

Sundström et al.;

On the use of satellite remote sensing based approach for determining aerosol direct radiative effect over land: a case study over China

## Response to Reviewer #1 comments

We thank the Reviewer for valuable comments concerning our manuscript. These suggestions improved the presentation and quality of the original manuscript. Please find below authors response (A) to Reviewer's comments (R):

### General Comments

**R1:** *I think that the authors must give a better description about the satellite data that they have use in their analysis. For example, the analysis has been based on daily or monthly data? What about their accuracy? What about their Level?*

**A1:** The satellite data is described in more detail in the new version of the manuscript, as suggested. The satellite based method for deriving ADRE at TOA uses daily observations of MODIS AOD and CERES TOA SW fluxes that are collected over one month period. In the linear fitting these daily values from one month are used, and the resulting ADRE represents a monthly value. The CERES SSF-data is edition 2G, which includes also the MODIS coll. 5 AODs merged into the CERES footprint. The CERES team reports an uncertainty of 3% for the instantaneous clear-sky CERES SW TOA fluxes at midlatitudes. For MODIS coll. 5 the expected error for AOD is  $\pm(0.05 + 15\%)$ . However, it is noted that in the CERES SSF-files the 10 km resolution AODs have been merged into 20 km CERES footprints as point-spread-function averages, which might change the expected error for AOD.

**R2:** *Which aerosol and cloud parameters have been selected?*

**A2:** The aerosol parameter that is used is AOD at 550 nm from MODIS collection 5, which indeed has not been indicated clearly in the current version of the manuscript. The cloud parameters that are used in this study are described in Sec. 4.1., but this could be moved to the earlier section where the actual data is described for clarification.

**R3:** *Add a short paragraph about LibRadtran.*

**A3:** A short paragraph about LibRadtran has been added.

**R4:** *Lines 147 - 153: Based on the caption of Fig. 3 this analysis about the critical albedo has been made for the September of 2009 but in the manuscript it has not been stated. Why the authors have select only this month?*

**A4:** We agree that overall Section 3.2. needed clarification, which has been done. The critical albedo and the simulations were meant to be only an example of how the aerosol properties together with different surface albedo affect the ADRE at TOA, and on the other hand to give some kind of pre-guess of how the ADRE pattern should approximately look when derived from satellite data (mainly to identify where ADRE is most probably negative, and where positive). Therefore, just one month was selected as an example.

**R5:** *Lines 169 - 170: "In each 0.5o grid cell the cloud-free flux and AOD observations were collected over one month....". According to the caption of Figure 4, this analysis has been made for the October of 2009 but it has not been stated in the manuscript. Why the authors select this month and not all the observations during the study period based on their criteria?*

**A5:** Figure 4 shows an example of how the fitting between CERES flux and MODIS AOD looks like in one grid cell for one month (October 2009). The procedure described in lines 169-170 is carried out in each grid cell for each month considered in this study, not only October 2009.

**R6:** *Lines 176 - 178: I think that the threshold of the correlation coefficient is too low. Why the authors didn't select only the statistical significant R values?*

**A6:** As stated in the text, we have adapted this method from the previous publications, where a correlation coefficient of 0.2 was selected as the threshold. Since in the previous publications the selection of this value has not been justified in anyway, we also looked how the selection of the correlation coefficient would affect results. This is illustrated in Fig. 11 where the difference between modeled and satellite based aerosol-free flux at TOA has been plotted against the correlation coefficient. It shows that indeed higher correlation more probably provides aerosol-free TOA fluxes similar to model, even though large differences may still occur. In the previous studies the p-value (or significance of correlation) was not considered in the fitting procedure, and we did not consider that either.

**R7:** *Lines 202 - 203: Could you please explain in more detail the second part of this sentence?*

**A7:** A more detailed explanation has been added to the new version of the ms.

**R8:** *Line 205: How these fixed values came up?*

**A8:** Some indication for a realistic SSA over the study area was obtained from the AERONET data. Before deciding the aerosol type that will be used in the simulated reference fluxes, a number of tests with different aerosol types were carried out to evaluate the sensitivity of the normalization procedure to the changing aerosol type. The tests indicated only small differences between the reference flux values associated with changing aerosol types, and the effect on the normalization and linear fitting against AOD was minimal.

