Manninen et al., " Atmospheric effect on the ground-based measurements of broadband surface albedo"

Answers to the reviewer

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.
There were no systematical errors in the simulations. This is described in more detail in the answers to the major comments. The simulations will, for comparison, be carried out also in an alternative way suggested by the reviewer. The intention in developing this method was not to replace an existing atmospheric correction method, but to offer a robust way to estimate the magnitude of the atmospheric effect on measured broadband blue-sky albedo data in cases when too little is known about the atmospheric characteristics to enable using more refined methods. The need is obvious when using old ground based albedo data sets, which are not accompanied with accurate atmospheric data. The possibility to obtain robust but realistic error bars for the ground based albedo measurements using, for example, climatological aerosol information is considered valuable by the authors.

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.
It is true that the normalization is developed for black-sky values. Yet, it works quite well also for blue-sky data, such as that from Cabauw, where the sun zenith angle dependence practically disappeared when applying the normalization. However, from the theoretical point of view it is better to apply the equation only to black-sky values. Thus the comparison of the Cabauw results with simulations will be changed to not normalized values.

Simulations: The authors used the radiative transfer model SPCTRAL2 to calculate the diffuse irradiance Fdiff . The direct irradiance Fdir is calculated by the law of Lamber-Beer. I do not understand why both Fdiff and Fdir are calculated with different methods. SPCTRAL2 also provides Fdir. So there is no reason to do it yourself. Further the calculation of Fdir 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.
This is a misunderstanding caused by the authors not writing precisely enough. Because the aerosol contribution usually dominates in the broadband case, the authors concentrated on describing that. However, all calculations were carried out using the complete Bird-Riordan formulation also for the direct irradiance (which is needed to calculate the diffuse irradiance) taking into account contributions from the earth-sun distance, Rayleigh scattering, water vapour absorption, ozone absorption and uniformly mixed gas absorption. The authors will edit the manuscript on page 6 to make this point clear. Thanks for the reviewer for pointing this out.

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.
The reason the authors preferred to use real data is as follows. A simple method for taking into account the atmospheric contribution to the radiation is sought for cases when only some of the parameters describing the atmosphere characteristics are available. It is evident, that then the match can't be as good as when using a full RT approach with all required input parameters available. The point was to optimize the regression parameters to the match best the most typical cases. If a complete grid were used for the regression parameter retrieval (with equal weights), the match would certainly cover better the extreme cases one almost never meets, but likewise the match to the most common cases would be poorer. However, it is of interest to compare, how much the regression parameter values would change if a regular grid of aerosol values were used. The authors will test this.

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.
The irradiance values were used as input, because then one has several independently measured parameter values as inputs for the regression. From the theoretical point, naturally, they provide nothing extra, but this is just to benefit from several independent measured values (even redundant), when applying the method to real data.

The diffuse component may be rather small sometimes; therefore its accuracy might not be good enough to encourage using it after all as independent measured data. Thus the authors will check an alternative to apply only total irradiance and AOD values as inputs. One would rather use at least the total measured irradiance, since that is always available in the BSRN data sets. When comparing old satellite measurements and old ground measurements it may often be the case that one has just the global and reflected radiation measurements and the AOD values have to be estimated from climatology. Therefore one would not like to give up the measured irradiance altogether, even if they are redundant from the modeling point of view.

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.
This is a suggestion worthwhile testing, although the authors are somewhat skeptical about using fewer independent measurements. The need for this kind of an approach is, however, evident where no measured aerosol values are available. For that purpose another regression will be derived and the results will be compared to the previous results of the more complex regression.

Use of BSRN-Data: Further I do not understand why no single measurement of F_{diff} and F_{dir} is included in the study. The data which was used in the study comes from a BSRN station where F_{diff} and F_{dir} are measured. At least show that your model results agree with F_{diff} and F_{dir} 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_{diff} and F_{dir}.
As the regression will be changed to apply only the total irradiance, this comment is no more relevant.

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.
As explained before, there was no error in the simulation method.

The reviewer expects a perfect match between the simulated and parameterized albedo. Unfortunately this is not the case, because we are dealing with the integrated broadband albedo, not the spectral one. Therefore the relationship between the albedo and AOD is so complex. For the spectral albedo, the equations by Bird and Riordan could be inverted to derive the black-sky albedo as a function of the blue-sky albedo. (Just by solving a second order equation.) Because the effect of the atmosphere varies drastically from wavelength to wavelength, a simple relationship between the black-sky and blue-sky broadband albedo does not exist.

