Response to the reviewers

We thank Dr. Kokhanovsky and the two anonymous reviewers for their remarks. We made modifications to the initial manuscript accordingly.

Note : original reviewers's comments are in black while our responses are in blue. Modifications of the manuscript are reported in bold and italic. All references can be found at the end of the document.

Preliminary modifications:

In the manuscript, it was stated that TARTES used the delta-Eddington approximation. In fact the version used for the paper was based only on the Eddington approximation. Hence we changed « delta-Eddington » into « **Eddington** » where it was necessary.

We added a reference in the introduction to Haussener et al. (2012) :

P2805, l.7:
“[...] twice greater than those predicted by a model with spherical grains (their Fig. 6). **More recently, Haussener et al. (2012) have compared the transmittance of snow slabs obtained by Monte-Carlo ray tracing performed on computed tomography images to the transmittance calculated with DISORT assuming equivalent spheres. They found that transmittance computed with DISORT is always much larger than that obtained with the ray tracing method (their Figs. 9b and 9d). They attribute part of the difference to the simplification of snow morphology in DISORT**.”

Figures 2 and 3 have been modified so that the irradiance profiles are now normalized at the surface to be more easily compared one with each other.

Response to Dr. Kokhanovsky (reviewer #1):

The paper is aimed at the studies of the influence of the grain shape on light extinction in snow. The paper is well written and can be published after minor corrections given below:

1. I would suggest the modification of the title. Interaction of light with snow is described in terms of extinction, scattering, and absorption. Extinction is usually referred to the attenuation of direct light beam by a snow (falling or on the ground). The authors consider not extinction coefficient given by Eq. (1) but rather AFEC given by Eq. (8). This must be specified in the title and also in the paper.

The title was changed according to Dr. Kokhanovsky's remark:
"**Influence of grain shape on light penetration in snow**"

To avoid confusion with the original definition of extinction, the term "extinction" has also been removed throughout the manuscript. It has been replaced by various formulations:
"irradiance profiles", "e-folding depth", "irradiance measurements"...

2. I suggest that the authors modify Table 3 and Fig.6. They mention that the values of B below 1.0 are not physical. The corresponding values above 2.0 are also hardly physical. The reason that the authors obtain too low or too high values of B is because they applied the retrieval algorithm at the conditions, where their theory does not work.

It is not pretended that all the retrieved B values are physical, they are just the result of our algorithm applied to data that were estimated reliable. While 1.0 is probably a lower theoretical limit for B, in Kokhanovsky and Macke (1997), the maximal value of B is obtained for spheroids with parameter ξ=0.3 and equals 3.16. According to this theoretical upper value, Fig. 6 was modified so that all values above 3.16 and below 1.0 are dashed and explicitly referred as questionable. This was also specified in Sect. 4.3.2 :

P2821, l.15:
"A **very** wide range of B is obtained, from 1.0 to 9.9. **The largest theoretical value of B found in the literature was obtained by Kokhanovsky and Macke (1997) for spheroids with shape parameter xi=0.3 and equals 3.16. As a consequence, the values obtained from the literature that exceed 3.16 are questionable.** Intuitively, high [...]"

Table 3 caption:
"The 5 values of B **larger than 3.16** and the corresponding \alpha values are bold. **These values are questionable**."

An important point of the discussion is to address the reasons why such hardly physical values are retrieved. The limit of validity of the theory is part of the explanations, as well as the quality of the measurements. However no data can be excluded from our analysis on objective criteria. The assumptions made in the theoretical framework are not obviously broken. The assumption that all scatterers are independent may be questionable for high density snowpacks, but still it is widely used in the snow optics community. For these reasons no value is excluded from Table 3 and Fig.6. However, as suggested by Dr. Kokhanovsky, it is pointed out that in some cases, the algorithm may have been applied outside the range of validity of the theory:

P2822, l.3:
" […] the quality of the diffuse incident flux may also be accounted for. **Eventually, the snow physical properties for those experiments (density, grain size) may be outside the range of validity of the**

theoretical framework presented in Sect. 2 and partly explain these high B values. "

3. I suggest to make a reference to the following papers, where similar problems have been considered: Kokhanovsky, A., 2013: Spectral reflectance of solar light from dirty snow: a simple theoretical model and its validation, Cryosphere Discussions, 7, 533- 550 Kokhanovsky A. A., Breon F.-M., 2012: Validation of an analytical snow BRDF model using PARASOL multi-angular and multispectral observations, IEEE Geosci.
Rem. Sens. Letters,9, 928-932. Negi, H. S., Kokhanovsky, A., and Perovich, D. K.: Application of asymptotic radiative transfer theory for the retrievals of snow parameters using reflection and transmission observations, The Cryosphere Discuss., 5, 1239- 1262, doi:10.5194/tcd-5-1239-2011, 2011. Kokhanovsky, A. A., 2006: Scaling constant and its determination from simultaneous measurements of light reflection and methane
adsorption by snow samples, Opt. Letters, 31, 3282-3284.

