Authors' Response to Interactive Comment by Z Ulanowski

The authors appreciate the Interactive Comments made by Z Ulanowski. Below we provide our answers to the raised issues. The Interactive Comments are in blue and authors' replies in black.

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.
We thank Z Ulanowski for acknowledging the importance of this study and address his comments below.

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 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.
In this study we only discuss the effect of ice crystal complexity to the SWCRE and omitting the LW effect will not in any way change the conclusion of this work. There are two reasons why we do not discuss the LW effect. First, the focus of this study is the effect of ice crystal complexity on the ice cloud asymmetry factor. In the ECHAM-HAM model the ice particle asymmetry factors are only considered for calculation of the SW effect and are not included in the calculations of the LW effect. This is due to the fact that the LW effect is less sensitive to the ice crystal morphology than the SW effect. For example, Yi et al. (2013) showed that changing the ice crystals from smooth to complex will not significantly affect the LWCRE.

Secondly, our optical measurements are in the SW region and, therefore, we can only make conclusion of the SW asymmetry factors. We agree that optical measurements in the LW region would be of interest to validate LW parameterizations in the future. Furthermore, we think that the term "salami-slicing" is more than misplaced with regard to this work, which - in our opinion - represents one of the most comprehensive studies on ice crystal complexity and its influence on the cloud radiative forcing. The experimental data used here are from dedicated cloud chamber simulation runs as well as from the field, gathered in a dozen of aircraft projects around the globe. Further, the data are used to construct a new, more realistic parameterization of the asymmetry factor to be used in climate models - a scientific span that is not common in the field.

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.
The data presented in this study covers all the geographical areas where the KIT SID-3 and the PHIPS instruments have been flown and a large amount of the campaigns where PN measurements were available. We agree that the TWP/MC region (where these instruments have not yet flown) is highly important for the cirrus cloud radiative impact. We hope that in future more field campaigns will be focused on this area, where this study demonstrates the largest SW 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.
The SID-3 instrument was developed by the University of Hertfordshire and a version of the instrument was purchased by KIT in 2008. It is true that many collaborative efforts between KIT and Hertfordshire has taken place to improve the hardware, software and data analysis methods to the current status and we believe that these collaborative efforts are correctly documented in the corresponding literature. The instrument itself is cited through the original Hertfordshire publication of Kaye et al. (2008). The University of Hertfordshire was involved in the first field deployment of the instrument in the MACPEX campaign, which is acknowledged by co-authorship in the Järvinen et al. (2016) and Schmitt et al. (2016b) publications. The SID-3 scattering pattern analysis methods for atmospheric ice particles were also developed in close collaboration between KIT an University of Hertfordshire by conducting at least five joint AIDA cloud simulation campaigns. This effort is acknowledged in the original work describing the use of the complexity parameter, k_e, as a complexity measure (Schnaiter et al., 2016), where the University of Hertfordshire (Z Ulanowski) is listed as co-author.

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.
Analysing scattering patterns to retrieve information on surface roughness has been previously used in industrial applications for surface quality control (e.g. Lu et al., 2006) but it is true that the Hertfordshire group was the first to use this technique for ice crystal surface roughness. Therefore, we have added the citation to Ulanowski et al., 2010, 2014 to the following sentence: "*The crystal complexity is quantified from the 2-D scattering patterns using a grey-level co-occurrence matrix (GLCM) method (Lu et al., 2006). This method was developed for industrial quality control of surface treatment processes but was later adapted for analysis of complexity features of three-dimensional ice particles (Ulanowski et al., 2010, 2014; Schnaiter et al., 2016).*"

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).
We refer to the in situ measurements that were presented in Ulanowski et al. (2014). The authors argued that in situ observations in a mid-latitudes cirrus showed differences in the ice crystal complexity based on the airmass origin: "*polluted airflow showed significantly lower roughness for all measures apart from kurtosis. We speculate that this was due to higher concentration of inhomogeneous ice nuclei (IN) in the last case".* Of course it is difficult to investigate the origin of ice crystal complexity based on in situ measurements, especially if the ice particle history is unknown. Therefore, such laboratory studies will be valuable to interpreted in situ field results.