The reviewer suggests fitting the model to measured uncorrected albedo and AOD. This could be done for comparison.

This method is really not meant to be an alternative for complete atmospheric correction, but a tool to estimate the difference between black-sky and blue-sky albedo values when the atmospheric conditions are poorly known (i.e. simultaneous measurements of AOD etc. and albedo don't exist and in that case one has to apply climatological aerosol values).

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?

The manuscript is already long enough without adding a third data set to describe and analyze. In addition, the satellite data set brings always with it an additional problem related to the heterogeneity of the land cover within a pixel.

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.
The nomenclature will be checked.

Figures: The labeling of most figures is to small. Different data points are not capable of being differentiated in some figures.
It is -evident that all individual points can't be detected, when there are more than 2000 points in one figure.

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.
To the authors' mind it would be preferable to define the atmospheric effect in more detailed way rather in the text part than in the abstract, but according to the suggestion of the reviewer this has now been edited. Yet, the main idea of the paper is not to produce an atmospheric correction method, but a semi-empirical method to estimate error bars for the black-sky estimates of broadband albedo derived from ground based measurements. This may sound a semantic detail, but this is the way the authors aim to use the method. The wording will be edited somewhat.

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."
In principle outdoor goniometer measurements really offer the possibility to estimate the black-sky albedo, although the goniometer actually measures directional reflectances, not the hemispherical albedo. The integration of the reflectance values to the hemispherical albedo has its own problems as well. However, estimation of the albedo requires that the target characteristics and the illumination remain essentially constant during the set of measurements, which usually take at least 15 minutes (Peltoniemi et al., 2010) per one albedo value. From the point of view of satellite product validation this requirement is a major drawback. The text is edited somewhat and a reference to broadband goniometer measurements is added.

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.
The text is edited according to the comment.

P387, 20: I do not understand. What changes for the analysis of the satellite measurements? Here you still have to consider both effects.
It is true that both effects still have to be taken into account, but they can now be separated. This study is aiming at estimating the difference between the black and blue--sky albedo estimates based on the ground measurements. Then it is analyzed, how much the land cover heterogeneity can really cause difference

between the satellite and ground based black-sky albedo estimates. The text will be edited to make this point more clear.

P387, 28: Change "radiation flux density" into "radiant flux density"
Edited as requested.

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".
Naturally there have been irradiance studies long since, just as the reviewer says. The idea was to mention, that the effect of atmosphere on the observed surface albedo has not been studied, because there is normally no need for that. The text is modified.

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.
The authors and the reviewer agree about the physics, so it seems the language was too imprecise to be acceptable. The text has now been edited.

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

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.
Both effects were included in the number given. So far these two effects were not studied separately, but it is possible to check this.

P389, 20: Which type of pyranometers have been uses? Are there any references for the measurement uncertainties.
The pyranometers used are Kipp&Zonen CM22. The text is edited accordingly.

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.
The value used for f is from Briegleb et al.(1986) as Eq. 1.Now the comparison of measured and simulated values will be carried out without any normalization.

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

P390, 11: What do you mean with "in practice"? Simulations? Pyranometers cover 305–2800nm in practice...
The shortwave spectrum of the solar irradiance covers the whole wavelength range up to 4μm, but practically all energy comes within the shortwave range. Therefore it is sensible to limit the simulations in that wavelength range.

P390, 11: 300 should be 305.
The definition of the shortwave range is not identical to the measurement range of pyranometers, therefore this number is different. See next comment.

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?
In literature the shortwave range varies somewhat. The range 0.3μm ... 2.5μm was a common definition and chosen for that reason to simulate the shortwave albedo. The measurement range of pyranometers is not identical to that range, which in principle would provide one error source for estimating shortwave albedo using pyranometer measurements. However, since the amount of the TOA irradiance between 2500 and 2800 nm is less than 1 %, this difference has no real effect. Yet, it is considered better to simulate the shortwave albedo using the defined wavelength range, not a wavelength range that a certain measurement instrument is sensitive to. We are after the shortwave albedo value after all. The difference in the wavelength range is already explained in pages 8-9.

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 sw can be omitted as you only deal with solar radiation.
Unfortunately the notation of radiance related parameters varies in literature. The authors prefer to use the notation of the paper by Bird and Riordan (1986), since the simulations are based on that. Therefore I is used for irradiance. As the notation is given the interested reader should not be too confused.

