Replies to comments of Anonymous Referee #1

1. My comments mostly pertain to how the terms BRDF and HDRF are used. It still remains a bit confusing in your introduction the difference between the BRDF and the HDRF. Since this is still often used non-correctly in many publications it would make sense to be more careful and not use the terms so interchangeably. My understanding from Schaepman-Strub et al. 2006 is that the BRDF is not something that can be measured whereas the HDRF is. Thus, it is confusing when you then say on line 26 that comparison of in situ measured BRDF with simulations, as the BRDF is not measured in situ as far as I understand it. The equations for each are shown in the methodology, which is helpful, but it does remain a bit confusing they way it is discussed. Since the HDRF is what is actually being measured from the digital camera, I think that all needs to be stated more clearly upfront and it would be good to add discussion on how Eq.18 relates to Eq. 3 for those less familiar with these topics – i.e. students.

Thank you for this comment. We agree that the used terminology and the differentiation between HDRF and BRDF is done too sloppily in some other publications. We tried to be careful in this matter, however as you correctly pointed out, some inconsistencies slipped our attention. In case the actual measured quantity was the HDRF, we changed the term BRDF to HDRF (although stated differently in the respective publication). We also define an 'effective BRDF' as done in Gatebe and King (2016) in case the FOV is small and the atmospheric influence is considered. We believe this is reasonable for measurements with the CAR instrument and decided to reference the CAR database in the same way as reported by Gatebe and King (2016) as this constitutes an important dataset.

We agree that, so far, the relation between Eq. 18 and Eq. 3 was not obvious. We therefore omit the rather theoretical definition given so far in the manuscript (Eq. 3) and give already the more conceptual definition via the BRF. This way, Eq. 3 already has the form of Eq. 18 and the relation should become more clear to the reader.

Changes in text:

- Page 3 Line 29: The comparison of in situ measurements of the snow reflectance with simulations is essential in terms of model validation. Observations of the HDRF (or effective BRDF in case the FOV is small and the atmospheric influence is considered) are conducted using a variety of different measurement concepts.
- P4 L10: 'Several studies observed systematically less anisotropy for a typical snow BRDF than estimated from simulations [...]': we replaced 'BRDF' with 'HDRF'
- P4 L19: 'Thus, the latter are more suitable for studying the influence of macroscopic surface roughness on the surface BRDF.': we replaced 'BRDF' with 'HDRF'
- P4 L26: 'and the snow BRDF loses its azimuthal symmetry (Warren et al., 1998)': we replaced 'BRDF' with 'HDRF'
- P5 L8: 'Cox and Munk (1954) analyzed radiance calibrated analog photographs for the parameterization of the ocean BRDF': we replaced 'BRDF' with 'HDRF'

– P5 L10: 'The instantaneous measurement of multiple viewing angles facilitates aerial BRDF measurements with digital cameras.': we replaced 'BRDF' with 'HDRF'

– P6 L5: As the BRDF of an ideal Lambertian surface is $(\pi\,\mathrm{sr})^{-1}$, the BRF is given by

$$R_{\mathrm{BRF}} = \frac{\mathrm{d}I_{\mathrm{r}}\,(\theta_{\mathrm{i}},\varphi_{\mathrm{i}};\theta_{\mathrm{r}},\varphi_{\mathrm{r}})}{\mathrm{d}F_{\mathrm{i}}\,(\theta_{\mathrm{i}},\varphi_{\mathrm{i}})} \cdot \frac{\mathrm{d}F_{\mathrm{i}}\,(\theta_{\mathrm{i}},\varphi_{\mathrm{i}})}{\mathrm{d}I_{\mathrm{r}}^{\mathrm{ideal}}\,(\theta_{\mathrm{i}},\varphi_{\mathrm{i}})} = \pi\,\mathrm{sr}\cdot f_{\mathrm{BRDF}}.$$

In Eq. 2 and in the remainder of this section, the spectral dependence is omitted for reasons of simplicity. In atmospheric conditions, both the BRDF and BRF cannot be measured directly as the global irradiance F_0 reaching the surface is composed of a direct (F_{i}) and diffuse (F_{diff}) component. In this case, the measurable quantity is the HDRF. The definition of the HDRF is analogous to the BRF, but includes irradiance from the entire hemisphere (denoted with 2π):

$$\begin{aligned} R_{\mathrm{HDRF}} &= \pi\,\mathrm{sr}\cdot \frac{\mathrm{d}I_{\mathrm{r}}\,(\theta_{\mathrm{i}},\varphi_{\mathrm{i}},2\pi;\theta_{\mathrm{r}},\varphi_{\mathrm{r}})}{\mathrm{d}F_0\,(\theta_{\mathrm{i}},\varphi_{\mathrm{i}},2\pi)} \\ &= R_{\mathrm{BRF}}(\theta_{\mathrm{i}},\varphi_{\mathrm{i}};\theta_{\mathrm{r}},\varphi_{\mathrm{r}})\cdot f_{\mathrm{dir}} + R(2\pi;\theta_{\mathrm{r}},\varphi_{\mathrm{r}})\cdot(1-f_{\mathrm{dir}})\,. \end{aligned}$$