References to suggested papers were added:

P2812, after Eq. 25:
" **Similar expressions have been used by Zege et al. (2008) and Kokhanovsky (2013) to take into account analytically the influence of impurities on snow albedo.** "
P2815, l.6:
" **The median value for B/(1-gG) is 4.7, in agreement with the scaling constant determined by Kokhanovsky (2006) for natural snow (that corresponds to a ratio of 4.6). For spheres, B/(1-gG)=5.7 and there are shapes above and below** the horizontal line"

Response to anonymous reviewer #2 :

General Comments:

This paper evaluates the spherical snow grain assumption that many snowpack radiative transfer models use to calculate albedo and transmission of light through snow. A multi-layer snowpack radiative transfer model (TARTES) is developed that allows the asymmetry parameter and the absorption efficiency parameter to be altered to best match measured radiation profiles in snow at Dome C and in the French Alps. A Monte Carlo (ray tracing) model is used to estimate the asymmetry parameter and absorption efficiency parameter for various geometric shapes. The authors find that the absorption efficiency parameter in simulated snow containing only spherical snow grains is underestimated, which leads to higher e-folding depths compared to the e-folding depths measured in snow at Dome C and in the French Alps. Modeled absorption efficiency parameters are compared to inferred absorption efficiency parameters in the literature. It would be useful for the authors to consider transmission of radiation in the UV and near-visible wavelength regions.

It would be interesting to compare observed UV/near-vis radiation profiles with modeled profiles to obtain an estimate for B. Perhaps this could be a point of future study. This is a well-written paper with a good deal of insightful discussion. I recommend that this paper be published after the minor corrections are addressed below.

We are very grateful to the anonymous reviewer #2 for the multitude of helpful comments. We have carefully considered each point and our responses are provided below.

The optical measurements necessary to retrieve B were taken in the visible-NIR range but the retrieved B values should be very close to the values in the UV-near visible range since B depends essentially on the real part of the refractive index, which barely varies between 300 nm and 1350 nm (it varies from 1.33 to 1.30). From computations of Kokhanovsky and Macke (1997), variations of B in the working spectral range can be estimated to about 2%. Two modifications were made in the manuscript to make this point more clear:

P 2808, l.7

"In the **spectral range 300-1350 nm**, ice is weakly absorbing and the real part of the ice refractive index is nearly constant **(Warren and Brandt, 2008), so that variations of B and gG are less than 3% (Kokhanovsky, 2004). Hence in TARTES, B and gG are assumed independent of the wavelength. "**

P 2823, l.21

"In this paper […] and TARTES model. **Even though the optical measurements are taken in the visible-NIR range, the retrieved values of B hold for the whole range 300-1350 nm (see Sect. 2). These B values –** obtained [...] "

To estimate B, wavelengths where the impact of impurities is minimal are chosen. Using UV-near visible wavelengths would be problematic since several different impurity species should be considered, that strongly alter snow optical properties and the quality of the retrieval algorithm.

Specific Comments/Technical Corrections:

P 2802

12: state what the parameters B and gG represent

Their explicit names have been specified:

“[...] **the grain shape is fully described by two parameters: the absorption enhancement parameter B and the geometric asymmetry factor gG**.”

14: state which macroscopic optical properties are impacted

“ to understand how they impact **the albedo and asymptotic flux extinction coefficient”**

27: consider adding “but not necessarily on the albedo of snow” after AFEC. Also, what is meant by “natural” snow?

Suggesting complement added as follows:

“natural “ is opposed to the numerical study performed on geometrical grains. For clarity the term “natural” has been removed.

NB: According to the comments of reviewer #3, the whole abstract was rewritten (see response to reviewer #3):

P 2803
8-11: The amount of solar absorption in snow is sensitive to changes in grain size and impurity content. The light extinction in snow is influenced by both changes in snow grain size and impurity content, but the albedo is not influenced by impurities until the impurity levels are high enough [Warren et al., 2006]. Perhaps alters the sentence to acknowledge that grain size and impurity content (both considered snowpack physical properties) do not influence the albedo in the same way.

The physical properties of interest for each quantity were detailed (density, grain size and impurity content) and the dependence of light penetration to very low amounts of impurities was explicitly mentioned. As you say the albedo is not sensitive to very low amounts of impurities, but the co-albedo (which quantifies the amount of absorbed energy mentioned in the text) is. For this reason it is not specified that levels of impurities should be high enough to influence energy absorption.

P 2803, l. 11:
“slight changes of **grain size and impurity content**”

P 2803, l. 13:
“[…] below the surface (Colbeck, 1989). **Light penetration depends on density, grain size and is sensitive to very low amounts of impurities**. Although Brandt and Warren (1993) [...] “

13-14: Brandt and Warren [1993] state that IR radiation is absorbed in the top few mil-limeters. For photochemical reactions, the UV and visible wavelengths are of interest. It would be helpful to be more specific about the wavelength regions of interest in this sentence.

This was specified as follows:

“Although Brandt and Warren (1993) highlight that most of the absorption occurs **in the IR and takes place** in the very top centimeters of the snowpack, the penetration of light has a crucial impact on the thermal regime and on the availability of photons for photochemical reactions **in the UV and visible**.

19: No comma needed after “energy”
Corrected

27-28: Remove comma after "properties" to make the sentence clear.
Corrected

P 2804
7: It would be good to mention equivalent spheres explicitly in this sentence.
Done

26: Zatko et al., 2013
Reference updated

P 2805
14: Is "it" referring to the derivation?

" it " refers to the expressions of B and gG: so " it shows " was changed into " **these expressions show** "

P 2806
7: It would be helpful to mention whether these quantities can be measured in a bulk sense or if they can be measured in individual layers in the field.

