Response to review of "Linking glacier annual mass balance and glacier albedo retrieved from MODIS data " by Dumont et al., submitted to The Cryosphere (doi:10.5194/tcd-6-2363-2012)

Author's responses are written in blue just below the reviewer comments. Proposed changes to the manuscript are underlined.

J. Dozier (Referee #1)

The paper's quality is high and contributes significantly to our understand of cryospheric remote sensing. But you need to reconcile the text and Table 4, or at least clarify what your evidence shows.

We are really grateful for the relevant review you provide. You will find below a detail point by point response to all your comments. We hope it addresses your concerns and improves the manuscript.

Referring to the text on P 12 L16, unless I misunderstand Table 4, the data in the table do not really show the improvement by including the anisotropy. My first reading (I looked at the table before I read the text) was that including anisotropy provides hardly any improvement, and in some cases is less accurate than the isotropic method. Can you please clarify the sentences on P12? I just find them hard to reconcile with Table 4, so perhaps revising both would improve the communication.

Both Table 4 and P 12 attempt to show that the anisotropy correction provides an improvement on the retrieved albedo value compared to field measurements. This improvement can be seen in Table 4 by comparing the metrics on lines 'iso' vs 'ani'. In all cases (topographical corrections or not, DISORT or Eq4) the performance are better when anisotropy is modelled ('ani') compared to the same case with the isotropic assumption ('iso'). Nevertheless, it is true that the improvement induced by the topographical correction is larger in some cases than that caused by the anisotropic correction.

In order to address this comment and improve the clarity of the manuscript, the paragraph line 17 in the section 4.1 has been changed as follow (page 12): "In every case, accounting for the anisotropy of the radiation reflected by snow provides better performance (i.e., 'ani' versus 'iso' in Table 4). This sole correction made the RMSD decrease by 10 % all other things being equal. The correction of multiple reflections due to surrounding slopes (i.e., 'topo' vs 'no topo' in Table 4) also noticeably improved the retrieved value. The topographic correction appeared to have a greater impact under the Lambertian assumption (the RMSD improved 8.5 % in the case of Eq 4 and 19 % in the case of DISORT, not shown in Table 4). When accounting for the anisotropy of the reflected radiation, the topographic correction improved the RMSD by 3.9 % in the case of Eq. (4) and 3.8 % using DISORT ."

The statistics for anisotropy correction and no anisotropy correction, no topographic correction and DISORT have also been added in Table 4 in a sake of clarity.

Perhaps the reason that the anisotropic correction is of marginal value lies in Section 4.3.4. There you discuss the anisotropy correction used and note the effects of surface roughness. Perhaps the reason that Table 4 does not show that much improvement is that roughness generally produces the opposite anisotropic effect compared to smooth ice or snow. Generally smooth surfaces scatter in the forward direction (because the ice grains themselves have a forward scattering peak) whereas roughness produces shadows in the forward direction, thereby causing a measurement that integrates over a larger scale to be lower for a rough surface. That is, the grain scale BRDF is different than the scale of the surface geometry. An interesting side note here is that Li & Strahler (IEEE Trans Geosci Remote Sens, doi: 10.1109/TGRS.1985.289389) showed the similar effect for forests. Their realistic BRDF model is based only on the geometry of the trees, where every element in the model (leaves, grass, soil) scatters isotropically.

As noticed in our response to your first comment, the anisotropic correction did provides a substantial improvement on the retrieved albedo. Nevertheless, this improvement is smaller in some cases than that caused by the topographic correction. We agree that the reason you are describing above may indeed explain these different impacts.

Consequently, we have added this point to the discussion in the section 4.3.4. (line 25, page 2378) : '(…)~~Such refinements (i.e. detailed BDRF models accounting for the shapes of grains and for the surface roughness) will be the subject of future work.~~ The fact that the surface roughness generally produces opposite anisotropic effects compared to smooth ice or snow surfaces (Hudson et al., 2007) may explain why the benefit of the anisotropy correction was of lower magnitude than that attributed to the topographical correction. Indeed, the BRDF associated with the surface considered at the granular scale can depart significantly from that of the surface observed at a macroscopic scale and affected by the surface roughness. Accounting for this departure could further improve the impact of the anisotropy correction (e.g. Li & Strahler, 1985).

