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Atmospheric Measurement Techniques Discussions

Interactive comment on "Atmospheric effect on the ground-based measurements of broadband surface albedo" *by* T. Manninen et al.

Anonymous Referee #1

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1 General remarks

The manuscript provides a parametrization of the relation between measured surface albedo and black-sky surface albedo. The parametrization depends on aerosol optical thickness (AOD), solar zenith angle and the incoming direct and diffuse radiation and can be used for an atmospheric correction of surface albedo measurements. The parametrization was developed with radiative transfer simulations by using data bases of black-sky surface albedo and AOD. The application of the atmospheric correction to surface albedo measurements obtained from a BSRN station in Cabau showed differences of about 5% between measured and black-sky albedo while the simulations showed maximum effects of up to 20%.

The idea to have an simple and robust parametrization for the atmospheric correction of surface albedo measurements is highly welcome and worth to be published. However, the approach presented by the authors suffers of several systematic and methodical errors which have to be reassessed in detail before publishing the manuscript. The accuracy of the atmospheric correction presented in the manuscript is quite limited, which might be caused by some of the systematic errors. If the accuracy can not be improved, I doubt that this method is sufficient to replace an ordinary atmospheric correction which fits the simulation to the measurements.

Below, I compiled a list of comments which have to be considered in a revised version of the paper. When writing the comments I sometimes did not consider, which in direction the revised paper might be changed. This may result in some contradictory statements. I am sure the authors will know how to weight in such cases.

2 Major comments

Normalization of surface albedo: The authors normalized the measured surface albedo to a solar zenith angle of 60° using equation 1. This equation only hold for the black-sky albedo as stated in the introduction by the authors itself. The measured surface albedo is the blue sky albedo and affected by the illumination from both direct and diffuse solar radiation. The partition between direct and diffuse radiation strongly depends on solar zenith angle itself. This means that the surface albedo changes with solar zenith angle for two reasons. a) the black-sky albedo changes, b) the diffuse fraction changes. This normalization may hide some of the atmospheric effects and may explain some of the deviations between corrected albedo and black-sky albedo.

Simulations: The authors used the radiative transfer model SPCTRAL2 to calculate the diffuse irradiance F_{diff}. The direct irradiance F_{dir} is calculated by the law of Lamber-Beer. I do not understand why both F_{diff} and F_{dir} are calculated with different methods.

SPCTRAL2 also provides F_{dir}. So there is no reason to do it yourself. Further the calculation of F_{dir} is fundamentally wrong. In Eq. 3 only the aerosol optical thickness is used while the atmosphere consists also of molecules. The Rayleigh optical thickness has to be included here as well. See the description of SPCTRAL2 (Bird and Riordan, 1986) or just use the results of the model.

AOD: For the simulations a range of suitable AOD is derived from AERONET measurements at Cabau. From this data set, the parametrization is derived. What about AOD values which are not covered in the 7 month period? I suggest not to focus on the measured AOD in this case. It would be much more appropriate to use a distinct grid of AOD for the simulations. Vary AOD and the Angström parameter systematically within a certain range and run the model. The results can be interpreted much better than the data shown in the manuscript. E.g. in Figure 4 not all categories of solar zenith angles have the same range of AOD. How to interpret the different length of the horizontal bars, if the AOD range is different for each solar zenith angle? How the parametrization will work for AOD values which are not covered by the simulation?

To characterize the spectral behavior of AOD, the Angström equation is often used as mentioned by the authors in section 3. Angström exponents have been calculated but never be shown or used. In order to obtain a parametrization which has a more general character, I suggest to express AOD by the Angström exponent and the AOD at the reference wavelength throughout the manuscript.

Parametrization: The form of the parametrization does not suit the intention of the study providing a simple parametrization from which surface measurements can be corrected without big effort. There are redundant parameters in the equation. I do not understand, why the diffuse and direct irradiance are used as parameter. Both are calculated from the SPCTRAL2 model, as I understand, and they are functions of solar zenith angle and AOD. This means, a parametrization on solar zenith angle, surface albedo and AOD would be sufficient, F_{diff} and F_{dir} have not to be calculated additionally.

