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Interactive comment on "Albedo over snow and ice penitents" *by* J. Abermann et al.

Anonymous Referee #2

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General comments

The paper presents analyses of ground- and satellite-based albedo measurements over a penitent-covered glacier. The subject is very relevant because of the practical difficulty in carrying out ground-based measurements over those surfaces, the difficulty in assessing error estimates for both ground- and satellite-based albedo retrievals over glaciers, and the large impact of surface albedo on the mass budget of glaciers, which are undergoing substantial reduction in many areas of the world. The paper addresses the measurement uncertainties related to the vertical displacement of the sensor above the surface of the glacier, and the temporal evolution of the glacier albedo during two summer seasons. Both are very relevant scientific questions. However, the first part of the paper related to the vertical profiles of albedo suffers from serious misinterpretation and improper methodology, thus almost only the second part of the data analysis, related to the albedo time series, is acceptable for publication. I therefore recommend

a major revision of the paper, which should properly account for all the criticism listed below. Moreover, a through language check should be made to improve the fluency and eliminate the grammatical errors.

Main problems

1) The first criticism concerns the attempt to measure the albedo inside the penitent troughs. The utilized method is inadequate, as only the reflected radiation is measured below the tip of the penitents. To infer the effective albedo there, which would also give a measure of the solar radiation absorbed inside the troughs, the downward radiation should be measured at the same time and at the same vertical level as the upward looking sensor that measures the reflected flux. The authors define the albedo inside the penitent troughs as the ratio between the reflected radiation measured inside the trough and the downward radiation measured above the penitents, but this definition is physically inconsistent, as the amount of solar radiation reaching the interior of the troughs is much less than the downward radiation above tip of the penitents. The whole discussion related to these measurements inside the troughs is unphysical, as it is based on the idea that the shaded areas inside the troughs have a lower albedo than the areas exposed to direct sunshine. This is wrong, and in fact the opposite is true, the snow/ice albedo is higher when illuminated with diffuse radiation, which is richer in the wavelengths for which snow has higher albedo. It follows, thus, that a general misunderstanding characterizes the author's interpretation of the measurements: the effective albedo of a penitent field is lower than that of a glacier with flat surface not because of the presence of shadows inside the troughs, as suggested by the authors, but because the intense multiple reflection inside the troughs cause more radiation to be absorbed by the penitent vertical walls, and because the effective solar zenith angle over a rough surface is lower than over a flat surface (the authors are invited to read and refer to Warren et al. (1998), Effect of surface roughness on bidirectional reflectance of Antarctic snow, J. Geophys. Res., 103, 25789-25807).

2) A second serious problem is the misinterpretation of the measurements above the

tip of the penitents. In the author's opinion, the albedo measured at the level of the tip is the effective albedo of the penitent trough (p. 3833, line 6). In reality, what is measured there is the effect of shadows and tilted walls on the radiation received by the sensor, and it cannot be regarded as the albedo of anything, neither the walls nor the trough. It is rather an "apparent" albedo (see Pirazzini (2004), which is referred to in the paper). The real albedo of a single trough or penitent simply cannot be measured with hemispherical pyranometers. The optimal distance above the surface to measure the effective albedo of a rough field depends on the sources of errors (shadows, tilted walls), which are related to the geometry of the rough elements (again, see Warren et al., 1998). Below that distance, the calculated apparent albedo depends on the particular location of the sensor with respect to the nearby roughness features, on the solar azimuth angle, and it changes by moving the sensor few centimeters/decimeters apart. In conclusion, the whole discussion on the vertical profiles of albedo (including equations 1-4 and figures 6-7) is incorrect.

3) The study on the vertical profile of albedo had the purpose of assessing the measurement uncertainties of the albedo time series obtained from the AWS. This should be more clearly stated. Were the measured vertical albedo profiles sufficient to assess the quality of the AWS albedo? On p. 3834, lines 7-20, the authors claimed that their profiles demonstrated that the AWS measured albedo are representative of the surrounding surface. Maybe, but you have too little data to draw conclusions. On the basis of Figure 4, my impression is that the sensor should be placed three or more meters above the penitent tips to measure the effective albedo, but higher and more profiles would be needed to reach a conclusion. What was the range of distance that the AWS albedometer had from the nearby penitent tips? On the basis of the presented data, the authors could extract an averaged error for the AWS albedo measurements, but with the warning that it could be an underestimation due to the limited dataset.

Specific comments

-p. 3824, line 5 of the Abstract: the expression: "the vertical dependence of albedo"

is unclear. I would rather express the concept as "the dependence of the measured albedo on the vertical distance from the penitent-covered surface"

-p. 3826, line 4: here a definition of "effective albedo" is given referring to previous work, but a sentence explaining that the same definition will be applied in the present work (if so) is missing.

-p. 3828, lines 26-27: the vertical profiles of albedo are calculated from the ratio of measurements taken with sensors having different spectral range. How this is accounted for in the calculation?

-p. 3841, Table 1: in the caption (and everywhere else in the paper) the ratio H/D (penitent height / penitent distance) should be replaced by D/H, as it is calculated in Table 1.

-p. 3835, lines 7 and 12: how many digits after decimal point are really significant for the bias between AWS and satellite albedo? Definitely less than three, on the basis of the error estimation for AWS albedo.

Interactive comment on The Cryosphere Discuss., 7, 3823, 2013.