Response to review of "Retrieval of snow Specific Surface Area (SSA) from MODIS data in mountainous regions" by Mary et al., submitted to The Cryosphere (doi:10.5194/tcd-6-1915-2012)

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

Review RC C965, Anne Nolin, 09 Jul 2012

Review of Mary et al. "Retrieval of snow Specific Surface Area (SSA) from MODIS data in mountainous regions"

The authors describe a method that they developed in order to retrieve specific surface area for ice particles in mountain snowpacks using radiance data from MODIS. They attempt to assess their results using modeled grain size from the SAFRAN-Crocus models. One innovative aspect that I appreciated in this paper is their topographic correction of MODIS reflectance data. Otherwise, I had a number of serious problems with it.

We would like to thank Anne Nolin for the numerous, insightful and constructive comments of this review, which enabled scientific improvements of the paper, along with clarifications of some statements and language refinements. We needed substantial time to refine this work, which can explain the additional delay for answering the reviews and correcting the paper. Among the main parts of this work, we particularly wanted to explain the choice of the SAFRAN-Crocus model as a comparison dataset; topographic considerations upon SSA retrievals indeed require an extensive dataset. However, as it was already mentioned in the paper, we recognize the need for field measurements of SSA as validation data. Thus, as suggested by the reviewer, we included the first results of field measurements leaded as MODIS validation purpose. We also thought important to add comprehensive explanations with regards to some imprecise points we dealed with, such as : the definition of SSA and its relevancy within remote sensing of grain size, considerations upon surface roughness and its influence on retrieved SSA, or some statistics explaining processing choices. Note that in the new version of the manuscript, we used an updated version of Crocus (Vionnet et al., 2012) which allows more slope angles and orientations to be computed. **Finally, we would like to recommend that the reviewer reads the whole revised version of the paper since many changes have been performed also in the structure of the paper, for example the entire introduction has been rewritten.**

1/The main problem that I have with this paper is that the authors "test" their method using modeled snow grain size (from SAFRAN-Crocus). Testing a model with a model does not validate the results. It is only a comparison. You cite a paper that is in press that shows SSA values from Crocus "were in very good agreement with field measurements" but that is a different study for a different time period and location. The attempt to use model output to verify SSA derived from MODIS is insufficient for validation. At the end of the paper you state that you are pursuing a complete evaluation using groundbased measurements. I suggest that you resubmit the manuscript following your analysis of these field measurements.

Thanks for pointing this imprecision on the comparison purpose of the paper. In fact, we do not use data from the SAFRAN-Crocus model to validate the method we propose, but as a comparison set of data to analyse the topographic variability of the retrievals. We entirely agree that validation

of a method for absolute measures of SSA needs ground-based measurements instead of a model's output.
However, the purpose of the paper is to analyse the results of an SSA retrieval method including topographic correction of radiance and in mountainous areas, hereby with regards to topographic aspects. As such, we need to compare our results to a wide set of independant data over various elevations, aspects and slopes, at the same time.
Unfortunately, field measurements at the MODIS sensor's scale (i.e. 500 m), on different places (of various elevations, aspects and slopes) at the same moment of the acquisition, can only result in a small set of data with regards to topographic parameters. Indeed, even with a fast and portable field instrument for SSA measurement, such as the one of Gallet et al. (2009), accessibility and representability of mountainous areas at the 500 m scale of MODIS make measurements a difficult task. Thus, such measurements are not sufficient (in terms of data amount) for the purpose of this study, and can only be used as local validation data. This is why we required and used a spatialized and topographic-dependant set of data, for which the outputs of the SAFRAN-Crocus model were an adequate comparison data. Furthermore, this model has been validated over years of operational snowpack evolution forecasting and analysis by Météo-France on many ranges of the Alps and Pyrenees (Brun et al., 1992 and Vionnet et al., 2012) in addition to the specific SSA evaluation presented by Morin et al. (2012), so that we can lend quite some reliability to the model.

Yet, we managed to process some field measurements carried out this winter (March, 2012). Given the rugged terrain, the spatial and temporal resolutions of the sensor, and the need for sampling representative SSA over a 500*500m area, two consecutive days of measurements resulted in two pixels data, namely: one pixel of flat terrain at 2000 m.a.s.l. and a second one on a south-westerly facing slope tilted around 15°, around 2100m. These additional results, although in little number confirm the biases observed without topographic correction. They have been included in the paper:
1/ lines 250 to 260 : description of the data
2/ lines 391 to 406 : discussion of the results
3/ in the conclusion line 688

Note that the entire introduction has been rewritten to precise the objectives of the paper and the comparison with the simulated dataset.

2/ The authors motivate their research by stating the significance of snow grain size as a major control on snow albedo and therefore it's significance to climate and snow hydrology. To refine and constrain what is meant by snow grain size, they use the term "specific surface area" which is defined as "the ratio between the area of the air/snow interface and the mass of the snow sample, i.e., SSA = S/M = S/(ρ * V), where S and M are the surface area and mass of a snow sample, respectively, V the volume of the ice particles in the sample, and ρ is the ice density (917 kg m^{-3} at 0°C)." While this definition is appropriate for understanding gaseous exchange at the ice-air interface, it is not correct for light scattering. This definition of SSA only considers S to be the surface that is in contact with the air. Thus, it excludes grain boundaries, which serve as scattering interfaces. The SSA that is relevant for snow albedo defines S as the entire surface of the snow grain (here, I term this S_{grain}), not just that portion that is in contact with air (here, termed $S_{ice-air\_interface}$). Thus, S/V as used by Warren (1982) is fundamentally different from S/(ρ * V) as used here (and defined by Legagneux et al. 2002 and used by Domine et al. 2007). Results using stereology and chemical adsorption should be different because they are sampling difference surface areas (S_{grain}/V vs. $S_{iceair\_ interface}$/(ρ * V)). While there may be a relationship between S_{ice-}

$_{air\_interface}$/(ρ * V) and the optical properties of snow, it remains that the definition is not correct.

