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The Cryosphere
Discussions

# *Interactive comment on* “Rapid and accurate measurement of the specific surface area of snow using infrared reflectance at 1310 and 1550 nm” *by* J.-C. Gallet et al.

**J.-C. Gallet et al.**

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### Reply to referee’s comments

First of all, we thank the reviewers for their time and constructive comments. All of these have been considered in detail. We first describe the general changes made to the paper, and then address the main specific points. Minor specific points have been taken into account in the revised version, but are not all discussed here because of the size limitations (15 pages). To facilitate reading, we have reproduced the review texts in italics.

**Discussion of general changes** First of all, we have justified the choice of wavelength, a point raised by Painter. In the introduction (lines 122-144) and in the new

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Figure 1, we demonstrate that the wavelengths we chose (1310 and 1550 nm) are potentially more accurate, because a given change in SSA results in a greater change in reflectance. Working in the SWIR rather than in the NIR has the added advantage of reducing our sensitivity to the presence of absorbing impurities, as ice absorbs more in the SWIR, an effect that we quantify lines 140-145.

We have also added a detailed description of our sampling protocol (experimental section, lines 241-262) and added Figure 4. We demonstrate that our perturbation to snow is minimal, and no greater than with NIR photography and contact spectroscopy. A snow face has to be cut in all cases, but our measurement takes place within seconds of cutting the snow face, considerably reducing the risk of surface modification while the snow is exposed to the atmosphere.

We have improved the readability of the paper by moving all the tedious details to the appendix. We have also addressed the issue raised by Painter that density is not the variable of interest when considering the presence of artifacts, and that we should use the optical depth instead. From a theoretical standpoint, we agree that optical depth is a convenient variable, and we have made adequate changes throughout the text, whenever we felt that the use of optical depth was appropriate or improved readibility. However, we have two important reasons for using density in some parts of the text. The first reason is that from a practical standpoint, optical depth cannot be readily evaluated in the field while density can. If we want to estimate whether an artifact is likely using the 1310 nm wavelength, with the result that the 1550 nm wavelength must be used, we have to rely on a density measurement. Therefore, we believe that considering density does have a considerable practical interest, and we also do mention that. The second reason is that the optical depth is not perfectly adequate because our snow sample and the sphere aperture both have finite horizontal sizes. A consequence of the finite size is that the solid angle where reflected light coming from a snow grain is captured by the sphere (FOV in the appendix of the revised version) depends on the physical depth of the grain in the sample. Hence, the measured reflectance does

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depend independently on the physical depth and density. As an example, if the sample holder was infinitely deep, the optical depth would be infinite, but the measured reflectance would still depends on the density because in a dense snow, the reflectance would occur predominantly closer to the top of the sample than in a low density snow.

We have also corrected our incorrect assertion about the illumination in the system presented in Painter et al. 2007. The discussion about the the illumination angles have been reworded but we maintain that a tight control of the illumination is important and that measuring hemisperical reflectances instead of conical reflectances is beneficial to reach a high accuracy. Between submission and revision, we have built another sphere whose reflectance was slightly higher than that of the first sphere. Of course these new optical parameters slightly modify the SSA-reflectance relationship shown in Figure 5 and we explain how to take that into account in the determination of SSA from a reflectance measurement. The need to take into account the sphere reflectance is first discussed in the theoretical development of section 3.1 (lines 349-354), and the practical aspects are discussed in the first 2 paragraphs of section 5.1. The error if this is not taken into account is small, about 5%, but we feel it is necessary to mention it for scientists who need the most accurate measurements. The specific points mentioned by the referees are addressed below.

### Anonymous Referee 2

*This paper outlines a compelling new method for measurement of the SSA in field and in laboratory. A device is proposed which seems to be easy to handle and which allows reasonably fast measurements of the parameters in question. A very detailed sensitivity analysis of the method has been performed which outlines certain problems, specifically at high SSA values. The level of detail is sometimes apparently higher than required and leads to a hard read of the paper contents. Some shortening to the most relevant outcomes of this analyses would help to make the paper more concise.*

We have regrouped the parts that may seemed too detailed in an appendix, so that the core of the paper can now be read much faster. These details are important, however, and we believe they must be available to the interested reader.

