

Interactive comment on “Ice formation and development in aged, wintertime cumulus over the UK : observations and modelling” by I. Crawford et al.

Anonymous Referee #1

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General comments

Crawford et al. present observations of a certain case of wintertime cumulus over the UK, in which very high ice crystal concentrations were observed despite quite warm temperatures. With the help of different measurements, two models and a number of back-of-the-envelope calculations, the authors search for explanations of this finding. The problem splits up into two aspects: (1) How can the primary ice formation be explained? and (2) How important is secondary ice formation? For neither of these questions, definite answers can be provided. Most attempts for closure between models and measurements fail at least to some degree. Examples are: surface aerosol
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and vertical velocity - cloud droplet number; dust and biological aerosols - IN; splinter production. These mismatches are somewhat frustrating for the reader. In my opinion, it can be legitimate to conclude a paper with open questions and future research needs, and that the topic of this manuscript is of high enough interest to be published without providing definite answers. However, I found this manuscript quite difficult to read, because it is long and lacks a clear thread. The organization into sections and subsections does not seem well enough thought through. I recommend a number of revisions, mainly concerning the structure and the discussion of primary ice nucleation.

- The abstract should be shortened. Here it should become clearer what story the authors want to tell.
- The outline of this manuscript is unclear:
 - 1 Introduction
 - 1.1 Sampling strategy - this doesn't fit into the introduction
 - 2 Shallow convection on the Chilbolton radial
 - 2.1 Meteorological conditions
 - 2.2 Cloud properties
 - 2.3 In-situ aerosol properties
 - 2.3.1 Coupling of observed surface and Airborne Aerosol Measurements - it is not useful to introduce subsections if there is only one
 - 2.4 Sensitivity Studies of the HM Mechanism using WRF
 - 2.5 Sensitivity studies of the HM Mechanism using ACPIM - this includes a description of the ACPIM model. However, ACPIM was already introduced in 2.3.1 with a brief model description.

- 2.5.1 ACPIM Parcel Model Results - the title doesn't explain in how far this section is different from 2.5. One would rather expect an inverse order of 2.5 and 2.5.1 (from their titles).
- 3 Source of the primary ice nuclei - why is this not included under section 2?
- 4 The Hallett-Mossop secondary production mechanism - HM was already discussed in 2.4 and 2.5, so it is confusing that this is taken up again.
- 5 Summary of model results - Why only now if modelling was discussed in 2.4 and 2.5? And where is the summary of the observations?
- 6 Conclusions

The article would benefit from some reorganization, e.g. reordering as follows: introduction - description of instruments and flights - description of the models - observational results - model results - discussion.

Detailed comments

- p 30801, l 22: It should be mentioned already here that this discrepancy in older data might be due to issues with shattering.
- p 30809, l 28: Explain better how the composition was determined. Does this refer to a specific size range only? The dust and primary biological components are not mentioned here, please reconcile.
- p 30812, l 1: "a detailed modelling study simulating the passage of a thermal bubble": what model does this refer to? This needs to be better explained (or removed).

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- p 30812, l 23: The effect of a small number of sea salt aerosol on the activated droplet number might be significant at small vertical velocities due to the competition effect (Ghan et al., 1998).
- p 30811 and 30812: It is unclear to me how all the points discussed in this section fit together. ACPIM simulates 350/cc with the modal vertical velocity, observations show peak values of 150/cc (why not give the modal value as well?), but the aerosol concentrations entering ACPIM are apparently to be scaled by a factor 1/6. Nevertheless, it is stated that "the aerosols measured at the ground are strongly linked to the aerosols at 750m" (which is just below cloud base).
- p 30813, l 28: What is an "acceptable simulation"? Were any objective criteria applied?
- p 30814, l 12: The change of the model output interval is trivial and doesn't need to be mentioned.
- p 30814, l 20: "insufficient vertical resolution" - how many levels are below 2km?
- p 30816, l 4ff: This discussion of the Cooper and Meyers ice nucleation parameterization in the WRF model is of little relevance for this case study. Their relative contributions don't say much about the actually occurring ice formation process. It is important to note that neither Cooper nor Meyers were developed for such warm temperatures.
- Why isn't the DeMott et al. (2010) parameterization employed in WRF, same as in ACPIM? This would make the simulations more comparable.
- p 30818, l 18: No discussion about the different bin schemes needed here.
- p 30820, l 5: It would make more sense to multiply the value of $n_s(T)$ by varying factors > 1 instead of shifting the temperature. Strictly speaking, the temperature shifts would lead to ice nucleation at $T > 0^\circ\text{C}$ (which I'm sure was suppressed).

