General comments

This paper presents an overview of observations of the angular distribution of reflected radiance from natural snow samples, along with some comparisons of these observations to modelling results. The fact that the measurements were made under artificial light, allowing purely direct-beam incidence, makes them scientifically new and interesting. My main technical concern with the observations is related to the effect of the small sample size.

I think the impact of the paper could be greatly extended through the inclusion of data at additional wavelengths and viewing angles. My first recommendation would be to resubmit the paper at a later date with an extended set of observations. This suggestion is based not on significant scientific flaws in the paper, but rather on the fact that the current dataset does not make the best use of the new possibilities made available by using the new laboratory observation system rather than field-based measurements. Therefore, it feels like an incomplete work that will have to be supplemented later. If the dataset extensions are for some reason not possible, or if the authors and editor choose to publish the current paper as a 'part one', it could be published after revisions to address the specific comments below.

Specific comments

1. The snow sample that is observed is cylindrical, with a diameter of 30 cm and a depth of 12 cm. It is stated that this is "large enough to minimize side effects within a large range of wavelengths." Presumably, the large range of wavelengths is meant to include all wavelengths covered in the current work: from 500 nm. No reference or explanation is given for the statement that this is large enough to eliminate edge effects. A discussion in a reference given elsewhere in the paper (Brissaud et al 2004) refers to this issue, but it mostly addresses the question of errors caused by the finite size of the incident beam.

   The effect of the loss of light to the sides and bottom of the cylinder should be discussed in more detail, along with a description of what is around the cylinder of snow. If the container holding the cylinder is white on the inside then it may have much less effect than if it is black (though it would still need to be examined, and the albedo of the sides given).

   To investigate the potential problem, I used a Monte Carlo radiative transfer model that tracks photons through a cylinder of snow until they are absorbed, or leave the top, side, or bottom. Using the Henyey-Greenstein phase function and values of single-scattering albedo (1 minus 4×10^{-5}) and asymmetry parameter (0.891) calculated for 400-micron-radius ice spheres using the ice optical properties given in Warren and Brandt (2008) for light with a wavelength of 600 nm, the model showed that 8.7% of the photons exited through the bottom of the cylinder and 5.0% through the side of the cylinder, while only 1.1% were absorbed. These results were for normal incidence (θ_i=0°) with all photons incident at the exact center of the top of the cylinder. The number of lost photons decreased some with increasing incident zenith angle, but remained 5.9% and 3.4% from the bottom and sides for θ_i=60°.

This model was obviously done as a quick check, and I am open to the possibility that I may have made an error in it. However, a similar result is seen in Figure 3 of Warren et al (2006), which shows transmission of about 10% of flux at 600 nm through 12 cm of snow in Antarctica, which has smaller grain sizes, giving a higher optical depth per unit of geometrical depth. Both of these results suggest the edge effects need to be much more thoroughly examined in the current paper.

2. One of the advantages of the laboratory measurements is the ability to measure something closer to the true BRDF, rather than the HDRF; i.e. to measure the reflection resulting from purely direct incident radiation. This advantage provides a reason to accept the risk of disturbing the snow by moving it to the lab. Unfortunately, no data are presented for the wavelengths where this advantage is most useful: the shortwave visible and ultraviolet. Field measurements made at wavelengths less than 500 nm are often dominated by diffuse incidence due to Rayleigh scattering, while the strong wavelength dependence of the scattering means that field measurements at longer wavelengths are made under a nearly direct beam. Using the SBDART atmospheric radiative transfer model to calculate the fraction of incident flux that is diffuse at sea level under a clear, subarctic winter atmosphere shows that, with a solar zenith angle of 60°, less than 25% of the incident flux is diffuse at wavelengths longer than 500 nm; less than 10% is diffuse at wavelengths longer than 630 nm, and less than 1.5% at wavelengths longer than 1 μm.

   Given that the measurement under direct radiation is the main motivation given for the method, it seems very surprising that no measurements are presented at wavelengths between 300 and 500 nm, where up to 90% of the incident flux in the field may be diffuse. The instrument description in Brissaud et al (2004) says it is capable of measurements down to 310 nm. Since it is the observations at shorter wavelengths that would provide the most scientifically interesting and new results here, I would suggest making those measurements and including them in the manuscript. If there is some reason that is not possible, then it should be explained here, and noted that it means that this method is currently limited to being used at the wavelengths where it provides the least advantage. Of course, the snow is even less absorptive at these short wavelengths, making the edge effects even larger, but it should not be much more significant than it is at 500 nm.

