

Interactive
Comment

***Interactive comment on* “Observations of ice multiplication in a weakly convective cell embedded in supercooled mid-level stratus” by J. Crosier et al.**

Anonymous Referee #2

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This paper presents observations which constitute another demonstration of the workings of the Hallett-Mossop ice-splintering process. Similar evidence has appeared in a handful of other papers. Because that number is relatively small, there is value in augmenting it. This is not one of the strongest of the cases but useful nonetheless.

The major merit of this case is its origin in forced convection along a front allowing the situation to be considered steady-state. The RHI radar sections in Figs 5-8 justify this description; the authors were for some reason taking this for granted or shied away from this interpretation so that the paper doesn't explicitly make this point but refers to 'local convection' and other vague terms. To judge how valid a steady state

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interpretation may be there should be a clear spatial (drifting) reference established and data presented in relation to that.

The most important data that is missing is in situ vertical air velocity. Probably because of this, the ice particle observations are presented in statistical form over several kilometers. No clear relationship can be ascertained between the locations of various ice crystal forms and whether they are being transported upwards or are moving downwards. Observations of small crystals at lower altitudes and larger ones higher up is reasonable within an updraft. This evidence is missing. Also, the question may be asked where do all those small crystals end up. Not all of them grow? Was the outflow from the convection diagnosed? This too is related, of course, to the view of the system as steady state, or not. Lack of clarity on this aspect leads to unease about the interpretations.

Specific points (page/line as reference).

It would be helpful to show clearly the relative locations of the flight track, the satellite image and the time series in Figs. 5-8. As it is, conversions are needed from lat/lon to radial distance along an oblique line.

19383/14: not all coalescence is between supercooled drops

19384/11: “ ... majority of ice in convective clouds ...” is too general

19384/17: “ ... may provide ...” in place of “ ... can provide ..”

19387/1: The section includes radar data as well, not just meteorological conditions.

19388/18: warm clouds are not “seeded” by ice crystals in the usual sense

19388/21-24: Why mention this event many hours earlier? What is the evidence for rimed particles and graupel?

19389/25-27: This is confusing: 2 m/s correction is 10% of the vertical velocity? Suspect that it should be horizontal velocity.

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19392/18: The relative locations of these data segments (distance) are more relevant than the length of the data segment. Were these data collected in updrafts?

19392/27: This reference implies a lack of small-scale organization in spite of the steady updrafts depicted. Is there a conflict between this feature and the assumption of vertical continuity?

19393/13- 19394/14: The aerosol data are fairly marginal to the main theme of the paper.

19394/24: what is meant by 'activation' here?

19395/21: Can the term "in-efficient" be better defined?

19397/11: Steady state is assumed here. Does this conflict with 19392/27?

19398/3: The crucial thing is the concentration of droplet >24 μm in conjunction with graupel. Is this reflected well enough by reference to the mean concentration? Mean values were taken over what flight segment?

19398/30: Is there any evidence backing up the suggestion that the crystal type on which riming is occurring has any impact on the HM mechanism? Size and hence riming rate are the important parameters, so there is only an indirect and rather weak link to crystal shape. Presenting this factor as a possible explanation for the discrepancy between observed and calculated values is without good basis.

19399/13: The caption for fig 15 identifies the source of the data as a flight different from the one discussed in the paper; this is probably ok as long as the same probes and settings were used. In a broader sense, the appendices are important analyses of probe performance but they have no significant impacts on the issue raised in the paper. Perhaps they should be given more detail in a separate paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19381, 2010.

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