Some minor comments:
End of Section 2.1 (P 6, L3) - I think you mean 30 m resolution.

Agreed. Page 6 Line 3 is now 'generated at 30 m spatial resolution'.

P8 (section 3.2) says 7 bands are used to retrieve broadband albedo, but equations 2 and 3 use just 4 bands. Please clarify.

Agreed. While the method presented in Sirguey et al. (2009) allows the seven reflective bands to be corrected, only bands 1, 2, 4, 5 and 6 are used for the broadband albedo retrieval.

To clarify this point, we have changed P8 line 18 as follow: '(…) to be estimated for each spectral band accounting for atmospheric effects and multiple reflections occurring in rugged terrain. Five bands are used in total in the retrieval method for albedo described below. The method is applied only to pixels identified as snow (i.e., …)."

Also page 9 line 13 is changed to 'The broadband albedo is inferred from five spectral albedo…' and page 10 line 8, 'The band 7 is not used here since the signal is really low for these wavelengths.'

Finally, in the caption of table 2 we removed 'to retrieve broadband albedo'.

Given that the analysis is restricted to pixels with more than 50% snow, how does the variability in reflectance of the rest of the pixel affect the analysis?

In general, the variability in the reflectance of the rest of the pixel will affect the analysis. For the inner pixel of the glacier, the pixels are either snow or ice. On the contrary at the edge of the glaciers, certain pixels might contains rocks as commented in page 15 line 1-3. When comparing to the results of MODIS with the albedo retrieved from the terrestrial photography as explained in section 4.2, most of differences between the two is concentrated in the pixels of the edge of the glacier. Part of the effect is due to the difference of spatial resolution (regions with high intra-pixel variability of the slope) and part of it is due to the presence of rock (this is also quantified in response to comment 4 of the second referee). The separated impact of each effect is difficult to evaluate. Nevertheless, a way to avoid this contamination of the reflectance would be to apply the albedo retrieval method only to the fraction of reflectance from the pixel which is attributed to snow and ice by the linear unmixing technique. A quantification of the error due to the mixed pixels can be done indirectly by comparing the discrepancies between the albedo retrieved from the terrestrial photographs and from MODIS at the edge and on the central part of the glacier. This is presented on Figure 3.

To clarify this point, we have changed the end of the discussion on mixed pixels page 15 lines 1-3 by 'debris cover. To address the problem of mixed pixels, it would be interesting to apply the albedo retrieval method only to the part of the pixel reflectance which is attributed to snow and ice by the linear unmixing technique. '

See also response to comment 4 of S. Lhermitte.

P 9 L13 - There is a word missing in this sentence but I cannot infer with certainty what it is.

Agreed. Page 9 line 13-15 has been changed to "The broadband albedo is inferred from the spectral albedo derived from five MODIS bands and using Look-Up-Tables (LUTs) generated with DISORT (Stamnes et al., 1988). The methodology is the same as the one described in Dumont et al., 2011. "

Added references :

X., Li, and A. H., Strahler, Geometric-optical mapping of a conifer forest canopy, IEEE Transactions on Geoscience and Remote Sensing, 23 (5), 705-721, doi: doi: 10.1109/TGRS.1985.289389, 1985.

S. Lhermitte (Referee # 2)

1 Summary

This study introduces a new methodology to obtain broadband albedo of snow and ice surface on a 250 m spatial resolution from MODIS. The methodology consists of i) multi-spectral data fusion, ii) conversion of obtained radiance to spectral albedo using the anisotropy factor and estimates of incoming radiation, iii) conversion of spectral albedo to broadband albedo using a LUT or quadratic combination. The results of these methodology are subsequently compared with field measurements and terrestrial photography albedo maps and applied on the Saint Sorlin Glacier over the period 2000-2009 to i) understand the evolution in albedo over time and ii) relate albedo to variationsin mass balance.

2 General comments

I think the authors provide a high quality paper that clearly illustrates the advantages of improved satellite data processing on the understanding of spatial and temporal variations in surface albedo. Although I think the paper is rather complete, I still have some comments related to some small changes/additions, that, in my opinion, can be performed to improve the paper.

