The Cryosphere Discuss.,
doi:10.5194/tc-2016-294-RC2, 2017

The Cryosphere
Discussions
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Interactive comment on "Comparison of different methods to retrieve effective snow grain size in central Antarctica" *by* Tim Carlsen et al.

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Received and published: 19 May 2017

General comments

The paper compares four time-series of surface snow grain size (specific surface area to be more precise) obtained around Kohnen station in Antarctica derived from different albedo measurements: one is from in situ manual, one from in situ automatic, another one using an airborne sensor, and the last one using MODIS satellites. The paper describes the datasets, the methodology to retrieve the grain size from the different sensors, then the results and provide a short discussion and a conclusion.

The paper theme is of great interest as the grain size controls the albedo which is a significant driver of the surface energy budget in Antarctica. Because time-series of grain size are scarce, in Antarctica and elsewhere, comparison of methodologies to

obtain them is an important ongoing effort in the snow community.

The paper is well written and easy to follow except in a few places where critical information are missing (see detailed comments). This requires minor work but is necessary for the read to understand the assumptions used by the authors, and to allow reproducibility and comparison with other studies.

Overall the paper is quite short. The authors should address in their response to reviewers the reason why this study is not merged with the paper in preparation (Freitag et al.). The review of the literature is also relatively light and should be completed (see some personal suggestions below, but works from other groups should be considered as well). The discussion should be completed with an analysis of the results with respect to other previous studies.

Uncertainties are given at different stages of the methodology, which is very useful, but the way these uncertainties have been estimated is not described, and they seems to me often overoptimistic. Even though it is notoriously difficult to estimate uncertainties, some justification are required. In addition, the authors do not consider the effect of the roughness, and they neglect the impact of wavelength-dependent errors of spectra calibration on the ratio R. Both are probably not negligible and should be evaluated or at least indicated.

The paper does not show albedo spectra from which SSA are derived. Given the focus of the paper, it could be acceptable. Nevertheless I suggest to add example of spectra for one date to illustrate the derivation of SSA from albedo and to possibly help in the analysis of the bias observed between the airborne timeseries and other time-series which remains unexplained.

To foster comparison in the future, I recommend to publish the four timeseries once the paper is accepted.

Detailed comments:

Abstract:

- The term "effective size of snow grain" is not well defined, especially "effective" is relative to the domain (optics, microwave, chemistry, etc). Optical grain size is more adequate.

- The abstract tells what has been done, but is not an abstract of the paper. The objective of the study is missing, as well as the conclusion.

Introduction

P2 L3 "most of the sea ice are covered with snow with little seasonal variability". The variability of sea ice is huge in Antarctica.

P2 L9 "Furthermore, snow surface albedo varies with snow depth and the liquid water content.". Need a reference for each effect. The dependence to snow depth is almost irrelevant in Antarctica.

P2 L11: Add references of published works (Libois et al., Munneke et al., Picard et al., Pirrazini et al., Warren and co, Zender and co, etc).

P2 L18: Are these values here for optical grain size ? Is the reference about optical grain size or other grain size metrics ? The sentence should be more precise about this.

P 3 L7: It is not clear if the sentence is about the impact of the albedo-grain shape or BRDF-grain shape. Both are quite different and have different impact depending on the algorithm used for the retrieval. For the former issue, the value of 2.6 is extreme. See Kokhanovsky and Zege 2004, Picard et al. 2009, Libois et al. 2013 for other works on this question.

About P3, Libois et al. 2015 and Picard et al. 2016 (both in The Cryosphere), have produced time-series of SSA (3 years) at Dome C in Antarctica which seems relevant to the present study. See also Jin et al. 2008 and Picard et al. 2012 for earlier (and

shorter) time-series of SSA in-situ and satellites measurements.

P3 L12-13: "However, polar orbiting satellites do not provide a sufficiently high temporal resolution that may resolve snow precipitation events and snow metamorphism that typically can advance in a matter of hours.". With the number of satellites (MODIS, Sentinel, SSM/I, AMSU, ...) and the convergence of the orbits in the polar regions, sub daily resolution can be achieved.

P4L8: According to Gallet et al. 2009, IceCUBE (i.e. DIFUSSS with 1300 nm only) can not be used from SSA of 60m2/kg and above. If the "in prep" paper shows different results, it would be interesting to add a sentence here. Otherwise the limitation should be indicated.

P4 L15: invert the temperatures

Fig 1: I'm not convinced that this first figure (the octa plot) is useful for the paper. It shows raw data that are not used as is in the following and the interpretation is short and inconclusive. Instead, I suggest to plot a time-series (a normal one as for the air temperature, not a time versus date graph) cloudy/not cloudy as used by the algorithm to switch between diffuse/direct radiation.

P5L1: "polar day". This term is ambiguous and the sentence is not strictly logical, air temperature decrease observed before the end of the "polar day" is due to a change of solar elevation, not a change of day duration.

P5L10: where these values of uncertainty are coming from ? As explained in the general comment, the description of the CORAS instrument and the evaluation of the uncertainty are missing. This part needs to be expanded but at least one paragraph. Please also consider the shadowing effect, frost formation, . . . Information such as the cosine correction method, the frequency of observation, and other instrumental details are needed as well.

P7L20: Regarding the choice of the values of B and g in the "middle of the range":

1) Values obtained from measurements are now available. Libois et al. 2013 and 2014 have measured B and recommended some values of g based on measurements.

2) The uncertainty of these values impacts the SSA estimation and needs to be taken into account in the evaluation of the uncertainties proposed in this paper which only considers instrumental uncertainties.

P8L5-L8: A few critical information are missing about the MODIS algorithm. - Which BRDF parameterization has been used and how ? - How the radiances have been converted to ground-level surface reflectances ? - Has an atmospheric model been used ? - Which product and version have been used here ?

P9L13: The argument implicitly assumes that the calibration error is wavelength-independent, which is often not the case. Even if the ratio R is less sensitive to calibration issues, the residual wavelength dependence can greatly affects the estimation of SSA as shown in Picard et al. 2016 (e.g. cosine correction, 100% direct assumption...).

P8L16: these values have been obtained for perfect clear-sky. To my personal experience, direct/diffuse ratio shows huge variations in the infrared due to thin cloud (more than in the visible because the diffuse component is already quite large). Calculations of the atmosphere model with cloud with varying optical depth and crystal size would be useful, because the estimation of SSA using ratio (i.e. Eq 9) is sensitive to the spurious wavelength-dependence resulting from cloud cover.

P10L7: I suggest to improve a little the justification of the constance of SSA during the day. E.g. doi:10.5194/tc-8-1205-2014

P10L10: the surface roughness/slope at the footprint scale is another likely player that affects the theory used here. See e.g. Dumont et al. 2017 in TC (see the slope estimation).

P14L5: the argument on the scales seems weak because the airborne data are between the in-situ and MODIS data in terms of scales and MODIS shows little bias w/r

to in-situ measurements. I guess that the authors have analyzed in details the spectra and various sources of errors from the different sensors without success. May be add part of this analysis. For instance, showing coincident spectra from the different sensors could help to understand the differences, and at least to rule out some potential defects of the sensors.

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