The Cryosphere
Discussions

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

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:

Weiser and colleagues present a methodology to correct tilted albedo measurements over a non-flat snow/ice surface using nearby leveled shortwave incoming radiation. The method is based on the assumption of constant diurnal atmospheric conditions, constant diurnal albedo values and constant sensor tilts and surface slopes. They apply the methodology for some days by comparing the modeled albedo with the observed albedo, but never validate the retrieved tilt, slope angles.

2 Assessment Although Weiser and colleagues present a study on a interesting topic for the TC community, the manuscript has too many issues in its current status (see major and specific comments) to warrant publication in TC. Therefore, I would recommend a resubmission where the authors can tackle the issues mentioned in this review and in the review reports of my colleagues which all raise valuable concerns.

3 Major comments: âĂć The manuscript is poorly written. The English needs a thorough revision and the structure of the paper is poor. For example, both the introduction and discussion lack a comprehensive overview and seem a collection of loose ideas without a clear rationale. Both sections also fail to put the methodology in a context in terms of applicability. Also the references lack a clear overview of the state of the art in the domain. The methodology section on the other hand is extremely technical and often difficult to follow. âĂć The results are based on some illustrations and examples, but fail to provide any idea on the accuracy of the method, applicability, etc. âĂć Although the methodology is interesting, it is based on some assumptions which are difficult to defend. Firstly, the methodology assumes a constant diurnal albedo and therefore does not account for diurnal variations in albedo, which can range above 0.1 depending on the solar zenith angle. Secondly, it does not account for any other physical condition that can have an effect on diurnal albedo (e.g. surface roughness; Lhermitte et. al.). Thirdly, the method fails to provide any correction of cloudy conditions and therefore still limits its use to calculate daily albedo values, etc.). Fourthly, the method assumes that the sensors only 'sees' the sky (upward sensor) or snow/ice (downward sensor), whereas this is often not the case for tilted surfaces: e.g. differences in skyview factors or a downward facing sensor which receives radiation from nearby mountains etc. âĂć Although the method is very interesting, it is, given its dependence on a 'third' leveled sensor nearby, very difficult to apply in real polar conditions, where the installation of an unattended leveled AWS is practically almost impossible. I think this drawback of the method should be clearly discussed.

4 Specific comments: p2710 L20: automatic tilt meters can be installed to make such

measurements in realtime (e.g. PROMICE data set) An explanation on why it is difficult to use automatic tilt meters for the accuracy of the described method is added and described in details.

p2710 L22: "changing physical conditions" What is meant by that? Does this include changes in diurnal albedo (e.g. Gardner and Sharp, doi:10.1029/2009JF001444.) and/or changes in roughness and homogeneity of snow cover in the surrounding of the measurement site (e.g. Lhermitte et. al., www.the-cryosphere.net/8/1069/2014/) Due to changes in the manuscript this part is now changed. Explained in details.

p2710 L25: 'ideally southwards': ideally a sensor is not tilted at all. On the other hand, the only reason to prefer a direction, has to do with the shadowing effect. Because all other tilt effects could theoretically be corrected for if the tilt is known. In this context, 'southwards' is only true for the northern hemisphere. Due to changes in the manuscript this part is now changed.

p2711 L1: the cosine law does not introduce the errors. The assumption of a flat surface/sensor when it is not flat, introduces the error and this error could be corrected using the cosine law. Due to changes in the manuscript this part is now changed.

p2711 L3: Many publications: true, but the introduction and discussion should benefit strongly of a more comprehensive overview of these publications. Some examples of publications worth including are: MacWhorter, M. (1991). Error in measurements of incoming shortwave radiation made from ships and buoys. Journal of Atmospheric and Oceanic Technology. Van den Broeke, M., van As, D., Reijmer, C., van de Wal, R. (2004). Assessing and improving the quality of unattended radiation observations in Antarctica. Journal of Atmospheric and Oceanic Technology, 21(9), 1417–1431. Thank you, many references added.

p2711 L28: 'measured tilts and directions': the words tilt, surface, angle, directions are often used confusingly. A sensor can have a tilt, which has a zenith angle and azimuth angle in a cer tain direction, whereas a surface can have a cer tain slope with

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corresponding slope angle and azimuth angle. In my opinion, a glacier/snow surface is not tilted. Glaciers that represent the slope in the described model are not horizontally flat, they are tilted with an aspect or in a certain direction. Sensors (pyranometers) are inclined with an aspect or in a certain direction.

p2713 L13: what about variations due to variations on solar zenith angle, cloudiness, etc. (Gardner and Sharp) Due to changes in the manuscript this part is now changed. SZA dependency of albedo was eliminated and cloudiness was explained in details.

p2713 L19: Not all AWS's are drilled into the ice. Some setups use tri- or four-pods standing on the ice. The AWS used for this method were drilled into the glacier, but even AWS that use 3- or 4-pods are tilted.

p2713 L20 "estimate reasonable diurnal mean albedo values". This is certainly true, but how is the proposed method going to change this without a valuable method that accounts for cloudy albedo values Due to changes in the manuscript this part is now changed.

p2713 L24: What is the slope of albedo variation? The Method section 2.1 should clearly indicate how many times was measured etc. Due to changes in the manuscript this part is now changed.

p2715 L4: changes continuously? What is meant by continuously (ever y day, five minutes) and how much does it change? Moreover, if a data logger is connected to the inclinometer the sensor tilt can be logged over time. Due to changes in the manuscript this part is now changed.

eq. 2 is irrelevant for the rest of the story It is relevant and explained why.