**R9:** *Lines 218 - 219: It would be better to provide geographical distributions showing the R and RMSE values before and after the normalization procedure.*

**A9:** By showing Fig. 4 as an example we wanted to demonstrate that the normalization procedure is relevant, and how it not only increases the correlation and decreases RMSE in majority of the cases (not in every case), but also changes the estimate for the aerosol-free TOA flux. We have added a new figure illustrating also the spatial distribution of R and RMSE as suggested by the reviewer.

**R10:** *We cannot assume that there is not variability in the AOD and the WV values during the day. Since the authors made this assumption, they must analyze the diurnal variation of the AOD and water vapour based on AERONET or other measurements. If this study shows that there is a substantial variability this part of the manuscript must be removed.*

**A10:** Arola et al. (2013) showed that the mean impact of diurnal AOD variability on 24 h ADRE estimates, averaged over all AERONET sites, is rather small and it is relatively small even for the cases when AOD was chosen to correspond to the Terra (or Aqua) overpass time. They also demonstrated that the strongest observed AOD variability (the strongest morning afternoon contrast) does not typically result in a significant impact on 24 h ADRE. The highest effect on 24h ADRE was obtained for cases when AODs have either maximum or minimum near local noon, then the impact could be 5-10%. Based on this, we estimated that the uncertainties related to diurnal variation of water vapour and AOD are likely smaller than other uncertainties related to this method, and thus would have insignificant effect on the results. Therefore only the solar zenith angle variation has been taken into account. We have been clarifying the text on this part and referred to Arola et al. (2013).

Reference: Arola et al. (2013). Influence of observed diurnal cycles of aerosol optical depth on aerosol direct radiative effect. *Atmos. Chem. Phys.*, 13, 7895-7901. <http://www.atmos-chem-phys.net/13/7895/2013/acp-13-7895-2013.html>

**R11:** *Lines 258 - 260: Is there any explanation about this?*

**A11:** At the moment we don't have explanation for this, but it might be related to some method artifact. The cases of positive ADRE are discussed in more detail in Sec. 5.2.

**R12:** *Section 5.3: I think that this part of the manuscript must be moved to Section 4 since it is described the agreement of the outgoing SW fluxes without aerosols, based on satellite measurements and LibRadtran outputs.*

**A12:** We have reorganized the sections as suggested.

**R13:** *Lines 326 - 335: I think that the interpretation of these results is poor. Please provide more details about the spatial and temporal characteristics of these differences*

**A13:** More detailed description is provided in the new version of the ms.

**R14:** *Lines 341-351: Is there any possible explanation about these results?*

**A14:** Some of these results might be related to the MODIS aerosol retrievals themselves, e.g. problems to retrieve AOD over bright surfaces.

**R15:** *Lines 354 - 356: I cannot understand the second part of this sentence. I suppose that the MODIS SW black-sky albedo values are almost uniform into the grid cell. Is that correct?*

**A15:** Yes, that is correct. These sentences has been clarified.

#### **Technical comments:**

**General Answer:** Several Reviewer comments concerning typos, or modifying the structure of the text have been changed as suggested. Also the requested references have been added. Below the answers to those comments that were not specifically related to the language or references.

**R2:** *Lines 38 - 40: It must be also considered that there are variations in the type of aerosols expressed by variations of other aerosol optical properties (e.g. single scattering albedo).*

**A2:** This has been added to the text.

**R8:** *Lines 126 - 127: "...where all other quality criteria for lev 2.0 were met except the AOD threshold (Arola et al., 2013)". I think that the authors must put a short sentence describing the AOD thresholds.*

**A8:** The description of AOD tresholds has been added.

**R9:** *Lines 143 - 144: It must be added in this sentence that one of the factors that determine the magnitude and especially the sign of ADRE at TOA, is the position of the aerosol layer relative to the clouds.*

**A9:** This sentence has been added as suggested.

**R10:** *Lines 180 - 181: In this sentence, it is better to state that the upward SW fluxes are changing due to the variances of aerosol load.*

**A10:** Has been changed as suggested.

**R19:** *Figure 5: Add at the right y-axis the corresponding percentages.*

**A19:** The y-axis "number of cases" has been changed to "percentages" as suggested.

**R20, R23:** *Figure 5: Re-write the caption of this figure. I didn't understand the sentence "White areas denote pixels where enough data has not been available for a successful linear fitting for any of the months." Figure 8: Re-write the caption of this figure.*

**A20:** This is an obvious mistake which is now corrected. Caption for Figure 8 has been re-written.

**R25-26:** *Figure 10: Add at the right y-axis the corresponding percentages. Figure 11. Replace colorbars with the corresponding percentages.*

**A25-26:** The Figures have been modified as suggested.