This properties can be measured for the whole snowpack (classic albedo measurements) or layer by layer using a profiler such as ASSSAP. This is now specified:

"only macroscopic optical properties **of the whole snowpack or of individual snow layers** are measurable"

13: "profiles" instead of "profile"
Corrected

26: Ice is weakly absorbing throughout this region (compared to the IR), but ice becomes more absorptive towards the IR, which might be worth pointing out. At 1350 nm, ice is considerably more absorptive than at 300 nm.

This distinction between 300 and 1350 nm is now specified :

"we limit our study to the spectral range 300-1350 nm, where ice is **globally** weakly absorbing (Warren and Brandt, 2008), that is $1-\omega \ll 1$, **even though ice is considerably more absorptive at 1350 nm than at 300 nm**."

P 2807
E6: Please mention that this equation calculates the "co-albedo of single scattering"

"it follows " has been replaced by "**the single scattering co-albedo is given by** "

P 2808
3: Briefly explain why gD does not change when the snow grain size and shape is changed although gG does change.

Details are now given:

" […] and the diffraction term gD. **For particles large compared to the wavelength, diffraction is essentially forward so that $g_D \approx 1$ and** : »

8: Cite Warren and Brandt [2008] (or another comparable reference) when stating that the real part of the imaginary index by constant at those wavelengths

Reference added

12: Please add a sentence or two (or perhaps another intermediary equation) to describe how the authors used E10 and E11 to obtain E12.

To obtain E12 from E10 and E11 the log of E11 should be taken and then multiplied by E11. Reference to the two parent equations is now explicit. It is also specified that both ke and α are wavelength dependent so that the wavelength dependence in E12 is not disturbing. After E12, the following text was added:

" **In Eqs. (8)-(11) the quantities ke and α are wavelength dependent since γ is. From the combination of Eqs.(10) and (11)**, the value B of a semi-infinite homogeneous slab of snowpack can be estimated.»

P 2810

2: "independent"

Corrected

P 2812

6: "proportional"

Corrected

E23 and 25: should lm be ln?

Im refers to the imaginary part of the complex number in parenthesis

P 2813

7: g is the asymmetry factor, which contains gG. Because the asymmetry factor from other studies is used for comparison in the discussion section, it would be more clear to refer to gG as the geometric diffusion term rather than the asymmetry factor throughout the manuscript.

The nomenclature was changed throughout the manuscript: "**asymmetry factor**" now refers to g only and gG is called the "**geometric asymmetry factor**" (no equivalent name wast found in the literature)

P2815:

6-8: Considerably more shapes are below the iso-albedo line than above in Figure 1b. It is not clear to me that a statement about median albedos being that of spheres can be made from the data in Figure 1b.

The statement about median albedos being that of spheres has been removed. The sentence has been altered and reformulated to be more precise :

" […] are very similar to spheres in terms of albedo, but not in terms of AFEC. **Fig.1b also highlights the position of spheres among all other shapes. The median value for B/(1-gG) is 4.7, in agreement with the scaling constant determined by Kokhanovsky (2006) for natural snow. For spheres, B/(1-gG)=5.7 and there are shapes above and below the horizontal line, suggesting that spheres may be a fairly good approximation to a mixture of various geometric shapes.** This might explain the success [...]"

To be consistent between the albedo and AFEC analysis, quantitative information is also given for the position of spheres in terms of AFEC:

" […] since the product B(1-gG) is minimum for spheres, **almost two times smaller than the median value**."

9: "those" instead of "that"
Corrected

22: Clarify the difference between AFEC and ke in the manuscript.
There is no difference between both quantities. ke is **called** the AFEC:

P 2808, l.12:
"[...] the bi-hemispherical albedo α (albedo of a semi-infinite scattering medium illuminated by a diffuse source, hereafter referred as albedo) and the AFEC ke are expressed in terms of g and ω [...]"

The commas have been removed in sentence P 2815 l. 22:
"Knowledge of **the AFEC ke, and the albedo α,** of a homogeneous snow layer"

25: E12 is still used if impurities are present, right? Is it correct that E25 is used in E8 and E9 and then that E8 and E9 are used to calculate B in E12.

Method 1 is based on E12 in its actual form or in a modified form taking into account impurities, deduced from E25, E8 and E9 as you mention.
To analyze data from the literature, E12 in its actual form is used. It has been clarified:
P 2821, l.13:
"B values are calculated using Eq. 12 for all these cases, **that is impurities are not taken into account**."

P2816:
5: What are the values most commonly used for lambda1 and lambda2 (e.g. UV and visible, or visible and IR). I think that this information is reported a bit later in the text, but it would be helpful here to mention the wavelengths used or at least quickly state where this information can be found in the manuscript.

Precisions about the wavelengths used for intensity measurements are given:

P2816, l.9:
"[...] the faster the convergence of the iterative process. **In Sect. 4.2 both wavelengths are in the range 500-780 nm**. Each numerical layer of TARTES [...]"

10: Does the(1-gG)/V term have a straightforward word description to go along with it? If so, include it here.

First, (1-gG)Σ/V is now used instead of Σ(1-gG)/V that looks like a sum.
There is no existing term for this quantity. It could be named "scaled SSA" but this term does not exist yet.

11-12: Mention here why and how the density and reflectance optically measured at 1310 nm.

The use of reflectance and density profiles is detailed further in the algorithm description. The details of the measurements are given in the section Materials, but reference to this section has been added. There is no particular reason to choose 1310 nm, which was not clear in the manuscript. The text was modified to point out that lambda_alpha can be any NIR wavelength. It was also specified that in this particular study 1310 nm is used.