P390, 14: How the reflectance is defined?
In the previous version of the manuscript the reflectance was a purely material property telling how much of incoming light is reflected back in nadir. Reflectance multiplied by the BRDF then told how large fraction of the radiation coming from a certain direction is reflected to another certain direction. Now the notation has been changed to f_r, which contains both the magnitude and the azimuth angle dependence of the reflected radiation.

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 θ = 0, ..., _ and φ = 0, ..., 2_ to obtain irradiance. 3) You have to define $r(\lambda)$.
The equation will be updated. It is true that the angular dependence of all terms should be shown explicitly, not only that of the BRDF. The given form was a (poor) result in trying to keep the notation simple. In the simulations all angular dependences were naturally included. The text is edited now to make this clear. See the appendix of these answers.

P390, 19: "sun" change into "solar" zenith angle.
Edited as suggested.(Everywhere).

P390, 23: "were" change into "is"
Edited as suggested.

P390, 24: The extraterrestrial solar irradiance at TOA is usually defined for perpendicular incident. Then you have to multiply with cos(_z) to derive the downward irradiance. Is this done? If yes, adapt equation 2 accordingly.
The angular dependences of the radiation were omitted from Eq.2 for simplicity, but it is true that they should be indicated as it is shown for the BRDF. The cosine needed for tilted surface is naturally taken into account when using the model by Bird and Riordan.

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.
When the sun is close to the horizon, the 3D characteristics of the land cover, such as forest, start to show up. Every now and then the sun is seen directly between the trees (or houses, hills etc.) and at other times it is behind them so that the amount of direct solar irradiance varies drastically.

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

Yes, edited.(Everywhere)

P391, 10: _z was already defined. No need to repeat "solar zenith angle".
Edited as requested.

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

P391, 13: _Aer or _a? Use only one.
Yes, absolutely so. Obviously notation of a previous version has remained in some places. Sorry about that. Edited as requested.

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.
The sentence was ill written. Should be: An example set of Ångström exponents was derived for the simulations by regression of Eq. (4) to the measured AOD values at wavelengths of 440 nm, 675 nm, 870 nm and 1020 nm.
P391, 23: The restriction to clear sky cases should be mentioned earlier. e.g. P388, 13.
Edited as requested.

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.
Yes, Cabauw is a good validation site because we have both AOD and albedo measurements. However, the spatial distribution of albedo validation sites would be seriously limited, if simultaneous AOD measurements were a mandatory requirement. The exact level of the albedo is not as crucial for a long time series data set as the seasonal variation characteristics and long term stability. Climatological information about AOD characteristics and real measured albedo values are still valuable information for satellite product validation, when continuous AOD measurements are missing.

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 Rsw? 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?
Obviously when trying to avoid repeating too much the text by Bird and Riordan (1986), the authors shortened the description so much, that the baby was thrown away with the washing water. The simulations were carried out using the Bird and Riordan model for all components, direct and diffuse. So this comment is happily enough completely unnecessary. The authors are sorry for not writing clearly enough.

The reflected radiation is determined using the spectra and BRDF and the irradiance. Both the direct and diffuse albedos are determined in red and near infrared channels. The total albedo is obtained from them. This is now added to the text for clarification.

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.

Although they are spectrally important, they don't dominate the broadband irradiance, when restricting the simulations to clear sky cases, which are relevant for satellite comparison. However, sensitivity analysis concerning water and ozone will be carried out.

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?
The simulations will be carried out both with and without BRDF.

P392, 26: Change "infrared" into "near-infrared".
Edited as requested.

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 Isw?
The simulated black-sky albedo was calculated using the Bird and Riordan model. Now the equations used are in the appendix of these answers. The albedo values were derived for various spectra and varying sun zenith angle.

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.
Quite right, the language is not precise. The text is edited.