Alternatively, for the case, that F_{diff} and F_{dir} are measured at a radiation station, but AOD is not, I suggest to derive a parametrization on measured albedo, F_{diff} and F_{dir}, without AOD. This would be a simple and helpful parametrization.

Instead of the AOD at two different wavelength, I suggest to use the parameters of the Angström equation.

Use of BSRN-Data: Further I do not understand why no single measurement of F_{dir} and F_{diff} is included in the study. The data which was used in the study comes from a BSRN station where F_{dir} and F_{diff} are measured. At least show that your model results agree with F_{dir} and F_{diff} from the BSRN station. I know the comparison may lack due to strong forward-scattering but with regard to the parametrization it is worth to include measured F_{dir} and F_{diff}.

Atmospheric correction: The atmospheric correction using the proposed parametrization does not obtain good results. As shown in Figure 7, the difference between measured and black-sky albedo is reduced only by about 50 %. This is surprising as in Figure 7 simulations have been used as input for the atmospheric correction. As the parametrization is based on the same simulations, I would assume a perfect agreement between corrected and black-sky albedo if the parametrization is good. This seams to be not the case. Reasons might be diverse. One might be the above mentioned errors in the method itself.

To show that the parametrization is a useful alternative to an complete atmospheric correction, both methods have to be compared in the study. I suggest to apply an atmospheric correction using model simulations by fitting the model to the measured parameters (uncorrected albedo and AOD). I suppose for the problem presented here using irradiance only, such an atmospheric corrections is not time consuming. The results will show, if the method using the parametrization equation is needed at all.

Satellite Data: The authors motivate their work by claiming that their method will help to validate satellite surface albedo estimates. Why this comparison is not done?

Wording: The nomenclature of the different measured and simulated albedos is totally confusing. Some examples: "simulated pyranometer measurement estimate of a surface albedo", "regression based atmospherically corrected value" "simulated pyranometer measured broadband surface albedo". The naming of the different albedo has to be consistent otherwise the reader can not follow. The best way is to define the albedo once and than use the symbol of the quantity only.

Further, I strongly recommend the paper to be proofread by a native English speaker for grammar and punctation.

Figures: The labeling of most figures is to small. Different data points are not capable of being differentiated in some figures.

3 Minor comments

P386, 3: Specify in which way the measurements are affected by the atmospheric conditions. What do you mean with "atmospheric conditions". Mention that you propose an atmospheric correction.

P387, 10: Ground based measurement with goniometer using an artificial radiation source can be used to derived BRDF and thus the black sky albedo. There are several publications on such kind of measurement, e.g. "Dumont, M., Brissaud, O., Picard, G., Schmitt, B., Gallet, J. C., and Arnaud, Y.: High-accuracy measurements of snow Bidirectional Reflectance Distribution Function at visible and NIR wavelengths - comparison with modelling results, Atmos. Chem. Phys., 10, 2507–2520, 2010.", "von Schoenermark, M., Geiger, B., and Roeser, H.-P., eds.: Reflection Properties of Vegetation and SoilWith a BRDF-Data base, vol. 1, Wissenschaft und Technik Verlag, 2004."

P387, 12: Satellites do not directly measure blue-sky albedo. Only radiances are

measured which are used in atmospheric correction schemes to derive the surface black sky albedo, blue sky albedo and white sky albedo.

P387, 20: I do not understand. What changes for the analysis of the satellite measurements? Here you still have to consider both effects.

