Four days' worth of microphysical and dynamical data area analyzed from the COnvective Precipitation Experiment (COPE) over Southwest England. The cases are chosen in a spectrum from one almost entirely glaciated by -10°C to one still primarily liquid at -13°C. The dynamic conditions also vary from low shear-low CAPE to high shear-high CAPE. Although the analysis and insight are not as deep as in the Taylor et al. COPE study, the work adds to a body of in-situ secondary ice production observations, and the discussion of ice recycling in different dynamic environments is interesting.

While the cases present a nice variety, I am wondering whether it would ease readability to refer to them with acronyms or names as opposed to dates. Table 1 is useful in this regard, but I did find myself flipping back and forth quite often to recall which case was which.
A strength of the work is that it considers different dynamic environments, but I wonder if this could not be made more quantitative. Could shear profiles / hodographs or CAPE evolution (The values in Table 1 are spatiotemporally averaged?) be included? This would make the discussion in Section 3a more rigorous. My other questions and comments are associated with specific lines or figures:

We thank the reviewer for their constructive comments. We share the reviewer's concern that the presentation of the four cases can be quite overwhelming especially given the wide variety of information. We tried different methodologies and acronyms, but all seemed to be rather confusing. The most logical way we found to label these cases for the reader would be to classify them as Cases A to D. In the revised manuscript, we refer to these alpha-numeric codings (introduced at the beginning of section 3) throughout sections 3 and 4. However, in the conclusions section we present the cases by their date.

Regarding the reviewer's comment on quantification of shear profiles and/or CAPE evolution, we are unable to provide an evolution of CAPE for the following reasons below.

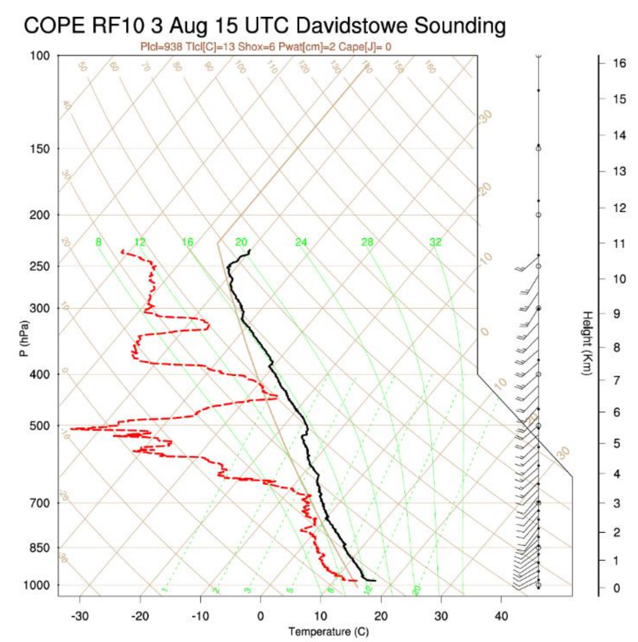

Figure R1. Skew-T diagram of the sounding launch at 15 UTC 03 August. The black line denotes temperature, the red dashed line is dewpoint. The pink dashed line shows the temperature of a parcel lifted from the surface.

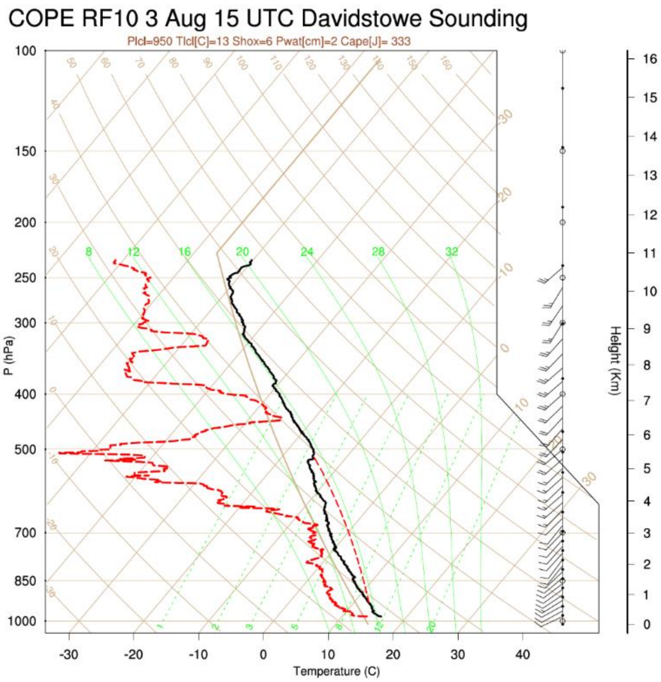

Figure R2. As Figure R1, but after the surface temperature is cooled to 16 degrees Celsius.