3. The choice to make observations at only 4 viewing zenith angles and 5 azimuth angles seems to limit the analysis. For example, in one case it was not possible to say whether a double maximum in the forward reflectance was present because it wasn't clear if there was one broad maximum, or if the minimum between the two maxima was not resolved. It also makes it difficult to interpret the double peak in reflectance when it is observed because one cannot say if the peak at θ_v=30° is actually located at the location where specular reflection would be observed, or if it is

actually at a slightly different angle, something which would be very helpful for interpreting the results. Having observations at θ_v=80° and at azimuth angles of 5° to 30° would help to better understand the shape of the forward peak.

This is another case where the advantages of the laboratory setup do not seem to be fully used (field measurements typically have a wider observation field of view to be able to complete the measurements over the hemisphere before conditions change too much). I would be quite interested to see the detailed angular structure of the BRDF and comparisons of that with model results, but instead, I am left wondering whether the linear interpolation is really accurate, and I can only see a sort of general comparison. Measurements at finer angular resolution would also provide an argument for making observations at longer wavelengths, where the direct beam is less of a laboratory advantage. If observation time is a problem, then reducing the wavelength resolution from 20 nm to 100 nm would probably be fine, especially as only two wavelengths are presented.

4. Also, it would seem useful to include larger incident zenith angles (70° and 80°), since these are common over snow and are most affected by diffuse incidence in the field.

5. Despite the large set of measurements, observations from only two wavelengths are shown. It would be interesting to include those from some other wavelengths as well, especially for comparison with the modelling results. Given that the modelling results are for comparison with the observations, it is not very useful to show results from the model for wavelengths that are not also shown in the observations. Also, including the full dataset, or a more extensive subset of it, as supplementary material would be useful to others who wish to make use of this work.

6. In Sections 1 and 2 (particularly line 26, page 19280), Nicodemus et al (1977) seems to be the defining reference for reflectance terms, including BRDF, bi-conical, hemispherical-directional, etc.

7. In the discussion of equations 1 and 2, at lines 6 and 17 on page 19282, the denominator being discussed [$F(\theta_i, \phi_i, \lambda)$] is the incident flux (or irradiance), not radiance or intensity. It is the radiance times the cosine of the incident zenith angle.

8. Line 11, page 19284, perhaps cite Wiscombe and Warren (1980) since Warren and Wiscombe (1980) was part II and included impurities, something not discussed here.

9. In section 4.3, since the definition of BRDF was given precisely, it should be noted that what is actually measured is the directional-conical reflectance factor, since the observation is not for an infinitesimal solid angle.

10. Line 18, page 19287, the quantity in the second set of parentheses should have $\theta_i \approx \theta_v$.

11. At the end of page 19287, it says linear interpolation was used to cover the whole hemisphere. a) Was it linear in θ_v or μ_v? It's not clear which is better, but often the latter is more physical, and it should be stated which was used. b) Filling the hemisphere would require *extrapolation* as well as *interpolation*, which should be stated for clarity. c) Is it possible to estimate the uncertainty in the calculated albedo, due to the interpolation? d) If ρ were reported, rather than R, then it would not be necessary to use the calculated albedo, which may be in error due to having measurements at relatively few viewing angles.

12. Line 2, page 19289, the larger absolute deviations of R from unity are in the forward-scattering direction. However, the relative difference may be nearly as large in the backscattering direction.

13. Line 6-7, page 19289, at nadir incidence, R should be circularly symmetric (varying only with θ_v), not only symmetric with respect to the 90-270 axis.

14. Line 22, page 19289 says "At 1.5 μm, the variations of R(S1)/R(S3) are stronger at 30° than at 60° incident angle." This doesn't really seem to be true; the maximum value is larger at 30° (1.8 versus 1.4), but the variability in the side and backward directions is larger at 60° (0.5 compared to 0.8), and a ratio of 0.5 is as different from 1 as a ratio of 2, so the difference is a bit more complicated than explained in the text. Also, the maximum values in the forward peak at 60° are likely not observed, so the ratio might get larger there if observations extended to 80° viewing angle.

15. Lines 24-25, page 19289, what does 'sharper and higher' mean? Why is it stated that the observed differences are definitely due to the grain size, when it is acknowledged elsewhere that grain shape can also be important?

16. Section 7.1, was it really a power law distribution that was used? If so, were there some limits placed on the range of sizes, since an unrestricted power law would make a lot of very small particles? A log-normal distribution, or something similar, seems more common for size distributions.

17. What shape and size were the cylinders used in the SnowRAT model. The effective radius is given, but what was the radius and length of the cylinders?

18. Lines 2-3, page 19292, the darkening at grazing angles also appears in the model at 1.3 μm (maybe at 1.5 also, but no details are visible there).

19. The reference to Aoki et al (2000) in line 25 of page 19292 should be replaced with Warren and Brandt (2008), which is a review and compilation of work done since Warren (1984), the main source for Aoki's figure.