We are really grateful for the in-depth review you provide. You will find below a detail point by point response to all your comments. We hope it addresses your concerns and improves the manuscript.

3 Major comments

1. I think the methodology of inferring mass balance variations from variations in minimum albedo (and the comparison of this technique to the technique of Rabatel (2005) is now well hidden in the

paper and when reading it it feels as a surprise add-on. I would recommend to add this method (and the comparison with Rabatel's method) to the methodology section 3 and clearly state it as an objective of the paper to see if mass balance variations can be derived from variations in albedo. This will make it more logical for the reader.

We agree that the final result showing a relationship between the mass balance and the albedo is only addressed as an additional section at the end of the paper. Although we appreciate the reviewer's comment that this may feel disconnected, we believe that it would not add to the clarity of the paper to include the methodology for this comparison in the main methodology section. We think that doing so would result in even more disconnection between methodological points and their associated results. We have preferred to present the final comparison as a separated application of the method presented in the paper.

Nevertheless, in order to make this application more prominent at an earlier stage in the manuscript, we have added a paragraph at the end of the introduction that stresses the potential link between albedo, mass balance and ELA. We also emphasized the methodology developed for mass balance reconstruction in the objectives of the paper page 2367 line 9.

Page 2367 line 4 "The glacier surface albedo is closely related to its mass balance since it constrains its surface energy balance (Six et al., 2009). Several methods have been developed to characterize the relationship between albedo and mass balance variations (e.g. Greuell et al., 2007). Alternative methods based on remotely sensed data have also been developed to estimate the variations of mass balance from those of the equilibrium line altitude (ELA), namely the line of null mass balance which separate the ablation zone (ice) from the accumulation zone (snow) at the end of the ablation season (Rabatel et al., 2005, 2008). Both areas have greatly contrasting albedo. Consequently, it is hypothesized that the albedo of the glacier surface at the end of the ablation season may contain a valuable signal related to the annual mass balance. In proposing and validating a method to retrieve glacier albedo from MODIS, this study aims at characterizing further the relationship between the albedo measured at the end of the ablation season from MODIS data, the annual mass balance of a glacier and the ELA retrieved by using the methods described in Rabatel et al., 2005 and 2008."

Page 2367 line 9 : "Section 5 presents the first results obtained on Saint Sorlin Glacier while applying the method over the 2000-2009 period. This section characterizes the relationship between the annual mass balance of the glacier, its albedo and the ELA".

2. The proposed method and also Sirguey (2009)'s method are based on the assumption that each pixel represents identical surface areas over time. This is, however, often not true as the effective sensor footprint is often much bigger than 250-500 m (e.g., the work of Tan et al. (2006)). I propose some discussion on this is added to the error sources section.

Agreed. We included some discussion on this issue in the section 4.3.3. The section has been renamed (page 2378, line 1) 'Geolocation and gridding artifacts'. We also added page 2378, line 13: 'In addition to the geolocation error, MODIS data are subject to mismatch between the location on the ground from which MODIS observations are derived and the predefined grid cells used for storing observations (Tan et al., 2006). This 'pixel shift' is particularly problematic at high view zenith angles (at the egde of the scan line) for example and can also affect the retrieval especially in highly irregular terrain. '

4 Minor comments

1. p2368,L2-5: a reference to the accuracy of the GDEM product will help the reader to understand the associated uncertainty

The estimation of the planimetric accuracy of the GDEM is about +-30 m. We added p2368 line 5 'The estimated planimetric accuracy of the GDEM is about +- 30 m (Zhao et al., 2011).'

2. p2371,L7: I think it is helpful if it is repeated very shortly how E_s and E_{diff} are derived, so the reader is not obliged to read earlier works.

Page 2371 line 7 has been changed to : 'respectively. Es is inferred from a clear sky single layer atmospheric model (Bird and Riordan, 1986). Ediff is derived using the same atmospheric model together with an iterative method to account for the contribution of the neighbouring slopes to the diffuse irradiance. This is described in more details in Dumont et al. (2011) and Sirguey et al. (2009).'

3. p2371,L12 "seven spectral albedo": Typographical error?

