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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Incidence of rough and irregular atmospheric ice particles from Small Ice Detector 3 measurements" *by* Z. Ulanowski et al.

**Anonymous Referee #1**

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The paper provides the first SID-3 in-situ measurements of high-resolution spatial light scattering patterns of atmospheric ice. From the SID-3 they derive measures of ice crystal size (through the inverse of median speckle area) and ice crystal complexity, with the latter being described under the general term of "roughness." The fundamental methodology of estimating the size and roughness is based on laser speckle, which the first author published in 2012 (JQSRT 113, 2457-2464 (2012)) and in that paper, it was shown that a measure of size and roughness could be obtained separately. In this paper, the methods previously detailed are applied to SID-3 scattering patterns to obtain measures of size and ice crystal complexity in 3 cases of cirrus and 2 cases of mixed phase. The interesting point about the cirrus is that the distribution of roughness appears different if the cirrus was sampled in an oceanic or continental air mass,



with the latter appearing to have a lower roughness index. Moreover, the paper finds a negative correlation between the halo ratio and surface roughness, and no relationship between roughness and RHi. They combine the in-situ measurements with results from the laboratory under a variety of conditions, and show how surface roughness changes under such conditions, and these findings are generally consistent with recent laboratory studies of mesoscopic surface roughness reported by Pfalzraff et al. (2010) and Neshyba et al. (2013). These papers demonstrate that surface roughness can change with changing environmental conditions, and that surface roughness may not be uniformly distributed about the surface of ice crystals, but rather, some surface roughness may appear on some facets, whilst on others, no surface roughness may appear at all. Theoretical work by Shcherbakov 2013 (JQSRT 124, 37-44) applied higher distortion to the basal facets and lower distortion values to the mantle facets of hexagonal ice to approximate the results of Neshyba et al. (2013). Deeply rough surfaces may appear under sublimation rather than mere "rounding" as is usually supposed, and this can occur at low supersaturations (Pfalzraff et al. (2010), and references therein).

The paper employs a physically independent method to measure ice crystal size to what is routinely applied using the conventional suite of microphysical instrumentation. Moreover, the measure of ice crystal roughness or complexity allows, to some degree, for the first time, the quantification of the light scattering properties of small ice, not currently achieved by current imaging probes, which are not able to resolve structure on or of small ice. I agree with the authors that it is not the details of the ice crystal morphology that matter, as far as radiation is concerned, what is important is how this complexity manifests itself through light scattering. It is well known, that as ice crystals become progressively more complex their intensity and polarization properties become simplified, to such an extent that "retrieving" shape is meaningless. The findings of this paper that simplified scattering properties generally best describe the light scattering properties of cirrus is consistent with previous studies using Polar Nephelometer, aircraft and satellite radiometric measurements. The difference between this paper and the previous studies is that the authors now try to put a measure on ice crystal complexity, which in turn should lead to a constraint on the asymmetry parameter. The paper is very interesting and innovative, which should have wide appeal to the atmospheric science community.

Although there is nothing scientifically major that is wrong. The paper would be improved if the points below were included in a revision. These are (i) clarifications with regard to the aircaft sampling of the cirrus (ii) Show other independent measures of size and RHi using some aircraft profiles, if available (iii) a reasoning as to why the presented results are not affected by shattering and some proof of this. (iv) Quantitative statistical analysis on the histograms presented in Figure 6.

1. (a) On inspection of Figure 7, apart from (a) and (d), how much dependence does the light scattering patterns of the other samples have on orientation? For samples (f) and (b) the symmetry properties of those crystals should preclude any dependence on orientation, and with very rough irregular particles represented by the more spheroidal and ellipsoidal particles. There should be some discussion of the dependence of the selected particles on orientation. Were some specific orientations considered for samples (a), (d) and (e)? If so, how many orientations were considered? (b) Table 1 considers only 10 samples, were other samples considered but gave the same results? If so please state and the total number of samples, but not shown for reasons of brevity. (c) Figure 7, does not seem to consider hollow crystals, in the introduction cavities are mentioned. How might hexagonal cavities affect the results or are the authors saying that roughness and complexity, without, hollowness, is the leading contribution to speckle? (d) The size range given in Table 1 is that the size range of SID-3? If not, please state the size range of SID-3. (e) Do the samples used in the in-situ analysis exhibit surface roughness on all facets? If not, then please state in the text.