Fig.2: Seems rather irrelevant Relevant for a better understanding of a complex method

p2716 L16: 'are used from here on': Why aren't uniform symbols used from the start. It would certainly increase the readability of the manuscript. Moreover, I would recommend to use clear subscripts. The subscript tilt for example can create confusion as

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both the pyranometer and surface can show a 'tilt' in the definitions that are given. An appendix with all used symbols, their meaning and their units was added.

Eq. 9: this is not necessarily true as the downward facing pyranometer, might also be receiving radiation from other terrain parts within its field of view. For heavily tilted pyranometers, for example, the downward facing pyranometer might 'see' parts of the horizon or nearby mountains etc. Explanation added that downfacing sensor only sees the glacier surface.

p2718 L11: and how is the downward facing pyranometer leveled? Due to the fact that both pyranometers use the same housing (see also: Section Albedo measurements), they have the same tilts.

p2718 L12: This also assumes that the surface is completely flat and homogeneous (e.g. Lhermitte et. al) Explanation added.

p2718 L18: part: I assume this means fractions between 0 and 1? Part is very unclear and does not necessarily imply that the values are between 0 and 1. On clear sky days the total incoming radiation consists of a direct beam (part) and a diffuse part (blue sky).

Eq. 16: I don't understand the logic for Eq. 16 and I think it should be proven in this case. Normally the diffuse radiation is dependent on the sky view factor, the solar zenith angle (i.e. longer atmospheric path -> more scattering, etc) Explanation and assumption of the diffuse part of the radiation, including references, added.

p2719 L17: 'Irrelevant': this is not true, depending on the tilt angle, the sky-view factor (or perhaps better ground-view factor) will change, i.e. the pyranometer will see more of the surrounding mountains, horizon, etc. Explained earlier.

Eq. 18: The derivation of Eq. 18 is very confusing as in Eq. 10 it is still defined based on solar zenith angle. Moreover, eq. 18 is only true for 100 Eq. 18 'rephrased'

Eq. 19: Where does the $\cos \Theta$ s come from Explained in the text.

p2721 Assumption 3 is really problematic as the solar zenith angle, changing surface properties (Gardner and Sharp) as the surface properties (Lhermitte et. al.) will have an effect on the diurnal albedo which certainly cannot be neglected (i.e. variations of 0.1 on an albedo of 0.7 due to SZA alone) Due to changes in the manuscript this part is now changed.

p2721: Why a constant diffuse fraction is assumed if the diffuse fraction could be expressed as a function of the solar zenith angle? Due to changes in the manuscript this part is now changed.

The workflow is often very unclear. E.g. p27222 L1 'for one specific day': so the sensor tilt is only determined once and assume it constant afterwards? Moreover, it is always guessing what has been performed exactly on what data. This should be clearly clarified. The workflow is described in details for one example day. This was made for every clear-sky day and days with at least 2 hours of clear sky in a time period of almost two years.

p2722 L12: it is very unclear which equations to minimize. Explained in details which equations are used.

p2723 L15: As constant as possible. First, it is physically wrong to assume a constant albedo (see previous comments). Second, what is 'constant as possible'? What is the range that is allowed? Is this based on some minimization? Rephrased and information added. C is a constant value for the day in progress.

p2723 last line: equation does not have a number Removed.

Eq. 27: how can you estimate errors in balance if the method does not allow to account for cloudiness? Explained in details with references earlier in the text an in the Discussions.

p2725 L4-10: This should be in the method section. All these setups are now never explained in the method section and appear in the result section, where they don't

belong Due to changes in the manuscript this part is now changed.

p2726 L4: So there is no diurnal variation in pdiff? The diffuse part of the incoming radiation has no diurnal variation, see also Methods and Discussions.

p2726 L16-20: Ok, but how certain are we that corrected one is effectively the correct one, when there is no correct measurement to compare it with? Due to changes in the manuscript this part is now changed.

p2726 L21-22: this is kicking in an open door as it is already the motivation of the article Due to changes in the manuscript this part is now changed.

p2727 L12 'Over the year 2011' how is this assessed when the method has no way to account for cloudy observations? Explained earlier, in an appendix I send you two figures that show typically diffuse/cloudy days and should clarify why albedo does not have to be corrected.

P2728 L1-3: True, but also for small angles (and even for all non-flat surfaces) the sensor can also 'see' neighboring mountains, etc. . . Explained earlier.

p2728 L12: without reference measurement the method can indeed not be used and this completely limits the use of the method as getting such a reference measurement is practically impossible (e.g. no flat nearby terrain on large ice caps etc.) Due to changes in the manuscript this part is now changed.

p2728L17: adjusted parallel. This is practically impossible and I challenge the authors to perfor m such a setup. Installing a flat unattended sensor is already 'impossible' Due to changes in the manuscript this part is now changed.

p2728L21: 'High' or low etc. Due to changes in the manuscript this part is now changed.

p2728l9: Winter months: Sep and June are not a winter months. Moreover, this is hemisphere dependent. I also challenge the authors to apply this method in polar

areas as it practically impossible to do a flat unattended reference measurement over ice caps etc. Due to changes in the manuscript this part is now changed.

Fig.9 is SWout, not albedo Albedo is defined in Eq. (1) $\rightarrow$ reflected radiation/incoming radiation $\rightarrow$ the measured and corrected values are presented in a scatter plot, the result is the measured and corrected albedo.

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.