

The paper examines 4 convective cases from the recent COPE campaign using aircraft and radar data. I found the paper generally well written. The approach is largely qualitative and I would like to see more work done to provide quantification of secondary ice processes that would be useful to the modelling community. My comments are generally minor in that I do not expect them to undermine the main messages of the paper. However, I think they would strengthen the paper and make it more useful to the community.

General comment This was a multi aircraft campaign. The BAe146 data is referred to in terms of reports from the Taylor paper. Why wasn't the data included in the C1 analysis to confirm or extend the observations from the Wyoming King Air? I only mention this because whenever observationalists request resources for aircraft there are often cases made for the use of two aircraft. This would be an excellent opportunity to demonstrate the success of using multi-aircraft that future proposals can point to.

We thank the reviewer for their constructive comments.

COPE was a multi-aircraft campaign, with the intention of coordinating sampling strategy between platforms to leverage the strengths of each platform and provide much greater spatial and temporal coverage within the study domain. However, it is not possible to utilize the data from the BAe146 for this study for three important reasons. This study utilizes a statistical approach to characterize the microphysics near the top of turrets as they grew through specific levels. For this, it is important to be able to measure the distance to cloud top from the penetration level. In the UWKA data set this is provided by the WCR. Indeed it was shown in this study that more than 80% of cloud penetrations were within 1 km of cloud top. No such verification is available in the BAe146 data.

Because of the instrument complement on both aircraft, the UWKA flew at a higher altitude than the BAe146. During multi-aircraft missions, while the UWKA was penetrating the tops of turrets, the BAe146 was obtaining measurements much lower down in cloud. We feel that combining these two datasets would be mixing apples with oranges.

As detailed in Taylor et al. (2016b), the study utilizing -146 measurements from 3 August detailed how that aircraft made repeated penetrations in clouds along the convergence line in order to study the development along the line and also the time evolution. However this study focuses only on cloud characteristics as clouds first ascended through a specific level. These are two fundamentally different strategies.

Lastly, of the four cases examined in this study, the -146 only flew on 29 July and 03 August. No additional data would be available for 02 August or 28 July. For these three reasons we believe it best not to include the -146 data in this study but rather use this study to provide a compliment to the results of Taylor et al. (2016b).

In the revised manuscript, we add additional text at the beginning of section 3b describing in more detail the sampling strategy of the UWKA for this study.

We have embedded the responses to the reviewer's specific comments in bold.

Specific comments:

Section 1 – there is also a recent COPE modelling study by Miltenberger (<https://www.atmos-chem-phys.net/18/3119/2018/>) and another by Yang that looks at updraughts from COPE that may provide some nice context (<https://www.atmos-chemphys.net/16/10159/2016/>).

We have added text in Section 1 briefly describing the key points of these two papers as they relate to the current study in order to provide a better overview of previous COPE studies relevant to current work.

P7 line 17-27: I agree that threshold need to be chosen to make the analysis tractable but it would be worth a statement to say if there was any sensitivity to the choice of these thresholds (0.05g/m³, 300m, 100m, 1m/s, 3m/s) in terms of the results and conclusions drawn.

We conducted various sensitivity tests to the choice of the thresholds for LWC, updraft width, and the minimum velocity. We found that our overall conclusions were not sensitive to the choice of our thresholds. We have attached an example of a sensitivity test where we define a penetration using a 0.03 g/m³ threshold and one with a 0.05 g/m³ in Figures R1 and R2 that show the analyses in Figures 3 and 4 using these thresholds. In Figure R3 we have attached a version of Figures 3 and 4 using a minimum penetration (or updraft) depth of 300 m in place of 100 m. We notice little change in the conclusions drawn when such analyses are conducted with these new thresholds. In order to summarize these sensitivity tests, we added a sentence to the first paragraph in Section 3b:

“Sensitivity tests conducting analyses using differing thresholds for LWC and updraft width showed that this conclusion was insensitive to the thresholds used to define a penetration or updraft core (not shown).”