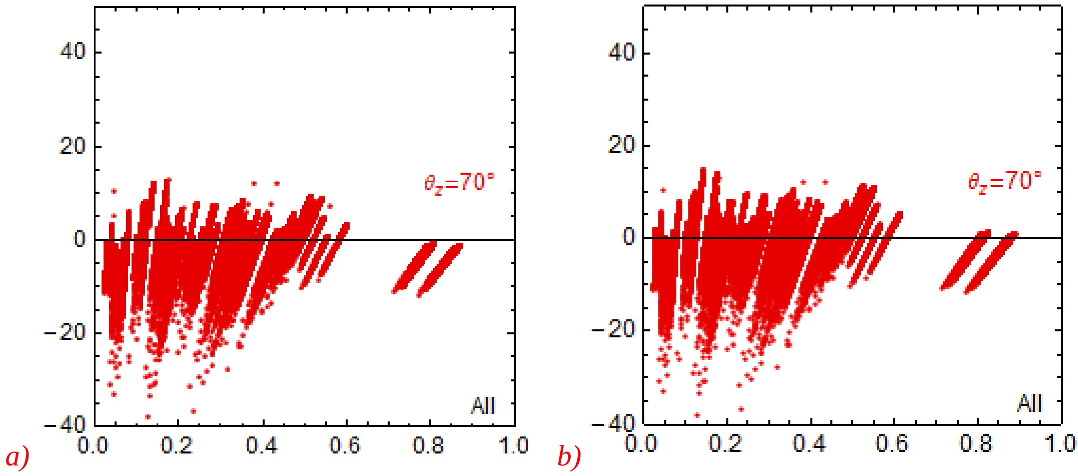
P393, Eq 5: The equation does not look right. **1)** I suppose the "1+..." must be a "1-...". Still the equation is confusing. I would assume the following equation for a correction of pyranometer measurements.
$Ic = fdir \cdot I \cdot Cdir + (1 - fdir) \cdot I \cdot Cdiff$
with $fdir = Idir/(Idir + Idiff)$
2) Further the correction coefficients CbTP 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 CbTP 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 $100 Wm^{-2}$.
1&2) The cosine correction was taken from the paper by Michalsky et al, but there was a typo in the manuscript. The latter coefficient should be C_{dTP}, not C_{bTP}. The simulations were carried out using the right coefficients.
3) As the cosine correction is always individual the authors just demonstrate what its typical size is. Therefore it is OK to use the solar constant without paying attention to the variation of the earth-sun distance (like in the paper by Michalsky et al.).

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...".
The text is edited.

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.
It is rather understandable, that the thicker the atmosphere the challenging will be the atmospheric correction, whatever method will be used. An AOD of the order of 0.3 will be expected to cause problems for the simultaneous surface albedo and AOD determination using RT.

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.
The grass albedo varies in a relatively small range compared to the variation from open water to snow albedo values. Thus one could think that the atmospheric effect might also be less pronounced for grass only. The authors wanted to check this and it turned out that this is not the case. Figures 4a and b are presented to demonstrate the point.

P394, 13: Discuss why this behavior is observed? Is there still a dependence on AOD due to increasing scattering with increasing solar zenith angle?
At large solar zenith angle values the fraction of diffuse irradiance is larger. On the other hand the increasing AOD increases the diffuse irradiance. Therefore the observed total solar irradiance is larger for larger AOD values, which then results in underestimation of the albedo for large solar zenith angles.
P394, 14: Start a new paragraph with "In the Cabau...".
Edited as requested.
P394, 18: What "spectra" do you mean have been studied?
The spectra of Figure 1. The text is edited accordingly.
P394, 19: "Reflectance" should be "albedo"?
No, Figure 1 shows reflectance spectra, not albedo.
P394, 20: This sentence is difficult to understand. Rewrite, split or rearrange.
The authors did not come up with any better sentence despite hard thinking. We shall try once more.
P394, 22: Indicate where the reader can see the snow cases in the figure.
The text is edited as requested.
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 nonideal 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.
The authors are sorry to have caused confusion. The simulations have been carried out both for an ideal pyranometer and for a pyranometer for which the cosine correction was estimated. It is not always trivial to apply cosine correction to measured data. When the measurements are carried out continuously all year round, the cosine response is not checked more than once a year, maybe less frequently. If there has been a change in that value, it is difficult to know, how to update the correction properly. All aging processes are not monotonous and smooth. Therefore it is of interest to check how big the effect is and whether the atmosphere makes things even worse or not. The wording has now been checked to clarify what was done.
P394, 26: Your wording is wrong: The correction does not give overestimated values. The uncorrected measurements overestimate the albedo.
The wording will be checked.
P394, 28: The effect of the cosine correction is not shown in Figure 5. Specify what "a few percents" are!
Quite right, the cosine correction is not shown, it is just mentioned in the text. A few percent is about 2 percent, see figures below.



Relative difference between simulated albedo with and without atmospheric contribution as a function of simulated albedo with atmospheric contribution. a) Ideal pyranometer b) Unideal pyranometer (i.e. one requiring cosine correction).