P2816, l.2:
"[...] when vertical profiles of density, \rho(z), **near-IR reflectance, alpha(z,lambda_alpha),** and spectral intensity [...]"

P2816, l.11:
« […] the type and content of impurities must be specified. **Here we consider that density and reflectance at lambda_alpha are given (see Sect. 4.2 for measurements details, where lambda_alpha=1310 nm)**, while B and impurity [...]»

12-13: B should change with depth in actuality because the snow density and extinction coefficient values change with depth. It's okay to assume that B is constant, but acknowledge that it should change with depth.

B should change with snow morphology (rather than with density changes, even though density and snow type can be correlated), hence it should change from a layer to another. It has been specified:

"while B and impurity contents are unknown parameters. **Although B is likely to be different from a snow layer to another since snow type varies with depth, here** B is assumed uniform"

To be consistent, we specified that BC is also assumed uniform:

P2816, l.17:
"**The black carbon content used in TARTES is denoted BC and is also assumed uniform within the snowpack.**"

13-14: It is true that BC is a dominant absorber above 600 nm, but nonBC material dominates the absorption in the UV and partly visible wavelength range. State here that BC is assumed to be the only absorber because this study is concerned with light transmission through snow in the part of the visible/near-IR wavelength range that is closer to the near-IR. If transmission of UV wavelength radiation were considered, it would be necessary to consider nonBC absorbers.

It was specified that measurements are taken in the visible-NIR range. Also, to be consistent with the wavelength used for Dome C measurements and Figure 3 of France et al. (2011), 600 nm was replaced by 500 nm as a lower limit:
"only BC is considered since **it is the major contributor to light absorption by impurities above 500 nm, where optical measurements are taken** (Sergent et al., 1993; France et al., 2011a)."

P 2818:
E29: What is the relationship between lambda1 and lambda2 and lambda_ke and lambda_alpha? It would be helpful to make relationship more transparent so that readers can follow the progression of equations more easily.

Method 1 is based on E12 that requires only 2 measurements, one for reflectance and one for intensity. lambda_alpha and lambda_ke are the corresponding wavelength.
Method 2 is more complicated since it takes into account BC (this is a third unknown) and thus requires

1 reflectance profile and 2 intensity profiles (at two different wavelengths). For consistency between both methods **lambda_1 and lambda_2 were renamed into lambda_ke[1] and lambda_ke[2] throughout the text** and the wavelength used for reflectance measurements in Method 2 was named lambda_alpha.

17: Are the measurements described in this sentence spectral e-folding depth measurements or reflectance measurements? Please clarify why the measurements were conducted in two different layers.

The e-folding depths were measured in visually identified layers. The two different layers correspond to two snowpits and are independent measurements. We clarified the text as follows:

P2818, l.10:
"A first set of measurements were conducted at Dome C (DC,75.10°S, 123.33°E, 3233 m a.s.l.), Antarctica, **during the summer campaign 2009/2010**. Spectral e-folding depths were measured **in 2 different snowpits** following the procedure detailed in France et al. (2011). **In each snowpit, e-folding depth was measured in a layer identified visually**. Density at 3 cm resolution [...]"

P2818, l.18:
"Method 2 (case 2) described in Sect. 4.1 is applied **to each snowpit** with [...]"

P2819:
27: Are the vertical profiles of density and reflectance in Figure 2b used to calculate the radiation profiles in Figure 2a and Figure 3?

The profiles in Figure 2b were used to calculate the radiation profiles in Figure 2a, but not those of Figure 3 (for Figure 3, the corresponding density and reflectance profiles are not shown)? This is clarified in the text and in captions of Fig. 2 and Fig. 3:

P2819, l.27:
"Modeled intensity profiles using the density and reflectance profiles **shown in Fig.2b** are also plotted "

caption Fig. 2b :
"Vertical profiles of density and reflectance at 1310 nm **used to compute the modeled intensity profiles in (a)** "

caption Figure 3 :
Same as Fig. 2a for the 4 other Alpine sets of measurements, **except that lambda_ke[2]=780 nm (the corresponding vertical profiles of density and reflectance at 1310 nm are not shown)"**

P2820:
13: The large range of B does not necessarily indicate that the spherical grain assumption is not valid. Freshly fallen snow has a very low B in this study while older snow has a higher B. B should be estimated more times for freshly fallen snow to determine whether the value of 0.8 is typical. Also, B should be estimated for transmission profiles of UV and visible radiation before making claims that the spherical assumption is inadequate.

Regarding the interpretation of the large range of retrieved B, the sentence was altered and the conclusion on the spherical assumption was removed:

P2820, l.11:
« B=0.8 is found for the fresh snow layer of the 9 March 2012 measurement. **The 7 values of B retrieved experimentally, ranging from 0.8 to 2.0, are shown in Fig. 6. This large range indicates**

that the AFEC can vary of a factor 1.6 solely due to the impact of grain shape on B. »

Regarding the representativity of the measurements, it is not claimed that B=0.8 is typical of fresh snow. On the contrary, in the Discussion it is underlined that such a value is contradictory with the definition of B. Also, to insist that measurements are scarce the following text was added:

P2824, l.1
« Indeed, the value B=0.8 is incompatible with the definition of B (Kokhanovsky, 2004) that constrains its value to be larger than 1. **Until more measurements are performed, this value should not be considered as typical of fresh snow.** »

The overall conclusion about the spherical assumption has been altered:

P2823, l.23:
"It means that assuming spherical grains for **the dedicated experiments would lead** to an overestimation of the e-folding depth. **B values calculated for various geometric shapes and deduced from data from the literature are also essentially larger than 1.25, which suggests that the spherical assumption is inadequate to model light intensity in snow**. By sampling different snow types [...]"

