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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of snow bidirectional reflectance from ARCTAS spring-2008 campaign" *by* A. Lyapustin et al.

Anonymous Referee #1

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1 GENERAL COMMENTS

This paper represents an important advancement in Arctic and Antarctic remote sensing in that it validates BRF models for snow surface with high angular resolution measurements from CAR. The analytical models used for MODIS and MISR, respectively, are RTLS and MRPV. While those are applied for all surface types, AART (analytical) and SHARM (numerical) were developed specifically for snow-covered areas by the co-authors within previous research. CAR-based BRF retrieval results are represented in the RTLS and MRPV framework, and the deviation of the RTLS- and MRPV-forward-modeled BRF from CAR measurements is used for evaluating the adequacy of the two models for satellite remote sensing over snow.

Although snow BRF's were measured in the past (mainly surface-based, mainly Antarctica), this study introduces at least two novel aspects: (1) A "side-product" of the CAR-derived surface BRF: surface albedo, is compared with simultaneous independent spectral surface albedo measurements near the sampling site (Elson Lagoon). This serves as validation of the BRF product with independent measurements and ensures that the atmospheric correction was applied correctly. (2) Since plane-parallel radiative transfer models (one of which being SHARM) introduce biases in the BRF distribution in two directions, the authors pursue the following ways to improve the agreement between CAR measurements and forward-model BRF representations: (2a) varying underlying snow microphysics (crystal shape/size) and (2b) introducing surface structure (sloped facets and shadows) into the model. In doing so, they are able to achieve a far greater consistency of their slope-averaged shadow-corrected SHARM, with the CAR measurements. Two new parameters related to the topography of surface are introduced.

The paper is generally very well written (excellent introduction!). Section 3 appears to have a slightly lower language quality. I recommend some minor changes to the flow / structure of the paper (see specific comments), and some clarifications, as well as typo corrections. Other than that, this is an excellent paper that I recommend for publication after minor revisions.

2 SPECIFIC COMMENTS

2.1 Instrumental issues

There are some unclarities in instrument description and data processing for the ground-based instrumentation:
p21998,l7-11 : The first sentence is incompatible with the rest: "The cosine collector

is designed to accept incident radiation with equal efficiency from any angle in the hemisphere." – this would be a light collector with isotropic characteristics where in fact the incoming radiation is weighted by the cosine of the incidence angle as you state in the next sentence. I would simplify these lines (just keep "cosine weighting" or "projected detector area"). This will also make it easier to understand.
p21998,l19-21 : Describe how the two light collectors are different – are they diffusers or integrating spheres – or something different?
p21998,l24 : Figure 1 caption is incorrect. It is a leveled, and not a leveling cosine collector.
p22000,l5 : Should 3.1 be called "ground-based measurements"? Otherwise the reader will wonder why the other cases are not discussed. (I believe they are not because there were no ground-based measurements available on that day.)
p22000,l19 : Can you discuss the effects of SZA, or is it not relevant for this manuscript?
p22000,l20 : "Due to differences in cosine collector design..." - Please explain what type of cosine collector was used (please see also comment above).
p22000,l23 : It should really be explained how this rescaling was done. If both UW ASD and NPI have "good" accuracy between 800-850, that means they should measure the same, why can you rescale the ASD instrument and how does this help the SW and near-UV wavelengths and/or the NIR wavelength range of the instrument of lower quality. Did you rescale all across the spectrum? What was the scale factor that you applied?
p22003,l6 : "convert digital numbers into reflectance" - this is a bit confusing: First of all, you should get **radiance** from "counts" by multiplying with the spectral instrument response function. Then, use modeled (or ideally, measured) irradiance to convert this radiance into reflectance. You have omitted step #1 in your discussion.
p22003,4.2: Have you compared your modeled spectral irradiance with airborne measurements?
p22010,l10: Is it not τ^g, the band-integrated absorption rather than the monochromatic

absorption $\tau^g(\lambda)$ that you need? You write it the other way round.
section 4.3: Is it possible to give error estimates for (a) CAR-derived radiance, (b) CAR-derived reflectance and (c) CAR-derived surface albedo? It should be possible to give the reader an impression of how much of an impact the atmospheric correction has for the accuracy of each of these parameters - I assume it is negligible compared to radiometric uncertainty.

2.2 Model related issues

p22002,formula(4): Is τ_0 really column optical depth, or path integrated? How about $\tau(z)$ - vertical integral above or below aircraft, or path-integrated?
p22002,l20: From the manuscript alone, it is not clear why you should need additional RT calculations for computing the MRPV parameters. Can you clarify?
p22005,l20-l21: "Although the use of SIM irradiance reduces the reflectance..." - how so if SIM is your only source for irradiance how can it reduce radiance - compared to what?
p22011,l16: Define "SPD".
p22012,l15: Define parameter D.
p22014,l8-l9: You can make this discussion easier by introducing something like "effective" surface roughness as a term.
p22014,l10: Replace "a" with "actual".

2.3 Suggested re-structuring of manuscript

In light of the issues raised in the instrument section, it might be helpful to have a separate section dedicated to "instruments". Writing this as a separate section

has the advantage that you can add a discussion of CAR, its calibration etc. which is completely missing so far. Also, you should probably expand a little bit upon uncertainties, as far as they are relevant for the message of the paper. If you decide to omit CAR error bars in, e.g., Figure 2, please provide at least a qualitative discussion.

There could also be some benefit in having a separate "model" section. It may be confusing to a non-expert to clearly categorize the various models used. For example, RTLS and MRPV are simply used for MODIS and MISR, as analytical BRF models. SHARM, in contrast, is a numerical code that delivers the "true" BRF based on any given microphysical snow composition. When it comes to AART (specialized for snow, but also analytical, that is, it has to live with assumptions) in section 7, it is sometimes unclear what it is that you use: Even though the title of section 7 is AART, you use SHARM in 7.1 and 7.2

A brief (one-two paragraph) general introduction which model belongs to which class: numerical, analytical, (semi-)empirical, and telling which one(s) are used for MODIS, MISR (is AART also used for satellites, e.g., from the ESA fleet?) right up front could help.

3 TECHNICAL CORRECTIONS / TYPOS

p21997,l6: Insert , after Alaska
p21997,l22: Insert , after sampling
p21998,l23: "...roughness was in the form of..." is grammatically incorrect (you need another verb in addition to was) – maybe "...roughness was dominated by..."?
p22000,l13: "incident" - Do you mean "incidence"?
p22001,l18: replace: term non-linear –> term that is nonlinear

p22002,l3: replace: according to current –> according to the current
p22002,l12-l14: insert "the" in 3 instances: after "Once", "altitude z of", and "representing".
p22005: insert "the" in two instances, after "coverage of" (l11) and "used in" (l18).

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 21993, 2009.