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

Anonymous Referee #2

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General comments

This paper presents a method to correct snow and ice broadband albedo measurements affected by tilts of the surface and pyranometer, when the latter are unknown. For this, the tilt of the pyranometer is first estimated using a reference measurement from a nearby leveled sensor. The tilt of the surface is then fitted in a simple radiative transfer model to match the measured diurnal cycle of albedo, assuming that the true albedo is constant over a day of measurement. Once both tilts are determined, the true albedo of the surface can be computed from the measured one. The question of albedo measurements errors due to tilts is critical because i) these errors can significantly impact the estimated surface energy budget of snow and ice surfaces and ii) such albedo measurements are used in a wide range of applications by users not nec-

essarily aware of the complexity of performing accurate albedo measurements. Hence proposing a method to correct albedo measurements is of great interest and the ideas developed in the present paper are interesting. Unfortunately, the method proposed relies on questionable assumptions. In particular, it neglects the dependence of snow albedo on solar zenith angle, which represents a significant shortcoming. In addition, the overall manuscript is poorly written, the introduction and discussion being particularly hard to follow for the reader. The structure generally lacks of organization and clarity which makes very difficult for the reader to understand the ins and outs of the method. The multiplication of inappropriate or approximate terms along with too abundant equations exacerbates this feeling. As is, the manuscript does not meet the standards required for publication in The Cryosphere, and should not be accepted unless substantial parts are entirely rewritten and major corrections are made.

Specific comments

1) The manuscript is overall poorly structured, with many repetitions, misplaced information and inappropriate content. Several paragraphs are made of a single sentence which perturbs the flow of reading.

The abstract could be substantially improved, for instance by adding a context sentence and illustrating the main results with numerical values.

The introduction fails to introduce the context, issues and approach of the study. These ideas are indeed presented in a very fuzzy way, without an obvious consistent organization. Hence it is difficult for the reader to understand what the authors really aim at doing before the Methods section. The last paragraph is more clear but a description of the paper organization would be very helpful at this point. I would recommend the authors to rewrite completely the introduction, following generic steps such as: i) Context: surface energy balance of snow surfaces critically depends on snow albedo ii) Problematic: accurate albedo measurements are difficult to perform because of tilt errors iii) Objective: developing a method to correct albedo measurements since current

methods are not satisfying iv) Approach: simple geometric considerations and use of a leveled pyranometer to estimate successively pyranometer and slope tilts, from which the true albedo can be retrieved.

The Methods section is more clear but several paragraphs are unnecessary or should be moved to the introduction. Many equations are displayed while some of them could/should be skipped. There is some redundancy between the model description and the algorithm description that come in two distinct sections. See more details in Technical corrections.

The Results section is too abrupt. For each experiment described, the context should be reminded to the reader for more clarity.

The discussion is currently a succession of independent sentences that form individual paragraphs. It contains information that should be placed in the introduction or Methods and does not discuss much about the results.

As for the conclusion, it does not provide any perspectives for future work or consequences of this research, while this is the main interest of proposing a method to correct albedo measurements.

2) A major flaw of the study is the assumption that the albedo of a snow surface is constant throughout the day. In fact, snow albedo varies with the solar zenith angle (SZA), which generates a diurnal cycle (e.g. Wang et al. 2011). This effect might be negligible compared to tilts errors when the latter are very large (e.g. 25° in the text) but probably becomes significant for small tilt errors and at high SZA. As a consequence, using a concrete surface to validate a method dedicated to snow surfaces is not ideal because snow and concrete do not reflect light the same way. I'd recommend to use a parameterization of albedo that accounts for this dependence. Carroll and Fitch (1981), Gardner and Sharp (2010) and Kuipers Munneke (2011), among others, propose that kind of parameterizations. At least, the limits of the constant albedo assumption should be discussed in more details, and the method adapted in consequence.

