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Interactive comment on "Correction of albedo measurements due to unknown geometry" *by* U. Weiser et al.

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We thank the reviewer for the helpful comments and suggestions on how to improve the manuscript. In the course of revision of the manuscript several changes were made. Therefore, some of the suggestions have been included and others were obsolete due to the changes. Please see our answers to the points below:

The authors present a method to correct errors that are introduced to glacier surface albedo measurements by sensor tilt relative to the surface. The method is based on a comparison of the measured radiation data at the study site with measurements made at a nearby, not-tilted sensor installation. I compliment the first author on the development of a method that is well worth to form the master thesis cited in the references section. Such a master thesis is mainly meant to show and demonstrate methodical

knowledge. However, and unfortunately, this is not enough for the content of a scientific paper. The study in its current stage and as it is presented in the given manuscript does not at all meet the high quality criteria demanded by a high-ranked journal like The Cryosphere. The manuscript is poorly written. It that lacks any red thread over most parts except for the method description. The introduction is missing a clear and comprehensive over view and explanation of the theoretical background of snow albedo physics. Topicspecific terms (e.g. "cosine measurement error", "cosine law" or "cosine error") that are not straightforward to understand for readers without background in solar geometry are never explained. Measurement principles of sensor are, in contrary, explained in too lengthy detail. The methods section in total is also much too lengthy and given with far too little illustration so that the reader easily gets lost on the way from Eq. 1 to Eq. 27. The results section is exclusively limited to examples and needs to be much more comprehensive. The discussion section is simply a stringing together of single notes without any identifiable ideas behind. Apart from these more editorial concerns I have a couple of very serious, methodrelated issues that prevent me from supporting any further consideration of this manuscript. I therefore refrain from giving detailed comments and only list my major concerns in the following:

1) The presented method is only applicable for days with at least 2-3 hours of sunshine, it needs to be calibrated separately for each day (but this cannot be done in a fully automated way) and a reference measurement that needs to meet very high quality criteria needs to be available in the vicinity. I assume that the method is meant to be applied in glaciology. However, given the above mentioned serious drawbacks regarding its straightforward applicability I cannot see any benefit at all with respect to potential future applications of this method. Especially as there are simple, small and rather inexpensive sensors that can be mounted to automatic weather stations to continuously measure the instrument's tilt adequately (and without cloud-cover related restrictions or the need of high quality reference measurements that are rather unfeasible in the framework of a glaciological field measurement setup). The fact that this method is also applicable for days with 2-3h of sunshine improves and expands it, because com-

pletely clear-sky days are rare. For accurate corrections tilts and directions have to be calibratd for each day, which can be improved and it is explained in the Discussions. We expanded the method for cases where no reference measurement is available with the usage of a high resolution radiation model. This model is explained in details and the results are compared with the results from the reference measurements. With the introduction of this radiation model the dependency of horizontally leveled reverence measurements is eliminated. It is explained in details why automatic tilt sensors are difficult to use for the accuracy of this method.

2) The consideration of diffuse radiation that is known to have the potential to strongly influence snow surface albedo is rather insufficient and maybe even misleading. The method compares albedo measurements at two sites without taking into account (or at least discussing) influences of differing sky view factors or cloud conditions. The method does also not account for different spectra of light (induced by these varying cloud conditions) that are reflected differently at the snow surface and thus lead to different surface albedos. Finally, and most important, the differentiation between direct and diffuse radiation in Eq. 15 and 16 is a very rough assumption rather than any profound physically based theory. The basis for these two equations is formed by Eq. 5. This equation describes the reduction (not the "weakening") of direct solar radiation due to absorption and scattering on the way through the atmosphere. Diffuse radiation originates from these scattering processes so how can it be calculated like given in Eq. 16? This does not make sense at all. Or even further, if this is not complete nonsense it needs to be by far better motivated, explained and referenced. Apart from that, the partitioning between rho(dir) and rho(diff) seems to be based on assumptions only. If this is really the case, it is not a valid approach for an in general so accurate and complex correction method. It is explained why albedo does not have to be corrected on diffuse days. Furthermore the diffuse part of the incoming is considered and explained in details why it can be assumed with p_diff$\sim$10%. Data from different Suntrackers and a radiation model are considered over a long time period to come to this assumption. The reflection of snow surface is assumed isotropic, considering additional added

references and explained in more detail.

3) The most crucial step of the presented method is the calibration of the two parameters epsilon and V. However, the description of the calibration process is completely insufficient and limited to a single statement regarding which method of fitting is used. No calibration results are visualised or explained in detail. No error assessments or sensitivity studies are carried out at all. This is not acceptable as the main parameters that form the heart of the method need to be given with appropriate uncertainty ranges in order to be able to judge about the reliability and final accuracy of your albedo correction. Also a visualization of the C values (Eq. 24) is completely missing and the reader is not able to judge whether or to which extent the criterion of similar C values across the diurnal cycle is really met or not. Epsilon and V are based on references. These parameters are explained in more details now, more references are added and the ranges of their results are discussed. The workflow of the method and the used equations are explained in more details for a better understanding. All results are visualized in the Tables and Figures, which have been expanded. Sensitivities and uncertainties of the used sensors are explained in the method section. Standard deviations are listed with the results and an additional table was added, where the mean bias error (MBE) and the mean absolute error (MAE) of corrected albedo values are shown. C is an auxiliary factor to find accurate tilts and directions of the slope. C is introduced and omitted later. Some major changes were made for a better understanding why C is needed for a calibration with the method of least sqares.

4) In total, calculations have been done for four days only. The question that needs to be asked is if the introduction and demonstration of a newly designed method for the example of only four days is sufficient to prove reliability, stability and transferability of this method. I doubt that. Issues of varying solar zenith angles over the year are not taken into account. Nothing can be derived about systematic temporal cycles or pattern of the method's parameters. No statements can be given about the performance of the model under varying cloud cover conditions (which is crucial when dealing with the

accuracy of albedo measurements). At the current stage, your study does not at all prove to be transferable, not in time nor in space. The calculations were done for a time period of almost two years, for every clear-sky day and days with at least 2 hours of clear sky. Also Figure 9 shows all corrected and not corrected albedo values and their over- and underestimations in two scatterplots for the two observed glaciers. It is explained in details and referenced, why albedo doesn't have to be corrected on cloudy days.

Taken together there is no other possibility than to reject this study (and thus the related manuscript) in its current stage. However, it would be great to see the authors investing more work in this topic. The above mentioned issues needs to be accounted for and, most important, the dependency on any unfeasible reference measurements and the manual determination of data cut-offs definitely need to be eliminated. If these goals could be accomplished I would strongly encourage a resubmission of a (better written and better structured) manuscript as in this case the method could really be of importance for postprocessing of glaciological fieldwork.

Additional changes of the manuscript: Abstract, Introduction, Discussion and Conclusions rewritten. High resolution radiation model introduced to improve and expand the described method. Solar zenith angle limited to 50°, where no albedo dependence occurs. Appendix with all used symbols and their units was added.

Interactive comment on The Cryosphere Discuss., 9, 2709, 2015.