We thank the reviewer for the helpful observations and suggestions which have considerably improved the manuscript. We have also taken the opportunity to further refine the text for minor grammatical and typographical issues.

Reviewer 2:

I acknowledge the changes made by the authors in response to my comments. Specifically, I agree that the addition of a paragraph citing the previous works by (Collier et al., 2016, Ulanowski et al., 2014, and Butterfield et al., 2017) sets the presented method of measuring surface roughness into the framework of optical properties of cirrus clouds.

However, the connection between the roughness measurements and calculation of optical properties has not been addressed in a satisfactory way.

1. The authors added a sentence where they "...explicitly state that the profilometer approach can yield roughness metrics directly comparable to those used in optical scattering studies, namely correlation length and height standard deviation (σ)..." (I am citing from their response to my previous comments). This "explicit statement" reads as follows (lines 96 -97 of the revised manuscript): "Building on all these approaches, the formvar replication method combined with optical profilometry enables such surface features – including correlation length and RMS height – that are directly comparable to values used in scattering simulations."

This statement, again, is a just a statement, which is neither supported by any further arguments, nor by comparing the actual data obtained in this manuscript with the data of other studies. This statement is also inconsistent with what authors write in response to my comments: the manuscript contains the values of RMS (root mean square) of height coordinate (equation 3); the standard deviation of height was not shown anywhere in the manuscript.

2. Further, in response to my comments, the authors write: "Finally, in lines 315–326, we present our dimensionless results which was produced by following Collier et al. (2016) and normalized to λ = 532 nm in a table and explicitly compare these values with those reported in prior studies, noting that they fall within the ranges used in light scattering simulations (e.g. Collier et al., 2016; Butterfield et al., 2017)."

The authors do not explain even briefly how these "dimensionless results" have been obtained. There is also an intrinsic inconsistency in their statement (lines 316 – 317 of the revised manuscript): "...we derived the correlation length (Lc) and standard deviation of height (σ) for one representative ice crystal replica which was normalized to the incident wavelength (λ = 532 nm), following the approach of Collier et al. (2016)." If the Lc and σ have been derived "for one representative ice crystal", then why are values for four different size parameters X are given? For a given wavelength, a single ice crystal can have only one size parameter, so where are the others are coming from?

3. The choice of size parameters is another mystery. From the definition of the size parameter and a wavelength of 0.532 μm, the largest ice crystal used for this derivation (X = 80) was not larger than 13.5 μm in diameter, and the smallest just above 3 μm (X = 20). However, all other images in the manuscript (figures 3, 4, 5, and 8) show ice crystals that are at least 50 μm in one dimension,

corresponding to X = 300 for the wavelength of 0.532 µm. So how such a crystal could be declared "representative"?

4. It should also be mentioned, that Collier et al. (2016) used the experimental values of correlation length and standard deviation to simulate model rough ice particles and then to calculate their scattering properties using the DDA (Direct Dipole Approximation) method. The DDA method, being computationally very expensive, is not capable of dealing with size parameters above 100. To mitigate this issue, and to keep the number of dipoles in the DDA limited, the roughness parameters (correlation length and height variance) were scaled up (artificially !!!) with size parameter – see section 3 "DDA Results" of the Collier et al. (2016): "Four crystal size parameters were considered; 20, 40, 60 and 100, with roughness being scaled proportionately with size". While this explains why the normalized correlation length and sigma both increase with simulated crystal size (see Table 1 of Collier et al. (2016), there is no physical reason to expect such proportional increase in your case. However, the Table 3 of the manuscript clearly demonstrate an increase of the normalized correlation length and standard deviation of height with the increasing size of ice crystals.

This argument renders the statement (lines 319 - 320 of the revised manuscript) "The values are consistent in magnitude with those reported for mineral dust analogues measured by an AFM (Collier et al., 2016) and with SEM-derived features of ice crystals (Butterfield et al., 2017)", essentially, meaningless.

Finally, the comparison with SEM-derived features of (Butterfield et al., 2017) is not valid because (Butterfield et al., 2017) does not report values of correlation length or standard deviation of height. Reading (Butterfield et al., 2017) carefully, one immediately notices that their definition of Z (equation 1 in Butterfield et al., 2017) has nothing to do with height variability but gives local gradient of a tilted microsurface on a rough ice surface.

In my opinion, these inconsistencies can be resolved in two possible ways:

Option 1. The authors explain in great detail how the correlation length and sigma have been calculated from the actual measurement data; the original measurement data has to be made available in supplementary material or via data repository.

Option 2. The authors remove the newly added "dimensionless results" and reduce the claim that the method is capable of providing roughness characteristics required for calculations of optical properties of rough ice crystals. The scope of the manuscript is reduced to the description of a method to measure surface roughness of ice crystal replicas with a profilometer. The demonstration of similarity between the roughness parameters retrieved for replicas of salt crystals and of the original salt crystals implies the possibility to apply this method for characterization of atmospheric ice crystals, but the applicability for optical calculations is yet to be demonstrated.

RESPONSE: We thank the reviewer for the detailed and constructive comments. After careful consideration, we have decided to rollback the previously added "dimensionless results" and the associated claims regarding calculation of optical properties.

The manuscript now focuses solely on the description and validation of the profilometer method for measuring surface roughness of ice crystal replicas. In particular:

The statement in lines 90–91 has been retained to describe the method, but any claims about direct applicability to optical calculations have been removed.

The concluding remarks in lines 385–395 have been revised to clarify that the method provides reliable roughness measurements as demonstrated for salt crystals and ice crystal replicas, implying potential applicability for atmospheric ice crystals, without claiming direct relevance to optical scattering calculations.

We believe these revisions address the reviewer's concerns while maintaining the focus and integrity of the manuscript.