

Interactive comment on “Snow spectral albedo at Summit, Greenland: comparison between in situ measurements and numerical simulations using measured physical and chemical properties of the snowpack” by C. M. Carmagnola et al.

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Received and published: 5 February 2013

General:

The paper addresses an important question and presents a very useful data set on spectral albedo, which has carefully been measured at Summit, which can be regarded to be quite representative for a large part of the Greenland ice sheet. The combination of in situ data with modeling is attractive and the combination of individual measurements including specific surface area (SSA), density, black carbon (BC) and dust in the

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snow profile with the spectral albedo is unique. Overall the paper is well written, easy to follow and I support publication in TC. In the following, I discuss points that should be considered to improve the presentation.

1) Title: The title is long but descriptive. Maybe use a shorter version “Snow spectral albedo at Summit, Greenland: measurements and RT simulations based on physical and chemical properties of the snowpack”

2) I share the view of anonymous referee #1, in particular with respect to the RCR correction and the effect of the highly non-isotropic bidirectional reflectance and suggest that these points are carefully addressed, although I will not try to repeat the content of the detailed and appropriate assessment provided by referee #1. You may consult and discuss Odermatt et al. (2005) and Bourgeois et al. (2006) for detailed measurements of HRDF over snow in this context.

3) The introduction is very general, which may be useful for some readers. In particular talking about the general behavior of snow and ice albedo (p. 5122, l. 15 ff) and scattering and absorption (p. 5123, l. 3 ff) is almost textbook style and may be somewhat shortened. On the other hand, on p. 5124, last paragraph, it is stated that “direct ground-based measurements of snow and ice albedo are sparse...” and I therefore suggest a few of the more interesting studies are discussed in this introduction. In particular, I would suggest that the publication of Bourgeois et al. (2006), which I already referred to above, is discussed since it has data from the same place and with a similar (or even the same) instrument. Also the earlier study of Meirold-Mautner and Lehning (2004) from the same location could be discussed although it did focus on the absorption aspect but has again measurements from a similar instrument and used the same model. Another interesting study in this context is (Banninger et al. 2008). These earlier studies could also help in the discussion of the deviations between measurements and simulation in the two wavelength bands at 1430 and 1800 nm.

4) What I also miss is a comparative discussion of the relative effect of BC and dust

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from other areas in the world. I would like to point the authors to a recent study (and references therein), which is currently in press (Ming et al., 2013, ADWR), in which it is found that BC influence of albedo is also small for the Himalayas but that (naturally) dust has a larger influence there. Please discuss your specific findings with respect to what is found in other areas of the world.

Editorial comments:

p.5124, l.5: I do not understand why SSA is important for assessing the energy budget of the snow cover.

p.5124, l.12: I suggest that you either provide a more in depth description of the DU-FISSS instrument or just work with the reference. The sentence “an integrating sphere . . .” is not helpful if you don’t already know details about the instrument.

p.5124, l.19: It is not clear what you mean with “features an effective SZA. . .”.

p.5124, l.21: You may add that surface roughness can trap radiation and lead to lower albedo values.

p.5127, l.17ff: You should clearly say in this paragraph that you used the Col de Porte measurements to better understand the discrepancies between your model predictions and the measurements at the two wavelength bands of 1430 and 1800 nm.

p. 5128, l.13: Please replace “ratioing”

p. 5129, l.12: The assumption of isotropy is clearly not a good one over snow as shown by the additional references, I pointed the authors to above. You should at least discuss what errors you expect from this assumption.

p. 5130, l.26: The smoothing reported here is probably explained in the original publication presenting the instrument. It needs explanation here, however, because in principle you would want as high a resolution as possible and the “integrating sphere” (see also comment above) is already providing some spatial average.

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p. 5138, l.15: As discussed just above, it is desirable to have a high spatial resolution for the RT modeling.

I really like the presentations on pages 5135, 5136, 5140 and 5143.

p. 5144, l.27: I would add here "...except for the two wavelength bands with high discrepancies, which are discussed below", or something like this.

p. 5146, l.9: I would start a new paragraph here.

References: Odermatt D, D Schläpfer, M Lehning, M Schwikowski, M Kneubühler & K I Itten, 2005. Seasonal study of directional reflectance properties of snow. EARSel eProceedings, 4(2), 203-214.

Banninger, D., C. S. Bourgeois, M. Matzl and M. Schneebeli (2008). "Reflectance modeling for real snow structures using a beam tracing model." Sensors 8(5): 3482-3496.

Bourgeois, C. S., P. Calanca and A. Ohmura (2006). "A field study of the hemispherical directional reflectance factor and spectral albedo of dry snow." Journal of Geophysical Research-Atmospheres 111(D20).

Meiold-Mautner, I. and M. Lehning (2004). Measurements and model calculations of the solar shortwave fluxes in snow on Summit, Greenland. Annals of Glaciology, Vol 38, 2004. P. M. B. Fohn. 38: 279-284.

Interactive comment on The Cryosphere Discuss., 6, 5119, 2012.

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