Response to Referee #1

We very much appreciate the valuable comments of referee #1, they target remaining issues to fine polish the manuscript.

Response to the general comment:

1) and 2) We have rewritten the summary accordingly to emphasize our conclusion:

In summary, we presented a unique multi-dimensional dataset with respect to SZA (time), wavelength and position. Our study of global irradiances in a highly heterogeneous albedo environment shows that even for the relatively simple clear sky situation, a variety of parameters have to be considered which illustrates the complexity of modeling solar irradiances at the Arctic coast. The associated uncertainties of both measurements and model input parameters conceal many of the model effects and reduce the suitability for a stringent validation of the 3D model and BRDF parameterizations.

No doubt, more data of a series of at least two completely clear sky days with stable atmospheric and sea ice conditions would be desirable to better constrain the model input. Measurement uncertainties are expected to be reduced by technological advancement of DA systems and improved input optics. This leaves room for future efforts to more accurately map the irradiance distribution along a large albedo gradient.

3) Direct solar measurements were not part of the instrumentation in this campaign due to logistic limitations. Direct sun measurements are available from the sun photometer and are indirectly used for the AOD input.

Regarding the scenarios, we have thought some time about the structure and although we agree that the reviewer's suggestion is also a good alternative way of structuring the analysis, we concluded that the *tilt* should remain part of the scenarios for the following reason. We start with the *standard* scenario which might be regarded as the naïve first model result. We then discuss all relevant (environmental and instrumental) model input parameters that affect the model comparison with the observations.

These "scenarios" are not treated on the same footing as uncertainties but are, in general, meant to illustrate their individual influences. Some scenarios like *albedo, drift ice* and *cloud* are environmental uncertainties. *Tilt* is an instrumental uncertainty (an unknown error is indeed an uncertainty, this has now been considered in the complete manuscript). Some scenarios like *aerosol* and *topography* are not associated with an uncertainty at all. In effect, the scenarios reflect the various factors that have to be considered in modeling the situation, they are a summary of what we learnt to include during our analysis.

Response to specific comments:

The effective albedo is defined when it is introduced in section 4 first paragraph. The term 'local albedo' (p.8, L18) has been dismissed in lieu of just albedo. The term Lambertian albedo is always used in conjunction with the word 'equivalent', i.e. equivalent Lambertian albedo, which should be comprehensible as the albedo equivalent to a Lambertian surface. P.8. first paragraph has been modified accordingly. The term 'integral albedo' has been dismissed in lieu of just albedo, since the albedo is always the integrated BRDF (over all viewing angles).

Page 2, line 13. The reference Bernhard et al., 2007 has been included.

Page 3, line 18. The term *discrepancies* has been eliminated in the sentence.

Page 4, line 21. Spectra were integrated for 100s, as stated (not averaged).

Page 5, line 22-23. Terms *deconvolved* and *convolved* are used now.

Page 5, line 28. *Error* has been replaced by *uncertainty*. But the ratio of direct to global irradiance for 500 nm at 80° SZA is 0.35 (as written on page 9, line 27) which induces a diurnal variation 2% for each instrument (the azimuth error is 5%, which has been corrected in the manuscript). This uncertainty is included in the grey bands indicating the total measurement uncertainty in Fig. 8 (as stated on page14, lines 22-24).

Page 9, second paragraph. The aerosol optical properties are given by the rural type aerosol according to Shettle (1989). This has been stated on page 6, lines 18-20: 'Aerosols properties are specified according to (Shettle, 1989), rural type, (with the extinction scaled to the measured AOD).'

Page 9, line 21. With an optical depth of 1, the cloud is indeed not particularly thin, and the word *thin* has been omitted (also in the abstract).

Page 10, third paragraph. The issue we discuss here is that in the extreme regime of large SZA and long wavelengths the effective albedo can exceed one. This is not unphysical *per se*, it just means that a surface with a sharply spiked BRDF can have a higher albedo *relative* to a Lambertian surface (which we understand as an atmospheric effect, a longer path through the atmosphere for the photons reflected with high zenith angle results in an increased chance of backscattering). So it all makes sense (there is no 'bias'), just that the use of the concept of the effective albedo becomes unphysical in that regime, and one must always bear in mind the definition and the meaning of the effective albedo.

Page 11, line 6. Regarding the effect of sastrugies, the paragraph has been rewritten, in order to improve the justification of neglecting the effect. An additional relevant reference has been included (Warren et al., 1998) and the last sentence of the paragraph now specifies a quantitative bound:

This geometric effect on snow albedo is difficult to model realistically but is only relevant for long wavelengths and vanishes for diffuse illumination. So global irradiances considered here are not affected significantly (<1%).