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

New figures will be added as requested.

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

The symbol is changed as requested.

P395, 14: AOD=0 does not mean, that Idiff = 0! There are still a lot of molecules in the atmosphere which scatter radiation and contribute to the diffuse radiation.

The authors did not mean that. The point was to say that black-sky conditions are achieved, when both AOD and Idiff are zero. Presumably the sentence was ill formed. It is now rewritten.

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

Figure 6 shows absolute differences, not relative ones. In a scatter plot a few individuals catch the human eye, although the points have been plotted somewhat transparent. Figure 7 gives more information on the error statistics. The relative error 0.9 quantile is about 8 %.

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.

Yes. A simple method will not easily provide better results. Using a more complex approach would certainly enable a better fit between simulated and true values, but then one would need a lot of input parameters, for which normally no values are available. It does not necessarily make the situation any better, if one uses a fine model, but provides it with erroneous input values.

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

Edited as requested.

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

Edited as requested.

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

Edited.

P396, 21: Idiff and Idir are needed as well.

Where albedo is measured, the total irradiance is measured and Idir and Idiff can be derived as mentioned later in the same paragraph.

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

Edited as requested.

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

Edited as requested.

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

Quite right. Usually AOD dominates in the broadband case, therefore we varied only AOD. We shall change this so that a sensitivity analysis about the effect of water vapour and ozone will be added. The text will be edited to inform more precisely what was varied.

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.

The authors prefer to keep this comment. It is just to remind that no wavelength band range or something else satellite specific assumption is hidden in the method.

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.

The sentence is edited.

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

Will be done.

P401, Table 1: c3 and c4 are not dimensionless. Give the units.

The units are added.

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

The figure is edited as reuqested.

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.

The point of this figure was to show that the AOD values used really represent a wide range of variation without significant intercorrelation between the two wavelengths. (Intercorrelation does not necessarily mean interdependence.) The alternative representation will be tested.

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.

The normal diurnal variation of albedo naturally exists before normalization.

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 oppor-tunity 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

To be sure it is not possible to show more than 2000 points so that they would be completely separable. The 1:1 line is now added. Yes, the horizontal bars are due to the AOD variation.

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.

Will be done.

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

Will be done.

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

The authors do not understand this request. However, since Figure 8 will be changed in any case the comment is obsolete.

Answers to Reviewer #2

This paper offers a parametrization for deriving the black-sky albedo by correcting for the fraction of the ground-based measurement of blue-sky albedo that is caused by the diffuse radiation component. A simple radiative transfer model (SPCTRAL2) is used to calculate the clear-sky diffuse irradiance using aerosol optical depth (AOD) measurements at two wavelengths and ignoring every other variable except the solar zenith angle. The AOD range measured at the Cabauw BSRN site and 87 tabulated albedo spectra serve as inputs to the model to simulate the deviations between bluesky and black-sky albedos that can reach 20%. Calculations specific to the Cabauw site using actual AOD and albedo measurements demonstrate typical blue-sky albedos that were 5% higher than black-sky albedos.

Many issues of this paper have been pointed out by Referee #1; I agree with most of them, and these will not be repeated. I think this paper could have been much better with not much more effort. I would have used the more accurate SMARTS model that the authors reference, but do not use, for the direct and diffuse calculations. I would have used ozone and water vapor inputs from the Cabauw site for the model. Using these and a nominal single scattering albedo and asymmetry parameter would have improved the Cabauw calculations as has been demonstrated by Wang et al. JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 114, D14206, 10 PP., 2009 doi:10.1029/2009JD011978.
The SMARTS model was not chosen, because it requires even more input values. It is evident that a description of the atmosphere can't be very precise, when only information about aerosols and irradiance is provided. Therefore using a more refined model would not have provided added value. Taking into account that the only goal is to derive a method to estimate the difference between the blue and black-sky albedo values in clear sky conditions, the coarser model is sufficient. A sensitivity analysis for water vapour and ozone using the Cabauw data is being carried out.