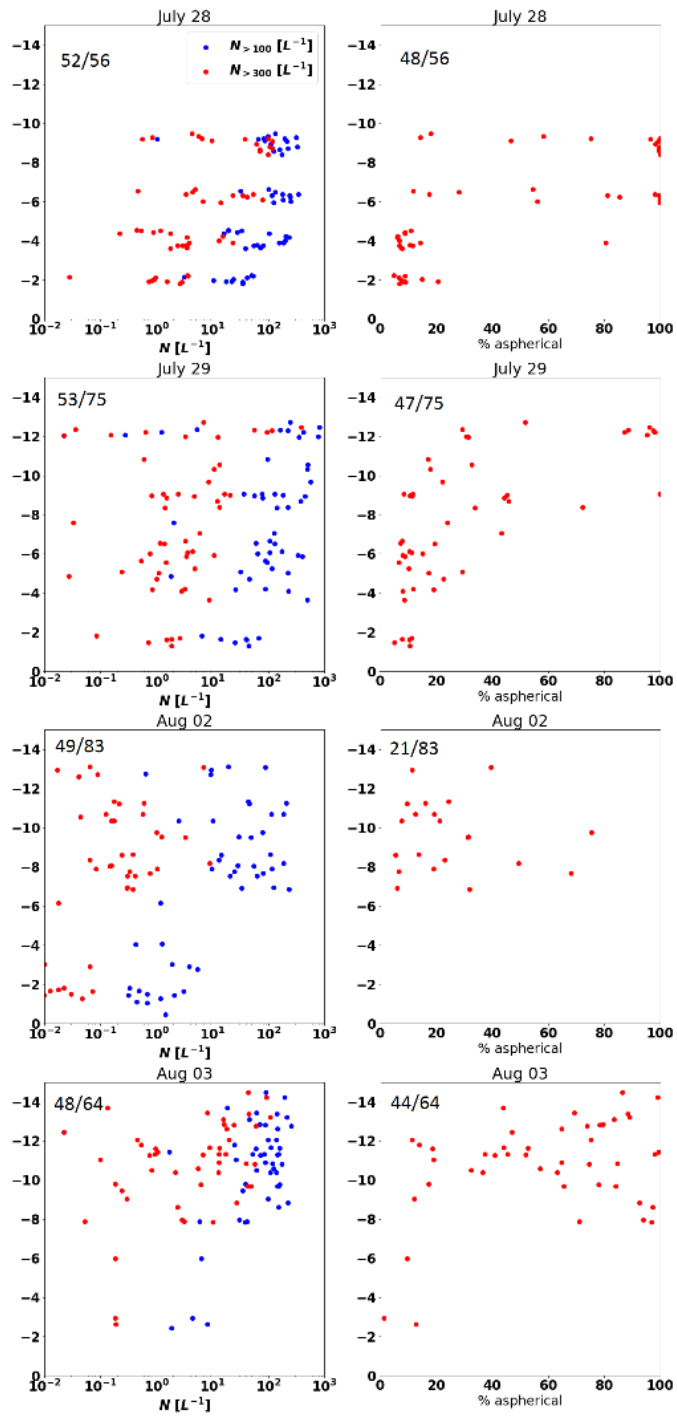


Figure R1. (left) The median total number concentration and (right) percentage of aspherical particles in each penetration defined by $LWC > 0.03 \text{ g m}^{-3}$.