Thanks a lot for this interesting and constructive comment. It is true that two definitions of the SSA can be found. For light scattering, the SSA is proportional to the entire surface of snow grains divided by the total volume of ice (Grenfell and Warren, 1999) [definition a]. For more chemical issues, the relevant surface is related to the area of the air/snow interface and so excludes the surface of grains boundaries (ice/ice interface) (Legagneux at al, 2002) [definition b]. The two definitions are equivalent in the idealized case of disconnected grains. However for natural snowpack, tomographic 3D snow images show that the specific grains contact area, i.e. the ratio of the grains contact area to the ice volume, varies from 10 (fresh snow) to more than 50 % (evoluted snow) of the total SSA [definition a] (figure 6 in Flin et al., 2010).

In this study, DISORT was used to model disconnected spheres. In the Crocus model, grains are not spherical, but they are disconnected too, and the calculation of an equivalent optical radius or SSA also consider the grains as disconnected (Vionnet et al. 2012). And lastly the SSA 'measured' by the DUFISSS device also used the assumption of disconnected spheres (Gallet et al, 2009). Consequently all the SSAs presented in this paper refer to the same definition [definition a] and you are perfectly right in your comment of the misused of the definition in the paper (page 1918, lines 9-10).

The disconnected spheres assumption have been added in the text of the manuscript and precision on the definition of the SSA and the optical radius have been written in the introduction (lines 75 to 85).

3/ In their review of the literature, the authors neglect the early work on grain size and albedo by Wiscombe and Warren (1980), the potential for grain size retrieval from remote sensing by Dozier et al. (1981) and Dozier & Marks (1987), and early successful demonstrations of grain size retrieval by Nolin & Dozier (1993, 2000) and Bourdelles & Fily (1993).

Thanks for this comment. These references are all quoted in Tedesco and Kokhanovsky, 2007 as written in page 1919, lines 11-12 that is why we first did not quoted them but we agree that it might be better to include them namely. These references have been added in the new version of the manuscript (lines 87 to 89).

4/ The study is focusing on "the effect of 1/ the local topography, 2/ the anisotropy of snow and ice reflection, 3/ the shape of snow grains". There seems to be a scale gap here you go from looking the scale of snow grains (50-1000 microns) to the scale of local topography (0.1-1.0 km) but you neglect surface roughness (0.1-10 m). You consider the case where snow has a Lambertian reflectance and where the snow surface is anisotropic. Please address how surface roughness will affect the anisotropic pattern of reflectance and what this means for your SSA retrievals.

Thanks for underlying this issue which is probably not clearly stated in the first version of the manuscript. Surface roughness will largely affect the anisotropic pattern of the pixel reflectance and will consequently lead to error in the SSA retrieval in both cases (Lambertian assumption or applying snow BRDF corrections). The impact of snow roughness e.g. sastrugi has been studied in many papers (e.g. Leroux and Fily, 1998, Hudson and Warren, 2007, Kuchiki et al., 2011, Zhuravleva and Kokhanovsky, 2011). The exact impact of the surface roughness on the retrieved SSA, i.e. if the

SSA retrieved using for example the Lambertian assumption is larger or smaller than the truth, is difficult to predict since it will depend on the type of surface roughness, its orientation with respect to the sun and also the viewing geometry of the sensor. To clarify this point we add a paragraph on surface roughness in the discussion (lines 551 to 568). These discussion was also include in the conclusion (lines 702 to 704) and to the abstract (line 16)

See also response to your comment n/ and to comment 4/ of T. Painter

Minor comments:

a/ Line 69: “the grains shape distribution”. This is not grammatically correct and is unclear. Perhaps you mean “the distribution of grain shapes”? Please clarify and correct.
This is indeed the meaning of the expression. It has been corrected as you suggest.

b/ Line 73: Use of the slash character in your numbered list is visually confusing since at first glance it looks like a ratio. Please use a right-parenthesis rather than a slash.
The list has been corrected this way.

c/ Line 94: “Alpine” should be lower case.
Thanks for this grammatical correction, ''Alpine'' has been corrected to ''alpine''.

d/ Line 100: “precipitations” should be “precipitation”.
Thanks for this grammatical correction, ''precipitations'' has been corrected to ''precipitation''.

e/ Line 108: “impurities content” should be “light absorbing impurities”
Thanks for this precision, it has been corrected as you suggest.

f/ Section 2.2: It would be worth mentioning that, in addition to the other advantages, the MODIS data are freely available for download via ftp.
Sorry for this unwanted omission. Indeed, that is one of the main advantages of the MODIS data. The item ''it provides daily coverage of the area of interest'' has been replaced by ''it provides daily coverage of the area of interest. Besides MODIS data have the tremendous advantage of being freely available via ftp download.''

g/ Line 125 and numerous sentences throughout the manuscript: You use the passive voice and do so in a way that obscures the meaning of the sentence. For instance, you write: “It is illustrated on Fig. 1. SRTM data were assessed by Rodriguez et al. (2005) that the absolute geolocation error is…” When it would be more clear and concise to say: “showed that “SRTM data have an absolute geolocation error of…(Rodriguez et al.,2005)”.
We thank you for correcting this dull grammatical trend. The following sentences will be changed (former lines numbering) :
Page 1919, line 18-19 : “..., topographic effects, such as the illumination angle or the reflected terrain irradiance, are ignored, despite ...” replaced by ”..., these methods ignore topographic effects, such as the illumination angle or the reflected terrain irradiance, despite ...”
Page 1920, line 4-5 : “while custom corrections are used by Lyapustin et al. (2009) or Zege et al. (2011).” replaced by “while Lyapustin et al. (2009) or Zege et al. (2011) used custom corrections.”
Page 1920, line 14-15 : “The same inversion technique is then applied to MOD09 reflectance” replaced by “Then we apply the same inversion technique to MOD09 reflectance”.
Page 1920, line 20 : “The SSA retrieved by the different methods are then compared to ...” replaced by “We finally compare the SSA retrieved by the different methods to ...”.