P387, 28: Change "radiation flux density" into "radiant flux density"

P388, 9: There must be a plenty of studies investigating the diffuse and direct fraction of solar radiation. This is a basic measurement at any meteorological site since many years and also used for retrievals of atmospheric properties. A quick web search lead me to the following publications, randomly chosen: "Continental aerosol properties inferred from measurements of direct and diffuse solar irradiance, Marsden et al, JGR, 2005", "The diffuse-to-global and diffuse-to-direct-beam spectral irradiance ratios as turbidity indexes in an urban environment,Kaskaoutis and Kambezidis, JASTP, 2009", "Coupling diffuse sky radiation and surface albedo, Pinty et al, JAS, 2005".

P388, 11: "albedo values .... contain contributions from the atmosphere...". The wording is physically incorrect. Also the following reason is not correct. Not only the spectral shape of the downward irradiance is modified by the atmosphere. The second problem is, that the surface albedo is defined for incoming direct solar radiation only, but in nature you always have a diffuse component. As the albedo depends on the direction of the incoming radiation, a different diffuse fraction will lead to different albedos.

P388, 12: "surface irradiance spectra" change into "surface downward spectral irradiance".

P388, 18: Are the 20% only from the spectral change du to scattering and absorption? Or do they also include the effect of the diffuse incoming radiation component? It might be worth to try to separate both effects and quantify which one is more important.

P389, 20: Which type of pyranometers have been uses? Are there any references for the measurement uncertainties.

P389, 25: Where does $f = 0.22$ comes from? Is it a literature value? If yes, I strongly suggest to derive f from the measurements itself. Later (P396, 14) it was stated that a wrong f might be a reason for deviations.

P390, 7: "atmospheric effect estimation" change into "estimation of atmospheric effect on measured..."

P390, 11: What do you mean with "in practice"? Simulations? Pyranometers cover 305–2800 nm in practice...

P390, 11: 300 should be 305.

P390, 12: 2500 nm. I suppose that is the spectral range of the simulations. Why do you not simulate the same range as measured by the pyranometer?

P390, 14: Instead of R_{SW} and I_{SW} I suggest to use $F^{\downarrow}$ and $F^{\uparrow}$. I is usually used for radiances and R for the reflectivity. F is common for the irradiance or radiant flux density. The index $_{\text{SW}}$ can be omitted as you only deal with solar radiation.

P390, 14: How the reflectance is defined?

P390, Eq. 2: Especially the last part of the equation is uncorrect. This equation does not follow the definition given by **??**. 1) α_{bb} is a function of θ_{z} and ϕ_{z}, and so is R_{SW} and I_{SW}.

2) Which viewing direction θ and ϕ is used for BRDF? You somehow have to integrate for the entire hemisphere $\theta = 0, ..., \pi$ and $\phi = 0, ..., 2\pi$ to obtain irradiance.

3) You have to define $r(\lambda)$

P390, 19: "sun" change into "solar" zenith angle.

P390, 23: "were" change into "is"

P390, 24: The extraterrestrial solar irradiance at TOA is usually defined for perpendicular incident. Then you have to multiply with $\cos(\theta_{\text{z}})$ to derive the downward irradiance.

Is this done? If yes, adapt equation 2 accordingly.

P391, 3: What means "noisy"? The uncertainty of the albedo is just higher because the relative error of the two values used to compute albedo is higher.

P391, 7: What is "irradiation"? Do you mean radiant flux density=irradiance?

P391, 10: θ_{z} was already defined. No need to repeat "solar zenith angle".

P391, 11: "depends" change into "can be parameterized". Often the spectral dependence of AOD does not perfectly follow the Angström equation.

P391, 13: τ_{Aer} or τ_{a}? Use only one.

P391, 15: What do you mean with "example set of AOD values". I do not understand the structure of your approach. Did you use the Angström parameter anywhere? In Equation 6 you use two separate wavelength and not Angström.

P391, 23: The restriction to clear sky cases should be mentioned earlier. e.g. P388, 13.

P391, 24: This argument does not hold. 1) For your Cabau case you have AOD measurements. 2) Each satellite validation site should have a sun photometer to measure AOD. 3) in Eq. 6, which is your atmospheric correction, you have to insert AOD values at two wavelength. Here you need AOD measurements as well.

