality control process we looked at the histograms of number concentrations from CIP15 and CIP100 (which responds to particles larger than 100 microns) and considered individual channels concentrations from CIP15. The figure here shows the distribution of particle concentrations from CIP15 (green) and CIP100 (orange). Data from CIP15 are clearly bimodal, with the higher concentration mode having number concentrations greater than 0.5 L^{-1}. Plotting only

the data from the smallest size channel (Channel 0) on CIP15 (dark red) shows that these particles are the ones smaller than 15 microns, and are almost certainly part of the liquid cloud population. CIP100 does not observed this mode. Channel 1 only from CIP15 (30 micron particles) (red) and channel 2 (45 micron particles) (pink) do not observe this mode. The total concentration of particles larger than a particular size is shown in the blue colours, with particles larger than 30, 45, 75, 135 and 255 microns shown in ever lighter shades of blue. These distributions shows only the ice population and look similar to the observations from CIP100. We took this to show that in this situation with the observed liquid cloud population on the day that the CIP15 only responded to the liquid cloud in the smallest size channel, bin 0 (less than 15 microns). We therefore have confidence that bins larger than this are responding only to ice, but leave a buffer of two adjacent bins.

Any contamination by small particles would not impact the IWC calculation significantly. We do not use the CIP 15 for effective radius calculations, instead we rely on CIP100.

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.

In figure 5 we have added a trace showing the TKE estimate when considering all three wind components. This shows the good performance of just considering w' when above -100 m, and that the estimate only using w' is biased high compared to the three wind component estimate, when below -100 m. This is discussed in the text.

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 als 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 doesnt make any sense, or at *least is NOT CONSISTENT with DeMott et al (2015).*

We agree with the reviewer. Reference to uncorrected INP concentration is removed from the figure and the text.

2 Technical Comments

Line 4: change Turbulence data to Turbulence measurements

We have changed 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

We agree, began was not the correct word. Text has been altered.

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

Abbreviation changed to kts

Line 275: Fig. 12@(b) ?

typo corrected

A.A. Sinkevich and R.P. Lawson, A Survey of Temperature Measurements in Convective Clouds, J. Appl. Met. 44, 1133-1145, 2005

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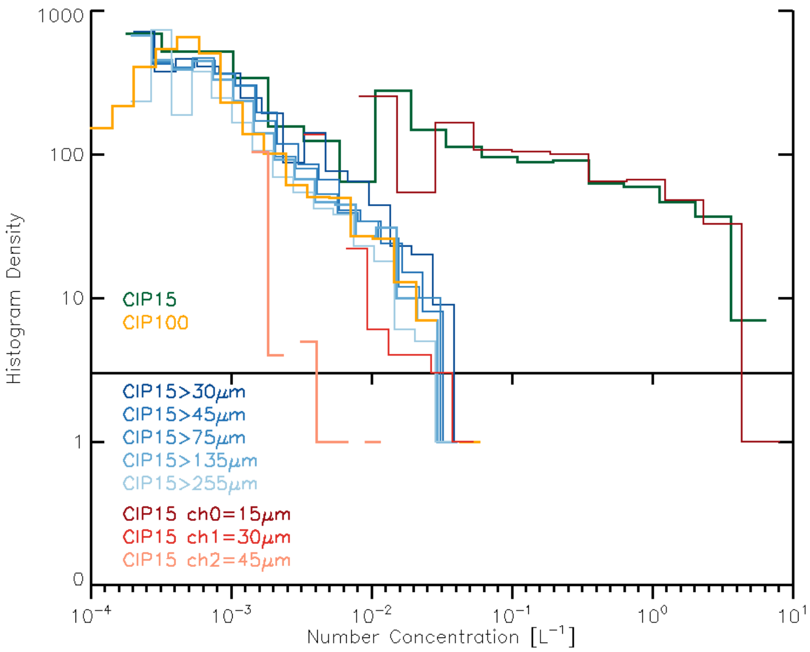

Fig. 1. Cloud particle number concentration histograms from CIP15 and CIP100