Since it is a BSRN site a much better downwelling is available that does not require a cosine correction. The downwelling irradiance can be calculated by summing the direct * cos(sza) + diffuse (assuming an offset correction for the diffuse is made, if needed). Since most pyranometers are calibrated at 45 degs and the weighted effective angle of incidence for both the diffuse and upwelling is not far from 45 degs, there may be negligible cosine correction needed if the sum is used instead of the single pyranometer measurement of downwelling.
I think this paper needs more effort to produce a useful parameterization that is acceptable for publication.
On p 390, lines 11-13, I am confused by their definition of shortwave and what a pyranometer measures; the shortwave is usually considered to extend from 290 out to 3000 or even 4000 nm, although there is little energy there and 4000 overlaps the thermal infrared
Yes, shortwave range definition varies quite a lot. Even the two reviewers don't suggest the same numbers. (290…305 up to 2800 …4000) Although pyranometers are used for shortwave measurements, it does not mean, that their wavelength range matches that of the shortwave definition exactly. The deviation is then a source of error for shortwave measurements. As the reviewers don't suggest the same numbers, the authors have decided to keep the wavelength range they used originally, since the longer wavelengths than that would constitute less than 1 % of the irradiance and the lower limit is now between the two numbers suggested by the reviewers.

*The equations will be given in more detail, which should relieve the concern of the reviewer about what is thought to be measured by the pyranometers. They sense the total irradiance on the surface (direct*cos(tsunz) + diffuse), which is integrated over the whole hemisphere and the wavelength range 0.3 ... 4 µm. The directional reflectance of the surface affects the diffuse component. We use essentially the nomenclature by:*
Schaepman-Strub, G., Schaepman, M.E., Painter, T.H., Dangel, S., Martonchik, J.V., 2006: Reflectance quantities in optical remote sensing – definitions and case studies. *Remote Sensing of Environment*, **103**, 27-42.
This reference will be added to the list of references.

Eqn (3) neglects Rayleigh scattering, ozone absorption, water vapor absorption, and mixed gas absorption; moreover, why not use the SPCTRAL2 direct irradiance output if you are going to use the SPCTRAL2 model
Quite so, Eq. 3 was originally meant to describe only the aerosol component, but the wording is incorrect as it is. The authors tried to avoid too much repeating the paper by Bird and Riordan, which unfortunately ended in compressing the text so much that finally the message was distorted. The authors really used the Bird and Riordan model in its complete form taking into account also contributions from the earth-sun distance, Rayleigh scattering, water vapour absorption, ozone absorption and uniformly mixed gas absorption are taken into account. The authors have will edit the manuscript on page 6 to make this point clear.
(In fact the earth-sun distance does not matter in the end results as it cancels away from the ratio of the blue- and black-sky albedo values.)

Sentence on lines 9-11 on p 392 is not correct
The effect of water vapour and ozone is being tested using the values measured in Cabauw. Previous test on the Ozone effect on the broadband albedo have suggested that in reality its effect is small.

In last paragraph before section 4 Results, there is a discussion about cosine correcting irradiance in which the same cosine correction is applied to the diffuse as is applied to the direct; the cosine correction for diffuse is not the same as the direct since the diffuse impinges on the pyranometer from the entire hemisphere.

The cosine correction was taken from the paper by Michalsky et al, but there is a typo in the manuscript. The latter coefficient should be C_{dTP}, not C_{bTP}. The simulations were carried out using the right coefficients.

Sentence beginning on line 15, p 394 is wrong, or at least confusing as to the meaning (if there is less attenuation in the near-infrared compared to the visible, then there is relatively more infrared than visible, not less). *This sentence has been turned up and down during the writing process. It seems that the logic does not appear in English as it is meant. The authors tried to improve this part of the text.*

See line 14 p 395; if AOD is zero, Rayleigh scattering is still producing diffuse irradiance

The Rayleigh scattering decreases the direct shortwave irradiance a few percent of the TOA irradiance. Its effect on the albedo itself is even smaller than on the direct irradiance. But to be consistent, a small correction term will be provided for it when no aerosols are available.

Appendix. Updated section 3.

1 Simulation of the atmospheric effect on the broadband surface albedo

The broadband surface albedo α_{bb} is the ratio of the total reflected radiation and the incoming radiation. In practice only the shortwave part of the spectrum (300 nm - 2500 nm) is taken into account. Pyranometers usually measure the total radiation integrated over the bandwidth 305 nm – 2800 nm. The reflected short wave radiation R_{sw} is related to the total short wave irradiance I_{sw} at the surface and the spectral albedo $\alpha(\lambda)$ via

$$\alpha_{bb}(\theta_z) = \frac{R_{sw}(\theta_z)}{I_{sw}(\theta_z)} = \frac{\int \alpha(\theta_z,\lambda) I(\theta_z,\lambda) d\lambda}{\int I(\theta_z,\lambda) d\lambda}. \quad (1)$$

