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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observations of cloud microphysics and ice formation during COPE" *by* J. W. Taylor et al.

Anonymous Referee #2

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Review of "Observations of cloud microphysics and ice formation during COPE" by Taylor et al.

Recommendation: Requires revision before acceptance in ACP.

This study examines data collected in two lines of cumulus clouds over the Southwest Penninsula of the UK acquired during COPE. Sampling was performed first along a line of closely packed cells, followed by repeated penetrations through an isolated cell as it grew and became glaciated. The evolution of the observed cloud and aerosol properties is explained in terms of the action of the Hallett-Mossop process. The continued passes through the developing cumulus cell are especially unique, and hence should indeed be published. However, I find that some of the writing in the paper is not pre-

cise, overly speculative, and not totally justified by the presented data. In particular, the authors state that the observations show that the H-M process was initiated from the recirculated ice, with ice splinters causing the drizzle drops to freeze on contact, forming additional instant-rimers. Although it could be argued that the results presented in the paper are consistent with this explanation, there could also be other mechanisms acting that could also have explained the observations. Thus, I recommend that the writing in the paper be carefully examined to note what trends are consistent with the H-M process, rather than stating that the H-M process explains the results. There are also a number of locations in the paper where the writing could be made more precise and quantitative, with the numbers of speculative comments reduced. This is further explained below in the context of some passages of the manuscript.

A second major point relates to questions about some of the microphysical analysis that is presented in the paper. It seems that there is an overreliance on the images from the 2DC/CIP for identification of the phases of the particles, and that other information should be additional considered. The definitions of circularity and roundness are important for many of the results that are presented in the paper. With this in mind, I think that the definition of circularity should be stated in the manuscript. Also, how sensitive are your results to the definitions of low, medium and high irregularity and roundness? If a minor adjustment is made in the threshold to change these classifications are your results significantly impacted? More importantly, I am surprised that a lot of the shape analysis in the manuscript is based on the 2DS images rather than the CPI images which give more detailed pictures and would allow the riming to be much more easily determined. I also have difficulty in understanding why particles that occurred on the edges of the photodiode array would be any different than those occurring in other portions of the photodiode array: you are sampling the same population of particles so why would they be any less likely to be mixed-phase? No size distributions are presented in the manuscript. How well do the 2DS and CIP distributions agree? How well does the CDP distribution agree with that of the 2DS in the overlap range? Answering these questions would help justify the robustness of the data. And, finally, I

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have some questions about the phase identification analysis. It would seem that there would be some additional information that might help better determine the phase, especially given the out of focus 2DS images that are used in some of the analysis. For example, was the shape of the CDP distributions examined? They tend to be more peaked in liquid clouds and flatter in ice clouds. Other questions include the following: was the H or V channel of the 2DS used, or some combination? Were depth of field corrections applied for the small particles? It is stated that varying the inter-arrival time threshold had little impact on the derived concentrations and that a constant threshold was used: although this would be problematic in conditions of large or heavy ice, this seems reasonable for the conditions sampled. Nevertheless, it would be nice to know quantitatively what "little" effect means: 10% or a factor of 2? And, is there any way to be more quantitative about the amount of riming that is occurring?

I find the naming of the runs (e.g., 10.3.1, and 11.1) rather strange. Can the runs simply be designated by their times, or is there something more significant about these naming conventions that is used. If they are named by time or altitude, it would help give me a perspective of when/where they were. I think some of the figures could be better presented to give a better perspective of where the data were obtained to help interpret the patterns within them. For example, in Figure 5 can the concentration plots be on the left hand side of the plot and the vertical velocities on the right hand side of the plot? Then the plots could be sorted vertically by altitude giving a better graphical manner to interpret the variations between the runs.

Abstract: I think the abstract should be considerably tightened and made more quantitative, stating what the observations were with a minimal focus on speculative comments explaining the observations. In addition, there is too much introductory material in the abstract that should be removed (e.g., the first two sentences are fine for an introduction, but not needed in the abstract). That would allow extra space so that it could be explicitly stated what conditions were present to justify statement that conditions of H-M process were met. In the third paragraph, the statement of "a few drizzle

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drops" should be made more quantitative. Also, it could be noted that graupel formed after the drizzle drops, but the freezing of the drizzle to form graupel was not explicitly observed. In the fourth paragraph it is stated that ice splinters were captured by supercooled drizzle drops causing them to freeze: but, again this was not a process that was observed so more quantitative comments about what was actually observed shoud be noted. In the fifth paragraph, can you stated what quantitatively a "majority of precipitation-sized particles" means. The second to last sentence in the abstract is consistent with the observations, but is not necessarily the only explanation for the observations and hence this statement should be reworded.

Page 16054, line 10: Why was this particular case (3 August) chosen for analysis? A couple of sentences of explanation should be given.

Page 16060, line 11: Was there any evolution of the altitude of the cloud base during the height?

Page 16061, line 3-4: Could there have been any possibility of APIPs from the King Air that could have generated ice, ultimately affecting the measurements from the BaE-146?

Page 16062, line 9: What do you mean by three runs? I only see a single line in Figure 3 so I am wondering how you define a run.

Page 16062, line 10: There were several times in the paper I was wanting to see a vertical profile of the wind. Could you show one?