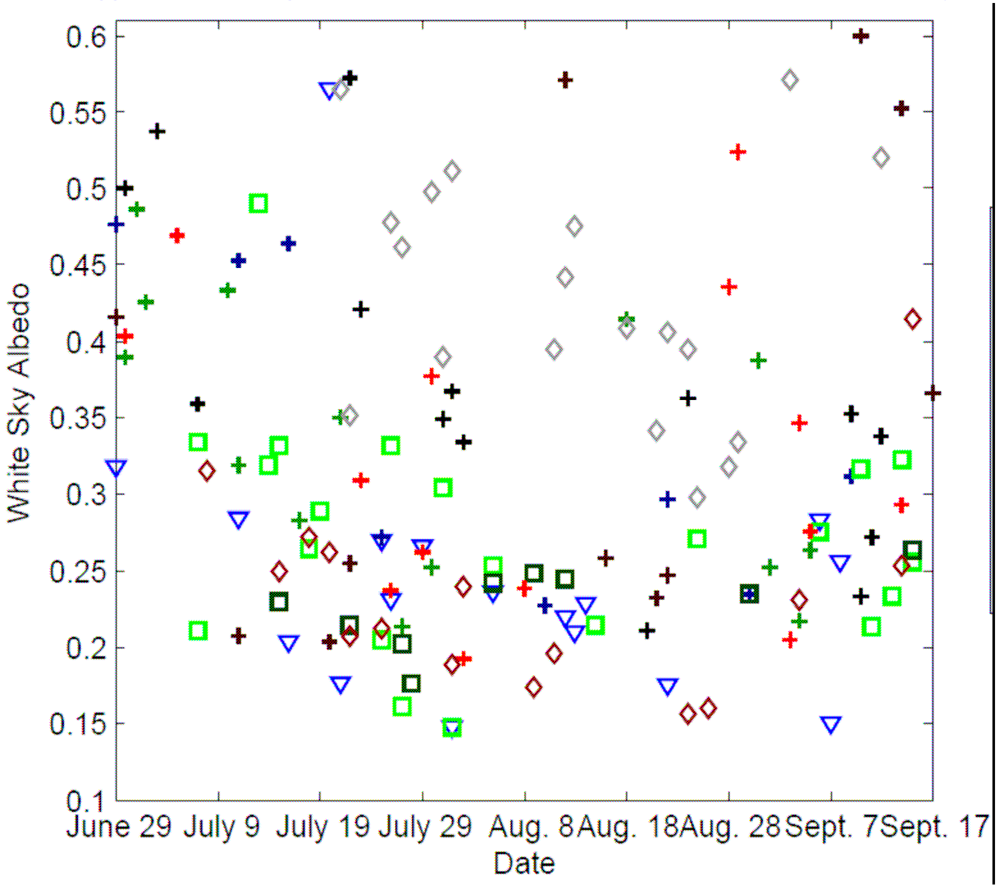
Agreed, this sentence was rephrased as follow : "The broadband albedo is inferred from the spectral albedo derived from five MODIS bands and using Look-Up-Tables (LUTs) generated with DISORT (Stamnes et al., 1988). The methodology is the same as the one described in Dumont et al., 2011. "

4. p2376, last paragraph: it would be interesting to see the standard deviation or variance in terrestrial photograph albedo per MODIS pixel. This should illustrate the increased variance in albedo near the edges.

Agreed. We have computed the standard deviation of the albedo from the photographs inside each MODIS pixel and included this points in p2377 line 1: " For instance, at the end of the ablation season, the standard deviation of the albedo values derived from the photographs within each 250-m MODIS pixel at the edge of the glacier is typically twice larger (from 0.02 to 0.04) at the egde of the glacier than standard deviation computed from pixels in the central area."

5. p2379,L20 + Figure 5: I understand the logic of plotting only composites of minimum albedo, but I think it would be good for completeness and clarity if you plot the actual observed albedo (e.g., in a semi-transparent color with fine lines) and the composites of minimum albedo on top of it (e.g., in an opaque color and in a more bold line type).

Thanks for this suggestion. The figure below shows the actual observed albedo for each year.



We believe that adding this information on Figure 5 would confuse the reader so we prefer just to keep the value of the minimum observed albedo since the goal of the figure is to demonstrate that there is no obvious trend on the minimum albedo. See also response to your comment 9 .