*However, the paper shows a novel approach and therefore should be published Some minor problems have been identified in the paper: In the introductory part, some thoughts about the relation of SSA and snow grain size would be of interest. This is due to the fact that in optical spectroscopy, often the grain size is considered being a driver for reflectance properties and I assume that these two parameters are highly correlated.*

Eq 1, line 50, shows this relationship.

*The usability of the method to measure real in-situ SSA values is questioned as the snow apparently needs to be taken from its original location and put into a box for measurement. Wouldnt it be interesting having a measurement directly on the natural snow surface without destroying its structure? It also would be of interest to know, why a single-wavelength approach has been taken for the SSA determination even though two wavelengths are available. Maybe even better results would be achieved if the two signals would be combined. Also, the single band approach is prone to snow impurities. Their potential influence on the method should be mentioned.*

Clearly our sampling procedure needed to be detailed and we have therefore detailed it in the text (lines 241-262) and added Figure 4. This shows that perturbation to the sample is minimal, and no greater than in the methods by Matzl and Schneebeli (2006) and Painter et al. (2007) who also need to cut a surface. In fact, we leave the surface exposed for a shorter amount of time, reducing the risk of modification. Regarding the choice of just one wavelength, Figure 5 shows that for SSA>60 m2/kg, 1310 nm cannot work, while Figure 8 shows that for SSA<50 m2/kg, reflectance is too low to be accurate. There is indeed a range where both wavelengths could be used simultaneously, and we have done so, to find that there is little benefit in doing it. However, these are details we feel are not necessary in this already long paper. The

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impurity issue is an interesting point, that we now address in the introduction (lines 137-144). We show that any realistic amount of impurities will not affect our measurement at 1310 or 1550 nm. On the other hand, measurements at shorter wavelength may be affected, and this supports our choice to work at longer wavelength.

*Equation 2 is somewhat confusing (at least to me). How has it been derived and what is the (1-r) term standing for? Also, the later mentioned parameter h does nowhere appear in the equations. Minor: r-dot is not necessarily r in eq. 4. (not because)*

Eq. (2) was derived by applying the recipe given by Hidovic'-Rowe et al. (2006, their section 2) for our detector configuration. Since the full derivation of Eq. (2) is well beyond the scope of our manuscript, the revised manuscript clarifies that Eq. (2) is derived following the formalism of Hidovic'-Rowe (2006) (lines 328-331). The term (1-r) is one minus the isotropic reflectance of the snow sample. In other words, it is the absorptance of diffuse radiation by the snow sample. r is clearly defined in the text. The parameter h is mentioned because the important parameter alpha, the surface area of the sphere walls, depends on h as shown in the normalization condition. Formerly it was not clear that we computed (rather than measured) alpha; from the normalization condition applied to d, h, and s (which are all measured). We have slightly reworded the text (lines 333-337) to make the need for h more obvious. The revised manuscript omits the because phrase (line 350).

**M. Schneebeli (Referee)**

*GENERAL COMMENTS The paper describes a new and useful additional method to determine optical grain size. The comparison to DISORT and the identification of instrumental artifacts is detailed and useful for future instrument improvement. This kind of instrument is a very useful an timely addition to near-infrared photography, and will help to calibrate simulation models. The method described here is very similar to the method developed by Painter et al, using surface spectroscopy. I think the title is somewhat misleading, as it is still necessary to take samples. Compared to NIR pho-*

*tography, the method is comparatively slow (number of samples / second). I would like to propose a title like: Dual-wavelength reflectance spectroscopy of snow to measure the optical diameter and surface area The accuracy of 10 and 12%, respectively, is similar to other methods. In this respect, a quasi-volume (NIR-photography) or full volume (adsorption, tomography) are much better concerning the representative sampling volume. Unfortunately, the representative sampling volume in a natural snow cover is poorly known. However, high-resolution stratigraphy indicates that for alpine snowpacks the variability in SSA is very high (see vertical sections in Matzl and Schneebeli (2006), and underestimated by point sampling methods and by traditional stratigraphic profiles.*