3) The manuscript makes reference to only 17 studies (10 in the introduction). This is clearly not enough for a topic that has already been largely investigated. This number should be at least doubled to strengthen the argumentation and method. Some suggestions are made in the Technical corrections. Currently, the few studies used as references are poorly used. In the introduction they mostly appear as a concatenation of previous works without any clear progression from one to another. Furthermore, the description of these studies is often unclear (e.g. Dirmhirm and Eaton (1975)).

4) The use of inappropriate or unusual terms in the text (e.g. "global radiation" or "flat zenith angle") sometimes makes it complicated to understand their meaning. The unnecessary multiplication of intermediate symbols in the formula also participates to an apparent complexity of the method while it is actually not complicated. Efforts should be made to make the reading easier.

Technical comments

NB: Italic indicates suggested vocabulary changes

Title: It is too fuzzy. What kind of albedo is corrected? Broadband, spectral? On which surface? Any, concrete, snow, glaciers? What does geometry refer to? Also, the correction is not "due to" unknown geometry, it is a necessary consequence of it. Suggestion: "Correction of [broadband] snow albedo measurements affected by unknown slope and sensor tilts"

Abstract: l.1: This first sentence is vague. "can be relatively high" should be more quantitative. The tilt errors (slope and sensor) should be mentioned as soon as possible. l.2: Clearly state that the present paper proposes a general method of correction. Then describe the method. l.6: is needed – is used l.10: can be corrected – are corrected

Introduction: p.2710 l.1: remove "reflected solar radiation and hence" because reflected solar radiation is determined by (not "depends on") albedo. Add reference. l.2: the surface energy balance of a glacier defines... l.20: before saying that tilts are dif-

ficult to estimate, state that tilts alter albedo measurements l.23: what are “physical conditions”?

p.2711 l.1: what is “the cosine law”? l.2: “other measurement errors and uncertainties” is unclear l.3: you mention “Many publications” but don’t provide a single one with such numerical values l.9: “specular components of daily albedo” is unclear l.19: add reference l.23: detail the kind of “problems” mentioned in that paper?

p.2712 l.7: specify why snow albedo changes with time? Maybe mention SZA effect, metamorphism, preferential orientation of surface roughness... (e.g. Pirazzini et al., 2004) l.9: it is not clear why they consider “the extinction through the atmosphere”. More generally the work of Allen et al. (2006) is difficult to understand l.12: “in the following” is awkward. Prefer “contrary to” in the next sentence. l.19: clouded – cloudy (also elsewhere in the text) l.20: it is not clear what Weiser (2012) has done with regards to Mannstein (1985)

Methods p.2713 l.4: title of subsection does not sound like a method. Most of this subsection should be merged with the introduction l.5: avoid to make a reference to an equation that appears later in the text l.5: “in turn” is inappropriate, there is no causal relation between both assertions. l.8: global - incident l.13: add reference l.15-17: add reference l.18: explain why tilts increase over time

p.2714 l.1-4: too general. The reader has no idea how the method concretely works l.9-12: consider removing this paragraph and adding a reference after “pyranometer” instead l.17: cosine error should be defined or a reference should be added (e.g. Grenfell et al., 1994) l.20: maybe add GPS coordinates and Table 1 here l.26-28: is the full description of the inclinometer necessary for the understanding of the method?

p.2715 l.8: at this stage, it is not clear what the optical properties of the atmosphere are and why they are needed? l.18: the method has not yet been described, so the reader does not understand why measurements on concrete are presented. l.20-21: how does this reference serve the manuscript? l.22: for sake of clarity, it might be useful

to have an overview of the method with the main steps before to start the detailed description of each step. This might correspond to subsection 2.3.

p.2716 l.3: check the consistency of the terms (irradiance is in Wm-2). The various terms “irradiance”, “solar radiation”, “radiant flux” are quite confusing. Do they all actually correspond to distinct quantities? l.15: This formula is probably valid only for clear atmospheres without multiple scattering. If this is the case, specify here (and maybe in the abstract and/or introduction) that the method is valid only for clear skies. l.22: please clarify the difference between Sterr and I. Also it seems that Sterr is the measured solar radiation, not the full solar radiation as suggested by the definition l.10.

