Atmos. Chem. Phys. Discuss., 14, C126–C130, 2014
www.atmos-chem-phys-discuss.net/14/C126/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Solar irradiance in the heterogeneous albedo environment of the Arctic coast: measurements and a 3-D-model study" *by* A. Kreuter et al.

Anonymous Referee #1

Received and published: 17 February 2014

The manuscript presents a unique study on validating 1D and 3D radiative transfer modelling with spectral solar irradiance measurements in the challenging Arctic environment with heterogeneous surface properties, high solar zenith angle, and instrument uncertainties.

The measurements were performed in 2009 during a field campaign in Ny Alesund, and the analysis of the measurements has obviously been an ongoing activity since then. The thought processes and insights developed during the analysis are reflected in the structure of the manuscript, which resembles more a report than a final manuscript. As this manuscript has the potential of becoming widely cited in the community, and

considering the considerable amount of work that was put into the field campaign and in the subsequent analysis, I suggest that the authors should invest a little bit more of their time in working out the conclusions that can be drawn from this study. Generally speaking, I was left with the following questions after reading the manuscript:

1)As a succinct statement, was the 1D and 3D modelling conclusive with respect to the observations, or do the 3D (and 1D) modelling miss some crucial aspect in the radiative transfer? e.g. do the discrepancies between model and measurements only result from a) measurement deficiencies, and b) missing input parameters to setup the model or is there a fundamental problem in the RT transfer?

2)Obviously, a lot has been learned from this study, and as stated in the last sentence of the manuscript, a successive measurement campaign is desirable. However it should be also clearly stated which targets such a successive campaign should have; What can be improved in terms of solar measurements and collection of ancillary information to better constrain the modelling? Which part of the models need to be improved?

3)The challenging part in modelling global solar irradiance is the diffuse component, while the direct irradiance could be used to better constrain the model atmosphere. Knowing the co-authors and their equipment, I am convinced that direct solar spectra are available from this field campaign and I wonder why they were not used here.

As mentioned previously, I would like to see the manuscript structured around answering the above questions, instead of following the thought processes of the authors through the analysis: a) the time shift between stations, the ice drift and the diurnal aod variation should be discussed initially and then implemented in the data analysis. This would reduce the scenarios in Table 1 considerably.

b) The Detector Tilt can either be treated as uncertainty, or as an error to be corrected. It should be decided by the authors, not left as a scenario.

General comment concerning Error and Uncertainty:

A (known) systematic error is corrected, and what remains is the uncertainty of the correction. An unknown error is not an error, but an uncertainty (http://www.bipm.org/en/publications/guides/gum.html) . Please rephrase the text accordingly.

Specific comments:

Please add definitions for local albedo, effective albedo, lambertian albedo, and integral albedo somewhere in the beginning of the document, for those readers (like me) that are not that familiar with these terms.

page 2, line 13, a very nice study on effective albedo in the arctic is the one by Bernhard et al, 2006, 2006JD007865.

page 3, line 18-19: Actually, the measurements were in agreement with the model , only the statement of the domain of influence being 2.5 km around the measurement location made by Smolskaia et al was misleading.

page 4, line 21: Were the spectra integrated for 100s, or were the measurements averaged over that time period?

page 5, line 22-23: The right term to use here is deconvolved, and convolved.

page 5, line 28: Assuming that at 500 nm most of the global irradiance is direct (for example 90%), then an azimuth error of 10% would produce a 9% diurnal variation, not 3.5%! Since the orientation of the diffusers is not known, this effect introduces an uncertainty in the measurements of +-4.5% at 500 nm.

page 9, second par: What were the choices for the SSA and g, and how important might they be for the modelling?

page 9, line 21: a thin cloud will usually have an optical depth of around 0.2 (0.15 and below is termed sub-visible clouds). An optical depth of 1 is actually quite a thick cloud, so I question the term “plausible” in this context.

page 10, third par: I do not follow the argument made by the authors that the unphysical result of an albedo larger than 1 does not put into the question the whole 3D modelling, especially since the sun glint has such a strong effect for the analysis. Why was it not possible to correct the 3 D model, if the effect is understood? From figure 6 the effect of sun glint is clearly gradual, and extends to low SZA and short wavelengths, which might therefore bias the results also in these model situations.

page 11, line 6: Can the effect be quantified, or some bounds stated?

page 12, Section 5: As stated by the authors RT modelling at high SZA poses several challenges: Apart from the ones mentioned, what about the effect of atmospheric profiles, especially of aerosols on the diurnal variation?

page 13, first par: There are now algorithms available claiming SZA calculation precision to several arc-seconds. One such algorithm can be found at http://rredc.nrel.gov/solar/codesandalgorithms/spa/ and claims to be accurate to +/- 0.0003°.

page 14, line 10: The effect of ozone on the back-scattered UV radiation is due to tropospheric ozone.

page 14, line 25: Here and other places, please use the term uncertainty, and specify the confidence interval (e.g. 66% for one standard deviation and normal distribution, or 95%, for an expanded uncertainty)

page 14, line 32: Please add “in the figure”, after anti-clockwise direction ...

page 16 line 28 and page 18, line 12: The effect of the tilt is estimated at 3% and ,later at 4%. Please homogenise.

page 19, line 24: Here a tilt of 1° is mentioned, while previously a tilt of 0.5° was discussed? For a 1° tilt, the effect would be 6%?? I suggest to take the tilt in account for Station West, and not as a scenario (see previous general comment). An alternative would be to keep it as an uncertainty of the measurements, but that would increase the

grey band in Figure 8 considerably.

Table 1: Please add a definition for rho0, alpha and beta in the caption.

Figure 4: correct left panels with right panels

Same for Figure 6

Figure 8: The overall discrepancies observed between the model and the measurements are large and could not be explained. In Figure 8a, there is a general offset of 5% between the model and the measurements. Maybe the relative uncertainties of the two instruments was underestimated?

Figure 8b: The significant diurnal variation has not been captured by any of the model scenarios. Is it possible to combine several model scenarios (tilt, aod, cloud, ice drift, topography), in a way and using parameters such that the diurnal variation can be modelled?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 3499, 2014.