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- p 30821, l 12: Is this for the "low" of the "high" aerosol run?
- p 30821, l 15: Does this refer to droplet or ice nucleation? If droplet activation, why wasn't this discussed in 2.3.1?
- p 30823, eq. 2 and the following discussion: I strongly recommend to disregard this equation and the attempt to derive IN numbers at a given temperature from it. As noted later, the droplet volume dependence included in this equation is not reasonable for the case of particles including only one particle. The whole discussion would be more conclusive if only ice active fractions were discussed.
- p 30824, l 10: 5×10^5 : The correct number from Levin & Yankofsky (1983) is 5×10^6 bacteria per 1mm drop (page 1965, second sentence of their section 3). However, note that the 50% freezing from their Figure 1(b) refers to droplets of radii of 220 to 360 μm (see their section 2a).
- INA fractions of bacteria: Only one study is cited here to derive the estimate of bacterial IN. This is problematic, as INA fractions vary greatly between different bacterial species, strains and can even change as a function of the sample preparation. For alternative estimates, see Phillips et al. (2008) and Hoose et al. (2010). For recent measurement of INA fractions, see e.g. Möhler et al. (2008) or DeMott et al. (2011).
- INA fractions of pollen: Because the number of pollen per droplet in the immersion freezing experiments of Diehl et al. (2002) is very uncertain, I recommend using the condensation freezing results shown in Diehl et al. (2001) instead. These refer to individual pollen grains.
- p 30824, l 16: Number of bacteria per particle: I disagree with the statement that it is likely that the bacteria occurred as single cells. Burrows et al. (2009b) and Huffman et al. (2010) find that bacteria are likely to occur in clumps. Individual cells are mostly around 1 μm .

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- p 30824, l 23: pollen diameter: The pollen grains studied by Diehl et al. (2002) had diameters around 30 μm . Here, 50 μm are assumed, while on p 30826, l 6, it is stated that "most pollen particles range in size from 15-40 μm diameter". Please harmonize.
- p 30825, l 9: Here it is argued that the observed biological particles in the size range of a few micrometers could be fragments of pollen, and these are assumed to have the same ice nucleation properties as the intact pollen grains. This is highly unlikely, as ice nucleation scales with the surface of particles, and therefore smaller particles should be worse ice nuclei.
- Why don't you estimate the pollen IN based on the number of particles $> 10 \mu\text{m}$?
- p 30825, l 11ff: This paragraph comes back to the discussion of bacteria. Please move this part to the previous discussion of bacteria. This would then deal with my previous comment.
- p 30825, l 13: "could account for IN concentrations of around 0.01 L^{-1} ": Where does this number come from? Is an INA fraction determined from the experiments used?
- p 30825, l 13: "we do not have any measurements specific to this species of bacteria": This is also a problem when using the Levin and Yankofsky (1983) data.
- p 30825, l 17: This discussion is indeed inconclusive and somewhat confusing. Putting all the information together, it could be tried to obtain estimates of the possible contributions of pollen, pollen fragments, bacteria and dust to the total IN number, as a function of temperature. This could look somehow like the sketch which is attached. I'm not sure whether the result will look as in my sketch, but if it does, then it would support your claim that biological IN could be significant at the temperatures of the observed cases.