Page 1922, line 2 : "two products were used" replaced by "we used two of these products".

Page 1922, line 12-13 : "It is illustrated on Fig. 1. SRTM data were assessed by Rodriguez et al. (2005) that the absolute geolocation error is lower than 8.8m and the relative elevation error is lower than 8.7m over Europe." replaced by "Figure 1.b illustrates the DEM on the studied area. Rodriguez et al. (2005) showed that SRTM data have an absolute geolocation error of 8.8m and a relative elevation error of 8.7m over Europe."

Page 1926, line 6 : "it is assumed that" replaced by "we assume that".

Page 1927, line 23 : "the SSA was retrieved" replaced by "we retrieved SSA".

Page 1928, line 9 : "Grains were assumed to be spherical and their single scattering parameters were computed using ..." replaced by "We assumed grains to be spherical and computed their single scattering parameters using ...".

Page 1928, line 13-14 : "Reflectance values were computed" replaced by "We computed reflectance values".

Page 1928, line 15-16 : "The presence of impurities in the snow was ignored" replaced by "We ignored the presence of impurities in the snow".

Page 1928, line 24 : "The retrieval of SSA values was then performed" replaced by "Then we performed the retrieval of SSA values".

Page 1929, line 25 : "Shaded pixels were ignored" replaced by "We ignored shaded pixels".

Page 1931, line 5 : "values retrieved from MODIS are compared with" replaced by "we compare the values retrieved from MODIS to".

Page 1931, line 14 : "Four statistics were used" replaced by "We used four statistics".

Page 1932, line 16 : "when retrieval is processed using four bands" replaced by "when processing four bands retrieval".

Page 1932, line 17-18 : "that this spectral method was preferred here to ..." replaced by "that we preferred this spectral method here to ..."

Page 1937, line 12 : "Smoothed SSA maps were computed" replaced by "We computed smoothed SSA maps"

h/ Line 155: "function of" should be "functions of".

Thanks for this grammatical correction, ''function of'' has been corrected to ''functions of''.

i/ Line 279-280: "high and low albedo corresponds, respectively, to little and large amounts of energy absorbed." Awkward – please rephrase.

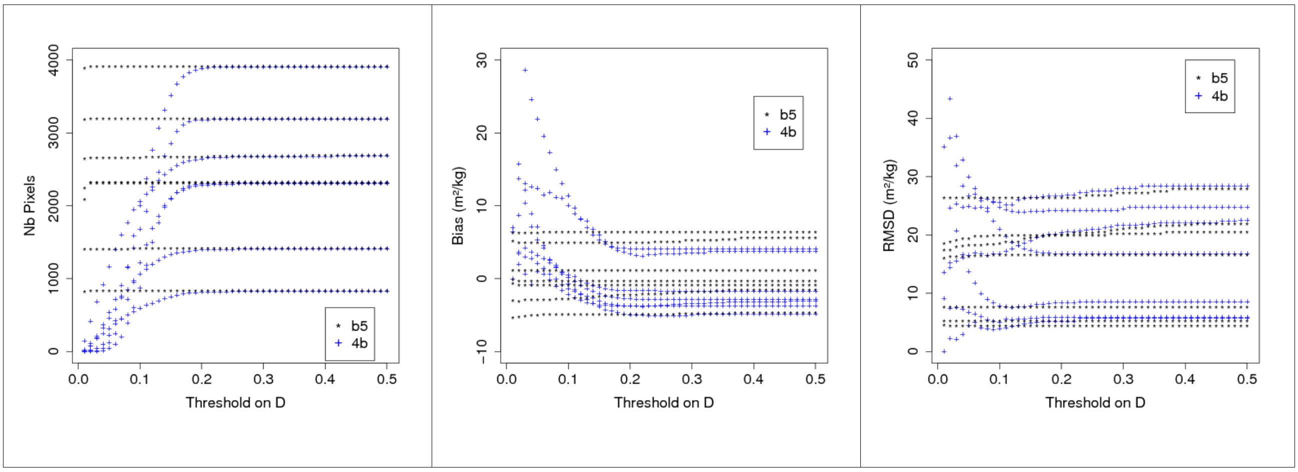
The whole paragraph has been rephrased (lines 381 to 390)

j/ Lines 289 and 298 (and elsewhere): Please use consistent formatting for dates.

Thank you for pointing these mistakes. We now use yyyy-mm-dd format everywhere.

k/ Section 4.1.1: It seems that you should perform an optimization study to determine the optimal threshold distance value and set of bands that give the best coverage and least bias. Your selection of bands and spectral distance threshold seem rather ad hoc (subjective).

Indeed, the choice of the spectral distance threshold was determined by a very simple test of a few values, which suggested a generic value of D=0,1 as a good candidate. Some sensitivity studies as the reviewer relevantly suggests, have now been performed with the DTA method on the six dates. The three resulting figures below show respectively the number of pixels obtained, the bias and the RMSD for methods with band 5 only (b5) and four spectral bands (4b). In sake of clarity, we will not include these figures in the paper, but we will modify section 4.1.1 with regards to the conclusions below.



Firstly, the value of the threshold has very little impact on the b5 method, only a little increase of RMSD with increasing D. Secondly, a threshold value lower than (roughly) 0,2 leads to a significant decrease of coverage (# pixels) for the 4b method, along with an increase of bias and an unsettled RMSD (depending on the date). Thirdly, an optimal threshold (on average) for the 4b method with regards to RMSD and bias would be around 0,14.

The conclusion from this analysis is that having both an optimal mean RMSD and an optimal bias and coverage is not possible for the 4b method, whereas it is possible for the b5 method. Hence, the 4b method leads to either a higher RMSD (with a high threshold) or a higher bias and lower coverage (with a small threshold), with regards to the b5 method.

This comforted us in the choice of the b5 method in the following.