20. Line 15 of page 19293 refers to Figure 3a of Hudson et al (2006); in that figure, the darkening at grazing angles occurs only at non-forward directions—there is still a

forward peak near the horizon.

21. I found the discussion of the source function in the paragraph going from page 19293 to 19294 confusing. I think it could be clarified in general. One specific point about the statement "the source function decreases from the surface to depth because downward diffuse radiation approaches zero close to the surface"—my understanding was that the source function generally decreases with increasing depth, but that in the uppermost layer it increases with depth because of the lack of diffuse radiation right near the surface. That is, the source function initially increases with depth from the surface, reaches a maximum at a small depth below the surface, and then steadily decreases with depth. Then the reduction in radiance as you look at very large viewing zenith angles is due to looking into the region above the maximum in the source function.

22. Case 2 in Section 8.1, the forward scattering peak, is also observed for short (low-absorption) wavelengths with large enough incident zenith angle. Line 15 on page 19294 should read something like, "At large incident zenith angles, or at wavelengths with strong absorption, R patterns show a strong forward scattering peak." Also, line 6 on page 19295 should read something like, "2) strong absorption or high incident angle…."

    With weak absorption, the forward scattering peak will still show up at large incident zenith angles (as seen in Figure 4c) because part of the forward peak of the phase function then sends photons toward the surface in the forward peak direction, so even though multiple scattering is possible, the photons are still progressively more likely to exit in the forward direction.

23. In the paragraph starting at line 5 on page 19296, it is not clear how a peak in the phase function at 22-25° would cause a peak in R at about (θ_v=30°, ϕ=180°). With the incident zenith angle of 30°, that viewing angle requires a deviation of 120°, so that is the angle where a peak in the phase function would explain the peak in R, but 120° is often a minimum in Xie's phase functions.

24. In the last paragraph of Section 8.2, it seems unlikely that diffuse illumination hides the double peak in field observations at 1.5 microns since there is essentially no diffuse illumination under a clear sky at such a long wavelength. I would expect the second explanation (that measurements are typically made with larger solar zenith angle), or the angular field of view of the field sensors to be the cause for this not being observed in the field.

25. Line 17 of page 19297, forward scattering is also stronger for shorter wavelengths with larger incidence angle.

26. In the conclusions (line 26 page 19297) it states that using non-spherical shapes for the grains improves the agreement between the measurements and model, but it is not clear that this is the case from the results presented here. First, there is only one case,

1.5 microns with incident angle 30°, that can be compared between observations and both models, so any such conclusion appears to be generalized from one point. Second, looking at that case, except for the rainbow, the results in Figure 8c look a lot more like the observation in Figure 4d than the results in Figure 9g or h do. Therefore, while it is probably true that using non-spherical shapes *can* improve the agreement, it is not shown here.

27. In Table 1, should the SSA for S2 be 126 m^2 kg^{-1}? That would be the more standard units and gives a radius of about 25 μm, while the current units seem to imply a radius of about 0.4 m.

28. Figure 1 would be clearer if the horizontal plane at z=0 were shaded to show the surface. The negative z axis is not needed and the reference surface would help show which lines are projected on the surface, and which are above it.

    In the text, ϕ is generally used as the angle between the incoming and reflected azimuths, but here it is an absolute direction.

    Offsetting the value of z where the θ arcs intersect the z-axis would make it clear that they are two different arcs.

29. The spectral albedos plotted in Figure 2 need to be discussed. The values at 0° and 30° incidence seem surprisingly low, compared to Wiscombe and Warren (1980), for example. Perhaps it could be explained by impurities. Also, the increase in albedo in the visible from 30° to 60° seems too large. Models, such as Wiscombe and Warren (1980), show little effect of zenith angle on albedo at non-absorbing wavelengths. Also, extrapolating that increase to larger incident zenith angles gives albedos well over 1. Perhaps calculating the albedo from a small number of radiances that do not cover all angles leads to some of these unusual features.

30. In Figures 4, 5, and 6, the interpolation between θ_v=30° and θ_v=0° could be done for all azimuths; there is no reason that the observation at θ_v=0° should be viewed as only being at an azimuth of 0°.

    For the θ_i=30° observations, why are there no data for θ_v=60° or θ_v=70°, with 0° azimuth (i.e. no data in the backward direction)?

    Similarly for θ_i=60°, I would expect to see data in the backward direction at θ_v=30°, and maybe also at θ_v=70°.

    The contours lines in the areas that are shaded white, or nearly white, should be made black or dark gray. This applies most to the contour for R=4 in Figure 4f.

