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Interactive comment on "Additional Global Climate Cooling by Clouds due to Ice Crystal Complexity" *by* Emma Järvinen et al.

Z Ulanowski

z.ulanowski@herts.ac.uk

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This extensive study investigates a very important area concerning the radiative impact of atmospheric ice. It could make an important contribution to this subject. However, several conclusions being made are too strong in my view and should be qualified. There is also one large flaw that should be addressed to increase the value of the study.

4.2 p.10. My main point is a significant weakness of this study, the omission of long-wave (LW) effects of cirrus. To illustrate the importance of this shortcoming, the cirrus radiative effect difference found here is dominated by changes in the Tropical Warm Pool (TWP) and Maritime Continent. Yet in this region the net radiative influence of

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Discussion paper

cirrus is determined largely by the longwave, with difference from even the zonal average of the order of many tens of W/mˆ2 (e.g. Xu and Guan, 2017; NOAA/ESRL), in contrast to the _peak_ SW value of about 8W/mˆ2 reported here. So potentially not just the magnitude but even the sign of the postulated effect could change. Hence the LW effect should be taken into account. The severely roughened hexagonal aggregate model that is adopted by the authors includes IR properties. Why were they not included to obtain the net radiative effect? Was the longwave parameterization done but the effects are not shown - why, it should be easy to do? Or was the parameterization not applied - which makes the model internally inconsistent? If this result is being kept "for later", I would strongly advise against it - salami-slicing climate science is a risky undertaking, e.g. the longwave cloud feedback is reported to be positive, mostly due to tropical cirrus (Zelinka and Hartmann, 2010), potentially negating the main conclusion from the work.

This brings me to a related point: the authors make strong statements about the radiative impact, with the largest impact being demonstrated in the TWP/MC region. Yet no in situ data from this region is provided, and very little data from the tropics altogether. What there is, refers to Amazonia, where modelling indicates very weak impact.

Some smaller points follow.

Introduction p.2 and section 2.1 p.3. I find it surprising that the authors do not properly acknowledge that SID3, the core instrument in this work, and long-term assistance with the hardware, software and data analysis techniques were provided to KIT by the team at University of Hertfordshire.

2.1 p.3. Likewise, the method for determining ice crystal roughness using pattern texture analysis (including GLCM) was developed by the Hertfordshire group (Ulanowski et al., 2010, 2014). This should be acknowledged too.

3.2 p.7. "enhanced submicron scale complexity of homogeneously formed ice crystals [...] and can be explained by an increased stacking disorder of homogeneously nucleated ice crystals" Firstly, it would be difficult to associate in situ measurements with the homogeneous mode of nucleation in such categorical fashion. The second part of this statement is extremely simplistic too, no proof of a general connection of complexity with stacking disorder exists yet, even in the lab let alone the atmosphere. While stacking-disordered ice has been produced in the supercooled water freezing experiments of Malkin et al. (2012), heterogeneous ice nucleation is equally important and there can be other reasons why roughness arises (Chou et al., 2018).

3.2. p7. While cyclic growth has been shown to contribute to increased ice roughness (Chou et al., 2018) the SEM experiments that are cited (Magee et al., 2014) are thought to have limited relevance to ice behaviour at tropospheric conditions, as growth in the near-vacuum of a SEM takes place under kinetically-limited, not diffusion-limited conditions typical of the troposphere (Kiselev, 2014; Chou et al., 2018).

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