

Interactive comment on “A Retrospective, Iterative, Geometry-Based (RIGB) tilt correction method for radiation observed by Automatic Weather Stations on snow-covered surfaces: application to Greenland” by W. Wang et al.

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Received and published: 23 December 2015

This paper presents a really interesting new methodology to correct automated short-wave measurements from stations tilt. This is an important issue for radiation budget and surface energy balance over snow covered areas and the solution proposed by the authors “only” required simulated broadband shortwave irradiance and cloud fraction. The results show improved SW and albedo compared to two reference datasets. The authors also provide an interesting investigation of station tilt variability versus melt intensity.

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This study fits well with The Cryosphere scope but I think that several comments should be addressed before it can be published. This is detailed in the following.

Main Comments

1/ My first main comment is on the equations used in the methodology and discussed in the section 3.2 and 3.4. From equation (2), it seems to me that $I_{b,h}$ is the direct incident radiation in the direction of the solar beam and $C=I_{d,h}/I_{b,h}$. However, no definition of C is given, $I_{d,h}$ is defined as the “direct part of SW radiation on the horizontal surface” and the above definition of C is, if I understand correctly, not compatible with equation (7) which requires that $C=diffuse/total$. These questions must be clarified in a future version of the paper. In addition, though I agree with equations 4 and 5, I think that they must be further explained and justified.

2/ The imperfect cosine response of the sensor is another of the major sources of error in radiation measurements especially when the solar zenith angle is high as noticed by the authors in the introduction. I think that the discussion should include a more quantitative assessment of the comparative errors induced by tilt and imperfect cosine response based on former studies on the angular response of optical sensor.

3/ The correction provided in this study is for broadband values. Some of the equations used in the study (e.g. equations 2, 4,5, . . .) are theoretically valid only when I , C , and are written as functions of wavelength. Though I completely agree that a spectral correction of the measured SW is beyond the scope of this study, I think that the paper should include a discussion on the impact of using broadband values only and on spectral corrections. This is also related to my main comment 2/ above.

Specific Comments

Page 6031 lines 24-26 : Does that mean that the shift of the diurnal maximum of upwelling radiation is an additional criteria to filter the data ? Does that imply that all stations over significant surface roughness (sastrugi . . .) are discarded from the

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analysis ?

Page 6031 line 27 : the reflected radiation is not isotropic. Snow surfaces are not lambertian (e.g. Painter and Dozier 2004). This sentence should in my mind be reformulated, and a discussion should be included on the effect of snow bidirectional reflectance on measured upwelling shortwave.

Page 6033 lines 3-4 : Maybe the authors should include values for Ozone, AOD, water vapour, etc ...

Section 3.2 Scheme describing all the angle and radiations would probably help the reader.

Page 6034 line 9 : "only the term of diffuse radiation is used". This should be either detailed, or removed considering the contradictory information given page 6035 lines 15-16.

Page 6034 line 20 : The value of 75° must be further justified.

Section 3.4. Line 4 : Could you quantify "negligible" ? In my opinion, it is quite difficult to understand from this section which tilt angles values are used to adjust the radiation: monthly mean over clear days ? daily values ?

Page 6035 lines 16-20: I don't understand this sentence.

Page 6036 lines 7-15 : This should be described in the "Data" section.

Page 6036 line 14 : What are the elevation and distance maximum difference between stations and grid points ? How would that impact the comparison?

Figure 3 : Is this for all-sky values or only for clear-sky ? This should be included in the legend.

Figure 3: It could worth including regression coefficients. It looks like the data are positively biased at least for high SW values. Maybe the authors could include a discussion

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on this result.

Page 6037 lines 6-8: I don't understand the sentence.

Page 6037 line 18: South Dome also has a large value of tilt angle.

Page 6038 line 7: I am not sure cloud interference is the main reason for the daily albedo product.

Page 6038 lines 14-17: Figure 5 gives really interesting examples of the efficiency of the adjustments but it's only a few examples. How does the algorithm performed over the whole data set with respect to the albedo diurnal cycle?

Table 2 and Figures 6,7 : The correction on the SW values is higher for the accumulation than for the ablation zone but the impact on the albedo values is higher in the ablation zone. It could be interesting to add a really short comment on that.

Page 6040 line 25 : Is there any effect of glacier dynamic on station tilt and rotation ?

Page 6042 line 8 : 10 W.m⁻² is the same order of magnitude of the adjustment effect (16 or 19 Wm⁻² according to results shown in Table 2). A comment of this should be included in the paper.

Page 6042 line 26 : "agree better with satellite observations" this has not be demonstrated in the paper ...

Minor Comments

Page 6027 lines 13-14 : "insolation on fewer than 40% of clear days ..." Is this related to corrected on to uncorrected data ?

Page 6027 line 15 : "biases" with respect to ?

Page 6027 lines 20-21 : I think it could worth giving the RMSE and correlation coefficient values after and before the correction so that the reader gets an idea of the relative efficiency of the correction.

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Page 6030 line 2: "quite sensitive" maybe highly and significantly is more appropriate.

Page 6032 lines 12-13 : is not corrected for ?

Page 6033 line 1 : "running average albedo" this one is not corrected for tilt ? Though I am convinced it won't make much a difference on the correction, it would worth mentioning it.

Page 6035 line 25: It would worth including a reference for solar noon time calculation.
Figure 2: What does "precision of solar noon" mean?

Page 6036 lines 2-3 : I guess this is related to unadjusted radiations ?

Page 6036 line 4: "The maximum shift is 0.5h" : this is included in the constraints of your optimization.

Page 6037 line 11 : Is it possible to quantify "small" ?

Page 6037 line 13 : Is it possible to add the value of RMSE over one month ?

Page 6037 line 26 : I guess 0.7 is used instead of 0.8 because of melting snow. It can be interesting to add the information in the paper.

Page 6038 line 10 : 'estimated by' -> simulated as a function of ?

Page 6039 line 20 : "Except for station south dome" : is there any explanation for that ?

Page 6040 lines 19-20 : What does "environmental factors take effect " means. Figure 10 A) : Could be clearer that change x-axis labels from 0 to 360 ?

Interactive comment on The Cryosphere Discuss., 9, 6025, 2015.