Page 12, section 5. The effect of atmospheric profiles at high SZA has been investigated in some detail. Using the pseudo-spherical 1D RT solver, we compared global irradiances at 80° SZA for different aerosol and atmospheric profiles. Homogeneously distributing all aerosols (with Angstrom α=1.77 and β=0.0134) up to a height of 1 km or 10 km made a difference of only 0.6% at 350 nm and less at 500 nm. Comparing the AFGL subarctic summer and winter atmospheric profiles, we found that the global irradiance is changed by a maximum of 0.6% at 500 nm. These are rather extreme assumptions and the uncertainties due to the profiles are expected to be much less in reality and can safely be neglected. We added a corresponding sentence in the relevant paragraph:

The influence of aerosol and atmospheric vertical profiles on the global irradiance has been investigated and was found to be negligible (smaller than 0.6% at 80° SZA).

Page 13, first paragraph. The suggested algorithm indeed claims the smallest uncertainty and agrees to within 0.01° with the one used in our work. The paragraph has been rewritten accordingly with the reference (Reda and Andreas, 2004) included:

First of all, great care must be taken in the calculation of the SZA. An SZA error of 0.1° (e.g. when neglecting refraction of the atmosphere) results in errors of up to 3% in the global irradiance at 500 nm and 80° SZA and 1.5% at 70° SZA. Comparing different available SZA algorithms (Duffett-Smith and Zwart, 2011, Blanco-Muriel et al., 2001, Spencer, 1971,), we found differences of up to 0.3°. The algorithm of Reda and Andreas (2004) claims an uncertainty of below 0.001° and agrees within 0.01° with the one used in this study (Duffett-Smith and Zwart, 2011) which includes the refraction of a standard atmosphere.

Page 14, line 10. The word *tropospheric* has been added.

Page 14, line 25. The term *uncertainty* is used now. The significance is specified in the sentence:

All uncertainties are independent, added quadratically and estimate the 1σ standard deviation.

Page 14, line 32. *In the figure* has been added.

Page 16, line 28, line 12. 4% in both instances.

Page 19, line 24. We had stated before in section 5.1 (page 16, line 28) that a tilt of 1° would be still be reasonable and would reproduce the magnitude of the observed hysteresis. We have also added that a tilt of 1° would, to a good approximation, have double the effect.

As reasoned above, we would prefer to keep the *tilt* as a scenario of explaining the discrepancies between the standard scenario and the observations.

Table 1. Definitions have been added: ρ_0 is the albedo parameter in the snow RPV model, α and β are the aerosol Ångstrom parameters.

Figure 4. Corrected

Figure 8a/b. We have discussed all factors (scenarios) individually to show each influence, in section 5.1 (page 16, line 8) and 5.2 (page 19, line 3) we did state that a combination of the scenarios yield the best result. We have also added that the effects can be added linearly to good approximation.

Response to Referee #2

Another reference for the description of instruments has been added:

Kouremeti, N., Bais, A., Kazadzis, S., Blumthaler, M., and Schmitt, R.: Charge-coupled device spectrograph for direct solar irradiance and sky radiance measurements, Appl. Optics, 47, 1594–1607, 2008.

The input optics were not aligned in any specific direction, so the azimuth error cannot be corrected in retrospect but is treated as a measurement uncertainty.

We considered the effect of sastrugies again in more detail (see also response to reviewer #1) and found that the effect is smaller than 1% and is insignificant here.

Page 17, lines 22-27. We agree and changed the sentence accordingly

This asymmetry is qualitatively reproduced in the 'standard' model scenario but with a much smaller magnitude.

The technical corrections have been implemented.

Editors comments

The work is very interesting worth published in ACP. The reviewers are raising a number of minor but very interesting questions concerning the work. I think that the answers to the questions raised especially by the reviewer #2 are very short. More specific:

a) The use of three diodearray systems are still not described with the Kouremeti et al. publication. I would suggest to dedicate a small paragraph in order to describe main instrument features and point also differences in the instrument characteristics, calibration and characterization procedures that can be linked with the discussion on the uncertainties of the actual experiment results, described later in the manuscript.

b) The points raised about sastrugis and diurnal effects are not followed up, apart from a statement that this has been reinvestigated and found insignificant in the UV.