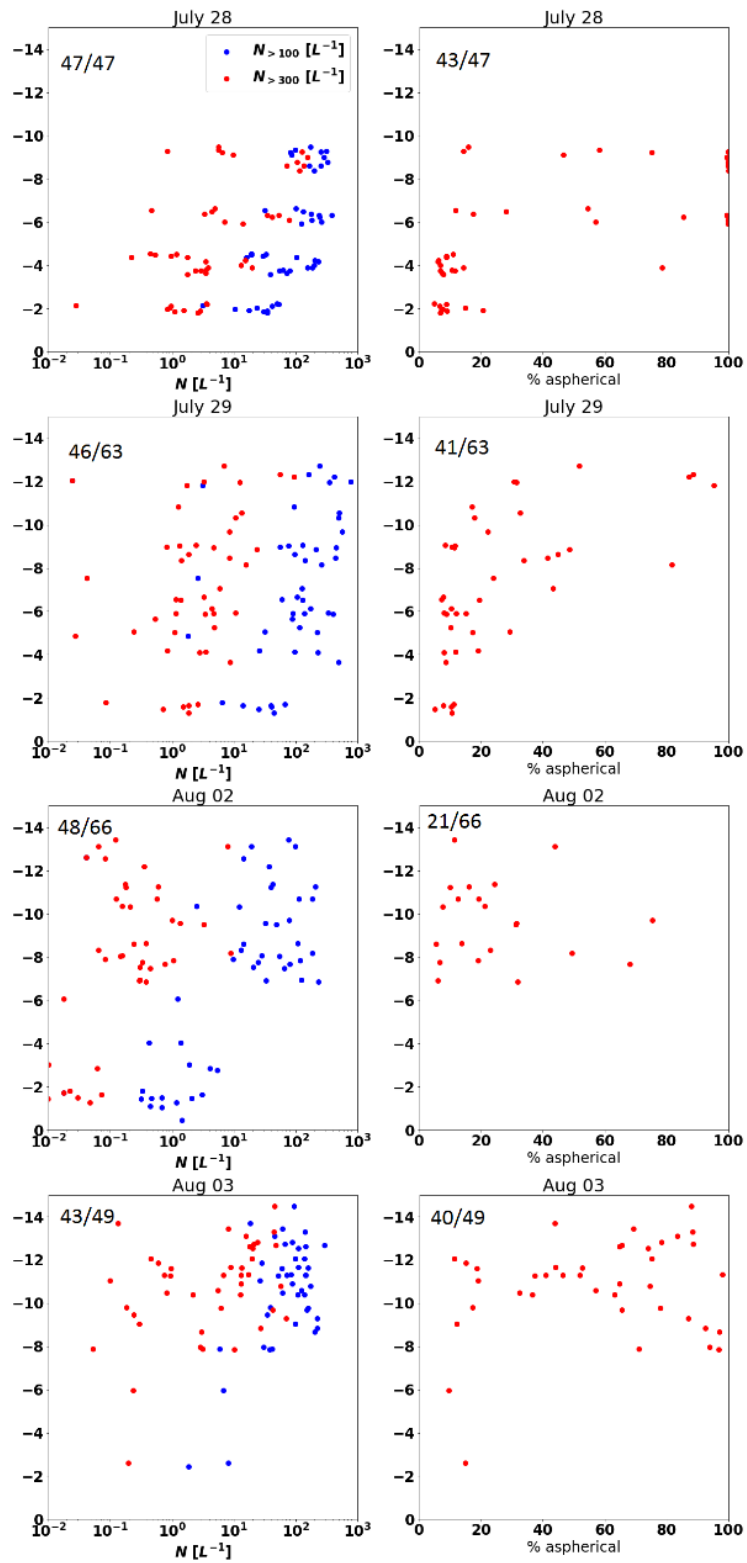


Figure R2. As Figure R1, but defining a penetration using a LWC > 0.05 g m⁻³ threshold.