31. The average value of R over the hemisphere (weighted by $\cos\theta_v$ and $d\omega$) should be 1, yet in Figures 8a and b there are few, if any, values greater than 1. What is

happening there?

32. If the values in Figures 9g and h really vary so much that the current versions of the plots are correct, then they need to be plotted with a different colorbar that allows the variations of R to be seen. Using one consistent colorbar is nice, when possible, but in this case it makes it impossible to examine the results.

33. Since the main point of the modelling is for comparison with and to help understand the observations, it would be useful to plot the differences between the model and the observations (possibly in addition to the plots of the modelled R). The comparisons by eye are difficult to make.

Suggested Corrections

1. 4/280 (line 4, page 19280): 'one of the first sets'
2. 19/281: 'They are also one of the first investigations'
3. 20/281: 'configurations'
4. 22/281 and elsewhere (search phenomenons): the plural of 'phenomenon' is usually 'phenomena'
5. 3/282: 'top of atmosphere' does not need to be capitalized
6. 11/282: replace 'supposed' with 'assumed'
7. 21/282: change to '$R(\theta_i, \phi, \theta_v, \lambda)$, a normalized BRDF value, is often used.'
8. 2/283: begin a new paragraph at 'In most field…'
9. Equation 4: the subscript 'o' in the denominator should be 'i' for consistency
10. 12/283: 'properties have been performed'
11. 23/283: 'field measurements' (no s on field)
12. 5-6/284: 'studies illustrate the main patterns'
13. 24/284: change 'lead' to 'led' if you want to use the past tense, or 'leads' to use the present tense
14. 2/285: 'are more appropriate for simulating snow'
15. 2-3/286: 'At nadir incidence, the illumination pattern at the sample surface is circular, with a 200 mm diameter.' (Otherwise it says it is always circular, but the diameter changes)
16. 17/286: delete the 'd' from 'refrozed'
17. 18/286: delete the 's' from 'grains' (could also add an apostrophe after the s, *grains'*, to make it possessive, but 'grain shape and size' would sound most natural to me)
18. 24/286: change 'have' to 'has'; it goes with 'a set', singular
19. 3/288: delete 'd' from 'changed
20. 9/288: change 'minima' to 'minimum'
21. 22-23/288: 'as a function of wavelength, for three different observation angles, with a fixed illumination angle' (the values in figure 3 are plotted as a function of wavelength)
22. 8/289: change '-30° viewing zenith angle' to '30° viewing zenith angle, in the forward direction' or something like that.
23. 5&6/290: change 'maxima' to 'maximum' (singular)

24. 7/290: change ‘maximum’ to ‘maxima’ (plural)
25. 21/290: change ‘is’ to ‘are’ (refers to ‘sizes’); add a comma after ‘1 mm’
26. 4/292: ‘maxima’ -> ‘maximum’
27. 24/293: change ‘ends’ to ‘tends’
28. 4/294: delete ‘e’ in ‘occurs’
29. 5/294: change ‘S. J.’ to ‘S. G.’ (assuming it refers to Steve Warren, Seattle)
30. 16/295: change ‘differ’ to ‘differs’ (refers to ‘ratio’)
31. 6/296: I think this should be Figure 6a; Figure 4 is for S3, not S1
32. 21/296: ‘maxima’->’maximum’
33. 22/296: change ‘illuminate’ to ‘illuminates’
34. 21/297: delete ‘factor’; the anisotropy factor increases in some viewing angles and decreases in others, but the overall anisotropy increases
35. 9/298: change ‘for its comments’ to ‘for her comments’
36. Table 1, footnote b: ‘Specific Surface Area (total surface of ice crystals accessible to gas, per unit mass of ice) was measured for S2 using the methane absorption method (Legagneux et al., 2002).
37. Caption for Figure 5: change ‘several’ to ‘two’
38. Figures 8 and 9: add a polar angle (θ_v) grid

References used in this review that are not in the manuscript

Nicodemus, F. E., J. C. Richmond, J. J. Hsia, I. W. Ginsberg, and T. Limperis (1977), Geometrical Considerations and Nomenclature for Reflectance, NBS Monogr., vol. 160, Natl. Inst. of Stand. and Technol., Gaithersburg, Md (available at http://graphics.stanford.edu/courses/cs448-05-winter/papers/nicodemus-brdf-nist.pdf)

Warren, S. G., and R. E. Brandt (2008), Optical constants of ice from the ultraviolet to the microwave: A revised compilation, J. Geophys. Res., 113, D14220, doi:10.1029/2007JD009744.

Warren, S. G., R. E. Brandt, and T. C. Grenfell (2006), Visible and near-ultraviolet absorption spectrum of ice from transmission of solar radiation into snow, Appl. Optics, 45, 5320—5334.