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- Fungi, lichen and plant fragments have also been suggested as biological IN. These should be mentioned as well.
- It should be emphasized in this section that in-situ IN measurements are not available for this case, and that there is no way to further constrain the estimated biological and non-biological IN concentrations.
- Figures 3, 4, 5: Please explain the variable " N_{round} " and how it is to be interpreted (in the text).
- Figures 10 and 11: Not much can be seen on these plots. Show the difference or relative change.
- Figure 11: The precipitation is shown for a 5-minute interval. This could mean that the precipitation enhancement is rather noise in the disturbed simulation than a consistent signal.
- Figure 12: "lower aerosol concentration reduced by a factor of 6": Isn't the lower aerosol concentration given by the high aerosol already divided by 6? Or is this even further reduced here?
- Figures 7, 13, 14: There is too much discussion of the results in the captions. This belongs into the text; the captions should only describe what is plotted.

Technical comments

- p 30799, l 26/27: There is usually no need to cite both IPCC TAR and AR4.
- p 30809, l 6: PBAB → PBAP
- p 30809, l 19: What is "this"?

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- p 30832, l 24: correct ':'
- p 30835, l 27: correct position of 'o'
- p 30836, l 17: remove '(1967)'
- Fig. 10, caption: II → 11
- Fig. 12 and 13: mention in caption that these are ACPIM results.
- Fig. 14, in x-axis titles: deMott → DeMott
- Fig. 14, left: It is really hard to distinguish the dashed and the dot-dashed lines. Use color.
- Make sure that the figures (in particular 4,5,6,14) will be significantly enlarged for publication in ACP (they are really hard to read).

References

- Burrows, S. M., T. Butler, P. Jöckel, H. Tost, A. Kerckweg, U. Pöschl, and M. G. Lawrence, 2009b: Bacteria in the global atmosphere - Part 2: Modeling of emissions and transport between different ecosystems. *Atmos. Chem. Phys.*, **9** (23), 9281–9297, <http://www.atmos-chem-phys.net/9/9281/2009/>.
- DeMott, P. J., et al., 2010: Predicting global atmospheric ice nuclei distributions and their impacts on climate. *P. Natl. Acad. Sci. USA*, **107** (25), 11 217–11 222.
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- Phillips, V. T. J., P. J. DeMott, and C. Andronache, 2008: An empirical parameterization of heterogeneous ice nucleation for multiple chemical species of aerosol. *J. Atmos. Sci.*, **65** (9), 2757–2783.

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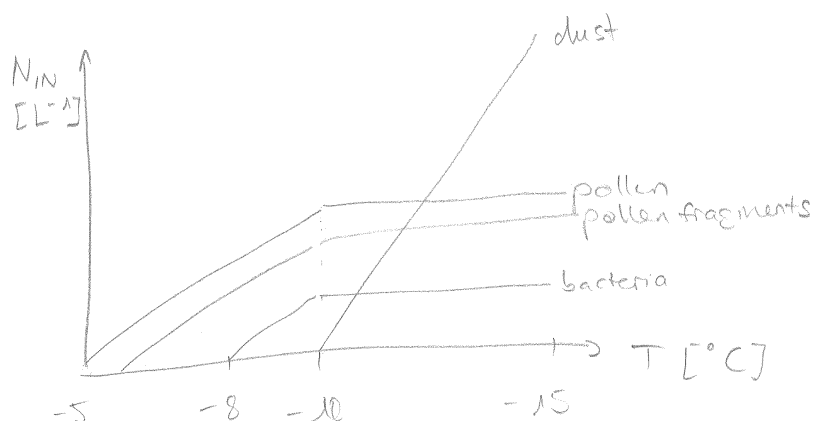


Fig. 1. Sketch of estimated IN concentrations

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