These modifications have been reported in the manuscript (lines 442 to 452).

l/ Line 325: You need to report the light transmittance depth for each spectral band. Not only is this dependent on wavelength, it also depends on grain size and density. It would be useful to report this in terms of grain radii thus the reader would have a sense of how many grain radii were sampled for each spectral band (see Nolin & Dozier, 2000; Table 2).

Indeed, light transmittance depth varies with wavelength, density and grains size. To address your comment we add the following table in the paper which describes e-folding depths for two extreme cases (fresh light snow and old heavy snow) and for each band of MODIS used in this study. Note as mentioned now in the text of the paper, these values have been calculated for spherical grains with DISORT and might thus be slightly higher than for real snow grains (Sergent et al., 1987).

	Band 2 (858.5 nm)	Band 5 (1240 nm)	Band 6 (1640 nm)	Band 7 (2130 nm)
Fresh light snow, R_opt = 50 um Density = 150 kg m^{-3}				
1/e Optical Depth	148.4	22.93	4.97	3.75
1/e Geometric Depth (mm)	30.0	4.56	0.97	0.73
Number of Grain diameters per 1/e Geometric Depth	60.0	9.12	1.94	1.46
Aged dense snow, R_opt = 1000 um Density = 350 kg m^{-3}				
1/e Optical Depth	33.66	4.32	1.31	1.27

Geometric Depth (mm)	58.6	7.51	2.28	2.21
Number of Grain diameters per 1/e Geometric Depth	29.3	3.76	1.14	1.11

The value presented in this table are presented in the new manuscript lines 234 to 241.

m/ Line 401: "whose anisotropy is less sided than small SSA snow" This doesn't make sense. Please rewrite.Thanks, the sentence has been rephrased ''whose anisotropy is less pronounced than small SSA snow''.

n/ Lines 444-446: "This suggests that nearly half the RMSD between MODIS and SAFRAN-Crocus SSA originates from the variability of the SSA retrieved from satellite measurements at constant altitude, slope and aspect." Could this be the result of surface roughness ?
Thanks for this interesting suggestion. Part of the additional variability found in MODIS retrieval could indeed be the results of surface roughness. Part of it might also be due to the use of semi-distributed modelling (e.g. shadowing effect due to local topography not taken into account, or intra-massif geographical homogeneity). Consequently we change line 601-603 by "… and aspect. This intra-classes variability in the MODIS retrieved values may stem from local topographic effects such as sun direct light shadowing and wind drift, from mixed pixels, and also from surface roughness."
Note that additional work on the variability has been added lines 604 to 616.

o/ Line 525: "The results expressed in optical optical radius are less sided." Doesn't make sense.The sentence has been changed line 539.

p/ Line 529: "However, despite SAFRAN-Crocus modelled SSA seems to be very close to field measured values…" Not grammatical, please rephrase.
The sentence has been removed in the new version of the manuscript.

q/ Figure 1: You need to produce a real map showing the location of the study area in the context of the European Alps. Just putting a rectangle on a DEM is entirely inadequate. Please contact a cartographer for advice or assistance.
This figure has been modified and separated to show both the location of the study (a) and its orography (b).

r/ Figure 2: This is really confusing. Please redraft or have a professional draft this for you.
This figure has been changed .

s/ Figure 3: The fonts are too small and the date format on the x-axis is difficult to understand and inconsistent with other date formats used in the text.
Agreed. Figure 3 has been changed

t/ Figure 6: It is not valid to compute a linear regression with so few points. You should not show the lines.
Agreed. Figure 6 has been changed and the regression lines were removed.

u/ Figure 7: Again, you have a new date format. Please choose one format and be consistent throughout the paper.

Dates formats have been modified throughout the paper (see answer to comment k/).

Additional and added references :

Flin, F., Lesaffre, B., Dufour, A., Gillibert, L., Hasan, A., Rolland du Roscoat, S., Cabanes, S. and Pugliese, P., On the Computations of Specific Surface Area and Specific Grain Contact Area from Snow 3D Images, Physics and Chemistry of Ice 2010, 321-328, 2010.

Nolin, A. W., and J. Dozier, Estimating snow grain size using AVIRIS data, Remote Sensing of Environment, 44, 231-238, doi: 10.1016/0034-4257(93)90018-S, 1993.

Nolin, A. and Dozier, J., A hyperspectral method for remotely sensing the grain size of snow, Remote Sensing of Environment, 74, 2, 207-216, doi:10.1016/S0034-4257(00)00111-5, 2000.

Nolin, A. W., and Payne, M. C., Classification of glacier zones in western Greenland using albedo and surface roughness from the Multi-angle Imaging SpectroRadiometer (MISR), Remote Sening of Environment, 107, 1-2, 264-275, doi:10.1016/j.rse.2006.11.004, 2007.

Dozier, J., Schneider, S. R. and Mcginnis, D. F., Effect of grain size and snowpack water equivalence on visible and near-infrared satellite observations of snow, Water Resources Research, 17, 4, 1213-1221, doi:10.1029/WR017i004p01213, 1981.

Dozier, J. and Marks, D., Snow mapping and classification from Landsat thematic mapper data, Annals of Glaciology, 9, 97-103, 1987.

Wiscombe, W. J. and Warren, S. G., A model for the spectral albedo of snow .1. pure snow, Journal of the atmospheric sciences, 37, 12, 2712-2733, doi:10.1175/15200469(1980)037<2712:AMFTSA>2.0.CO;2, 1980.

Bourdelles, B. and Fily, M., Snow grain-size determination of Landsat imagery over Terre Adélie, Antarctica, Annals of Glaciology, 17, 86-92, 1993.

Kuchiki, K., Aoki, T., Niwano, M., Motoyoski, H. and Iwabuchi, H., Effect of sastrugi on snow bi-directional reflectance and its application to MODIS data, Journal of Geophysical Research-Atmospheres, 116, D18110, doi:10.1029/2011JD016070, 2011.