We have changed the title to remove the words rapid and accurate. However, we have recently used simultaneously NIR photography and DUFISSS in the field, and given the long set up time for NIR photography (as also mentioned in Painter.s review), studying a profile is not significantly longer with DUFISSS. Data analysis may also be faster for DUFISSS. It is true, however, that DUFISSS will not detect lateral SSA variations, and this is a clear advantage of NIR photography, as was stressed in our conclusion. It is not clear that the data presented by Matzl and Schneebeli support the suggestion that the method has an accuracy of 10 to 12 %. There is a lot of scatter in the calibration data, and the shape of the calibration curve could not be explained theoretically, which might suggest sources of error not clearly identified.

*In my opinion, the existing data of direct measurements of SSA in snow shows that the uncertainty is dominated by the intrinsic variability of the snowpack. As two completely independent methods, adsorption and tomography, are on a 1:1 line, this is now a well justified statement. Therefore, the main problem is the selection of a representative sampling volume, and a sufficiently dense sampling interval (according to high-resolution penetration measurements and NIR-photography, SSA can easily vary by more than 20 % even in a layer of homogeneous traditional stratigraphy.*

We have added the calculation of our sampling volumes and compared them to those

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of CH4 adsorption and to those considered to be representative of bulk snow properties (section 2, lines 228-240). Of course, unless we sample several parallel profiles, DUFISSS will not solve the problem of lateral variations. This is where NIR photography nicely complements DUFISSS, as we already had stressed in our conclusion.

*p36 l 9 : This is correct sensu-strictu. But consider the hundreds if not thousands of traditional snow profiles were traditional grain size and density is recorded: it is for almost all snow types to get a reasonable estimate of SSA, especially if the grain size is measured on a reproducible way. This should be mentioned here.*

We discuss this lines 83-88. We do not believe that grain size is an acceptable substitute for SSA, as discussed in previous papers that we reference.

*p38 l5 The larger e-folding depth is rather an advantage than a drawback. Tomographic and stereological studies show that a volume of at least around 5 mm3 is necessary to receive a representative volume. In fact, with a larger e-folding depth, artifacts resulting from the preparation of the snow pit or the snow sample are much less important.*

Our addition in section 2 (lines 228-240) shows that our sampling volumes are much larger than that.

*Small particles at the surface of the sample, caused by preparation, could significantly influence the reflection, an by this the SSA. The argument of interference can be refuted by the combined measurements of stereological surface area and NIR-reflectivity of Matzl, where vertical surface sections of highly layered samples were measured.*

We have now detailed our sampling procedure, which does not perturb the snow more than NIR photography. In fact, the snow face that we cut remains exposed to the atmosphere less time than NIR photography, reducing the potential for transformation, for example by the action of warm air. (lines 241-262). The issue of small grains formed by preparation has drawn much attention on our part, and we address this issue lines 256-259.

*P54 L 20 ff : SAI is probable only useful by weighting with light penetration - which is SSA and density dependent. I think that SAI should be left in this article. However, the authors could discuss the method in the context of quantitative stratigraphy, see also the paper by Satyawali et al. http://dx.doi.org/10.1016/j.coldregions.2008.09.003 and the general lack in quantitative methods in the analysis and validation of snowpack simulation models.*

SAI has applications in snow chemistry, snow optics, and air-snow interactions. Then of course SAI data must be transformed to be useful for the intended purpose. We therefore limit our objective to providing SAI data, and let readers use it for their specific purpose.

**T. Painter (Referee)**

*This paper describes an implementation of traditional integrating sphere technology to measure the reflectance of snow at 1310 and 1550 nm in order to infer specific surface area more rapidly than gas adsorption techniques. The importance of this measurement technique is that it leverages a hemispheric measurement rather than a directional measurement and thus is less sensitive to anisotropy of reflectance that is modified by the morphology of the snow microstructure. The efforts to understand issues with optical depth and geometric effects with DUFISSS are heroic and the authors are to be congratulated on these. While the authors do not speak to this, its utility in the field is attractive because of its minor set-up time relative to the NIR photography technique of Matzl and Schneebeli 2006 and its lesser instrumentation expense relative to the spectroscopy technique of Painter et al 2007. This paper will be acceptable for publication after correction of the following relatively simple but extensive points. By the way, the acronym is outstanding.*

*The main issue here is that interpretations and claims are not necessarily substantiated by the data or physics. Among these are mismatch between measurement and calibration set, attribution of sensitivity to density rather than optical depth, erroneous* *use of reflectance nomenclature, erroneous discussion of Matzl Schneebeli 2006 and Painter et al 2007.*

We address these general comments below, when they are detailed by the referee.

