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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Surface roughness during depositional growth and sublimation of ice crystals" *by* Cedric Chou et al.

Anonymous Referee #1

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The authors describe a new laboratory setup for investigating roughness of single ice crystals grown in the apparatus. Their main result is the appearance of a ratcheting up of ice surface roughness/irregularity as crystals are subjected to cycles of growth and sublimation, a "memory effect". Motivation for the work is given in terms of the radiative properties of ice-containing clouds in Earth's atmosphere.

I found this to be a useful and interesting contribution to what seems to be a still poorly constrained topic (ice surface roughness). The experimental apparatus and analysis methods seem to be described in sufficient detail, with a few minor exceptions (see below). The time-lapse videos provided in on-line supplement were especially valuable in aiding the interpretation of figures 10 and 11, but the manuscript stands on its own even without them.

OK, now for some notes, recommendations, and complaints:

1. It seems that the discussion of whether ablation leads to more or less roughening should be improved in a couple of ways. Figures 10 and 11 do seem to suggest that ablation conditions tend to reduce roughness, but I think the videos of the 2-D scattering patterns tend to tell this story more clearly. And there are more interesting patterns evident in those videos: what are the bands caused by? Finally, while the literature references given by the authors seem to point in the opposite direction, it seems worth mentioning that at at least one SEM study (Butterfield et al, Quantitative three-dimensional ice roughness from scanning electron microscopy, 2016) appears to support the authors' findings that ablating crystals tend to be less rough.

2. I found the discussion of roughening mechanisms more speculative than the authors let on. In particular, the last paragraph of section 3.1: none of what is presented in that paragraph is substantiated by evidence given in the paper. I also have a problem with the attribution at the beginning of section 3.1, in which the statement "the growth rate is slow enough for the deposited molecule to diffuse on facets to well-separated attachment sites at steps, kinks, and ledges" is not really justified, even if one can find such statements in the literature. I would point the authors to numerous studies that show that the picosecond-scale sticking coefficient on the quasi-liquid layer of ice is close to 1, and conversion of quasi-liquid to ice occurs faster than horizontal diffusion permits Neshyba et al, A quasi-liquid mediated continuum model of faceted ice dynamics, 2016, offer an alternative view. In general, I found it puzzling that there was no mention of the role of the quasi-liquid layer in these sections; if they are going to speculate, at least that factor ought to be included. At the very least, the authors should flag these sections as more highly speculative than currently indicated.

3. Mentions of "diffusion-limited" and attachment kinetics are unlikely to be understood by many ACP readers; I'd suggest elaborating a little, or omitting these points of discussion.

4. I think it would be appropriate to point the reader to the authors own prior discussion of the possibility of roughness ratcheting-up, as in Ulanowski et al, 2014.

5. Some very minor points: I think the paragraph just preceding section 3.1 is misplaced; it seems to refer to figure 8, but that figure has not been introduced yet. In section 3.2, where reference is made to “small scale vertical motions” as a possible mechanism for formation of irregular crystals, it might help to clarify that those are (I presume) atmospheric vertical motions. And there are a few misspellings here and there (“closeer" in section 3.1) that I presume will be weeded out in the next round of editing.

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