**The Cryosphere
Discussions**

Interactive
Comment

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.

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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

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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.

The measurement's 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.

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.

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.

Several times, the text indicates "measured bidirectional reflectance factor" when referring to others' works. Note that the BRDF is a conceptual quantity and that the BRDF is merely approximated with measurements of usually the hemispherical-conical reflectance factor or the biconical reflectance factor. See Schaepman-Strub et al 2006

Remote Sensing of Environment for explicit treatment.

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 – 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 – 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 – 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 – see Fig 12 (left column) of Painter and Dozier 2004 – 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).

Page/Line specific comments:

p.33 The title should be modified to remove qualitative terms such as 'Rapid' and 'Ac-

curate' – these terms are relative. Perhaps "Integrating-sphere measurement of snow specific surface area at 1310 and 1550 nm".

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.

p.35/line2 given that the units are area/mass, give the equation in area/mass, not area/(density*volume) –

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

p.37/line 19 this was discussed before but the contact spectroscopy has a tight control on illumination

p.38/line 3 much of the paper contains terms such as 'fairly large' – these have no robust meaning as what you consider to be 'large' may be small to another – here you mean larger than at longer wavelengths – please modify these to be explicit.

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.

p.38/line 12 they measured hemispherical-conical reflectance factor, not BRDF – BRDF is a conceptual quantity – ref Schaepman-Strub et al 2006 p.38/line 15 as noted before and in the paper, the hemispherical spacer is painted black $< 2\%$.

p.38/lines18-22 while there may be heterogeneous illumination, you should also note that they calibrate the reflectances with Spectralon disks distributed across the pit face p.40/line 29 are these 1% and 2% absolute? Leave it at there without the modification

that these are highly reproducible – in a dynamic range for 1550 nm of ∼0-10%, 2% is relatively noisy.

p.41/line16 light is generally not reflected externally by crystals 8211; the vast majority is refracted (see Bohren and Barkstrom 1974, p. 4529)

p.42/line11 be more explicit with the statement "to optimize the agreement" – it would be interesting to know how the results vary if you use the compilation straight ahead – you've done the modeling so explain how it is different.

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

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've miscalculated many times – it has happened before.

p.44/line 15 The caption for Figure 5 should indicate that this is for 1310 nm.

p.44/line 17 Give the values instead of "reproduced fairly well"

p.44/line 27 While some points lie near a difference in reflectance of 0%, the range is 0-6% for SSA near 60 m^2 kg^{-1}. Remove the statement that there is excellent agreement and just indicate what the values are.

p.45 Give references for this approach.

p.48-49 This extensive discussion of sensitivity to snow density needs to be recast as a discussion of changes in optical depth. At the densities given, snow reflectance for a semi-infinite snowpack is insensitive to the density – the sensitivity that you have here to density is that for a fixed depth, an increase in density increases the optical depth

and hence increases the reflectance given that the sample holder is relatively dark. If the sample holder was made of Spectralon, the increase in density would affect a decrease in reflectance.

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

p.53/line 4 Be explicit as to exactly how an "accuracy" of 10% is arrived at – also, is this not uncertainty?

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.

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