*The measurements integrated dimension for the DUFISSS is markedly smaller than that from the gas adsorption technique by which the calibration SSA is retrieved. The former e-folding distances are 1.1 cm (1310 nm) and 0.2 cm (1550 nm) according to DISORT whereas the sample dimensions used in the methane adsorption appear to be several cm if I infer correctly from the papers that are provided as reference here for the technique. It is confusing that the discussion indicates that the e-folding distances at the 1310 and 1550 nm band centers are superior to the NIR when the calibration dataset has, and most likely necessarily so, a dimension that is several times to a couple of orders of magnitude greater. Moreover, as Reviewer Schneebeli pointed out, the lesser e-folding distances make the measurements more sensitive to the destructive preparation that occurs at the snow surface. In order to address this for publication, please include a more detailed discussion of the sample dimensions used in the methane adsorption and then rectify the discussion according to difference in dimensions.*

These are interesting comments that we have addressed in detail. RE. The sampling volumes, examples have been detailed in section 2, lines 228-240. We have also given a more detailed description of the sampling protocol (lines 241-262), and added Figure 4. RE. the intrinsic accuracy of wavelength, we decided to treat this important point in detail (lines 122-136) and added Figure 1. We demonstrate that at 1310 nm, a given change in reflectance will translate into a smaller change in SSA, illustrating that optical determination of SSA is more accurate at 1310 nm than at 1030 and especially at 900 nm. In the text, we also demonstrate using DISORT calculations (introduction, lines 137-144) that the longer wavelengths are less sensitive to contamination by impurities, and at 1310 and 1550 nm, we therefore eliminate one source of error that is present at 1030 nm, and even more pronounced at 900 nm. There are therefore at least 2

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reasons why working a longer wavelength is potentially more accurate.

*This discussion bleeds into the assertion that somehow the wavelengths used in Matzl and Schneebeli (2006) and Painter et al (2007) are inferior to those used here. So, while it is true that Figure 2 of Dominé et al 2006 indicates the greatest linear correlation between SSA and reflectance lies at wavelengths > 1450 nm, it should be noted that the relationship between SSA and reflectance is quite robust at 900-1000 nm but distinctly non-linear for the range of SSA used in Figure 2 of Dominé (and shown in Figure 1 of that paper). (Note: those spectra are directional reflectance factors and not spectral albedos. Hence, without controlling for solar geometry, these nadir measurements are not directly comparable.) It therefore makes great sense that the linear correlation would in fact be poorer in the wavelengths where the relationship is markedly non-linear. The features at 1030 nm and 1260 nm are arguably preferable in that the full range of reflectance corresponding to the possible range of SSA for snow are available without approaching asymptotes in near-1.0 or near-0.0 reflectance. The 1550 nm range has very low reflectance and as such is vulnerable to saturation at near 0.0 for smaller SSA. The 1310 nm range is fine with respect to range of reflectances but it is a problem for your experimental apparatus. So, please modify the text to indicate a better description of the sensitivities in wavelengths and how they migrate from nearly infinite slope in the visible wavelengths (exhibiting asymptotic reflectance for most SSA) to non-linear in the NIR to linear (but subject to asymptotic behavior at low SSA) in the SWIR.*

This has been addressed in the previous comment. The key answer is our Figure 1.

