

Review of “Observations of the microphysical evolution of convective clouds in southwest United Kingdom”

Four days’ worth of microphysical and dynamical data area analyzed from the COncvective 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:

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 $2 < D < 50 \mu\text{m}$ ”; “Concentrations of particles of $25 < D < 100 \mu\text{m}$ from the CIP are not reported in this study”; “Images ...with diameters less than roughly $250 \mu\text{m}$ are not included in the ice categorization”. Where do the number concentrations between 50 and $125 \mu\text{m}$ come from in Figure 5? Are the authors concerned that with a cutoff size of $250 \mu\text{m}$ for ice that some fragments are not accounted for?

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 $30 \mu\text{m}$* “were orders of magnitude less.” And on page 10 that there is a “larger cloud droplet number concentration” *of less than $30 \mu\text{m}$ 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.

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 stated explicitly somewhere in the analysis.

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 μm , are exclusively droplet numbers, right? (If not, I am surprised that spectra at different temperatures overlap between 30 and 50 μm .)

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.

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.*”?

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