My view is that you have to explain a bit more what was the work that have been done in order to end up with this statement. The answer to the reviewer #1 (and #2) concerning the sasturgis effect is quite short and leaves some open questions.

c) Finally, concerning the azimuth error. The reviewer suggest that as the input optics are similar, they either have the same azimuth (angle) response so they could be positioned in the same way (direction) in order to minimize the effect, or they have different directional azimuth responses so the question raised is: what is the error introduced and how it is linked with the results presented. So I think some more discussion is needed in the new manuscript and also to the reviewer #2 response for this matter.

Response to the Editors comments

a) The DA systems are now described in detail in a complete new paragraph:

"The DA spectroradiometer at station west (Ocean Optics, USB4000) has a 16 bit dynamic range and 3648 pixels over the range 180 nm – 890 nm and an average slit width of 1.7 nm full width at half maximum (FMHM) (Kreuter and Blumthaler, 2009). The DA spectroradiometer at station center (Zeiss, MCS-CCD) has a 16 bit dynamic range and 1044 pixels over the range 310 nm – 1000 nm and an average slit width of 2 nm FWHM (Kouremeti et al., 2008). The DA spectroradiometer at station east (Ocean Optics, S2000) has a 12 bit dynamic range and 2048 pixels over the range 190 nm – 850 nm and an average slit width of 1.2 nm FWHM. All instruments were temperature-controlled in a weatherproof housing and fitted with the same type of cosine-weighting diffusers as input optics."

The calibration of each instrument (and intercomparison), the radiometric stability, the correction of stray light, the homogenization of slit widths, and the correction and estimation of the cosine error, has been in detail explained in the following section:

"All DA spectroradiometers were radiometrically calibrated with the same calibration lamp as the absolute reference standard. While co-located at Ny Ålesund, before and after field deployment, all instruments were intercompared for several days under various sky conditions. Regular calibrations were also performed for each instrument in the field, to monitor their stability. The relative radiometric stability of each instrument was better than 1% over the whole day.

Data post processing of the DA measurements include the following steps. After dark current subtraction, the spectra are stray light corrected. The spectra from the DA at station west were corrected with a matrix method (Kreuter and Blumthaler, 2009). Spectra from stations east and center were corrected with a simplified technique using modeled spectra to estimate the stray light. We estimate an uncertainty due to a residual stray light error at 340 nm of <2% at 80° SZA.

Since the different instruments have slightly different slit widths, all spectra are deconvolved and convolved with a triangular slit function of a 1 nm width (Slaper et al., 1995). Finally, the spectra are corrected for deviations from the ideal cosine response of the input optics. For each instrument, the measured cosine response functions, measured carefully in laboratory before and after the campaign, were applied."

b) We have now laid out in more detail our investigation of a possible effect of sastrugis by modifying and extending the relevant paragraph:

"Diurnal variation of snow albedo can also be affected geometrically by anisotropic snow surface structures, e.g. so-called sastrugis, formed by wind erosion (e.g. Warren et al., 1998). Sastrugis are common around Ny Ålesund roughly in east-west direction (König-Langlo and Herber, 2006), and have been noted to some extent during our campaign as well. Wang and Zender (2011) have measured the broadband shortwave albedo in Ny Ålesund for clear sky days in April 2003-2008 and associate the observed symmetric diurnal variation of snow albedo (the minimum at local solar noon is about 0.12 lower than in the morning or afternoon) with sastrugis. The magnitude of this variation, however, is comparable to the SZA dependence of the effective albedo at 500 nm from the RPV snow model (Fig. 4). Also, Carroll and Fitch (1981) have reported shortwave albedo measurements from Antarctica and presented a parametric albedo model which exhibits a diurnal albedo variation of 0.15. They infer that the sastrugies, which were prominent in the measurement area, may only affect the albedo by a maximum of 4%. Furthermore, this geometric effect is only relevant for direct illumination, i.e. in our case for 500 nm, where on the other hand the albedo amplification factor (see section 5.1) is so low that the effect on the global irradiance is not significant (<1%)."

c) Regarding the azimuth error, we have worked on the corresponding paragraph which now should unambiguously explain how the azimuth error is estimated:

"The azimuth error of the global input optics cannot be corrected in retrospect (since the optics were not oriented in a specific direction) and is about 5% at 500 nm for direct solar radiation. By considering the direct contribution to the global irradiance (given below in section 3.2), the expected maximum uncertainty for each instrument is 1.5% and 1% for the global irradiance at 70° and 80° SZA, respectively, for 500 nm and <1% for 340 nm."