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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observations of the microphysical evolution of convective clouds in southwest United Kingdom" *by* Robert Jackson et al.

Anonymous Referee #2

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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

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. 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-chem-phys.net/16/10159/2016/).

Specific comments: 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.

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?

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.

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?

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

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 a fuller picture of the cloud characteristics.

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

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 by 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. . .

P10 line 1 – some mention of the 30micron 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.

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. . .

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.

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.

P10 line 22. Why can't the ice be carried up from the H-M zone to the colder temperatures?

P10 line 22-24. I think this is speculation that should be moved to the discussion.

P11 line 22. Concentrations – it would be good to quote the spatial scale over which this is appropriate to help with comparison to models.

P12 line 14. Can you comment on the requirement for smaller droplets alongside millimetre size droplets to allow H-M to proceed?

P12 in cloud temperature measurements are difficult. It might be worth commenting on this for situations where there are strong updrafts and latent heating occurring.

Conclusions. Between 1) and 2), I think it would be good to add a statement that primary ice concentrations based on DeMott 2010 and aerosol (D>0.5micron) measurements (need to add this analysis into results) are much lower than observed ice concentrations and therefore it appears that a secondary ice production mechanism was active. Conclusion 3. As mentioned earlier, the H-M process has a set of conditions defined from laboratory work that is more extensive than just the temperature range. Please could you assess whether all of the tests are passed? This would strengthen the assertion and/or motivate further laboratory work. Conclusion 5. I do not necessarily agree with this – see my comment above about the contending effect of a lower cloudbase. I think you can speculate about the effect of droplet number in the discussion, but I don't think it can be a robust conclusion.

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