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Atmospheric Chemistry and Physics Discussions

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

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This paper present in-situ measurements and modeling studies of a mixed-phase shallow cumulus cloud system over the southern UK. The paper focuses on the role of i) heterogeneous ice nucleation and ii) ice multiplication processes for precipitation formation in and development of mixed-phase cloud systems. Both are key issues and have received ample attention from the aerosol/cloud community in recent years, but many open questions still remain. While this paper does not provide any definitive answers to any of them, it re-emphasizes the importance of understanding ice formation and growth in the presence of supercooled liquid. It presents extensive

results both from aircraft and ground-based in-situ measurements as well as a range of model simulations, but I find it lacks focus and honestly is a bit of a struggle to read. It is long and not very well structured. Nevertheless, I believe the paper will be suitable for publication in ACP after the following comments and questions have been addressed:

General comments:
- Abstract: Too long and lacks focus. Leave out all the details, and keep only a brief description of the study and the key results.
- Section 1.1 should be moved to Section 2.
- Page 30803: Since 2DS-ice and 2DS-round are shown in Figure 3 and discussed separately, it would be interesting to know how the two particle classes are distinguished by the 2DS instrument.
- Figure 3 is not very appealing to the eye, most data points are squeezed very close to the vertical axis, and most of the space in the figure is left blank. Please change/experiment with the axes to make the figure more reader friendly. Also, I don't understand why apparently only four discrete temperatures were sampled. Is this a result of how you sorted the data? On Page 30805, where figure 3 is discussed, runs R1 and R2 are emphasized, which seems odd since the figure displays results from all runs.
- Page 30805: How close is typically the RHI scan closest in time to the cloud penetration?
- Page 30806: For run R1, the text reports drizzle droplets of a few per litre, while Figure 4 (2DS-round) shows concentrations that are more like a few per 100 litre.
- Page 30808: In addition to the absolute contributions of different aerosol species to the aerosol mass concentrations, it would be informative to also know the relative contributions to the total aerosol mass.
- Page 30808-30809: I'd like to see a reference or some more information on the "detailed modeling study" that suggested only 1/6 of the aerosol population would be

transported across the inversion.
- Throughout the manuscript: Decide on how you want to write “lognormal” and “in-situ”, and be consistent throughout the manuscript.
- Page 30811: I don’t see the 2-3 degree temperature inversion reported in the text in Figure 8. The inversion is really only visible in the Larkhill sounding, and even there it’s much smaller than 2-3 degrees.
- Page 30812: First, I don’t understand the purpose of this sentence: “Integrating the size distributions between 0.6 and 5um yields a total number concentration of 10cm-3, which is sufficiently small that it is unlikely to affect the CCN number calculations”. As far as I can see from the fitted lognormal size distribution that I assume enters the CCN number calculations, it extends all the way to 10um. What am I missing here? Second, I disagree with the second part of the sentence. Large particles grow rapidly and deplete the available water vapor, such that smaller CCN cannot activate in their presence, so they can have a tremendous effect on the CCN concentration despite relatively low number concentrations.
- Figure 10 does not really show a noticeable reduction in snow mass and number due to the inclusion of the HM process. I have a hard time seeing much of a difference between the output from simulations with and without the HM process. How about showing difference plots instead?
- Page 30816: In your discussion of what freezing mechanisms that are included in your simulations, please use the common terminology for the different modes of heterogeneous freezing (i.e. deposition, condensation, immersion and contact freezing).
- Page 30816: As far as I understand, the model is not accounting for the depletion of IN, so in theory the same IN could be activated again and again, which is obviously unrealistic, Please comment on this, and what the implications could be for the comparison with observations.
- Page 30818: In the DeMott et al. (2010) parameterisation, isn’t it appropriate to only include insoluble particles larger than 0.5um? I don’t see why e.g. large sea salt

particles should act as IN at these temperatures. Also, my impression from DeMott’s recent work is that if the large insoluble particles are predominantly dust particles, this parameterization likely underestimates the IN concentrations. Please comment.

Specific comments:
- Page 30800, Line 10: Remove “an”.
- Page 30801, Line 4: Remove one “been”.
- Page 30801, Line 4-8: Vey long sentence, needs some commas.
- Page 30802, Line 9: There’s one “)” too many.
- Page 30805, Line 18: “closet” should be “closest”.
- Page 30806, Line 15: Remove the “s.
- Figure 6, caption: Does dispersion in this context mean log of the standard deviation?
- Page 30814, Line 14: “than” should be “then”.
- Page 30816, Line 23: Remove one “the”.
- Page 30820, Lines 9-13: This sentence is unreadable, please rewrite.
- Page 30821, Line 22: Remove “have”.
- Page 30823, Line 24: Remove one “are”.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 30797, 2011.