P392, 2: As I understand, you use Eq. 2 (right part) to calculate the surface albedo. Downward irradiance as sum of diffuse and direct components are calculated from SPCTRAL2 and from Eq. 3, respectively. (which is already wrong as mentioned above). How do you calculate the reflected irradiance R_{sw}? From the text it looks as if you do it by using the right part of Eq. 2. If so, that would be wrong as you have to consider multiple scattering and diffuse radiation for the reflected radiation as well. Why do you not use the simulations by SPCTRAL2? SPCTRAL2 will provide upward and downward irradiance. And what surface albedo is used in SPECTRAL2? Or do line 16–26 describe how the input surface albedo is prepared for SPECTRAL2?

P392, 10: Be carefully. For high solar zenith angles the ozone absorption is visible in the spectral irradiance. Huggins and Chappuis bands. Also water vapor has significant absorption bands in the solar spectral range. That there is no effect changing the concentration of ozone and water vapor has to be verified.

P392, 23: I would assume too, that the BRDF is not crucial here. But you have to show that or give a reference. Is there any difference in the results when you change the assumed BRDF model?

P392, 26: Change "infrared" into "near-infrared".

P393, 5: I still do not understand the calculations reading the description in the manuscript. What is the "simulated black-sky albedo". Which equation or model was used? The black-sky albedo should be input to SPECTRAL2. Or did you use Eq. 2 also to calculate black-sky albedo just with different I_{sw}?

P393, 9: When I understand right, you do not correct the measurements for imperfect cosine response. You try to adapt the simulations to the measurements by "uncorrecting" the simulations. In this case it is not a "cosine correction". The equation has to be inverted.

P393, Eq 5: The equation does not look right. **1)** I suppose the "1+..." must be a "1-...". Still the equation is confusing. I would assume the following equation for a correction of pyranometer measurements.

$$I_{\mathrm{c}} = f_{\mathrm{dir}} \cdot I \cdot C_{\mathrm{dir}} + (1 - f_{\mathrm{dir}}) \cdot I \cdot C_{\mathrm{diff}}$$

with $f_{\mathrm{dir}} = I_{\mathrm{dir}}/(I_{\mathrm{dir}} + I_{\mathrm{diff}})$

2) Further the correction coefficients C_{bTP} are a function of solar zenith angle and can only be applied to the direct solar radiation. For the diffuse radiation a diffuse correction

coefficient has to be calculated by integrating the C_{bTP} over all solar zenith angles. See e.g. Feister et al. 2007 who assumed isotropic diffuse radiation.(Feister, U., R. Grewe, and K. Gericke, A method for correction of cosine errors in measurements of spectral UV irradiance, Sol. Energy, 60, 313-332, 1997)

3) In the equation 1367 seams to be the solar constant. You can not use this number because the solar constant is not constant at all. It varies with day of year due to the different Sun-Earth distance up to $100\,\mathrm{W\,m^{-2}}$.

P394, 5: You can not fit simulations to the atmosphere. You mean "performing an atmospheric correction, fitting the black sky surface albedo so that measured albedo is represented by the simulations with given AOD...".

P394, 5: Why no good results are expected for large AOD? Theoretically an atmospheric correction can be applied for all AOD. Sure, measurements uncertainties limit the correction. But the same holds for you method. Also specify what "good results" means and how large the AOD values have to be.

P394, 7: Why using gras surfaces only should improve the results? Explain that. How do you quantify that the results are improved. The main problem rises from the variability of the AOD and this does not change much between Figure 4a. and 4b.

P394, 13: Discuss why this behavior is observed? Is there still a dependence on AOD due to increasing scattering with increasing solar zenith angle?

P394, 14: Start a new paragraph with "In the Cabau...".

P394, 18: What "spectra" do you mean have been studied?

