

***Interactive comment on “Measurement of acidic ions and their qualitative effects on snow crystal morphology and the quasi-liquid layer” by T. N. Knepp et al.***

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This paper describes experiments looking at the growth of ice crystals as a function of temperature and in the presence of impurity gases. Growing ice crystals are known to exhibit some dramatic changes with temperature, and although this important phenomenon has been known for over 60 years, it remains essentially unexplained.

I would like to draw attention to the author’s statement that “no morphological diversity was observed in clean air experiments, for which all crystals were filament-like needles.” This statement is in dramatic contrast to numerous other experiments over the past 60 years, in which growing ice crystals have shown clear and reproducible mor-

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phological transitions between plate-like and needle-like morphologies as a function of temperature.

The authors suggest that this difference "could be due to the purity of our air source as compared to that used in previously reported experiments," and that "it seems that an impurity such as AA is required for morphological diversity."

If correct, this is a very important result, as it suggests that surface impurities are needed to produce "normal" ice crystal growth. However, it seems to me that the authors have not made a very convincing case for this result.

It would be more convincing if the authors reproducibly found this unusual "needles-only" growth in very clean air, and then reproducibly found the usual morphological transitions in not-so-clean air. If they did explore this and obtained these results, then it is not clearly stated in the paper.

I can think of an alternative hypothesis that explains these data. Their growth chamber was constructed from plexiglass, styrofoam, and RTV silicone adhesive, and the top part of the chamber was heated to 40C. Since these materials are known to strongly outgas, perhaps the air in the chamber was not extremely clean, but instead was rather contaminated. If so, this could explain the needles-only morphology, as impurities have been known to produce this behavior in past experiments.

My main point here is that the authors have not clearly excluded this alternative hypothesis in their paper. They at least need to do this if we are to believe that the needles-only morphology arises in very clean air.

In my opinion, the clean-air result is much more important than the acetic acid data, so the authors should first focus on proving this result beyond any reasonable doubt.

If impurities did affect the clean-air data (my alternative hypothesis), then these same impurities may also have affected the acetic-acid data in unknown ways.

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