Regarding the last point, B barely depends on the wavelength (see above)

20-22: According to Figure 4, the low end of your BC range (12 ng/g) does not influence more than 5% of the absorption, but the high end (85 ng/g) does at 700 nm. When the concentration of BC is 85 ng/g, there will be some impact on snow optical properties so it would be better to make this sentence less severe.

The statement that BC slightly impacts snow optical properties at 700 nm refers to Dome C measurements (BC about 5 ng/g). This is clarified in the text :

« such contents **at Dome C** have a negligible impact »

25-27: State that Figure 5a is for Dome C and Figure 5b is from the Alps in this sentence for clarity.
Done

P2821:
24-25: The aim is to minimize the uncertainty of the retrieved B value.

We are not sure to understand the remark, but the text was modified to detail how the wavelengths were chosen to analyze the data from the literature:

P2821, l.14:
"**In order to minimize the uncertainty of the retrieved value of B for each experiment, the largest available wavelengths are used in Eq.(12)** "

25: These values are in bold, not underlined.
Corrected

P2823:
26: Provide the value of B for spheroids here.
Value 1.60 was added

Figure 1a: I find Figure 1a quite confusing, but I think Figure 1b is very straightforward. Is there a way to put the field measurements on Figure 1b instead of 1a and get rid of 1a? Also, in 1b, do all of the different grain shapes have the same SSA? Maybe in 1b, add "albedo" to the y-axis and "AFEC" to the x-axis. If they all do have the same SSA, mention the value of SSA used. Also, it would be more clear to mention the wavelengths used to calculate these values.

Fig 1a shows the singularity of spheres for both B and gG and it highlights the correlation between (1-gG) and B that is addressed in the discussion. Therefore we consider it is useful and complements Fig. 1b. The shaded area in Fig 1a has been removed in the new version since it can be puzzling before the reader has understood that only B can be retrieved. Values of B deduced experimentally remains in Fig . 6. Adding a similar shaded area in Fig. 1b would require to choose a value for gG. Since gG is not retrieved experimentally, the shaded area cannot be reported on Fig. 1b

In Fig. 1b, the SSA of the different grain shapes is not specified since at first order, B and gG do not depend on grain SSA. This is true, while grains are large enough for geometric optics to be applied but small enough for absorption in a single grain to remain very low. At 900 nm, ice absorption coefficient gamma equals 5.9 m-1. For absorption to become significant, a photon should travel about 10 cm in a grain. The calculations were performed on crystals smaller than 1 mm, hence the impact of SSA on B and gG calculations is very low. This point is clarified I the text:

P2814, l.9:
"The values of B and gG for these shapes are summarized in Table 1 and reported in Fig. 1. **All numerical calculations were performed on crystals smaller than 1 mm so that absorption within a single grain is very low and the calculated values of B and gG do not depend on grain size**"

"albedo" and "AFEC" were added on the axis in Fig. 1b

The complex refractive index at which calculations were performed are specified in the caption of Fig. 1:
m=1.33 for the calculations performed by Kokhaniovsky and Macke (1997)
m=1.31-1e-7i for Kokhanovsky and Zege (2004)
m=1.30-4.2e-7i for calculations performed with SnowRat

Figure 2b: at instead of et for "Reflectance et 1310 nm"
Modified

Figure 3: Why is 780 nm used instead of 600 as in Figure 2? Please put the wavelength in the captions for additional clarity.

The algorithm is based on fitting the intensity profiles at 600 and 780 nm. Showing profiles at both wavelengths illustrates this fact. The profile at 780 nm is not very sensitive to the impurity content while at 600 nm it is. Showing the profile at 600 nm illustrates that BC is included in the algorithm, while at 780 nm this straightforward illustrates the impact of B.

The wavelengths have been specified in the captions of Fig. 2a and 3

Figure 4: Why are some of the lines dashed lines while others are solid?

The solid lines highlight the wavelengths that were used in Method 2 for Dome C data, while dashed ones refer to the wavelengths used for Alpine measurements. This is now detailed in the caption:

"The dashed lines highlight the values at 600 nm and 780 nm **(wavelengths used for the Alpine measurements)** and the continuous lines highlight the values at 500 nm and 700 nm **(wavelengths used for Dome C measurements)**."

Figure 5: In the caption note the depth of the depth hoar layer and the day of measurement to be consistent the details provided for the Lac Poursollet snow pit.

Details have been added in the caption:

"Spectral e-folding depth measured **between 24 cm and 42 cm** in the depth hoar layer at Dome C **(29 Dec 2009).**"

The exact dates for Dome C measurements were also updated in Table 2.