P394, 19: "Reflectance" should be "albedo"?

P394, 20: This sentence is difficult to understand. Rewrite, split or rearrange.

P394, 22: Indicate where the reader can see the snow cases in the figure.

P394, 25: I do not understand why there is a need to investigate the effect of cosine correction. The correction should be applied anyway to all measurements. Assuming, that the correction works perfectly your measurements will provide values similar to the simulations. You try to adapt your simulations to uncorrected measurements, which makes no sense, when you anyway intend to correct your measurements for a non-ideal cosine response. Again I have to ask, if you applied a cosine correction to the ideal simulations, or did you reversely calculate uncorrected measurements from the ideal simulations? That is not clear from you explanations.

P394, 26: Your wording is wrong: The correction does not give overestimated values. The uncorrected measurements overestimate the albedo.

P394, 28: The effect of the cosine correction is not shown in Figure 5. Specify what "a few percents" are!

P395, 11: Can you show in an additional Figure, that the ratio $\hat{\alpha}_{0\text{bb}}/\alpha_{\text{bb}}$ depends on τ, solar zenith angle and I_{diff}? This may help to follow your argumentation.

P395, 13: "AOD" use the symbol τ which you have introduced for AOD.

P395, 14: AOD=0 does not mean, that $I_{\text{diff}} = 0$! There are still a lot of molecules in the atmosphere which scatter radiation and contribute to the diffuse radiation.

P395, 18: Specify what is "quite small" I roughly calculated values up to 25% which is quite a lot!

P395, 20: Does "atmospherically corrected albedo estimate" mean the results from the regression? If yes, the regression is not good, because there are still large deviations from the ideal black-sky albedo.

P396, 7: Add a new section 5. Here you start to use the real measurements.

P396, 9: Change "the measured incoming... zenith angel values" with "the measured albedo".

P396, 11: Wording: The atmosphere can not increase the black-sky albedo.

P396, 21: I_{diff} and I_{dir} are needed as well.

P397, 2: Change " estimating the size" into "estimate the magnitude".

P397, 4: Change "could then be used in" to "could be used instead of sun photometer measurements to".

P397, 7: "atmosphere": you did not consider the atmosphere only AOD.

P397, 10: I do not understand why it is worth mentioning that the study do not involves any satellite date. If you want to validate satellite data with any method, these measurements should per default do not rely on any satellite product.

P397, 13: You did not show any study on satellite albedo products. Do this or delete this statement or explain exactly how this should work.

P397, 16: There are more conclusions in the text. Expand this section!

P401, Table 1: c_3 and c_4 are not dimensionless. Give the units.

P402, Figure 1: Label with (a), (b),... and also label with "snow/water", "vegetation",...

P403, Figure 2: Is it necessary to show this plot? What do we learn from the plot? AOD is per definition independent on solar zenith angle. You can remove the color code. And to present the relation between AOD of two wavelength it would be better to present Angström parameter.

P404, Figure 3: Is there a dependence of the albedo on SZA? This might be caused by increased diffuse radiation if AOD is constant but solar zenith angle is increasing.

P405, Figure 4: This is a bad illustration. single data points are not distinguishable. Include a 1:1 line! Why there are horizontal bars? Is this due to the variation of AOD? Where is AOD min and where max? Further it looks as if the AOD range differs for different zenith angles. I suggest to use artificial AOD ranges. This will give the opportunity to study the relation between AOD and atmosphere effect more systematically. With the 2226 cases you are fixed to the observed cases, which probably do not cover all possible situations

P406, Figure 5: "calculated black-sky albedo" this is not what you displayed here. Deviations in % are shown. Where is AOD min and max? To make interpretation easier, I suggest to change the figure into an 1:1 plot similar to Figure 4.

P407, Figure 6: Again a similar plot to Figure 4 showing 1:1 relationship will be better to interpret.

P409, Figure 8: Show a 1:1 plot in addition.

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