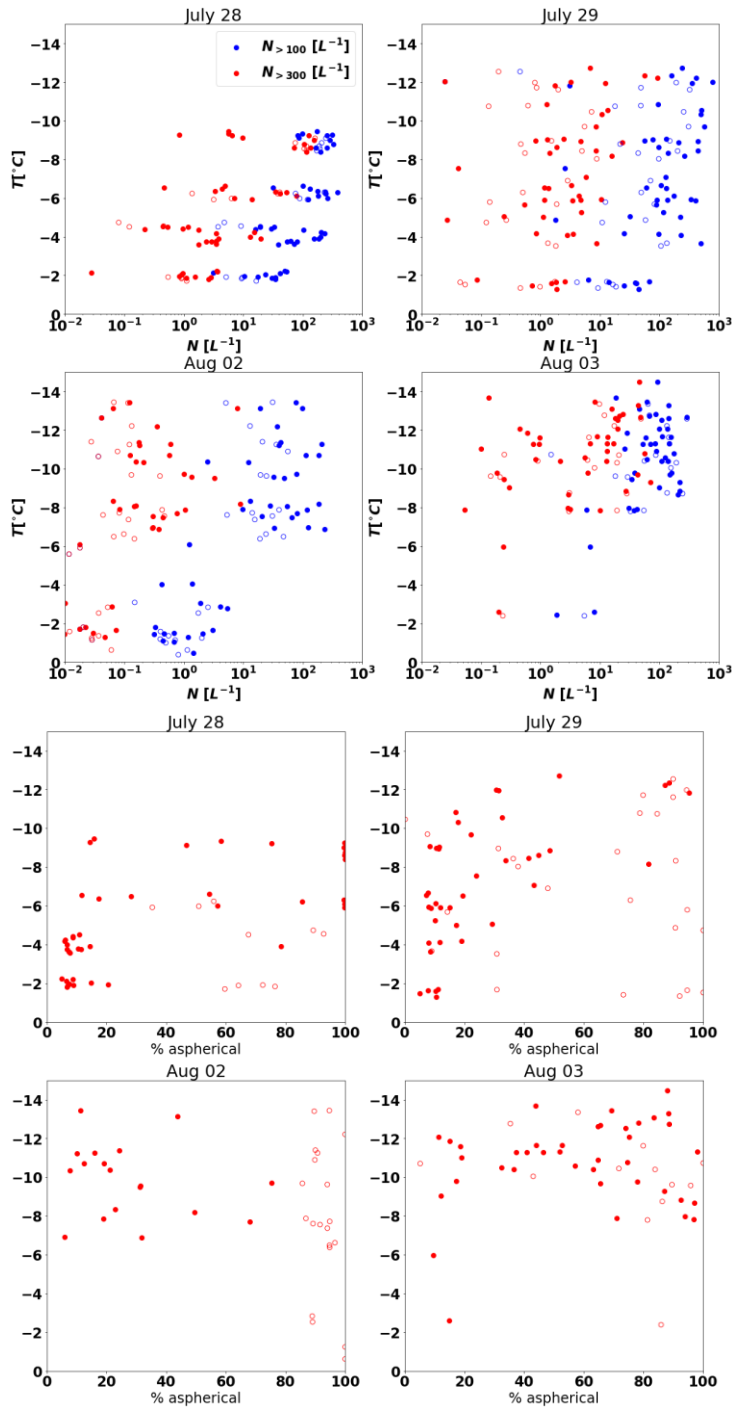


Figure R3. As Figure R1, but with a minimum penetration (updraft) length of 300 m.

P8 line 30: this is a surprising result given the underlying hypothesis that secondary production is active and linked to processes in the updraft rather than the anvil regions. Is this lack of difference between

the updraft and non-updraft region supported by the other aircraft observations? For the discussion - what do models show when segregated like this?

Reviewer 1 had a similar comment, below is our response to reviewer 1. It should also be noted that our sampling strategy focused on turrets as they were just passing through the UWKA level, so those devoid of updrafts were not anvil regions, but rather likely turrets that had just begun transitioning.

Our original analysis focused on just updraft regions. As we expanded our analyses to include cloud penetrations with 'no updraft' (actually updraft less than a 1 m/s threshold), we found no significant difference in our results. After much discussion, we attribute this to our sampling strategy. Since we targeted turrets as they first ascended to and just above the level of the aircraft, every penetration was a 'fresh turret'. All had hard, well-defined edges and none of the penetrations included anvil regions or clouds in their decaying stage. Because these turrets often extended above their equilibrium level, one may expect a rapid transition between a turret with an updraft and one whose updraft had weakened significantly over just a few minutes. It appears the microphysical characteristics of these two types of turrets were quite similar. This discussion is added to the paragraph in section 3b in the revised manuscript.

P9 line 1 – do you have plots of the penetration lengths as a function of T for the different days? Perhaps these figures would be improved by including some measure of the variability along the penetration. Could add 25th, 75th percentiles for example.