6. p2380,L15:p29, where does this square root come from?

The number 29 refers to the number of pixels used to calculate the mean albedo. The use of the square root is associated with the computation of the standard error of the estimator of the mean . Since the bias of the albedo value derived from MODIS is close to 0 (i.e., 0.014, see table 4), the RMSD (0.052, see table 4) identifies to the standard deviation. As per the central limit theorem, the estimator of the mean albedo computed from n independent samples has a standard error being σ/sqrt(n), or 0.052/sqrt(29) in our case.

7. p2380,L1 "are also indicated in Fig.6": are also indicated in Fig.6 as error bars?

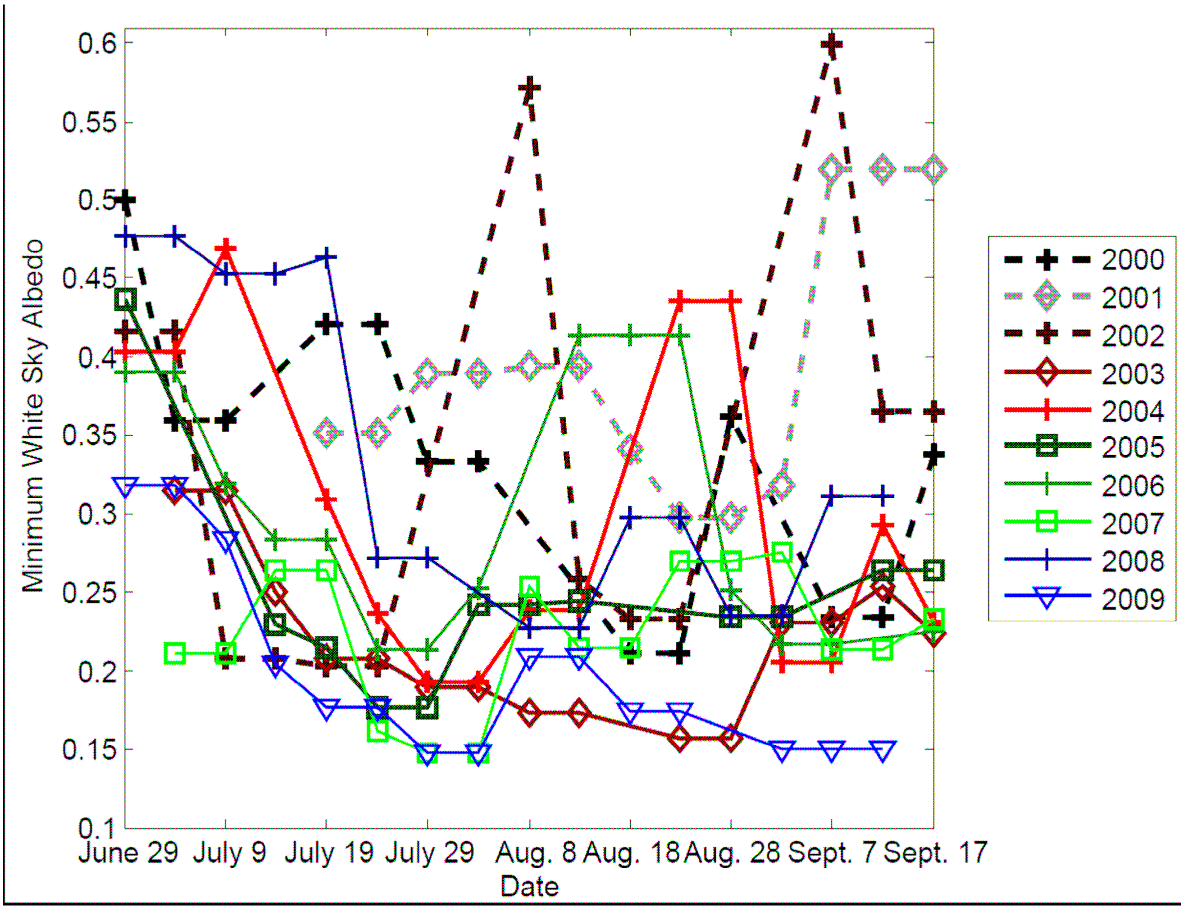
Agreed. Line 15-16 p 2380 is now ‘are also indicated in Fig 6a as error bars.’

