Note: Reviewer's original comments are in black text and author responses are in blue text

M. Lehning (Referee #2)

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 modelling is attractive and the combination of individual measurements including specific surface area (SSA), density, black carbon (BC) and dust in the 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.

We really thank M. Lehning for this positive evaluation and for taking the time to provide a detailed critique of our paper. We have carefully considered each comment and our responses are provided below.

(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"

We have adopted this suggestion and shortened the title as follows: "Snow spectral albedo at Summit, Greenland: measurements and numerical 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.

We thank M. Lehning for this comment. This critique has already been addressed in our response to the first Reviewer.

For the RCR correction, please see our previous reply to the first Reviewer's comments #1 and #2.

For the effect of the anisotropic reflectance (and for a discussion of Odermatt et al., 2005 and Bourgeois et al., 2006), please see our previous reply to the first Reviewer's comments #1 and #8.

(3) The introduction is very general, which may be useful for some readers. In particular talking about the general behaviour 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.

We agree with the Reviewer that the introduction is quite general. However, since in the paper we discuss all the factors influencing the albedo (ice index of refraction, snow physical properties [density and SSA], presence of impurities [BC and dust], angular and spectral distributions of incoming solar radiation), we think that a general presentation on the behaviour of snow spectral albedo can be useful for some readers to better understand our results.

(3 - continued) 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.

We thank M. Lehning for these suggestions. More informations regarding the studies of Meirold-Mautner and Lehning (2004) and Bourgeois et al. (2006) have been added to the revised manuscript. The paragraph has been modified as follows:

"Direct ground-based measurements of snow and ice optical properties are sparse and discontinuous (Hansen and Nazarenko, 2004; Wang and Zender, 2011). The available field data cover only few areas and a small range

of snow and ice types. The database is limited especially for remote regions such as the Arctic and Antarctic (Kondratyev and Cracknell, 1998). Meirold-Mautner and Lehning (2004), for instance, measured the transmission of light thought the snowpack at Summit using an ASD spectroradiometer. At the same location, high quality measurements of Hemispherical-Directional Reflectance Factors and spectral albedo were carried out by Bourgeois et al. (2006), under solar zenith angles from 49° to 85° and for different snow surface type s. These measurements are crucial in order to improve satellite-based albedo retrievals…"

After having revisited the Banninger et al. (2008) paper, we think that this work is not helpful in the context of ground-based measurements of snow albedo, since it describes reflectance measurements performed in a cold room and not in the field.

In addition, we do not believe that these papers provide a useful point of reference in regard of the deviations between measurements and simulations at 1430 and 1800 nm, as neither of these studies focus on wavelengths longer than 1050 nm: Bourgeois et al. (2006) used a specially designed Gonio-Spectrometer with wavelength range of 350-1050 nm, Meirold-Mautner and Lehning (2004) used a spectroradiometer sensitive in the 340-1050 nm spectral band and Banninger et al. (2008) also focused on wavelength smaller than 1050 nm.

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

The Reviewer makes a good suggestion. A comparative discussion of the relative effect of BC and dust from other areas of the world has been added to the discussion section of the revised manuscript, as reported below:

"The fact that dust, due to its higher concentration, may have a stronger effect on light absorption than BC is consistent with previous studies in other areas of the world. X. Wang et al. (2013), for instance, found that in inner Mongolia BC is responsible for only about a third of total particulate light absorption and in the Qilian Mountains (north of Tibetan Plateau) snow absorption is strongly dominated by dust. Gautam et al. (2013) pointed out the importance of dust deposition toward darkening of Himalayan snow cover. The weak effect of BC on the energy budget of High Asian glaciers was further confirmed by Ming et al. (2012), who estimated that BC deposition causes a mean forcing of only 5% of the total forcing and therefore is not the major factor impacting the melting of most glaciers in this area."

Editorial comments:

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

The Reviewer raises a question that we feel is adequately answered in introduction of the original manuscript.

The energy balance of the snowpack at short wavelengths depends on how much sunlight is scattered back by snow (albedo). Albedo is determined, in turn, by the profile of the physical (and chemical) properties of the snowpack; SSA is one of these properties, giving an essential information about the dimension of snow grains. In particular, in the IR region of the spectrum, where about 40% of the incoming solar radiation occurs, albedo is mostly determined by SSA.

(6) p.5124, l.12: I suggest that you either provide a more in depth description of the DUFISSS 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.

We agree that the description of DUFISSS instrument is not helpful in this context. For this reason, it has been removed in the revised manuscript and replaced by the reference to Gallet et al. (2009).

(7) p.5124, l.19: It is not clear what you mean with "features an effective SZA…".

Wiscombe and Warren (1980) showed that "… the purely diffuse radiation has an effective zenith angle near 50°", i.e. the albedo of snow under purely diffuse illumination is about the same as the albedo of snow for a collimated incident bean with a 50° incident angle.

In our case, albedo measurements were obtained at SZA ≈ 50°. Thus, since this angle is equal to the effect ive zenith angle for purely diffuse radiation, the impact of direct/diffuse radiation fraction on our measured spectral albedo is negligible.

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

We agree with this good suggestion. This information has been added to the introduction of the revised manuscript as requested.

(9) 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.

We thank M. Lehning for this comment. The paragraph has been modified accordingly in the revised manuscript.

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

The word “ratioing” has been replaced by “by making the ratio of” in the revised manuscript.

(11) 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.

We agree with this observation. For a more in depth discussion on the anisotropy of the upwelling diffuse radiation and its impact on the bi-hemispherical albedo, please see our previous reply to the first Reviewer’s comment #8.

(12) 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.

Our description of the procedure used to convert reflectance measurements from the DUFISSS instrument into SSA values was not accurate. The sentence “smoothed by using a radiative transfer model”, in particular, is ambiguous, since it seems to refer to the vertical resolution of the instrument. Actually, what we wanted to say is that SSA values are retrieved directly from reflectance using the DISORT radiative transfer model. Regarding the CH4 absorption technique, it was only used by Gallet et al. (2009) to validate this approach independently.

Since the presentation of the DUFISSS instrument is not helpful in this context, we have removed this paragraph in the revised manuscript (please see our previous reply to M. Lehning’s comment #6).

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

We appreciate the Reviewer’s point of view. As explained in the section “Profiles of the snow physical properties”, our SSA measurements have a vertical resolution of 1 cm (p. 5131, l.2). Since wind slabs thinner than 1 cm are present within Summit snowpack, a higher resolution in SSA measurements would be of course desirable for DISORT simulations. Moreover, a very thin, high SSA layer made up of precipitation particles, surface hoar or rime is often present at the surface (p. 5138, l.2). The impact of this thin surface layer on the simulated albedo has been already presented in the original manuscript in relation to Fig. 10.

In the discussion section of the revised manuscript (more precisely in the paragraph where we focus on the infrared wavelengths), this mismatching between the vertical resolution required by simulations and that of our input profiles has been discussed more clearly.

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

(14) 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.

We agree. The sentence has been added to the text.

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

Good suggestion. The revised manuscript has been modified accordingly.

References:

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