Page 16062, first paragraph in Section 3.2: Is it possible to also include a plot of the liquid fraction? I think that would be very useful for many of the analysis fields that you presented. Are the components of ice included or not included in the calculation of effective radius? It would seem important to exclude them for calculating the effective radius.

Page 16062, line 26 (also several other points in paper): I have trouble seeing where

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you are getting the information about the spatial scale of the vertical motion from. Was some sort of FFT analysis applied? Or what else was done to determine the spatial scale of the vertical motion?

Page 16063, line 11: Could look at shape of CDP to verify that the cloud was almost exclusively composed of liquid drops?

Page 16063, line 15: It would be interesting to show the size distributions to better visualize these comments on the contributions of drops with different diameters.

Page 16064, paragraph on Region II: I'm not convinced that I see evidence of liquid water from the images that are presented for Region II. It is claimed that this is a more mature cloud because of the presence of the ice crystals. Is there some way that is more objective that can be used to estimate the age of the cloud to make the analysis more objective?

Page 16064, line 13: Are some of the donut type crystals seen in region III assumed to be liquid particles? Out of focus ice particles can appear as round donuts in 2DS/CIP images, so it is not necessary that these particles are liquid.

Page 16064, line 17: See my comments on microphysics analysis. How well do you really know what the concentrations of graupel or round particles are? What is their uncertainty baed on the shape analysis?

Page 16064, discussion of Region III: Inevitably, you may be seeing some differences in the regions from what part of the cell was penetrated (edges or cores). This is certainly acknowledged in the text, but I am left wondering to what degree some of your conclusions can be affected by these differences.

Page 16065, line 8: I would recommend removing the last sentence of this paragraph unless something more concrete can be said rather than the speculative statement.

Page 16065, line 11: Is it differences in ages, or do what extend could some of these differences be associated with where in the cells the penetrations are made? Do you

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see any gradual maturation of the cells on the radar that can be quantified?

Page 16065, line 25-26: I think it would be more fair to say that your results are consistent with the action of the H-M process: it does not really infer that this process is going on.

Page 16066, line 3: Where does this 4 km come from? It seems to be more than 4 km away from the other cells on this figure.

Page 16066, line 14: can you quantify what you mean by relatively low?

Page 16066, line 16: Are these numbers correct? The 0.5 mˆ-3 of 1 mm drops seems a little high for this reflectivity.

Page 16067, line 17: Can you say something more quantitative than young clouds?

Page 16068, line 10: I find this hard to justify with presented data because so many of the images are out of focus.

Page 16068, line 18: Can something more quantitative be stated to show that the downwind updraft region was more turbulent?

Page 16069, line 10 and after: Can you refer to which particular figure you are seeing this information about the particle characteristics? I'm not convinced that you really have the resolution in the particle images to observe this. So many particles on the CIP/2DS don't really have sufficient resolution. Can't you be using the CPI and showing specific images to show that this is occurring?

Page 16069. line 22: What is the basis of saying that the downwind side is more turbulent?

Page 16070, line 8: You can say frozen drops but not recently frozen drops. There is nothing in the observations that says the time at which the drops were frozen.

Page 16070, line 18: Remember that you are not necessarily sampling the exact same

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locations in the cloud. Plus with the evolution and movement of particles, you can't necessarily equate one part of the cloud to the other. Thus, some of the discussion should be adjusted accordingly.

Page 16071, line 17: reword "following cloud upwind". I think I know what you mean, but this is worded awkwardly.

Page 16072, paragraph beginning line 3: A lot of the comments in this paragraph are overly speculative (suggesting, likely, may have, etc.). I think it would be better to say the data are consistent with these processes. Are there any more processes that the data are also consistent with?

Page 16072, line 23: Emphasize that the results are consistent with some processes, state if there are any other processes that should also explain the results, and note that there really is nothing that proves what is stated.

Page 16073, Section 4.1: Is this section really needed? I think the earlier reference in the paper to the 2004 floods should be sufficient.

Page 16074, line 13: It would be nice to show the wind gradient or vertical shear in the paper somewhere.

Page 16076, line 6: It should be emphasized that it takes time for any ice crystal produced at cloud top to fall to the measurement level, so the approach discussed here should provide a maximum estimate of the ice crystal concentration.

Page 16077, line 11: Can you be more quantitative rather than use words such as minimal?

Page 16078, line 2: I would argue that this is a possible explanation or that the results are consistent with this explanation.

Page 16078, line 3: classified rather than classed

Page 16078, line 13: Can there be some quantitative analysis presented to justify that

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ice in the downdraft became more rimed.

Page 16078, line 21: It is not clear these particles were generated by the H-M process, but the results are consistent with their generation by the H-M process.

Page 16079, line 8: Remove word “recently”

Page 16808, line 2: I think you determined that the H-M process was consistent with the observations, not necessarily that it was responsible for generating the ice crystal concentrations up to several hundred per liter.

Page 16084, lines 5-12: I don’t think this paragraph is needed. I agree that it would be a good subject for a subsequent paper.

Table 2: Is there some significance to the nomenclature that is used to label the runs?

Figure 1: Can you also show quartiles of the distributions on the plots as well as the individual points in order to give a better idea on what are the trends with respect to altitude?

Figure 3: Shouldn’t the upper panel be labeled a, and the other panels subsequently reordered?

Figure 4: Should there be a vertical wind field for panel (b) for the first penetration?

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