Figure R4 shows the length of each penetration. The penetrations lengths generally varied between 0.2 km to 2 km, and wording has been added to this paragraph to state the overall lengths of each penetration:

“The penetrations ranged from 0.2 km to 2 km in length.”

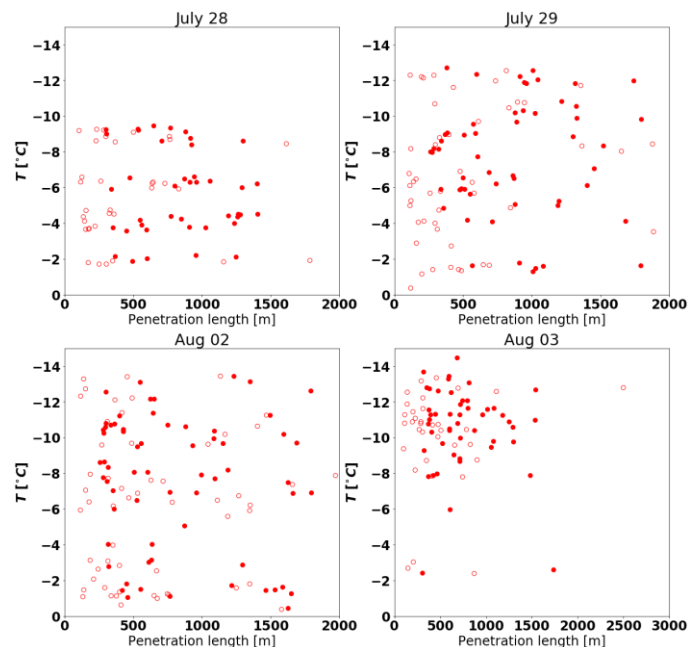


Figure R4. The length of the penetration (solid circles) or updrafts (hollow circles) as a function of temperature.

Regarding the inclusion of quartiles of these penetration plots; An early version of this manuscript had included just such information, however, we found that it was difficult to create a figure that was easily readable with this information included. Further, it did not alter the outcome of the analysis and therefore, in the end, we decided to only include the medians.

P9 line 1 – what do the droplet concentrations look like from CDP or something similar? Do they show a difference in and out of the updrafts?

The range of CDP concentrations for all of the penetrations: 98 cm⁻³ on July 28, 75 cm⁻³ on July 29, 175 cm⁻³ on Aug 02 and 96 cm⁻³ on Aug 03. We have added these values to Table 1 and modified the wording to say that there is no systematic difference between the ice concentrations and habits between updrafts and penetrations.

P9 line 6 – are growing turrets the penetrations with updrafts?

We attempted to sample *only* growing turrets. Turrets that were obviously collapsing and/or did not have sharp, well-defined edges were not sampled. We expect those that were still actively rising at the time of penetration were the ones that contained updrafts. Those that did not contain updrafts had likely lost all of their buoyancy at the time of penetration.

P9 line 10– what was the strategy for sampling clouds with the UWKA? Was it the same on all days? Was the sampling strategy for the BAe146 the same? The difference in results suggests that it would be good to combine datasets from both aircraft to provide C2 a fuller picture of the cloud characteristics.

We provide the answer to this in our first response above. Yes, we did seek to sample the clouds with the same strategy on all four days and it did differ from the strategy employed by the -146. The first paragraph of section 3b in the revised manuscript contains discussion of the sampling strategy.

P9 line 35 – figure 5 is from the updraft penetrations. Have you got the same plot for the non-updraft penetrations?