For direct and diffuse illumination the albedos (α_{bbdir} and α_{bbdiff}, i.e. the black-sky and white-sky albedo respectively) are related to the angular dependence of the material reflectance $f_r(\theta_z,\varphi_z,\theta,\varphi,\lambda)$ (i.e. the Bidirectional Reflectance Distribution Function BRDF) via (Nicodemus, 1970; Schaepman-Strub et al., 2006)

$$\alpha_{bbdir}(\theta_z) = \frac{\int \left(\int_0^{2\pi} \int_0^{\pi/2} f_r(\theta_z,\varphi_z,\theta,\varphi,\lambda) \cos\theta \sin\theta \, d\varphi d\theta \right) I_{dir}(\theta_z,\lambda) \cos\theta_z d\lambda}{\int I_{dir}(\theta_z,\lambda) \cos\theta_z d\lambda} \quad (2)$$

$$\alpha_{bbdiff} = \frac{\int \left(\int_0^{2\pi} \int_0^{\pi/2} \int_0^{2\pi} \int_0^{\pi/2} f_r(\theta_z,\varphi_z,\theta,\varphi,\lambda) \cos\theta \sin\theta I_{diff}(\theta_z,\lambda) \cos\theta_z \sin\theta_z \, d\varphi d\theta d\varphi_z d\theta_z \right) d\lambda}{\int \left(\int_0^{2\pi} \int_0^{\pi/2} I_{diff}(\theta_z,\lambda) \cos\theta_z \sin\theta_z \, d\varphi_z d\theta_z \right) d\lambda} \quad (3)$$

where the *BRDF* (f_r) describes which fraction of the reflected radiation coming from direction (θ_z, φ_z) is reflected to the direction (θ,φ), where θ_z and φ_z are the solar zenith and azimuth angles and θ and φ are the zenith and azimuth angles of the viewing direction. For simplicity the azimuth angle dependence of the radiant fluxes has been omitted. The *BRDF* is not typically a strong function of the wavelength, but slightly different values are often obtained for the visible and near infrared wavelengths. The blue sky albedo α_{bb} is then

$$\alpha_{bb}(\theta_z) = d\alpha_{bbdir}(\theta_z) + (1-d)\alpha_{bbdiff} \quad (4)$$

where $d = I_{dir}/(I_{dir} + I_{diff})$ is the fractional amount of direct radiant flux.

If there is no atmosphere the irradiance used in Eq. 1 would be the top of atmosphere solar spectral irradiance $I_0(\lambda)$ (ASTM Standard G-173-03) and the broad band albedo would be the black-sky albedo α_{0bb}. However, the presence of aerosol particles and gases in the atmosphere requires that both the direct and diffuse contributions to the irradiance at the surface are taken into account.

Surface albedo values measured at grazing incidence angles are typically very noisy and values measured at solar zenith angles larger than 70˚ are usually discarded from analysis. In that case the attenuation by the atmosphere can quite accurately be taken into account by assuming that the light propagates in the atmosphere along a straight path. Then the direct solar irradiance on a surface normal to the direction of the sun at ground level $I_{dir}(\theta_z, \lambda)$ for wavelength λ is related to the irradiance at the top of the atmosphere $I_0(\lambda) = H_0(\lambda)D$ by (Bird and Riordan 1986)

$$I_{dir}(\lambda,\theta_z) = H_0(\lambda) D T_r(\lambda,\theta_z) T_{Aer}(\lambda,\theta_z) T_w(\lambda,\theta_z) T_o(\lambda,\theta_z) T_u(\lambda,\theta_z) \quad (5)$$

where H_0 is the extraterrestrial irradiance at the mean earth-sun distance for wavelength λ, D is the correction factor for the earth-sun distance; and T_r, T_{Aer}, T_w, T_o and T_u are the transmittance functions of the atmosphere for molecular (Rayleigh) scattering, aerosol attenuation, water vapour absorption, ozone absorption, and uniformly mixed gas absorption, respectively.