*Multiple times the discussion focuses on the sensitivity to density of the DUFISSS reflectance without mentioning that this sensitivity is fundamentally a change in optical depth in the chamber. Given that the layer depth is fixed, the increase in density represents an increase in optical depth in an optically thin regime whereas for a semiinfinite snow layer, a change in density would have no effect for the range of snow densities here (Bohren and Beschta, CRST, 1979 - < 450 kg/m3). Please correct the discussion* *to indicate this and you can easily remove a great deal of text that refers to density sensitivity.*

We agree that the optical depth concept is useful to describe the dependence of our measurements on density. We have refomulated most of the sections using optical depth whenever possible. We have also specified the meaning of the word 8220;reflectance8221; to make it clear when we refer to a measurement using DUFISSS, in which case there may be artifacts and the issue of optical depth (or density) is relevant, or when we refer to modeling, in which case we have a semi-infinite layer and the issue is irrelevant, as noted by Painter. This will avoid misleading the reader. However, the paper aims at being practical and snow density is measurable in the field. Hence, we prefer to keep density values that most readers are used too, instead of optical depth values that only experts in radiative transfer are able to use. Finally, the appendix shows that the density artifacts manifest themselves in two ways. First, as mentioned by the reviewer, the optical depth is finite and the measured reflectance is then less than if the optical depth were infinite, Second, a low optical depth causes the geometric artifact, because of the the finite horizontal size of the aperture (and the FOV issue). In the later case, even if the sample holder were infinitely deep (=infinite optical depth), the reflectance measured by DIFUSSS would still depend on density.

*Finally, you should correct several misstatements about the NIR photography technique presented in Matzl and Schneebeli 2006 (M2006) and the spectroscopy technique presented in Painter et al 2007 (P2007). These techniques maintain the integrity of the snow structure that DUFISSS cannot in that the measurement is made in situ with the only manipulation being a shaving of the snowpit face. DUFISSS requires displacement of the sample to the sample holder 8211; you should mention that this puts the measurements at greater risk of modification. As above, the sensitivity of reflectance in the wavelength range 890 nm is non-linear but robustly invertible. Your argument regarding the directionality of irradiance in M2006 methodology is plausible however but not necessarily true 8211; take the camera out and prove it as such. With respect* *to P2007, the text suggests that the illumination is poorly controlled with contact spectroscopy as there is a hemispherical reflector that surrounds the illumination source 8211; however, as they indicate in their paper, the spacer is painted black with reflectance < 2% across the spectrum and hence the illumination is well controlled to direct. (the spacer is highlighted as white for visibility in the figure in that paper) It is true that Painter et al 2007 report optical grain radius and not SSA, but these are isomorphic under the spherical assumption implicit in the optical grain radius definition. The text claims that the retrievals of SSA from directional measurements are sensitive to anisotropy. However, the configuration of the contact spectroscopy system is well suited to be insensitive to the anisotropy of snow HDRF because of the geometry of the sensor and the illumination source 8211; see Fig 12 (left column) of Painter and Dozier 2004 8211; and includes correction for the anisotropic reflectance of the Spectralon panel. Note also that the scaled integral approach is more robust than a single band technique in that random band reflectance uncertainty is mitigated through the summation of multiple bands (this was shown by Anne Nolin in her dissertation).*

We have indeed detailed our sampling protocol, to conclude that sample perturbation is minimal (lines 241-262). In fact, we only cut the snow once, just like Painter (2007) and M S (2006). However, only a few seconds elapse between cutting and measurements, compared to many minutes in both other techniques, so that chances for modification, for example caused by temperature changes, are greatly reduced. With regard to the illumination in P2007 and the spacer, the picture was indeed misleading in their paper, our text is now corrected (lines 151-155). The anisotropy of the reflectance is a source of error to infer albedo and SSA (or grain size) that can indeed be reduced by a suitable choice of illumination/observation angles. At least some angles are worse than others as mention in Painter and Dozier, (2004). However, we have noticed numerically with SNOWRAT and experimentally with the NIR photography that the grain shape and the limited size of the footprint of measurement have a strong influence on the effect of the anistropy. This influence was not accounted for by the calculation in Painter and Dozier, (2004). These calculations may thus be over-optimistic. Therefore, we still believe

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that any design of the instrument that permits to measure reflectance with large solid angles, large range of wavelength, or large footprint is beneficial for the accuracy of the SSA determination. Hence, measuring hemispherical reflectance is an advantage with respect to measuring HDRF or bi-conical reflectance. For the same reason, we agree that the scaled integral approach probably helps to mitigate the influence of the angle. This point is now stated in the introduction (line 170).