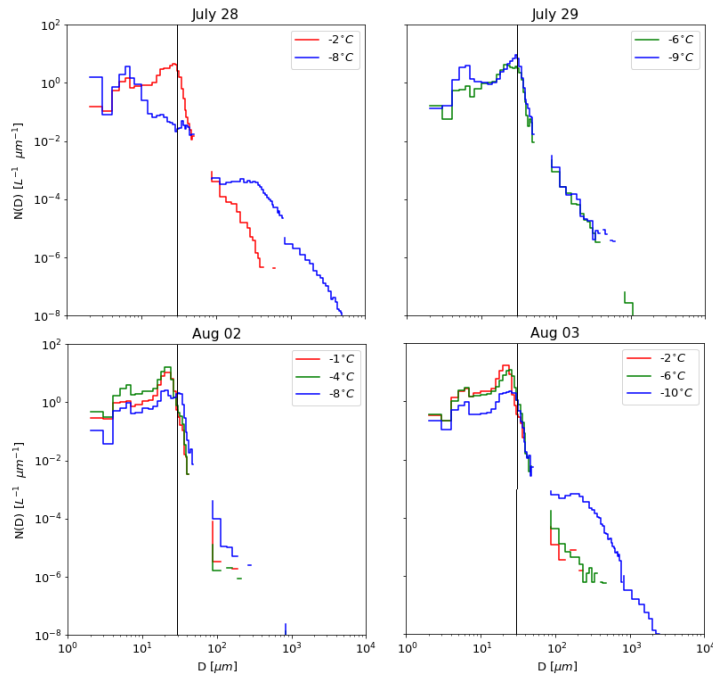


Figure R5. As Fig. 5 in the manuscript, but for penetrations without updrafts.

Fig R5 is the same as Figure 5 in the manuscript except for the penetrations without updrafts. For the precipitation size particles, we see similar trends compared to the analysis in the manuscript, except with fewer precipitation particles on 29 July outside of the updrafts. However, for the cloud droplets, we do generally see reduced number concentrations all days except July 29 as we approach colder temperatures, as presumably without an updraft the precipitation particles would act to deplete the cloud droplets via accretion and secondary ice production.

P10 line 2- agreed that there are twice as many droplets near cloud base and that would lead to smaller droplets for the same liquid water content – but it will only be 20% smaller. For 2 Aug, the cloud base is also warmer suggesting that more liquid water would be available that could offset the effect of increased droplet numbers. . .

The cloud bases between the days did not differ by more than 4 degrees Celsius. The modelling of the 02 August case by Lasher-Trapp et al. (2018) is consistent with the notion that the increased droplet numbers provided narrower droplet size distributions that led to the inhibition of the warm rain process.

P10 line 1 – some mention of the 30 micron threshold here, but not its importance to the Hallett-Mossop process as suggested in the caption to figure 5. There should be some more discussion about this here or earlier in the paper.

The Hallett-Mossop threshold is 24 μm. In Figure 5, this vertical line is meant to provide a reference diameter for the reader to easily compare distribution modes between the 4 days. We do not make an assertion that there are more (or fewer) droplets that have achieved this threshold diameter and

therefore act as the principal controlling factor for the onset of H-M. Rather we assert that the lack of warm-rain production on 02 Aug leads to a dearth of drops that can freeze and become ‘instant rimers’. In the revised manuscript we remove the reference to ‘minimum size water drops needed for the Hallett-Mossop process to occur.’ from the caption in Figure 5.

P10 line 12 – Fig 4 2d imagery suggests that July 29th when secondary production was thought to be less effective also has large rimed particles present. . .

The 2D images in Figure 4 are *examples* of particles from the penetrations, but not necessarily every penetration has particles with those habits. The aspherical percentage on July 29 at temperatures from -3 to -8 C (and even lower temperatures) is significantly less than on July 28 and August 03, suggesting that ice production at those levels (ie secondary production) is less effective on that day.

P10 line 20. If invoking the H-M process then I think you also need to comment on the conditions that are felt necessary for it to be active (e.g. p10 line 1 comment). Beside the temperature range there are other parameters such as the range of liquid droplet sizes present and accretion rate that could also be explored to understand if conditions satisfy what was observed in the laboratory. Additionally, to be useful to modellers some estimates of the splintering rate as a function of temperature, accretion rate etc would be a useful step.