References: Brandt, R.E., Warren, S. G.: Solar-heating rates and temperature profiles in Antarctica snow and ice. J. Glacio, 39, 131, 1993. Warren, S.G., Brandt, R.E., Grenfell, T.C.: Visible and near-ultraviolet absorption spectrum of ice from transmission of solar radiation into snow. Appl. Opt., 45, 21, 2006. Zatko, M.C., Grenfell, T.C., Alexander, B., Doherty, S.J., Thomas, J.L., Yang, X.,: The influence of snow grain size and impurities on the vertical profiles of actinic flux and associated NOx emissions on the Antarctic and Greenland ice sheets. Atmos. Chem. Phys., 13, 3547-3567, 2013. Warren, S. G., Brandt, R.E.: Optical constants of ice from the ultraviolet to the microwave: A revised compilation, J. Geophys. Res., 113, D14220, doi:10.1029/2007JD009744, 2008.

Response to anonymous reviewer #3 :

Abstract: Please eliminate AFEC and other abbreviations (SSA, etc.) from the abstract and elsewhere. Abbreviations save a tiny amount of space at the expense of comprehensibility. People do not think in terms such as AFEC. This is not a word. Authors who use an abbreviation again and again for weeks and months and years know what it means. But this is not necessarily true of readers. For example, I had never seen AFEC and SSA before. If I read it on one page, then I am expected to stop and make a conscious effort to memorize what it means in case I encounter it again. But I am unwilling to do this. If I encounter AFEC again I have to go back to find out what it stands for. In other words, the authors are training me like a rat in a maze.
Scientific papers are becoming ever more painful to read, and this is exacerbated by littering them with abbreviations the meanings of which may be unknown to readers. Every effort should be made to make papers as readable as possible to the greatest number of readers. Arcane abbreviations, initialisms, and acronyms run counter to this goal.
Papers by the giants of science (Rayleigh, Einstein, Stokes, Maxwell, Faraday, etc.) are completely free of ghastly abbreviations such as AFEC. And these older papers are quite readable, especially when compared to modern papers. How did all these authors write such beautiful and enduring papers without abbreviations? Modern scientific writing has become more and more infected with "abbreviationitis". There are much better ways of shortening manuscripts than resorting to abbreviations. For example, getting rid of all the references that merely serve as decoration (e.g., Mie, Chandrasekhar, Schuster,. . .).

The authors do not know how to write an abstract. An abstract is a condensed version of the text that follows and *completely independent of it*. But instead of writing an abstract, most authors write what amounts to a brief introduction. Statements such as "we present an experimental method" are inappropriate in a proper abstract. Such an abstract should stand completely on its own with no reference to what follows. Abstracts are very difficult to write, which is why most authors don't write them (they write brief introductions instead).

The abstract was entirely reformulated in order to be independent of the main text. All abbreviations in the abstract were eliminated:

New abstract:

"The energy budget and the photochemistry of a snowpack depend greatly on the penetration of solar radiation in snow. Below the snow surface, spectral irradiance decreases exponentially with depth with a decay constant called asymptotic flux extinction coefficient. As with the albedo of the snowpack, the asymptotic flux extinction coefficient depends on snow grain shape. While representing snow by a collection of spherical particles has been successful for numerical computation of albedo, such a description poorly explains the decrease of irradiance with depth in snow. Here, we explore the limits of the spherical representation. Under the assumption of geometrical optics and weak absorption by snow, the grain shape can be simply described by two parameters: the absorption enhancement parameter B and the geometric asymmetry factor gG. Theoretical calculations show that the albedo depends on the ratio B/(1-gG) and the asymptotic flux extinction coefficient depends on the product B(1-gG). To understand the influence of grain shape, the values of B and gG are calculated for a variety of simple geometric shapes using ray tracing models simulations. The results show that B and (1-gG) generally covary so that the asymptotic flux extinction coefficient exhibits larger sensitivity to the grain shape than the albedo does. In particular it is found that spherical grains propagate light deeper than any other investigated shape. In a second step, we developed a method to estimate B from optical measurements in snow. A multi-layer two-stream radiative transfer model with explicit grain shape dependence is used to retrieve the B parameter of snow by comparing the model to joint measurements of reflectance and irradiance profiles. Such measurements were performed in Antarctica and in the Alps yielding estimates of B between 0.8 and 2.0. In addition, values of B were estimated from various measurements found in the literature, leading to a wider range of values (1.0 -- 9.9) which may be partially explained by the limited accuracy of the data. This work highlights the large variety of snow microstructure and experimentally demonstrates that spherical grains, with B=1.25, are inappropriate to model irradiance profiles in snow, an important result that should be considered in further studies dedicated to subsurface absorption of shortwave radiation and snow photochemistry"

Abbreviations were removed from the manuscript when possible (see details below).
In particular, the abbreviation AFEC has been entirely eliminated.
The abbreviation SSA stands for "**specific surface area**" and is widely used in the snow community (Flanner and Zender, 2006; Domine et al., 2012; many others). The abbreviation was replaced by its full name when it refers to the physical quantity. However, it remains in formulas when used as a variable.

Page 2804. Please eliminate the reference to Mie. This is false scholarship. Did you use anything from Mie's paper? Have you read it? If you had used Newton's laws of motion would you have cited Newton? These days, I am pleased to say, people are becoming more scrupulous about calling the theory of scattering by a homogeneous sphere Lorenz-Mie theory. Lorenz preceded Mie by 20 years. I have read Lorenz's paper. He deserves much more credit than he is given. Mie's paper is excellent, but he was preceded by Lorenz (and others).

If readers want more details about scattering by a sphere, would you direct them to Mie's paper? There are more comprehensible modern sources such as the books by van de Hulst, Kerker, Bohren and Huffman, and by Mischenko, Travis and Lacis (Scattering, Absorption, and Emission of Light by Small Particles).