$$T_{Aer}(\lambda,\theta_z) = \exp\left(-\tau_{Aer}(\lambda)/\cos\theta_z'\right) \quad (6)$$

where (Bird and Riordan, 1986)

$$\cos\theta_z' = \cos\theta_z + 0.15(93.885 - \cos\theta_z)^{-1.253} \quad (7)$$

and $\tau_{Aer}(\lambda)$ is the aerosol optical depth (AOD) at wavelength λ. It can be parametrized using the wavelength according to (Ångström 1929)

$$\tau_{Aer}(\lambda) = \beta \cdot \lambda^{-\alpha_A} = \tau_{Aer}(\lambda_{ref}) \cdot \left(\frac{\lambda}{\lambda_{ref}}\right)^{-\alpha_A}, \quad (8)$$

where β is the AOD at the reference wavelength λ_{ref} (usually taken at 1 μm) and α_A is the Ångström exponent evaluated for the wavelength pair λ_1 and λ_2. An example set of AOD values was derived for the simulations by regression of Eq. 8 to the measured AOD values at wavelengths of 440 nm, 675 nm, 870 nm and 1020 nm. The regression was carried out separately for each quartet of recorded AOD values. Altogether there were 2226 quartets resulting in 2226 regression functions $\tau_{Aer}(\lambda)$.

The estimation of the diffuse solar irradiance at the surface per unit area $I_{diff}(\lambda)$ is much more complex. Here only clear sky cases are of interest and ground-based surface albedo measurements are usually not accompanied by simultaneous measurements, which allow for an accurate atmospheric correction. Therefore a relatively simple but informative model, using only a limited and readily available number of atmospheric parameters, would be desirable to estimate the diffuse irradiance. The model SPCTRAL2 (Bird and Riordan 1986; Gueymard 1995 and 2001) was chosen for that reason.

The diffuse irradiance computed in SPCTRAL2 consists of three components: 1) Rayleigh scattering, 2) aerosol scattering and 3) a component that accounts for multiple scattering of light between the ground and the atmosphere. The diffuse irradiance component contains separate transmittance terms for ozone, water vapour, mixed gas and aerosol absorption and for aerosol scattering.

The absorption bands of ozone and water vapour are mainly situated at longer wavelengths, so that the exact amount of ozone and water vapour are not crucial for the total broad band irradiance in clear sky cases. Therefore in the simulations the ozone and water vapour contributions are taken to be equal to those of the standard atmosphere (ASTM Standard G173-03).

The simulated broadband albedo that would be measured by a pyranometer is then obtained using the sum of the modelled direct I_{dir} and diffuse I_{diff} radiation for the irradiance I_{sw} in Eq. 1. The required reflectance spectra are directly derived from the USGS Spectroscopy Lab database (Clark et al. 2007). Then only the BRDF is needed to obtain the surface albedo estimate.

At this stage the idea is to simulate the variation range of the atmospheric effect for a chosen example set of land cover spectra, which covers the wide variation of typical land cover types. Therefore, it is not critical to have the BRDF description for the chosen individual model targets, which are just random representatives of similar targets, but the BRDF descriptions have to be realistic both in size and in characteristics. The BRDF values were obtained using the methods developed for visible and near infrared bands of satellite instruments (Roujean et al., 1992; Wu et al., 1995). The spectra were split into a visible and a near infrared part separated at 750 nm.

The total, visible and near-infrared irradiance values (I_{0sw}, I_{0vis} and I_{0nir}) at the top of the atmosphere are obtained by integrating the solar irradiance spectra (ASTM Standard G173-03) over 305 nm – 750 nm for the visible band and over 750 nm - 2500 nm for the near infrared band. Pyranometers usually measure the irradiance integrated over the band 305 nm – 2800 nm, but the irradiance in the band 2500 nm – 2800 nm is negligible (< 0.2%) compared to the whole irradiance or that of the near infrared band.

The simulated broad band surface albedo values that a pyranometer would observe were then compared to the corresponding simulated black-sky surface albedo values in order to assess the effect of the atmosphere on the albedo observation. A typical cosine effect was also added to check its effect on the results (Michalsky et al.1995). The uncorrected global irradiance is just the sum of the direct I_{dir} and diffuse I_{diff} irradiance values. The irradiance I_c with the cosine effect included was obtained from

$$I_c = \left(1 + \frac{I_{diff}}{1367\ cos\theta_z}\right) I_{dir} C_{bTP} + \left(1 - \frac{I_{dir}}{1367\ cos\theta_z}\right) I_{diff} C_{dTP} \tag{9}$$

The value of the correction coefficient C_{dTP} is 0.9789 and the correction coefficient C_{bTP} values for all solar zenith angle were interpolated/extrapolated from the values given by Michalsky et al. (1995) for 0, 10, 20, 30, 40, 50, 60 and 70 degrees.