2. What was the sampling pattern flown by the aircraft? Did the aircraft obtain complete profiles (from above cloud-top to below cloud-base) of each cloud? What were the typical cloud-top temperatures and base temperatures and how did RHi vary throughout the cloud? Were there thin regions of high supersaturations and regions of low



supersaturations? If so, where were the layers located as a function of distance from cloud-top? The image below shows DARDAR image of B504 obtained from the A-train, at about 1-00pm (UTC) or so, note that the cirrus extends up to an altitude of $\sim$12 km. I do not believe that the aircraft used can extend to such an altitude according to the NERC website which details such information (ceiling of $\sim$10.7 km)? This is important, as the fraction of incident radiation reflected back to space is weighted towards the cloud-top, and so it is important to sample the tops of cloud, and if the tops were not sampled, then not all possible conditions were considered.

3. The paper uses an independent method to determine size. Please show independent measures of size obtained using other microphysical probes. The Cotton et al. (2012) paper lists a whole suite of instrumentation that was deployed on the aircraft. Suggest you show the averaged PSD over the cloud profile for the cirrus cases.

4. How do the authors know that their results are not affected by shattering?

5. Please show a figure of measured size as a function of roughness for some profiles.

6. How confident are the authors that the FWVS is a good measure of RHi? Were any other instruments deployed on the aircraft that also measured RHi? If so, please show some profile comparisons.

7. The coefficient of determination from Fig. 9 is only 5%, whereas the halo ratio varies from about 0.5 to almost 2. Personally, I do not believe the authors have demonstrated a strong relationship. However, such a relationship is expected theoretically. This is the problem with regression analysis of the type presented in Fig. 9, due to the number of data points being large merely increases the likelihood of low correlation coefficients being deemed statistically significant. What should perhaps be stated is that the halo ratios can > 1, at a low measure of surface roughness, than at a higher measure. Please state the number of data points used for the analysis in Fig. 9.

8. Figure 6 is very interesting. Can the authors give more quantitative statistical mea-

sures using standard methods as to whether the differences between the roughness histograms in the oceanic and continental airmasses are statistically significant? Also please give the total number of data points used in each of the histograms. The reason is, in the abstract, the authors state that differences are only "slightly higher." Can they quantify "slight"? Then in the next sentence the authors state that their results bring into question the use of seeding cirrus. The authors need more examples of this, to make such general statements. They present only a couple of examples. Other examples may not necessarily show the same behaviour. At this time, I believe, it is too premature to make such statements, unless the authors can demonstrate numerically their confidence in the result.

Other points

1. Although the spatial scattering patterns were not as detailed as SID-3. SID-2 was successfully used by Field et al. (2003) [GRL vol 30, issue 14, July 2003 DOI:10.1029/2003GL017482] to show little evidence for their being significant halo formation at 22o in the data they sampled. This reference should be cited.

2. Another paper that should be cited is Shcherbakov 2013 (JQSRT 124, 37-44).

3. The authors state on page 3 line 5 that whether cirrus warms or cools the Earth surface depends on ice crystal morphology. It is true that ice morphology is important but other parameters are also important and the most important ones of these are the vertical distribution of ice mass, and the vertical variation of the size-shape distribution. Please re-write more generally.

4. Page 3 line 26. Not true, the results of Baum et al. (2011) do not uniquely show that rough particles explain their depolarization results. There is a narrow distribution at around a ratio of about 0.4 which is exhibited by smooth particles. It more true to say that rough particles explain the majority of their observations. At latitudes > 30oN, the smooth particles can explain the higher values of P11 at 180o. The P11 results at the higher latitudes cannot be ignored. Please re-write.