Zhuravleva, T. , and Koknanovsky, A. A., Influence of surface roughness on the reflective properties of snow, Journal of Quantitative Spectroscopy & Radiative Transfer, 112, 8, 1353-1368, doi:10.1016/j.jqsrt.2011.01.004, 2011.

Leroux, C., and Fily, M., Modeling the effect of sastrugi on snow reflectance, Journal of Geophysical Research – Planets, 103, E11, 25779-25788, doi:10.1029/98JE00558, 1998.

Hudson, S. R. and Warren, S. G. , An explanation of the effect of clouds over snow on the top-of-atmosphere bidirectionnal reflectance, Journal of Geophysical Research – Atmospheres, 112, D19, D19202, doi:10.1029/2007JD008541, 2007.

Sergent, C., Chevrand, P., Lafeuille, J., and Marbouty, D.: Caractérisation optique de différents types de neige, extinction de la lumière dans la neige, Le Journal de Physique Colloques, 48, C1–361–C1–367, doi:10.1051/jphyscol:1987150, 1987.

Comment RC C1119, Thomas Painter, 05 Aug 2012

This paper describes an analysis of topographic treatment of solar radiation in mountainous terrain with the intention of quantitatively retrieving snow specific surface area from NASA Moderate Resolution Imaging Spectroradiometer radiance data. The central model treats anisotropy in the hemispherical irradiance per pixel and snow anisotropic reflectance and the differences between spherical and nonspherical assumptions of particle shapes.

The motivation for this paper is strong – snow specific surface area/snow grain size is important for understanding the metamorphic state of the interface of snow with the atmosphere and incident radiation. In turn, the SSA/grain size partially controls snow albedo and modulates the influence of impurities on albedo and radiative forcing by those impurities.

This paper will be suitable for publication once some issues are addressed – mainly correction of the context of this retrieval relative to those already existing for grain size/SSA and more comprehensive discussion of the vulnerabilities of this approach. Nevertheless, it is worthwhile to pursue. Below, I describe the major issues that should be addressed before publication and then page/line-wise indication of issues.

We thank Thomas Painter for its constructive review and the relevant comments he raised upon the paper. We particularly spent time to refine the numerous imprecisions he indicates, including : the detection and treatment of mixed pixels, and the confidence in NDSI criteria; some misinterpretations from Painter et al. 2009 about ratioing technique; we also specified additive statistics about geometry considerations; the discussion on the sign of d(alpha)/dTE has also been improved.

Finally, we would like to recommend that the reviewer reads the whole revised version of the paper since many changes have been performed also in the structure of the paper, for example the entire introduction has been rewritten.

1/ Specific major issues: Mixed pixels The approach described here focuses on the topographic influence on retrieval of snow SSA but does not address mixed pixels. At the spatial scale of the MODIS pixel (even at nadir), homogeneous snow pixels are rare in rough mountain terrain such as the Haute-Alpes/Isère. Non-snow surfaces such as rock and vegetation have hemispherical-directional reflectance factors (HDRF) that are relatively orthogonal to those of snow. Therefore, a mixed pixel will have an inferred HDRF that is contaminated, influencing the retrieval of the SSA by any of the band scenarios described here.

Additionally, vegetation and rock through their surface roughnesses have directional reflectance distributions with prominent backscattering components unlike snow, which has distinct forward scattering with a minor backscatter. The mixed pixel then has a directional reflectance distribution that is a composite of those from snow and from the non-snow exposed surface.

The reviewer is correct that spectral mixture from non-homogeneous pixels would cause problem to the retrieval method. Nonetheless, as clarified in the response to the next comment, the retrieval method used here was only processed on pixels that could be considered to be fully covered by snow. Therefore, our research eluded the problem of mixed pixels at the expense of the inability to estimate SSA in partly snow covered pixels on the contrary of what has been done by

Painter et al, 2009.

To clarify this point we add a paragraph on surface roughness in the discussion (lines 551 to 568). These discussion was also include in the conclusion (lines 702 to 704) and to the abstract (line 16)

In order to address the reviewer's comment, this point was also emphasized in the response to the next comment. See also response to comment 4 of A. Nolin.

2/ The paper indicates that it uses the normalized difference snow index to mask for snow covered pixels but we know from Salomonson and Appel (2004, 2006) that for a given NDSI value, there is a large range of fractional snow covered areas. As such, the NDSI cannot be treated as a reliable metric by which to find pixels that are covered 100% by snow. No details are provided as to the interpretation beyond the simple calculation of the NDSI.

The reviewer is right to stress the fact that NDSI is not a perfect metric and that pixels less than fully covered by snow may still exhibit a NDSI value relatively high, possibly above a threshold considered to be representative of full coverage. However, Salomonson and Appel (2006) also noted the fact that the variability of snow coverage for a given NDSI originates from "variability of terrain and accompanying shadowing, variability in land cover (…), and atmospheric variability (…)." In the context of our research, we processed MODIS data to retrieve ground reflectance values corrected for both atmospheric and topographic effects as presented in Sirguey et al. (2009). We also excluded pixels in shadow from the retrieval method as stated in p1928-1929. By minimizing the main causes of variability in the NDSI response to the snow covered fraction, we believe reasonable to rely on a subjectively high threshold on the NDSI (computed from the corrected value) to identify pixels fully (or close to fully) covered by snow.
To address the reviewer's comment, we have clarified the use of the NDSI as a mean to identify pixel fully covered by snow (see changes lines 327 to 338)

See also response to your comment 1/ for further modifications on mixed pixels.

3/ Moreover, you use a 90 m DEM and downgrade it to 125 m (without acknowledging uncertainties injected by that step) and 500 m. In either case, 125 m or 500 m, rough mountain terrain is not represented well – sub-125 m variation in slope and aspect, and in turn local irradiance and local view geometry are markedly different from those calculated from a kernel of a 125m and 500m.

Indeed, the terrain roughness is a remaining issue of such retrievals in mountainous areas until one reaches decametric scales. We thank you to underline it, and we will in turn address it more explicitly in the paper (see changes lines 137 to 138).