Two embarrassing questions should be asked about a reference: (1) Did the authors read it? (2) Did the authors use anything in it? If the answer is no to both questions, the reference is merely a decoration. It is becoming increasingly evident that these days only a fraction of cited papers are actually read. One study claims that this fraction is only 20%.

Also, there is no need to cite Chandrasekhar's book. Again, have you read it? Have you used anything from it? It is not easy to understand, and there are more comprehensible sources on radiative transfer. This book is now more than 60 years old. It is mostly of historical interest.

References to "historical" papers were removed.

Snow is not "semi-transparent". It is translucent. A transparent medium is capable of transmitting images (more or less faithfully). A translucent medium transmits light.

Mie theory was renamed **Lorenz-Mie** theory.

We have read Mie's paper and Chandrasekhar's book. However, references to them were replaced by more recent sources as suggested.
References to Mie's paper was replaced by reference to van de Hulst (1981)
Reference to Chandrasekhar's book has been replaced by reference to Kokhanovsky (2004)

The term "semi-transparent" has been changed into "**translucent**"

Page 2806. See previous remark about Chandrasekhar's book. There are much better sources on radiative transfer. See, for example, Thomas, Gary E., and Knut Stamnes, 1999: *Radiative Transfer in the Atmosphere and Oceans*. Cambridge University Press. van de Hulst also published a two-volume treatise on radiative transfer. Also, Multiple Scattering of Light by Particles by Mischenko, Travis, and Lacis.

TARTES AART etc. Horrible. See previous remarks about abbreviations.

TARTES is the name of the model. It should be considered as a name and used in the same way as DISORT (Stamnes et al., 1988), which is widely used in the radiative transfer community.
The abbreviation TARTES has been removed from the title of Sect. 2

The abbreviation AART has been removed.

Page 2807 "independent scatterers" This is an archaic and misleading term. Scattering by an ice grain in snow is excited by the incident radiation and by light scattered by its neighbors. Thus radiative transfer theory is a theory of dependent scattering: scattering by each grain depends on scattering by its neighbors. The term independent scattering goes back at least to van de Hulst's 1957 book. But by 1980, in his treatise on multiple scattering, he changed this to the more correct term "incoherent scattering." The concepts of coherence and incoherence are well-established in optics and are unambiguous. Incoherent scattering means that phases (strictly phase differences) of scattered waves can be ignored.

Here, "independent scatterers" means that the bulk scattering coefficients are obtained by summing the contributions of all scatterers. The extinction and absorption coefficients are thus proportional to the extinction and absorption cross sections of the individual snow particles and the number density of scatterers. This property is referred under different names in the literature: "incoherent addition" (Bohren and Barkstrom, 1974), "independently scattering particles" (van de Hulst, 1981, p.31), "averaging" (Warren and Wiscombe, 1980), "isolated scatterers" (Warren, 1982), "summarizing rules" (Melnikova, 2008)... As suggested, the term "incoherent scatterers" is used.

P2806, l.27:
"**Snow grains are treated as incoherent scatterers (Bohren and Barkstrom, 1974, Wiscombe and Warren, 1980). Hence the extinction and absorption coefficients sigma_e and sigma_a are proportional to** the extinction and absorption cross-sections C_ext and C_abs (m^2) of individual snow grains:"

Page 2808 "The average cosine of the phase function" You mean the average cosine of the scattering angle.

It was modified:
"**The average cosine of the scattering angle, determined from the phase function, is called the asymmetry factor and is denoted g**"

Page 2809. "convex particles" As far as I know, there is no simple expression (such as Vouk's) for concave particles. But surely some snow grains must be concave or partly concave, partly convex. The authors should clearly state this.

This point is now clarified:

P2809, l.10:
"**Eqs (10) and (11) are valid for convex and concave particles. Although snow grains are partly convex and partly concave, in the specific case of convex particles Eqs (10) and (11) can be simplified.** The specific surface area SSA ($m^2\ kg^{-1}$) is the total surface area per unit mass."

Page 2810 Shuster (1905) Again, have you read this paper? Have you used anything in it? It is mostly of historical interest. There are more modern treatments of two-stream theories.

Reference to Schuster (1905) has been replaced by reference to Choudhury (1981)

Page 2811 What on earth is HULIS? And is it really necessary to abbreviate black carbon as BC?

HULIS has been replaced by its full name (humic like substances).
Like SSA, BC has been replaced by "black carbon" when it refers to the physical quantity but remains abbreviated when it refers to a variable.

Page 2812 What are "summarizing rules"?

"Summarizing rules" (term employed by Melnikova, 2008) state that the scattering and absorption cross sections of a medium are obtained by summing the contributions of all scatterers. It is equivalent to the incoherent scattering assumption. The term "summarizing rules" has been removed.

Page 2815 It is easy to show that the extinction coefficient is much more sensitive to particle shape (i.,e., to g) by considering the simplest possible two-stream theory of reflection and transmission by an incoherently scattering infinitely deep medium. The derivative of the albedo with respect to g is zero in the limit of a single-scattering albedo of 1. In the same limit, the derivative of the extinction coefficient is infinite. No elaborate calculations are necessary. What is missing from this manuscript is a simple sensitivity analysis.