5. The Gayet et al. (2011) also found evidence for regular particle shapes with distinct halo features at 22o, and these were found at the leading edge of the same cirrus. It is true to say that the majority of observations collected by Gayet et al. (2011) were dominated by non-regular plates, and they recommend that featureless phase functions should generally be applied. Please discuss these other findings and the range of asymmetry parameters reported by Gayet et al. (2011).

6. Can the authors give an uncertainty in their estimated roughness values? At what surface roughness value does the retrieval become ill-posed? Climate models require that the uncertainty in g be less than 5% (Vogelmann and Ackermann, 1995; J. Atmos. Sci., 52, 4285–4301).

7. Recent observations reported by van Diedenhoven et al. (2013)[ Atmos. Chem. Phys., 13, 3185–3203, 2013] found that tropical anvil cloud-top median asymmetry parameter values were between 0.76 to 0.78. Since the article discusses the asymmetry parameter then the currently estimated observational range in g should also be cited.

8. The authors are aware that the tilted facet method is often used to represent the surface roughness by large numbers of authors. Can they give an indication of how well this method describes their 2D scattering patterns? Recent theoretical evidence reported by Liu et al. (2013) [JQSRT 129, 169-185] indicate significant deviations between facet tilting and idealized high values of surface roughness in the intensity and polarized scattering matrix elements.

9. Please state the range of humidity reported by Kramer et al. 2009.

10. Page 16 line 24, can you not conclude the same from Figure 9 of this paper, as the scatter plot is somewhat similar? I agree with the comments on histories of ice crystals and the problems with instantaneous measurements, without knowing such histories. The former point is merely about scatter plots.

11. A comment on the discussion on page 18 line 10 about the SW bias in climate

models over the Southern Ocean. This bias tends to be general to most climate models. I agree that some lower value of g would have a positive impact on this bias, but there are other possibilities, which need to be considered. Such as getting the fraction of liquid water correct, at the moment there might be too much ice in the mixed phase in this region due to unresolved vertical speeds. An increase in the LWP would have an impact on the bias in the right direction, so would a more correct treatment of the indirect effect such as the uptake of sea-salt aerosol, as well as better sea-salt aerosol PSDs, and a more correct treatment of ocean-atmosphere surface flux exchanges. The problem will not be solved by one parameter alone but by a combination of improvements.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 24975, 2013.



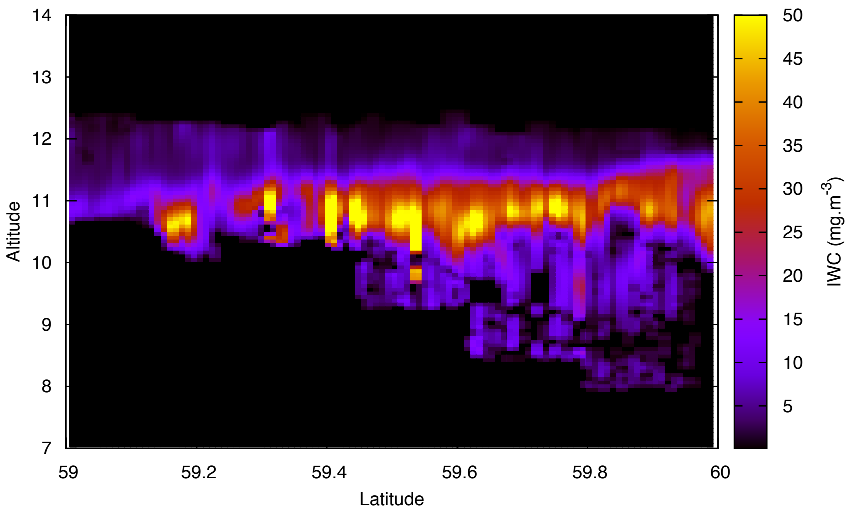


**Fig. 1.** DARDAR