For the second point, we agree that our knowledge of formation of surface roughness in a single crystal is still highly unknown. Therefore, we modified the sentence as: "*can be partly explained by…"*.

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).

We agree that discussing the results of SEM experiments in atmospheric context is challenging due to the near-vacuum pressure conditions experienced by the ice crystals. Therefore, it is important to have proof such results in atmospheric conditions as shown in Chou et al. (2018). We have added this reference to the sentence together with the Magee et al. (2014) reference.

References

Chou C., Voigtländer J., Ulanowski Z., Herenz P., Bieligk H., Clauss T., Niedermeier D., Hartmann S., Ritter G., Stratmann F.: Ice crystals roughness during depositional growth and sublimation, Atm. Chem. Phys., doi:10.5194/acp-2018-254, in review, 2018.
Kiselev, A.: Interactive comment on "Mesoscopic surface roughness of ice crystals pervasive across a wide range of ice crystal conditions" by N. B. Magee et al., Atmos. Chem. Phys. Discuss., 14, C4758–C4763, http://www.atmos-chem-phys- discuss.net/14/C4758/2014/, 2014.
Malkin, T. L., Murray, B. J., Brukhno, A. V., Anwar, J., and Salzmann, C. G.: Structure of ice crystallized from supercooled water, Proceedings of the National Academy of Sciences, 109, 1041–1045, 2012.
NOAA/ESRL http://www.esrl.noaa.gov/psd/
Ulanowski Z., P.H. Kaye, E. Hirst & R.S. Greenaway: Light scattering by ice particles in the Earth's atmosphere and related laboratory measurements, In: Proc. 12th Int. Conf. Electromagnetic & Light Scatt., Helsinki, 294-297, 2010.
Zelinka, M. D., and D. L. Hartmann: Why is longwave cloud feedback positive?, J. Geophys. Res., 115, D16117, doi: 10.1029/2010JD013817, 2010.
Xu, Q. and Guan, Z.: Interannual variability of summertime outgoing longwave radiation over the Maritime Continent in relation to East Asian summer monsoon anomalies, J. Meteorological Research, 31, 665-677, 2017.

References

Järvinen, E., Schnaiter, M., Mioche, G., Jourdan, O., Shcherbakov, V. N., Costa, A., Afchine, A., Krämer, M., Heidelberg, F., Jurkat, T., Voigt, C., Schlager, H., Nichman, L., Gallagher, M., Hirst, E., Schmitt, C., Bansemer, A., Heymsfield, A., Lawson, P., Tricoli, U., Pfeilsticker, K., Vochezer, P., Möhler, O., and Leisner, T.: Quasi-Spherical Ice in Convective Clouds, Journal of the Atmospheric Sciences, 73, 3885–3910, https://doi.org/10.1175/JAS-D-15-0365.1, 2016.

Kaye, P. H., Hirst, E., Greenaway, R. S., Ulanowski, Z., Hesse, E., DeMott, P. J., Saunders, C., and Connolly, P.: Classifying atmospheric ice crystals by spatial light scattering, Opt. Lett., 33, 1545–1547, 2008.

Lu, R.-S., Tian, G.-Y., Gledhill, D., and Ward, S.: Grinding surface roughness measurement based on the co-occurrence matrix of speckle pattern texture, Appl. Opt., 45, 8839–8847, 2006.

Schmitt, C. G., Schnaiter, M., Heymsfield, A. J., Yang, P., Hirst, E., and Bansemer, A.: The microphysical properties of small ice particles measured by the Small Ice Detector-3 probe during the MACPEX field campaign, Journal of the Atmospheric Sciences, 2016b.