– new Eq. 18:

$$R_{\mathrm{HDRF}}(\theta_0,\varphi_0;\theta_{\mathrm{r}},\varphi_{\mathrm{r}}) = \frac{\pi\,\mathrm{sr}\cdot I(\theta_{\mathrm{r}},\varphi_{\mathrm{r}})}{F^{\downarrow}\,(\theta_0,\varphi_0)}\,.$$

2. I also do not follow why the downward irradiance from SMART could not be used to calculate the HDRF. If it's good enough for the surface albedo, and thus the grain size determination, then I do not follow why it's not good enough for the incoming solar radiation. Also, if as you state you are mostly interested in the shape of the HDRF, not the absolute magnitude of the values, then the calibration shouldn't have mattered? I would like to see a comparison between your modeled and measured incoming solar irradiance.

The grain size determination relies on surface albedo measurements. The calculation of the surface albedo from the raw spectra measured with SMART involves three main steps: (a) the correction for dark and stray light, (b) the correction for the non-ideal angular response of the sensor–spectrometer system, and (c) the cross- calibration of the up- and downward looking optical inlets to account for different sensitivities of the sensors-spectrometer systems. The latter part (c) was performed in the field during the observations and comprised successive measurements of the signals of the up- and downward-looking sensors under the same illumination with an integrating sphere. Thus, for albedo measurements, an absolute calibration converting the digital numbers registered by the spectrometers into units of irradiance is not required. All calibration steps for measurements of the surface albedo could be carried out, which is why the retrieval of the optical-equivalent snow grain size from SMART measurements is not affected by these calibration issues.

This is different for the downward irradiance that requires an absolute calibration converting the raw signal into units of irradiance. The absolute calibration was performed in the laboratory. Normally, the cross-calibration is used to transfer the absolute calibration as measured in the laboratory to the field to account for changes in the transmissivity of the sensor–fiber–spectrometer system. Unfortunately, problems with the power supply of the integrating sphere in the field occured, and the integrating sphere got destroyed after the campaign before it could be used later for an additional laboratory calibration of SMART. Due to this failed transfer calibration, an absolute calibration was not possible and the downward irradiance measurements from SMART could not be used for the calculation of the HDRF. For the same reason, a comparison between modeled and measured incoming solar irradiance is not possible.

Instead, the global irradiance was simulated along the flight track with libRadtran using DISORT. The simulated irradiance was integrated over the wavelength range of each camera channel and weighted with the RSR function of the camera. The use of simulations limits the validity of absolute values of the measured HDRF to cloudless conditions. It is true that we are mostly interested in the shape of the HDRF. However, the retrieved values for the model parameter f_{iso} would be influenced by the absolute values of the HDRF. We therefore restrict our analysis to cloudless cases when we are confident that the simulated values for the downward irradiance were representative of the actual measurement conditions.

As both reviewers mentioned this, we added a clarification in the manuscript.

Changes in text:

– P14 L4: The downward irradiance measurements from SMART could not be used for the calculation of the HDRF due to calibration issues. Note that this pertains to the radiometric calibration only, which converts the digital numbers registered by the spectrometer into units of irradiance. For the albedo measurements with SMART, a relative calibration of the upper and lower sensors is sufficient and an absolute radiometric calibration is not required. Thus, the albedo measurements and the retrieval of the optical-equivalent snow grain size are unaffected by this calibration issues. For the calculation of the HDRF, the global irradiance was simulated [...] instead.

3. What were the cloud vs. clear sky conditions during these flights? Were synoptic cloud observations not also obtained? Could you not see whether or not there were clouds with the SMART measured incoming solar irradiance?

In approximately 75 % of the camera observations, cloudless conditions prevailed. Indeed, cloudless cases were identified from visual synoptic observations during the flights as well as using the downward measured irradiance by SMART. Periods with fast fluctuations in the downward irradiance were flagged as cloudy. Clouds influence the HDRF measurements in changing the direct/diffuse ratio of incoming solar radiation. For a quantitative analysis, the cloud scene during the times of measurement (cloud cover, optical thickness, etc.) needs to be well characterized, which is not possible from the observations available during the flights. Thus, we restricted our analysis to cloudless conditions only.