When calculating CAPE we carefully selected soundings that were closest to the aircraft penetrations in time. We also found that when we calculated CAPE there is known to be a large sensitivity to both the surface temperature used as well as due to the lifting due to the convergence line (Browning et al. 2007). We had to carefully check whether the calculated thermodynamic profiles were representative of the observed cloud bases and tops on those days. Figure R1 and R2 show an example of this sensitivity. Therefore, we decided to calculate CAPE based on the observed cloud bases and tops throughout the day. Using this process is impossible without detailed information about how cloud tops vary throughout the day to help verify the validity of the CAPE calculation. Therefore, we are unable to provide an evolution of CAPE throughout the day.

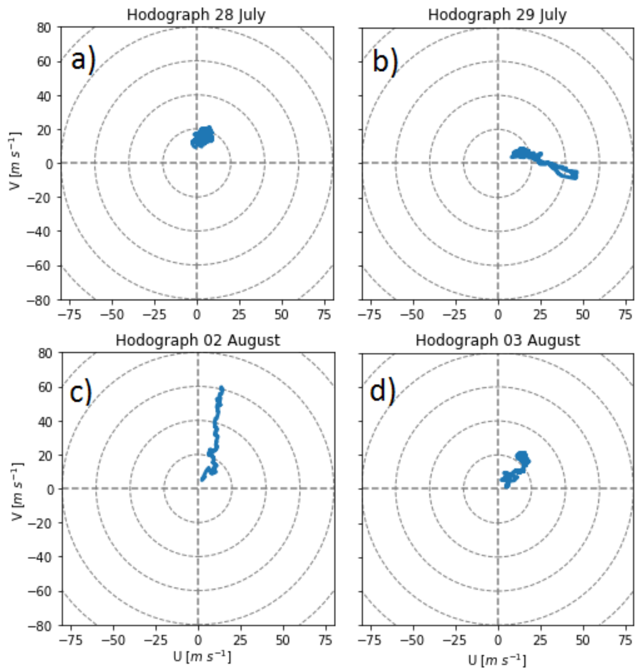

Figure R3. Hodographs for the 4 cases.

The hodographs shown for the 4 cases in Figure R3 does provide a qualitative visual confirmation of calculations of shear provided in Section 3a. However, no real additional information is gained. We see unidirectional shear on all of the days, and the shear is the greatest on 02 August (Case C), the least on 28 July (Case A). Furthermore, we feel that the most representative calculation of shear for these cases is the shear from cloud base to cloud top, as that is what will impact the cloud dynamics and in turn the microphysics. Therefore, we feel that we have, to the best of our ability, provided the most quantitative assessment of the thermodynamic and dynamic profiles on the 4 days that we are able to provide based on the data set available.

Specific comments

Particle numbers in different size range

Page 5, Lines 10, 30-31; Page 6, Line 12 – Could you clarify where particle number concentrations in the different size ranges come from? “CDP sampled particles with diameter 2 < D < 50 um”; “Concentrations of particles of 25 < D < 100 um from the CIP are not reported in this study”; “Images …with diameters less than roughly 250 um are not included in the ice categorization”. Where do the number concentrations between 50 and 125 um come from in Figure 5? Are the authors concerned that with a cutoff size of 250 um for ice that some fragments are not accounted for?

To clarify – the CDP measures particles with 2 < D < 50 um, the CIP nominally measures particles with 25 < D < 1600 um, and the 2DP nominally measures particles with 200 < D < 6.4 mm. In our processing of the data, since the concentrations from the CIP are unreliable at D < 100 µm, we removed any particles in the CIP data that had D < 100 um. For regions of overlap between the CIP and 2DP, we used CIP measurements for D<800 um and 2DP measurements for D>800 um. The ‘gap’ in the spectra in Figure 5, accounts for particles with 50 < D < 100 um. We have added a clarifying clause in the last paragraph of page 5 to clarify to the reader what size ranges are associated with which probes:

“To account for regions of diameter overlap between probes, and to remove significant uncertainty associated with poorly resolved particles from OAPs, for the remainder of the manuscript, concentrations of particles with 2 < D < 50 μm are reported from the CDP, 100 < D < 800 μm from the CIP, and 800 μm < D < 6400 μm from the 2DP.”