**Page/Line specific comments:**

*p.34/line4 The statement that current measurement methods are not simple to use in the field is not true. The contact spectroscopy method is in fact very simple to use in the field and it retrieves SSA (via optical grain size), albeit expensive. The NIR photographic technique is relatively simple as well once the pit face is well prepared and the illumination diffusion well prepared.*

Yes, we agree with this. We added up to recently line 79, to exclude both these recent methods from our comment.

*p.36/line 11 contact spectroscopy is rapid and accurate method for SSA via optical grain size.*

Contact spectroscopy is definitely rapid and accurate to obtain optical grain size. An interesting debate is the actual relationship between optical grain size and SSA, and how that is affected by grain shape and size distribution. In our DISORT calculations (Figure 5), we had to use slightly adjusted optical parameters and a given size distribution to fit our experimental data.

*p.38/lines 2-9 as mentioned before, an e-folding distance of 0.2 cm = 2 mm is very sensitive to the preparation of the surface and is not commensurate with the measurement from gas adsorption. The measurements at shorter wavelengths are closer to the scale of the gas adsorption.*

Yes, we agree. However, that value was for snow of SSA=35 m2 kg-1 and density=200

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kg m-3, which in fact is not measured at 1550 nm, so that was a poor choice. A more relevant choice is snow of SSA=100 m2 kg-1 and density=50 kg/m3, as now mentiooned in the experimental section (line 241), which brings the e-folding depth to 4.2 mm. 1550 nm is used only for fresh snow, where grains are very weakly bonded so that the surface is very little perturbed by sampling. We mention this line 260-262.

*p.43/line 5 given that snow is not Lambertian, explain how the results are different 8211; this is particularly pertinent given your arguments that the hemispherical treatment is more robust than a directional treatment.*

There is indeed a mistmatch between the Markov model assumptions (that cannot be relaxed to track the analytical calculation) and the non-Lambertian character of snow. We now do mention this clearly (lines 327-8), but at this point, we do not believe that the impact of this approximation can be evaluated, as mentioned in the text. However, calculations with SNOWRAT where we modeled as precisely as possible the sphere geometry show good agreement with the Markov approach, though the numerical noise is pretty large and does not reveal small differences. It is unclear where we argue that the hemispherical treatment is more robust than a directional treatment in the case of modeling the sphere response.

*p.43/equation 4 I tried several times to get this to have the same values that you plot in Figure 2 but could not, using the numbers presented here I get values of order 0.001. It appears to me that the equation is not correctly written here or you have scaled the results in arriving at mV. Perhaps I have miscalculated many times, it has happened before.*

Indeed, one needs another factor for the gain and one for a possible offset, as detailed in our new equation (6).

*p.50/line 10 Be explicit here that it is in the context of the fixed sample depth of DUFISSS that snow samples with high SSAs would have low densities. With no constraint on depth, SSA is independent of density, akin to optical depth.*

There is a misunderstanding here. What we mean is that snows with high SSA are always fresh snow. And fresh snow (almost) always has low density. Therefore, the use of 1550 nm wavelength for snow with high SSA always deals with low density snow. To remove the misunderstanding, we have reworded the text which now reads (lines 404-405) : Only snow samples with high SSAs were selected for these measurements. These were necessarily fresh or very recent snow samples, that therefore had low densities.

*p.54 Does this mean that the technique cannot be used for melting or near melting snow? What is the threshold temperature for use of the technique? Please explain how the sample is placed in the sample holder.*

We have now added a whole section and figure 4 on the sampling protocol. The measurement quality will of course be affected if there is any liquid water, whose optical constants are different from those of ice. If it is too warm, manipulating the snow may cause melting, which will perturb the measurement. We have added sentences to stress that in the experimental section (lines 251-253): This temperature aspect is crucial to study fresh snow having high SSA or any snow when the air temperature is near $0^\circ$C, as the presence of liquid water will of course reduce the accuracy of our method in a manner that still needs to be quantified.

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