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Atmospheric Chemistry and Physics Discussions

# *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 #1**

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Crosier et al. report measurements of ice crystal concentrations in mixed-phase stratus clouds over southern England. They present a detailed analysis of the cloud microphysics measured during the flights, and corresponding remote sensing measurements of the sampled clouds. Good evidence for the high concentrations of ice nuclei being primarily due to secondary ice crystal production mechanisms, the Hallett-Mossop process in particular, is provided. Seeding of ice crystals from above the measured cloud is also convincingly discounted. Where the analysis and arguments fall apart is in regards to the aerosol concentrations and chemical composition measurements, and the interpreted source of the initial ice nuclei responsible for cloud glaciation at rather warm temperatures > -15 C. The conclusions regarding the sources and efficiency of



the likely ice nuclei do not agree with the current understanding of atmospheric ice nuclei. Unfortunately, the Aerosol Mass Spectrometer is not an appropriate instrument for determining possible ice nuclei compositions as it detects only non-refractory aerosol components that evaporate. It cannot detect mineral dust or biological particles, which are the most efficient ice nuclei, particularly at warm mixed-phase cloud temperatures of > -15 C. The manuscript therefore requires major revisions to address these issues before it can be reconsidered for publication. The subject matter of this manuscript fits well with the scope of ACP.

My major issues are in regards to the aerosol measurements presented. First, it would be useful if the size range of aerosol and cloud particles measured by the various detectors was re-iterated when the data are discussed, and also given in the Figure Captions. Section 2.1 states "Aerosol particle size distributions (0.1 < Diameter <3.0 $\mu$m) were measured with a wing pylon mounted Passive Cavity Aerosol Spectrometer Probe (PCASP,PMS).", but Table 2 shows the smallest particle size detected as 0.61 um. Which is it?

On a related topic, the CDP particle concentrations are displayed in several figures. This instrument detects particles with 2.0 < $\mu$m < 50. It will therefore miss the majority of aerosol particles present, as number concentrations maximize at submicron sizes. These undetected particles are also a large potential source of ice nuclei concentrations (DeMott et al., 2010). The manuscript should be clarified to make these issues clear to the reader. It does not appear that any measurements of submicron aerosol concentrations were performed here, which severely limits any interpretation of likely ice nuclei sources and efficiencies responsible for cloud glaciation.

In Fig. 13 the AMS submicron aerosol mass concentrations are shown along with the CDP supermicron particle number concentrations. It should be clarified that the two instruments do not detect the same size range of particles. The AMS's transmission efficiency falls off rapidly above $\sim$ 600 nm. The likely explains why the two data traces are clearly uncorrelated in Fig. 13, perhaps even anti-correlated.

Why is no AMS data shown in Fig. 13 just above 1 km when the CDP counts maximized? It is important to specify that the AMS measures non-refractory aerosol mass. Therefore, no mineral dust, elemental carbon, bioparticles, or other components that don't vaporize are detected, and these particle compositions are the most likely source of ice nuclei. Why wasn't aerosol composition obtained for the below cloud runs? Was sampling suspended when flying through precipitation?

Remotely-sensed cloud-top and base temperatures are used to infer the temperature that both primary and secondary ice nucleation occurred at, between -12 and -10 C. Ice nucleation efficiency at T > -15 C is typically quite low for mineral dust particles. The only known possible source of ice nuclei at this temperature would be certain bioparticles such as some bacteria (DeMott and Prenni, 2010). Do you have any evidence to support or reject this?

The mixed sulfate/carbonaceous aerosol measured by the AMS is not a likely source of heterogeneous ice nuclei, based on the body of laboratory and field studies of ice nucleation. The known sources of efficient IN deserved further discussion with reference to other papers in the literature. Soot is a poor ice nuclei even at -30 C, and even worse at > -15 C (Karcher et al., 2007; Phillips et al., 2008). If the carbonaceous signal is from biological particles this could explain ice nucleation at these warm temperatures > -15 C. At this temperature the most likely sources of IN are biological particles and perhaps mineral dust particles, neither of which are detected by the AMS. It does not appear that the authors can make any convincing attribution as to the source of the ice nuclei with the presented measurements; it would be pure speculation. Mineral dust could likely be ruled out based on back-trajectory analysis, MODIS images, and the measured aerosol size distributions (which omit the submicron aerosol modes).

Page 14: "Roughly 1 in 500 particles entrained would need to be efficient IN at temperatures > -12.0 C." This is a preposterous statement completely unsupported by our current understanding of ice nuclei sources and efficiencies (DeMott et al., 2010). Only at lower temperatures of < -20 C do some mineral dust types display this high ice



nucleation ability via immersion-freezing (Connolly et al., 2009; Welti et al., 2009). Biological particles are the only known source of ice nuclei that could exhibit this high ice nucleation efficiency (DeMott and Prenni, 2010), but bioparticles acting as the ice nuclei are not discussed here. The fact that a large fraction of the total CN is also not being measured produces another large error in this estimated 1 in 500 IN efficiency. In Section 4 the cloud temperatures of R1 and R2 are stated to be above zero degrees. Is a negative sign missing?

Please justify using a collision efficiency between drops and ice crystals of 1. Was the sensitivity to this parameter tested?

"Relaxing the constraint where only drops with D >24 $\mu$m allow splinter production to occur. . .", does this mean that the constraint was completely removed, or was the size threshold moved to a lower particle size? The sensitivity to the size cutoff for this constraint should be tested.

Most of the Figures, especially Figs. 4-9, are too small and difficult to read.

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