4/ Geometry Despite the fact that you are addressing topography and anisotropic reflectance, there is never mention (that I can find) of the local sensor zenith angle for the particular scenes that you are using and the impact of sensor zenith on ground instantaneous field-of-view (GIFOV). The at-surface range of sensor zenith angles of nadir to 65 result in a variation in pixel size from 463 m at nadir to twice as large in along-track and nearly five times in cross-track – nearly 10 times the area. As such, pixels are far more likely to be mixed with respect to surface cover and distribution of subpixel surface slopes and aspects. In turn this markedly affects the directional reflectances and the topographic interpretation from the 125m and 500 m SRTM DEM sets. This

must be addressed before publication particularly given the core topic of the paper.
Note that later you indicate (p1921 line 23) that the MODIS sensor was chosen because “it provides daily coverage of the area of interest”.

The viewing and illumination geometry is fully taken into account when computing ground reflectance. The computation of the local angles that account of the local topography is provided in appendix in Dumont et al. (2011). We have addressed this point by adding a “tilde” on the view zenith angle in consistence with notations in Dumont et al. (2011) and clarified the definition of the angles in the text accordingly.
As for the panoramic (bow-tie) effect, we agree with the reviewer that the effective ground pixel size towards the side of the swath favours spectral mixture and may become problematic. However, as explained and clarified in our response to your comments 1/ and 2/, we ensured that only pixels that could be reasonably regarded as fully (or near-fully) covered by snow were considered in the retrieval method. The problem of mixture happening at the native or distorted pixel size is therefore not consequent. It is a problem inherent to all whisk-broom scanners that we believe do not needs further emphasis.
In addition, the range of absolute sensor zenith angle, i.e. not taking into account local topography, for the MODIS images presented in this study is 2-27°, and varies no more than by 4° within a single image.
Nevertheless to underline this problem at the borders of the swath, we add some details on this subject lines 155 to 157.

6/ Grain shape What are the non-spherical grain shapes that are used? “Fractal” is not descriptive enough. You allude to the modeling by Kokhanovsky and Negi/Negi and Kokhanovsky in which they used plates and columns. These too are not physically consistent with observations in the snowpack except immediately after snowfall. Please give more description.

Thank you for pointing this imprecision. The complete description of the fractal model is given by Macke et al. 1996. We agree that this is not physically consistent with observations but this grain shape model has proven its capabilities to correctly describes snow BRDF is comparison with observations (Kokhanovsky et al., 2005).
These changes have been added lines 368 to 379.

7/ Perspective This will be a clarification of Painter et al 2009 in light of the authors' misunderstanding or misinterpretation of our paper. This point has little bearing on the results of the Mary et al paper but given that it features prominently in the Introduction, it warrants correction.

The text states, “In addition, the snow end-members used in MODSCAG are based on theoretical spectra whereby snow grains are assumed to be spherical, the effect of soot on reflectance is ignored, as well as the effect of the anisotropy of snow reflection.” True that we model snow endmembers under the assumption of spherical particles, true that we consider clean snow as opposed to that affected by dust or soot. However, it is not true that we ignore anisotropy of snow reflectance. The snow endmembers are HDRF – so, they are expressly addressing the directional reflectance. However, the misinterpretation may come because we do not vary the directional snow endmembers with view geometry. This is based on the work presented in [Painter et al., 2003; Painter and Dozier, 2004] in which we show the relative insensitivity of the MEMSCAG/MODSCAG approach for local view zenith angles of < 40 and the relative paucity of local view zenith angles that exceed 40. We can then maintain the computational efficiency of the algorithm.

Thank you for this clarification and sorry for this misinterpretation. We modified the text as follow : "...the effect of soot on reflectance is ignored, and the effect of the anisotropy of snow reflection is addressed with assumption of constant view geometry. Recent updates of MODSCAG make it possible to include the impact of the surface radiative forcing of light absorbing impurities in the snowpack (Painter et al., 2012)"

8/ The text also states on p1929, lines8-9 "2. Relying on absolute reflectance values or on the relative shape of the snow's spectrum (i.e., the ration between SSA-sensitive bands and band 4, e.g. Painter et al 2009, ...). It is not at all apparent how the authors have interpreted that Painter et al 2009 suggests the use of band ratios. Perhaps it is a nomenclature issue? Band ratios are as follows: band1/band3. They are used in some remote sensing interpretations but we do not use band ratios. We use matrix inversions in which the solution vector space is spanned by the endmembers. The matrix inversion is solved with the Q-R decomposition in the Modified Gram-Schmidt orthogonalization.

Thank you for this explanation, it was also a misinterpretation. The text is modified as follows : « Relying on absolute reflectance values or on ratios between the above SSA-sensitive bands and band 4 (Negi and Kokhanovsky, 2011a,b; Zege et al., 2011). This ratioing enables to account for the relative shape of the snow's spectrum instead of absolute reflectance which could be affected by e.g. by atmospheric perturbations, or local shade. »

9/ Ultimately, the MODSCAG algorithm accounts for fractional snow cover in retrieving grain size (SSA), whereas the algorithm presented in the Mary et al paper requires homogeneous snow cover. MODSCAG does indeed though make the assumption that the directional reflectances do not vary markedly with sensor zenith angle so that it can maintain its computational efficiency.

Agreed. This was addressed in the response to your comments 1/ and 2/.

10/ Lack of field measurements The authors acknowledge that the uncertainties of this retrieval would be better understood with field measurements instead of the SAFRANCrocus results. There is no problem with the comparison with SAFRAN-Crocus. However, the authors mention the inference of SSA from the integrating sphere technique of Gallet et al 2009 for future validation. Given that Gallet et al have made measurements already and MODIS covers Earth, why can they not be used here? Please address.