Yes, since we do not factor in particles of less than 100 square pixels in our habit analysis, we are concerned that some of the smallest ice fragments are not accounted for. However, we cannot provide a reliable estimate of how many small ice fragments are being excluded from the analysis, as it is nearly impossible to reliably determine the shape of particles from the CIP that are less than 100 square pixels in area. However, any ice fragments that are produced are expected to rapidly grow in regions of ice supersaturation and significant cloud liquid water. Regardless, we acknowledge being unable to estimate the number of smaller crystals in our analysis with the addition of the following text in the last paragraph of section 2b:

“...corresponds to hydrometeors with diameters less than roughly 250 μm. While this threshold excludes some small ice fragments, it is impossible to provide a reliable estimate of how many fragments are excluded.”

Droplet size distribution broadening

Page 6, Lines 30-31; Page 10, Line 2 – I would make more explicit which droplet number concentrations were anomalously high and which were anomalously low on 02 August… for example stating on Page 6 that concentration of drops *with diameter greater than 30 um* “were orders of magnitude less.” And on page 10 that there is a “larger cloud droplet number concentration” *of less than 30 um diameter.* Alternatively, the analysis concerning droplet SDs could be reworded in terms of the size distribution *broadening* or *narrowing* as in Lawson et al. 2017 *JAS*.

On page 6 (second paragraph in Section 3), the discussion is focused on *precipitating drops* (i.e D > ~500 um). We clarify this in the revised manuscript stating:

“…concentrations of drops with *D* ~ 500 um and greater were orders of magnitude less during Case C than on the other three days.”

On page 10 (second to last paragraph in section 3b, beginning with “Figure 5 shows…”), at the beginning the focus of the discussion is on *cloud droplets.* Here we use a threshold of 30 um, because droplets larger than this will likely begin to grow more effectively through collision-coalescence. The following sentence connects that a higher concentration of these drops will result in a smaller median diameter and narrower droplet spectrum:

“…there is a larger concentration of cloud droplets with *D* < 30 μm in Case C. This is likely a due to the larger cloud droplet number concentration observed at cloud base in Case C (Table 1) and is consistent with a slower collision-coalescence process as expected given the smaller median droplet diameter, narrower droplet spectra, …”

Updraft dependence

Page 8, Lines 30-32 – I was rather surprised that neither the size-segregated hydrometeor concentrations nor the aspherical percentages had “any systematic difference … between observations obtained from cloud penetrations without updrafts.” I think more discussion is warranted here because many studies have noted an influence of updraft to secondary ice production rates, e.g. Mossop 1976 *QJRMS*, Heymsfield and Willis 2014 *JAS*, Lawson et al. 2015 *JAS* among others. And one might expect, given the highest percentage of strong updrafts on 02 August, that droplet shattering would be facilitated with a lofting of even larger droplets to high altitudes.

That is not the case here, presumably because the aerosol loading is higher. But I do think this should be stately explicitly somewhere in the analysis.

Quite honestly, we too were rather surprised by this finding. 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 signficantly 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.

Figure 5 – Is there a reason that the particle size distributions are not shown for the same temperatures for all cases? Clarifying which particle numbers are shown in which size ranges, the *N(D)* before the "break" in the spectra around 80 um, are exclusively droplet numbers, right? (If not, I am surprised that spectra at different temperatures overlap between 30 and 50 um.)

We showed size distributions at slightly different temperatures in Figure 5 because there was not always a penetration at the same temperature for each flight that was representative of the overall statistical analysis. We do not show concentrations of particles in the "break" (100 um in the revised manuscript as per the discussion earlier) as we do not have reliable measurements of the concentrations of particles between 50 and 100 um in diameter.

Section 3c – This is a nice comparison, but it would flow more logically to me if the fifth paragraph ("The two penetrations considered here…") came second (after "The observations demonstrate… few graupel particles"). This is the motivation of the comparison and the other paragraphs explain the differences.

We have rearranged section 3c according to this suggestion.

Page 12, Lines 6-8 – This sentence is unwieldy. Could you say "*Variation in the spatiotemporal distribution of ice and precipitation production for these CAPE cases is likely due to a variety of ice production mechanisms.*"?

We have replaced the sentence with your suggestion, except that we used "COPE" in place of "CAPE."

Figure 8 – This figure is not mentioned in the text. Some explanation should be incorporated, or it should be removed.

Figure 8 is referred to in the text in line 14 of page 11.

References:

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