We agree with the reviewer that a more quantitative analysis of the splintering production rate and accretion rate of the Hallett-Mossop process would strengthen the paper. However, there are two quantities required by the calculation of the measured and predicted splinter production rate from Harris-Hobbs and Cooper (1987) that are difficult to quantify with our dataset. One is the number concentration of columns of sizes 87 to 140 μm that are needed to calculate the measured splintering rates. The other is the number concentration of graupel particles that is required to calculate the predicted splintering rates according to the laboratory studies. There are large uncertainties in the measurements taken from CIP and 2DP probes for both such measurements, so therefore it is important to check the sensitivity of such process rate calculations to the measured concentrations of columns and graupel.

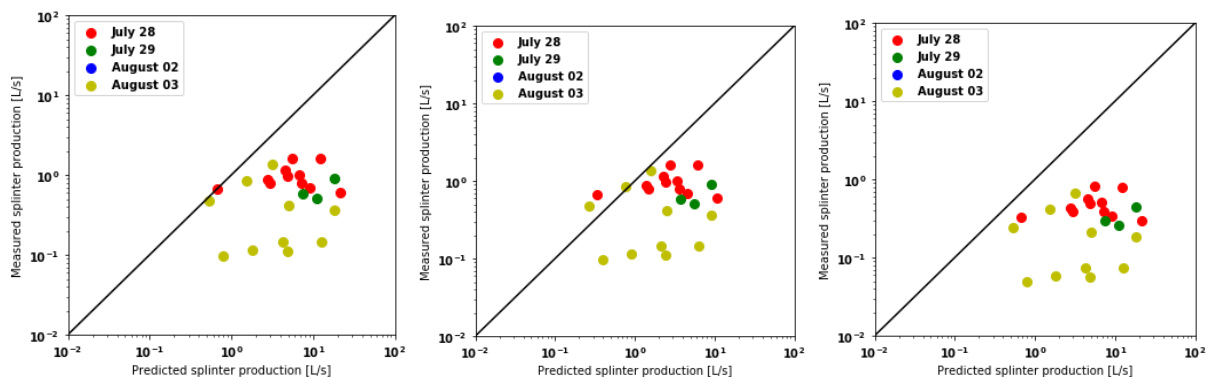


Figure R6. The splinter production rate measured versus those rates predicted using the methodology from Harris-Hobbs and Cooper (1987) assuming that all particles with D of 100 to 140 μm are columns (left) and all particles with D > 800 μm are graupel. (middle) as (left) but assuming only half of the particles with D > 800 μm are graupel. (right) as (left) but assuming that only half of the particles with D from with D of 87 to 140 μm are columns. Only penetrations with aspherical percentages > 80 percent are included to ensure that most of the precipitation is ice.

Figure R6 shows the calculated measured process rates compared against predicted process rates in the penetrations. It shows that the conclusions drawn can be very sensitive to the number concentration of both columns and graupel. For example, in the middle plot one can see that, for many of the penetrations on July 28, the process rates are within an order of magnitude of each other but this ceases to be the case for the other two plots. Therefore, with this level of uncertainty, it is nearly impossible to quantify whether the observed splinter production rates agreed with those predicted from the laboratory studies and we therefore chose not to include these in the paper.

P10 line 19. To be pedantic, the role of primary nucleation has not been ruled out. There was no ice nucleation information available, but there needs to be some discussion about the fact that these concentration likely outstrip the primary production rates. Perhaps using DeMott 2010 and tying that to observed large aerosol in the boundary layer is a means to estimating a bound for the primary ice nucleated particles. I see that this discussion occurs in section 4 but it might be good to combine this discussion with the comments about primary ice concentrations.

The original manuscript included text in the discussion demonstrating that the measured ice crystal concentrations were orders of magnitude higher than those that would be predicted by DeMott et al. (2010) {Paragraph 1, section 4 original manuscript}. In the revised manuscript we modified that to include a reference to the analysis from Taylor et al. (2016a)'s paper on the observed aerosol concentrations during COPE. They predicted that the concentrations of INP using the DeMott et al. (2010) parameterizations applied to measured boundary layer aerosol concentrations that the number of INP would range from 0.1 to 10 L^{-1} . Therefore, this shows that secondary ice production mechanisms must be occurring.

References:

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