p.2718 l.2: is it necessary to describe the idealized case with only direct radiation if later on the diffuse part is accounted for in the method? I’d recommend to introduce the diffuse part from scratch.

p.2719 l.9: “derived” is awkward because Eq. (13) does not contain Fdir and Fdiff as expected. Just keep the end of the sentence that introduces the albedo formula.

p.2720 Eq. (18): remove the last term and reverse the 2nd and 3rd terms for clarity. But again, why to introduce this equation when the more realistic/general Eq. (19) comes just afterwards? l.4: specify here that υp is derived as in Eq.(7) because “inclination angle” is not clear. Eqs. (19) and (20). Use αmeas instead of the ratio. l.16: assumptions can be made, but they should as much as possible be supported by relevant references and/or discussed in the discussion if questionable. l.19: two objects with different dimensions are compared: spectral range (wavelength in microns for instance) and irradiance (Wm-2)

p.2721 l.2: the assumption about constant albedo cannot be used without a reference to support it, especially because it is a major shortcoming. l.3: add reference

p.2722 l.3: Reference to Eq. (13) is not straightforward. l.8: Eq. (22) should appear on p.2720 when υp is first introduced l.22: use a proportionality sign rather than “=”

p.2723 l.9: Eq. (25) should appear on p.2720 when υt is first introduced l.11: the optimization method is not clear because l.11 suggests that C is optimized while σt and γt actually are. Eq. (26) meaning is not clear l.18: it seems that the true albedo could be derived simply from Eq. (23) now that υt is known, so that Eq. (20) appears useless. This equation should anyway not be rewritten here.

p.2720 l.1: what is the "opening angle" of a pyranometer? Is it a field of view, an apparent SZA? l.2: "flat" zenith angle – high SZA l.3-6: this sentence is probably not necessary l.7-14: this subsection seems useless. It could serve as a start for a discussion but should be removed from the methods

Results: p.2724 l.21: Do these values compare well with known measurements or previous studies? l.21: "which occurs" - as a result of

p.2725 l.4-5: Keep only (Step B) in parenthesis l. 4-6: Are you applying the method to a specific case study? Then mention it because it is not straightforward for the reader. l.7 subsequently (to what?). σp is very large, is it realistic for in situ measurements? l.8-10: consider removing this sentence l.11: keep this sentence with previous paragraph

p.2726 l.1-3: this should be mentioned when detailing the model assumptions l.9-10: this sounds more like a conclusion of this subsection rather than an introduction l.20: where the actual tilts measured at some point to validate the retrieval?

Discussion: p.2727 l.18-19: the method was described for clear-sky days. How can it be applied to mostly cloudy days? Why 2-3hrs? How does the method deteriorate with less time to perform the fit? l.24-26: I don't understand the point of this sentence.

P2728: l.10-16: this is certainly one of the most understandable paragraph in the paper and the whole discussion should be built on that kind of statement. l.17-20: after a whole study on the impact of tilts this sounds like a common place. This should be moved in the introduction or just removed

Conclusions: p.2728 l.24: This sentence is awkward. Just say that a method was

developed to retrieve the tilts and directions of sensors and slopes in the case these parameters can hardly be measured in situ. This could be moved to the introduction. l.26: to compensate - to overcome

p.2729 l.3-7: the description of the method is not understandable at all. l.11: "prove" - validate l.12: again the validation of a model dedicated to snow measurements using a concrete surface is very questionable

Tables: 2: Remove "results" from the caption. "Corrected" - retrieved 3: same as 2

Figures: 2: the solar azimuth is not clear. Maybe add dots starting from the incident beam to show the correspondence. 5: increase labels size 7: increase labels size and figure size. 8: Add ylabel

References

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