Thanks a lot for this remark. This is completely true that the method will greatly benefit from comparison to field measurements. The SSA measurements made by Morin et al., 2012 at Col de Porte site are nevertheless not usable for our purpose since the location of the measurement is surrounded by forest and the open area is too small with respect to the size of MODIS pixel. The measurements presented by Gallet et al., 2010 are a bit out of purpose in our case since they were made in Antartica over flat areas. Gallet et al., 2010 describes the comparison of the albedo modelled by DISORT using their measured SSA profiles and MODIS albedo products showing relatively good agreement between the two. In this area, we are not convinced that using topographic corrections will largely change the results of the retrieval. Consequently, we propose to add to the paper, a comparison between MODIS retrieved SSA with the methods presented in the paper and two days of measurements in the French Alps. All these changes have been reported in the response to comment 1/ of A. Nolin.

Specific points:
p. 1917 Snow covers a large part of the Earth's land surface.
The sentence has been modified as you suggest.

p. 1917 Since it is . . . -> reference [Flanner et al., 2011]
The reference has been added as suggested.

p. 1918 The justification of the use of SSA versus grain size is rather weak.
Sorry for this imprecision. If I understand properly you suggest to better justify the choice of SSA versus ropt ? In this paper we surely did not want to use SSA more than grain size (that is why we tried to present the results both in terms of SSA and optical radius and this is discussed lines 368 to 379. Changes have also been performed in the introduction lines 75 to 85.

p. 1918 Gallet et al are not the only ones making these measurements - [Matzl and Schneebeli, 2006; Painter et al., 2007]. Moreover, all of these measurements are natively point measurements and not spatial.
Thanks for this suggestion. All the section on the measurements of grains size have been removed from the introduction.

p.1919, line 4 spaceborne and airborne
Changes have been done as suggested. Thanks.

p. 1920, 22 be more specific about "radiation". Shortwave, longwave, reflected, emitted?
Thanks for noting this imprecision. We hope it has been clarified by changing line 187 into "precipitation, <u>and downward longwave and shortwave direct and diffuse incident radiation</u>. "

p. 1925, 2 point to the website – this paper is UV/VIS.
Thanks for this suggestion. Lines 179-180 have been changed to "has been made by Warren and Brandt (2008). <u>Note that the relevant database is available at http://www.atmos.washington.edu/ice_optical_constants."</u>

p. 1927, 14 Where does SCA actually get calculated ?
The Snow Covered Fraction is calculated after topographic and atmospheric correction. This has been clarified in the response to your comment 2/.

p. 1927, 1 alpha is usually used for albedo or spectral albedo
Indeed, usually alpha is used for albedo, i.e. bi-hemispherical reflectance, and rho is used for BRDF. Therefore, as the reflectances we used are partially integrated, we prefer to use alpha to represent it.

p. 1927, 4 explain better the place of R here while not addressing the Ediff
Thanks for this remark, indeed Ediff is not multiplied by R here since we used the (clearly unrealistic) assumption that the diffuse part of the illumination is isotropic. This is explained in Eq. 5. Consequently, while integrating over the hemisphere, the directional-hemispherical reflectance appears instead of the bi-directionnal reflectance for the direct part of the illumination (see Eq. 6). All this is clearly described in the paper page 1926 and we did not want to bother the reader with

too much equations so we did not make in change on this point.

p. 1927, 24-25 "measured values of the anisotropy factor R . . ." how many, how were they applied?
Values of bi-conical reflectances have been measured on four different snow samples in cold rooms for 108 different geometries. From this bi-conical reflectances distribution, the anisotropy factor is inferred. The correction applied here is the average anisotropy factor found for the three different snow samples (fresh snow, rounded grains and recognizable particles). This correction is really rough but here we just aim at evaluating its impact in comparison with more robust anisotropy corrections (ART). All this is described in Dumont et al., 2010.
To clarify the paper, we change lines 310-311 by "In other words, mean anisotropy factors inferred from measurements on three types of snow and under three different illumination angles by Dumont et al. (2010) were used in Eq. (7)"

p. 1927, 23 remember that mixed pixels will require a different directional "correction"
Thanks for this remark. We think this has been addressed and clarified in the paper as described in the responses to your comments 1/ and 2/.

p. 1928, 14 DISORT calculations to 88 will be highly uncertain
Thanks for this remark on which we completely agree. The proposed changes have been included line 342-344 "... MODIS band 1 to 7. DISORT calculations for high zenith angle are highly uncertain but these high zenith angles concern very few pixels of the MODIS data. Average viewing zenith angle varies between 23 and 32° depending on the date. "

p. 1928, 16 this paper ignores impurities as well.
Right. See response to your comment 7/
We also modified the sentence line 344-346 to "in the wavelengths available that are the most sensitive to SSA..."

p. 1929, 6 band 2 (0.858µm) is affected by impurities
The reviewer is true. This point of the four bands method has been inserted in the text : "using MODIS band 5 only or using all SSA-sensitive MODIS bands (i.e., bands 2, 5, 6, and 7), although band 2 is sensitive to impurities".

p.1929, 1-2 this relationship is highly uncertain – look at their plots – then how do you apply it here? What uncertainties do you have in SSA retrieval relative to the NDSI retrieval?
Thanks for this remark. This point has been addressed in the response to your comment 2/.

p. 1929, 14-20 you need to mention [Nolin and Dozier, 1993] for similar technique.
Thanks. The following precision has been now added : "Similarly to Nolin and Dozier (1993), for each pixel to be processed ...".

p. 1930, 1-9 how is this a test of grain shape if you do not use the same algorithm/method?

