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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.

I. Crawford et al.

i.crawford@manchester.ac.uk

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We thank the referee for their comments and suggestions. Our responses to the comments are given below

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:

Abstract: Too long and lacks focus. Leave out all the details, and keep only a brief description of the study and the key results.

Response: We accept the reviewers comments and the abstract has been shortened.

Section 1.1 should be moved to Section 2.

Response: We have restructured the paper according to the guidance of reviewer 1 which has this change included.

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.

Response: This question has been answered in a response to the first reviewer.

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.

Response: We tried several ways of plotting this figure and found a linear x-axis as shown to be the clearest, however, we have enlarged the figure in the manuscript for the sake of clarity. We only show in cloud data from the straight and level runs performed at each level/temperature. Run 2 was performed below cloud and runs 6 & 7 were above cloud.

Page 30805: How close is typically the RHI scan closest in time to the cloud penetration?

Response: An RHI scan was performed approximately every 90 seconds and the closest scan to the start of the cloud penetration was used.

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.

Response: We thank the reviewer for highlighting this. This is an error and has been corrected in the text.

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.

Response: We have now included the PCASP total mass as integrated from the size distribution in fig. 6 (2.05 μg mˆ-3) for reference.

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.

Response: This has now been removed from the paper. See response to reviewer 1.

Throughout the manuscript: Decide on how you want to write "lognormal" and "in-situ", and be consistent throughout the manuscript.

Response: We note the inconsistency and this has been harmonized in the revised 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.

Response: This is a typographical error and should read 0.2-0.3 degrees. The text has been corrected in the revised manuscript.

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.

Response: This has now been removed from the CCN discussion which has now been significantly reworked to concentrate on aerosol compositional effects.

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?

Response: We agree and difference plots have been added to the figure.

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).

Response: The sentence on lines 23-27 has been replaced with the following: "The stochastic nature of both the contact and immersion freezing parameterizations means that they operate independently of the existing total ice crystal concentration, and are limited only by the number concentration of liquid drops available. Thus new ice crystals can continue to be produced by the model so long as there is supercooled liquid present and the temperature is cold enough."

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.

Response: It is true that WRF does not account for the depletion of IN as there is no prognostic treatment of aerosol in the WRF simulations. Consequently we find that when ice crystals grow to precipitation size and fall out of the simulated cloud, new ice crystals are produced afresh at each timestep to replace them. This is because the heterogeneous nucleation schemes act in such a way that they maintain a steady ice crystal concentration for a given temperature. This has the effect of continually replenishing the cloud and sustaining precipitation in an unrealistic way. Thus we have identified a weakness inherent to mesoscale models that do not account for IN depletion when simulating shallow convection at relatively warm temperatures. In this particular case, this weakness is exacerbated by the errors in the dynamics which lead to colder cloud top temperatures than those observed.

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.

Response: The DeMott et al. (2010) parameterisation doesn't make the distinction between soluble and insoluble particles so we believe it is fair to use the total concentration of aerosol greater than 0.5 μm as input.

References:

DeMott, P. J., Prenni, A. J., Liu, X., Kreidenweis, S. M., Petters,M. D., Twohy, C. H., Richardson, M. S., Eidhammer, T., and Rogers, D. C.: Predicting global atmospheric ice nuclei distributions and their impacts on climate., Proceedings of the Na-tional Academy of Sciences of the United States of America, 107, 11 217–22, doi:10.1073/pnas.0910818107,2010.

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