Changes in text:

– P14 L13: The use of simulations limits the validity of absolute values of the measured HDRF to cloudless conditions. Cloudless cases (approximately 75 % of the camera observations) were identified from visual synoptic observations during the flights as well as using the downward irradiance measured with SMART. Periods with fast fluctuations in the downward irradiance were flagged as cloudy. Although mainly the shape of the HDRF is analyzed within this work (which is independent from the absolute value of $F^{\downarrow}$), the analysis is restricted to cloudless conditions only.

4. I don't follow exactly what was done in section 3.6 for the inversion. How was the HDRF used in this context? Also, seems you setting the HDRF equal to the BRDF in Equation 12 but we don't find that out until section 3.7 so be good to mention it earlier. However, in section 3.7 you then mention you don't expect the atmospheric conditions to be large, which is true though as you mention it is wavelength dependent with the blue channel more impacted. Also the discussion then focuses on the BRF and HDRF relationship with the proportion of direct vs. diffuse but again it's the BRDF that you are substituting with the HDRF so I think it would be better to keep the discussion in that context as the interchanging of terms is hard to follow.

Thank you for suggesting this improved structure of the manuscript. We agree that it is helpful for the reader to mention the substitution of the BRDF with the HDRF in Equation 12 earlier. We therefore changed the structure as follows: we switched Sects. 3.6 and 3.7 and added the suggested clarification at the beginning of the new Sect. 3.6. Concerning your second suggestion, we believe it is better to continue discussing the BRF and HDRF relationship as this is what was simulated by Schaepman-Strub et al. (2006). However, we added a reminder about the close relation between the BRF and BRDF by referring to Eq. 2 at this point.

Changes in text:

- P15 L2: For each averaged HDRF, the weighting functions f_{iso}, f_{vol}, and f_{geo} are retrieved by inverting the RTLSR BRDF model and setting the HDRF equal to the BRDF in Eq. 12. This is necessary as the BRDF is not measurable under atmospheric conditions. However, the atmospheric influence [...]
- P15 L11: Schaepman-Strub et al. (2006) simulated the difference between the HDRF and BRF ($R_{\mathrm{BRF}} = \pi \cdot f_{\mathrm{BRDF}}$, compare Eq. 2) for snow surfaces
- switched Section 3.6 and 3.7

5. It is well known that the anisotropy increases with increasing solar zenith angle so it would be good to reference some early publications that have already discussed this (i.e. some early work by Warren seems relevant here). It's also well known that surface roughness reduces the overall forward scattering as there is more backscatter, so again referencing earlier work is important here. This is also how data from MISR are currently being used to map surface roughness over ice sheets and sea ice.

We totally agree and added more references in the discussion part of the manuscript.

Changes in text:

- P19 L18: This expected increase in the anisotropy of the snow HDRF for increasing θ_0 is obvious in the changing model parameters f_k and has been discussed in earlier studies (e.g., Dirmhirn and Eaton, 1975; Kuhn, 1985; Warren et al., 1998; Hudson et al., 2006).
- P20 L1: This is in accordance with Warren et al. (1998) who observed a reduction of the forward reflection peak due to sastrugi during tower measurements at South Pole Station. The effect of surface roughness on the BRDF has been previously investigated in observational (e.g., Grenfell et al., 1994; Hudson and Warren, 2007) and modeling studies (e.g., Leroux and Fily, 1998; Zhuraleva and Kokhanovsky, 2011) and is facilitated by the multi-angular reflectance data from MISR to map surface roughness over ice sheets and sea ice from space (Nolin et al., 2002).

6. I'm surprised that there is no mention of how BRDF uncertainties translate into albedo and absorbed solar energy uncertainties as that is what is really important after all. While the paper is already quite long, this would complete the study. If the MODIS BRDF model is off, how much does it influence the albedo and the energy balance of the ice sheets?

Thank you for this suggestion. We agree that the influence on the surface energy budget would be an important point. While it would be possible to translate the retrieved model parameters into a spectral albedo corresponding to the wavelength range of the camera channel (490-585 nm for green), it is the broadband albedo that controls the surface energy budget. For the narrow-to-broadband albedo conversion, the MODIS albedo product uses precomputed coefficients and information from all spectral channels of MODIS. The camera observations could ideally only cover a spectral range between 400 to 700 nm (compare Figure 3). As the HDRF is strongly wavelength dependent (e.g., Hudson et al., 2006; Marks et al., 2015; Gatebe and King, 2016), it is not possible to imply the same discrepancies between the camera measurements and MODIS at other wavelengths. This holds especially true as the camera observations in the visible wavelength range do not cover longer wavelengths above 1000 nm where ice absorption becomes dominant. All in all, these points would make an attempt at a surface energy budget calculation doubtful in our view.