Thanks for this remark. This may be a bit confusing in the paper. Indeed, the method is not the same, so that this comparison embodies as much the effect of grain shape as the impact of the algorithm used. However, the benchmark of this comparison is the standard retrieval from MOD09 data, without topographic correction. In the one case, we evaluate the contribution of topographic

correction, and in the other case, the contribution of grain shape modelling and a more comprehensive algorithm. Combining both intakes will be object of future work, as both have proved to improve the results in their ways.
This point have been clarifier in the paper lines 378-379.

p. 1930, 24 the Grandes-Rousses massif has strong spatial mixing at the MODIS pixel resolution
Yes, you are completely right. The effect of spatial mixing is now discussed in the end of the first part of the discussion and in the conclusion. See response to your comments 1/ and 2/). Thanks for underlying this point.

p. 1931, 1-4 what are their view geometries in the Grandes-Rousses?
The mean viewing angle varies between 23 and 32° depending on the date. This information, which is missing as you noticed, has been added in section 3.3 (see comment about p. 1928, 14).

p. 1931, 2 need a figure that shows the setting, topography, etc. perhaps a photograph
If we understand correctly, you suggest to show the topography of the studied area. A new digital elevation model has been added to the paper as requested by A. Nolin (see response to her comment q/). We hope this answers to your suggestion.

p. 1931, 7 "reported as accurate in terms of SSA" – accurate is a relative term. Give the quantitative values.
The RMSD of their study is already reported at page 1923, line 3. We also add the value of the RMSD in the conclusion.

p. 1933 the discussion of the change d(alpha)/dTE is not sufficiently clear to justify the conclusion that ". . .leading to d(alpha)/dTE < 0." This needs considerable improvement. Moreover, a better description of TE is needed from the outset.
This comment have been taken into account by modifying the concerned parapgraph. See changes lines 478 to 499.

p. 1934 the description of the band ratioing is very difficult to find
Thanks for this remark. "The use of band ratios instead of" is replaced by "The use of band ratios (as defined in Sect. 3.4) instead of".

p. 1934 again with "Painter et al (2009) suggested the use of band ratio in order to overcome the error on absolute reflectance, largely due to ignoring topographic effects." Where does this come from? We do not use a band ratio. Are you talking about the shade-normalization? That comes from the additive complement to the sum of the coefficients in the matrix inversion.
Again after comment 8/, this reference to Painter et al. 2009 concerning band ratioing is a misinterpretation. The sentence was modified accordingly

p. 1935, 13 "non-neutral"??
By non-neutral we meant different from 1. This sentence has been removed in the new version of the manuscript.

p. 1935, 16 anisotropy correction is to the reflectance, not the SSA. Downstream of the correction of the reflectance is the impact on SSA.
Sorry for this shortcut. This paragraph (lines 536-544) has been entirely rewritten.

p. 1935, 21 anisotropy factor is not measured, it is inferred from measurements

Another shortcut. "anisotropy factor measured by" replaced by "anisotropy factor inferred from measurements by", and also, line 541 : "measured values of the anisotropy factor R given by Dumont et al. (2010) were used in Eq. (7)." is replaced by "mean anisotropy factors inferred from measurements on three types of snow and under three different illumination angles by Dumont et al. (2010) were used in Eq. (7)."

p. 1935, 25 seems to make sense that the fractal grains would have larger SSA, right?

We are not sure to understand exactly the point you made here, the considered paragraph has been entirely rewritten. Anyway, it makes sense that the fractal grains would have larger SSA (more surface than spheres).

p. 1936, 19-26 it is not clear what "asymmetry" you are describing here

Indeed we should clarify : the asymmetry described is between the SSA bounds of the Crocus model and that of the MODIS-retrieved SSA, as the first one goes from 1 to 65 m^2kg^{-1} and the second from 2 to 160 m^2kg^{-1}. The second is hereby more susceptible to have greater SSA. We propose to replace "This discrepancy introduces an asymmetry between the two SSA supports." by "This discrepancy between the two SSA ranges may introduce an asymmetry in their statistics, as extreme values of SSA would pull up mean values or standard deviations."

p. 1937, 4 please explain "signal entropy" in this context

The sentence is explained by the end of the paragraph, describing the degrees of freedom of both SAFRAN-Crocus data and MODIS. Anyway, we will add the following to be clearer : "... different signal entropy, i.e. even on the same number of pixels, the two data have different degrees of freedom. Indeed, while SAFRAN-Crocus ..."

p. 1938, 25 mixture of terminology – "grains growth at low SSA"

Thanks for pointing this imprecision. "grains growth" is replaced by "SSA decrease at low SSA".

p. 1939, 16-17 snow's forward scattering peak is more often sampled from MODIS here

Agreed. On these sun facing slopes the snow's forward scattering peak is more often sampled from MODIS than for the other slopes. Consequently when not applying any anisotropy correction, the MODIS retrieved SSA would be higher than the "real" one. Consequently we add the following sentence lines 664 to 668. "In addition, the snow's forward scattering peak is more often sampled from MODIS for sun facing slopes. Consequently , the MODIS retrieved SSA with DTA may be biased high. This suggests that the SSA decrease is even more pronounced in reality than the DTA method shows."

p. 1939, 24 monotonic

Thanks. "monotonous" has been replaced by "monotonic".

p. 1940, 5 replace "confrontation to" with validation against. Again, why have you not used the previous measurements of Gallet?

Thanks. "confrontation to" is replaced by "validation against". See answer to comment 10 for the second part of the question.

p. 1940, 19-20 "decreases with the incidence of the solar radiation" – this sentence is not clear.

To be clearer, we propose to replace it by "decreases with the amount of incident solar radiation which is linked to the solar zenith angle ".

p. 1941, 1 “very close” has no meaning. Be quantitative.
Thanks for underlining this imprecision. This sentence has been removed in the new version of the paper.

p. 1942, 16 not your title, but note that it is physically impossible to measure the bidirectional reflectance distribution function
We entirely agree with this. The quantity which is measured is in fact a bi-conical reflectance and this is discussed page 2510 of Dumont et al., 2010.

p. 1949 again, the “band ratio” is hard to find.
The clarification done page 1934 with reference to the definition of the ratioing in Sect. 3.4. should make the reading clearer.

p. 1955 no remotely sensed images are shown – please insert a color composite of MODIS images for each of these dates.
The new figure 4 has been inserted.

p. 1957 indicate the 1:1 line.
The 1:1 line is already present in black and we hope that the removal of the regression lines, requested by the other reviewer, makes it clearer. Please see answer to comment t/ of the other reviewer.

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