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Interactive comment on "Investigation of ice particle habits to be used for ice cloud remote sensing for the GCOM-C satellite mission" *by* H. Letu et al.

Anonymous Referee #1

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This is a review for the manuscript "Investigation of ice particle habits to be used for ice cloud remote sensing for the GCOM-C satellite mission" submitted to ACP by Letu et al. This paper essentially evaluates the use of the Voronoi ice model for ice cloud remote sensing for the GCOM-C satellite mission. POLDER multi-directional measurements are used to evaluate the Voronoi model and a selection of other models. It is concluded that the Voronoi model would be a suited model for ice cloud remote sensing for the GCOM-C satellite mission. The authors clearly put in a lot of effort in the simulations. Unfortunately, however, many things are unclear in the manuscript and much of the work is irrelevant considering previous publications.

The Voronoi model is compared to pristine hexagonal columns, plates, bullet-rosettes and droxtals. Already since the first POLDER publications in the 1990s that also used the SAD method, in addition to analysis of polarized reflectances, it is clear that pristine crystals with hexagonal parts are inconsistent with the measurements. Basically all the papers referred to on page 31669, line 21-23, and page 31670, line 5-6, come to this conclusion. Including pristine crystals in the analysis performed here is therefor pointless. Since discussion of the pristine models compared with the Voronoi model takes up most of the paper, either that analysis needs to be repeated with rough models or in my opinion the paper should be rejected in the current form. In figure 10, the rough 5-plate aggregate is shown. I assume the optical properties for that habit are obtained from the Yang et al. database. The authors may want to use the optical properties of other rough particles in that database instead of their own calculations. Otherwise, a shorter paper just focusing on the Voronoi particles may be suited for publication.

Aside from this main comment, there are various other major shortcomings in this paper, as well as some minor issues. Below my other major and minor comments are listed. Should the paper be accepted in some form, I recommend these issues to be addressed. Both the conclusions and the abstract should also be revised accordingly.

Major comments:

1) A model is searched to be used for optical thickness and size retrievals using the commonly used Nakajima-King approach as illustrated in Fig. 6. Such an approach requires the optical properties to be integrated over size distributions as shown by Baum et al. (2005). The authors define an effective radius in Eq 6., where a size distribution is used, although not specified. However, in their SAD analysis, as far as I understand, only single particle optical properties are used. To be consistent with their goal, I recommend using size-distribution integrated optical properties for the SAD analysis. It should be specified which size distributions are used. The conclusion that small bullet rosettes are consistent with the data is because these tiny crystals have size parameters well below 100 and therefore smooth phase functions. However, the contribu-

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tion of these small particles to scattering properties for any realistic size distribution is probably negligible. Thus, I expect only the Voronoi model to be consistent with the measurements once size distributions are used, since the other models are pristine and have phase functions with features. Furthermore, it should be pointed out that for particle size retrievals, a model is needed that is consistent with measurements over a large range of sizes and not just a single size.

2) Several definitions are unclear in the paper. On page P316672, $\tilde{r}$ is named the effective radius, but it is defined as the radius of an equivalent volume sphere. Effective radius for a single particle is usually defined as three-fourth of the volume over the projected area, which is a relevant size definition for determining the single scattering albedo. Using the term "effective radius" for $\tilde{r}$ is confusing and should be avoided. I suggest to name $\tilde{r}$ "volume-equivalent radius".

In equation 6, a size-integrated effective radius is defined, which adds to this confusion as it is unclear which "effective radius" is meant in the following parts of the paper. Furthermore, the effective radius defined in Eq. 6 is based on the size distribution weighted integration of $\tilde{r}^3$ and $\tilde{r}^2$. If I am correct this is not consistent with the usual definition of effective radius for non-spherical ice, which is three-fourth of the total volume over the total project area. Please use the common definition of effective radius or rename it and use a symbol other than r_e.

For the calculation of effective radius as defined in eq. 6, size distributions are needed. What size distributions are used here? For non-spherical particles it is not trivial to choose a size distribution to obtain a specific effective radius (as it is for spheres). For example, this problem was described and tackled by Baum et al. (2005, 2011) by applying about 14000 size distributions and sorting them for effective radius afterwards. It seems that here r_e is only used for figure 6 in the current version, but the authors should discuss the size distributions applied as also remarked in my previous comment.