8. p2381, L9 "close to the date": Can you give an indication on how close?

Unfortunately, it is impossible to be specific about the time difference between the minimum albedo value observed via MODIS and the date when the snowline reaches its uppermost. Since, MODIS images are only usable in clear sky case and the date at which the transient snow line reaches the equilibrium line can be a cloudy day, we cannot be certain as to the time difference. Besides knowing the exact date of this event would need a daily monitoring that is not in place at the moment.

This sentence has been changed in a sake of clarity. You can now read: “Indeed, the date of the minimum albedo matches the date when the snowline has reached its uppermost position (i.e. when the extension of the ablation zone, with low albedo values, reaches its maximum) and so the date when the snowline can be considered as an good indicator of the equilibrium-line (Rabatel et al., 2005, 2008).

9. Fig.5: A color figure would be much easier to read.
Agreed. Figure 5 has been changed as followed :



References

Tan et al. (2006) The impact of gridding artifacts on the local spatial properties of MODIS data: Implications for validation, compositing, and band-to-band registration across resolutions. Remote Sens Environ, vol. 105 (2) pp. 98-114.

Dear,
Sorry for this add-on, but I noticed I have forgotten to include a comment in my already submitted review RC C1759 of 02 Oct 2012.
p2362, L10-12: when minizing $D_{2VIS} + D_{2IR}$ you assign less weight to each VIS band (i.e., only one third) than to each IR band (i.e., a half) in the spectral matching. First, I don’t see any specific reason for that as I assume each band can have equal weights. Secondly, I think it is perhaps useful to discuss this shortly and, in the future, look at more advanced band weighting in function of the information content of each band (e.g., Somers et al. A weighted linear spectral mixture analysis approach to address endmember variability in agricultural production systems. Int J Remote Sens (2009) vol. 30 (1) pp. 139-147).

In order to respond to this comment, we believe that we must first provide a short clarification of the method. Dvis and DIR are minimized first independently. From Dvis, in which each visible band has the same weight, we retrieve the impurity content and one value of SSA . From DIR, we inferred another value of SSA as explained in the top sentences of page 2373. The only step in which we used Dvis and DIR is step 3/ for discrimination between ice and snow (There no minimization at this step). We agree that in this case the IR band has more weight than the VIS bands. The spectrum of ice and snow largely departs from each other after 500 nm. Consequently, as you suggested, it may be useful to rely on a more elaborated weighting scheme.

Thus, we add the following paragraph page 2373 line 4 : “In the steps described above, either each band has the same weights (steps 1 and 2) or more weight is assigned to the IR bands (step 3). In the future, more advanced band weighting depending on the information content of each band (e. g. more weight for band 4 which is the most sensible to impurity to retrieve the impurity content) could be implemented as described in Somers et al., 2009”.

Added references :

S. Zhao, W. Cheng, C. Zhou, X. Chen, S. Zhang, Z. Zhou, H. Liu and H. Chai, Accuracy assessment of the ASTER GDEM and SRTM3 DEM : an example in the Loess Plateau and North China Plain of China, International Journal of Remote Sensing, doi: 10.1080/01431161.2010.532176, 2011

B. Somers, S. Delalieux, J. Stuckens, W. W. Verstraeten and P. Coppin, A weigthed linear spectral mixture analysis approach to address endmember variability in agricultural productions system, International Journal of Remote Sensing, vol. 30, 1, 2009

B., Tan, C.E., Woodock, J. Hu, P. Zhang, M. Ozdogan, D. Huang, W. Yang, Y., Knyazikhin, and R.B., Myneni, The impact of gridding artifacts on the local spatial properties of MODIS data: Implications for validation, compositing, and band-to-band registration across resolutions. Remote Sens Environ, 105 (2), 98-114., doi : 10.1016/j.rse.2006.06.008, 2006

Other changes :

We added a definition of the Specific Surface Area page 2371, line 17.
We also replaced diffuse equivalent albedo by White Sky Albedo (WSA) which is a more common term in remote-sensing.