Our objective is to show that 1) the asymptotic flux extinction coefficient is sensitive to particle shape and 2) assuming spheres is inadequate. This can only be shown by considering both B and gG. Indeed, both appears coupled in the formula of asymptotic flux extinction coefficient (albedo as well) and both depend on particle shape. Partial derivation by g is insufficient to infer dependence to particle shape because it supposes that B is constant (by definition of the partial derivation) which is not true as in shown Figure 1a.

The fact that B and 1-gG are positively correlated (at least for the set of investigated shapes) implies that B(1-gG) varies in a larger range than B/(1-gG). The most important point is not the sensitivity (nor the range length) but the fact that spheres have a B/(1-gG) value in the middle of the range of the investigated shapes and B(1-gG) at the low end of this range. Furthermore the determination of B using experimental data taken on snow proves that snow has not the same B as spheres. It results that the spherical assumption is inadequate for extinction coefficients calculation. This can not be proved with radiative transfer formulation and derivatives as it results from B and gG that are driven by geometry.

Page 2816 "spectral intensity" The preferred term these days is radiance (if that is what is meant) or irradiance (if that is what is meant). Intensity is used carelessly to mean many radiometric or photometric quantities (radiance, irradiance, luminance, illuminance, proper intensity, the square of the electric field, the electric field, ...). Sometimes intensity is used to mean two different quantities in the same paragraph, or the meaning flips back and forth at random. I see that the authors also use "flux" for (I think), intensity (irradiance?), another example of changing terms at random

BC again and again is unnecessary. Is it such a terrible burden to write black carbon or simply carbon?

The authors have taken the data for carbon from Chang and Charalampopoulos. This is OK but the optical constants of carbonaceous substances vary considerably depending on the sample. There is no such thing as an invariable substance "black carbon". This is just an unpleasant fact of life about which nothing can be done. Better measurements won't help. There is an irreducible range of the absorption coefficient of carbonaceous substances misleadingly labeled "black carbon". I have no objection to the use of the term carbon as long as it is noted that it is not an invariable substance (in contrast with the element carbon).

As mentioned in the manuscript, the radiative transfer model TARTES computes fluxes (term employed by Jimenez-Aquino and Varela). "flux" was replaced by "irradiance" and the letter I is used instead of F throughout the manuscript. Measurements are taken with a fiber optic with aperture 25° half-aperture and hence are not strictly irradiance measurements. However, since our method uses relative variations of irradiance with depth, and since, according to radiative transfer theory, all the radiometric quantities follow the same exponential decrease under the weakly absorbing medium approximation, our measurements are proportional to the theoretical irradiance. The term "intensity" has thus been removed from the manuscript and is replaced by "irradiance" everywhere.

P2819, l.13;
"Series with variations of the incident flux larger than 3% were discarded. **Since only the relative variations of irradiance with depth are of interest, and since the theoretical framework introduced in Sect. 2 ensures that the measurements taken with the fiber optic are directly proportional to irradiance, the measurements are hereafter referred as irradiance measurements.** Profile of density with [...]"

The intrinsic uncertainty on black carbon optical properties has been detailed:

P2816, l.15:
"[...] only black carbon is considered […]. **The optical properties of black carbon depend on the nature of its elementary constituents (Bond and Bergstrom, 2006). Here the refractive index of black carbon** is taken from Chang and Charalampopoulos (1990) [...]

Page 2818 DC, POSSUM

The abbreviation DC, as well as LR, LP and CP, that refer to the measurement sites, were removed from the manuscript and replaced by their full names.
POSSSUM is the name of an instrument, it is always referred in the literature like that. It remains in the manuscript but is referred as "**the snow reflectance profiler POSSSUM**".

Page 2819 ASSSAP The authors certainly love abbreviations [or perhaps I should write TACLA]

What are "dedicated measurements"?

ASSSAP is the name of another instrument (like DUFISSS). They are now referred as "the instrument ASSSAP" and "the instrument DUFISSS".

Dedicated measurements refer to measurements that we took with the specific goal of determining B, that is the experimental conditions were taken to maximize the quality of the retrieval method. In particular, reflectance was measured at 1310 nm where ice is much more absorbing than in the visible range. The specificity of our measurements is detailed and the term "dedicated measurements" is replaced by "**measurements**":

P2815, l.20:
"The second method treats the case of a~multi-layer snowpack and uses field measurements **we have specifically performed for the B determination, undere experimental conditions chosen to maximize the quality of the retrieval method**.

Page 2820 See previous comment about BC. The policy of the authors seems to be that if a term consisting of two or more words is used more than once, it needs to be abbreviated.

See above.

Page 2825 I agree that “snow cannot be systematically represented by a collection of spheres.” Unfortunately, snow cannot be systematically by a collection of particles of any shape. Grains in snow are just too variable in space and time, and hence uncertainties in their shape will always plague any attempt to accurately model the optical properties of snow. Such is life. Sometimes one just has to accept that Nature is not kind.

We meant “**for optical calculations**”. It is added to the sentence.

The spherical assumption has been and is still used for optical calculations even by the majority of scientists that are aware snow grains are not spheres.

Recommendation

This manuscript can be published if for no other reason than that it presents measurements, which are always in short supply. The manuscript can be shortened by getting rid of all the references that the authors did not read, will not be read, and hence are not necessary. All the ugly abbreviations should be transformed into words.

I suggested a simple sensitivity analysis that shows why the albedo is much less sensitive to asymmetry parameter than is the extinction coefficient (provided that the single-scattering albedo is close to 1). No modeling, no detailed calculations are necessary.

We have addressed these points above

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