On page 31679, the comparison to other models from "conventional studies" is de-

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scribed. The optical properties of GHM model are provided by Bryan Baum. If I am correct, the optical properties available for the GHM are already integrated over size distributions and are given for specific effective radius (defined in the traditional way). However, here the authors say they compare the model using a single particle equivalent volume radius $\tilde{r}$ of 30 micron. I doubt that this is correct. Although this may not matter much for the analysis, it is all very confusing and inconsistent.

3) The calculations for the SAD analysis are very unclear. Equation 3 on P316674 is technically incorrect. The right part is an observed quantity, while the left part is a modeled quantity since the τ, r_e, ω and P_{11} dependency is included. In the following it is very unclear what is modeled and what is measured. The SAD analysis compares measurements with simulations, but nowhere in these equations there is a difference taken between measurements and simulations. Also, the step-wise description on page P316674-75 suggests that this is applied on a pixel-by pixel basis, while the model evaluation is done on globally, temporally averaged data. The SAD analysis is probably better explained in some previous Baran et al. papers, so I suggest to correct the equations and description based on such previous work.

It should also be pointed out that simulated R_{cld} is a function of τ, ω and P_{11}, but then not also of r_e. The r_e dependency of R_{cld} comes from the dependency of ω and P_{11} on r_e.

Aside, the SAD analysis is described here in terms of r_e, while the applied SAD analysis is in terms of $\tilde{r}$. This should be made consistent. As pointed out in my previous comment, the SAD analysis should also be performed using size-integrated phase function.

Minor comments:
P31669, L1: Perhaps the most important update in the 2013 version of the Yang et al. database was the addition of roughness to the particle surfaces. That should be mentioned here.

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P31669, L6: There is a bracket missing after “8-column-aggregate”.

P316670, L6: The van Diedenhoven et al. (2012) paper is cited here, but this paper only describes a method to retrieve crystal roughness. Other papers by this group show the method applied to data, such as van Diedenhoven et al. (2014; already cited) and van Diedenhoven et al. (2013; ACP, vol 13, 3185-3203, doi:10.5194/acp-13-3185-2013.). These would be better references here.

P316671-72: Are the Voronoi model optical properties based on a single realization of the Voronoi habit or are several averaged? Does the geometry of this model depend on size or not?

P316673, L27: Please point out that the scattering angle ranges observed depend on latitude, possibly adding lines to the figure to show the different sampling in various latitude bands.

P316674, L2: What is meant with a “changing peak”. Please explain in the paper or rephrase. Also on page 31678.

P316674, L16: Note in the paper that r_e cannot be retrieved from POLDER/PARASOL measurement because of the lack of shortwave infrared measurements.

P316674, L20: Move first “and” before “spherical”.

P31677, Figure 5: The y-axis in figure 5 is such that the relevant differences between the models are not apparent. The asymmetry parameters appear to range from 0.5 to 0.9, which is a huge range in terms of radiative effects. Please change the y-axis range to at least 0.5-1 to make the differences between the models apparent.

Page 31679, L8, Fig. 9: Please explain what is meant with “averaged over all distortion values”. Which distortion values are that? 0, 0.15 and 0.25?

Page 31679, L16: Please replace “most efficient” with something like “more consistent with measurements”.

Page 31680, L2: This conclusion is consistent with the conclusion of Liu et al. (2014) that of geometric irregularity and surface roughness are effectively equivalent. Please include this citation. (Liu, C., R. L. Panetta, and P. Yang, 2014: The effective equivalence of geometric irregularity and surface roughness in determining particle single-scattering properties. Opt. Express, 22, 23 620–23 627, doi:10.1364/OE.22.023620.)

Figure 8 and 11: These figures show similar information but for different particle types. I suggest to make the style the same so it is obvious that these are similar figures. In figure 8, the word “different” in the y-axis title should be “difference”. In figure 8 and 11, respectively, the “ and dollar signs should be the degree sign.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 31665, 2015.