Author response on “Effect of water vapour on the determination of Aerosol Direct Radiative Effect based on the AERONET fluxes” by J. Huttunen et al.

First of all, the authors express thanks for the comments. Changes to the paper based on the comments are marked in the revised reviewers' version with blue. The response is below to the reviewer comments in italics.

Anonymous Referee #2

1.) *At first, I basically agree with some of the comments of reviewer #1. In particular: - Concerning the use of F0 fluxes from AERONET and the fact that the assumptions at the base of the model calculations are not described. To add some details would help the discussion.*

1.) We added some description with more details in the revised version.

- What do you mean with “cosine correction of the SZA?”

Corrected. We meant that fluxes are cosine corrected, not SZA.

2.) *The fact of not using pyranometer data is explained, however to add direct measurements of surface irradiance from few sites with different aerosol and reflectance conditions would be useful to compare and possibly validate your results.*

2.) We agree. Still in this paper, use of the AERONET fluxes with the explanations is justified. This is motivated with a result in where no significant difference was found between the AERONET and measured fluxes. One example is shortly described below and we added some text about this into the paper also.

<u>A brief test considering the analysis with measured and the AERONET fluxes:</u> Here, the WVC effect is considered with measured solar fluxes (pyranometer fluxes from SolRadNet) side by side with the AERONET fluxes. The maximum time difference between the AERONET fluxes and measured fluxes is 5 minutes, otherwise the thresholds are the same as described in the paper. The analysis here is done

for GSFC and Alta-Floresta in 2006 for SZA=60±1.5 degrees. The results are shown in Figs. 1 and 3; the ADRE is the approximately the same for the measured and the AERONET fluxes, with no significant difference. Thus, we can state that the conclusion of the paper is holding also for measured fluxes. In Fig.1, the lower subfigure, the average AERONET F^0 is shown with the red cross. The linear regression's intersection with the y-axis is close to the red cross, thus giving the ADRE in a close agreement with the AERONET's ADRE, demonstrating the WVC effect. Fig. 2 shows AOD and WVC from Fig. 1, indicating that WVC is increasing while AOD increases and the change is significant; from below 0.5 cm to above 4 cm while AOD changes from below 0.1 to above 1.0.

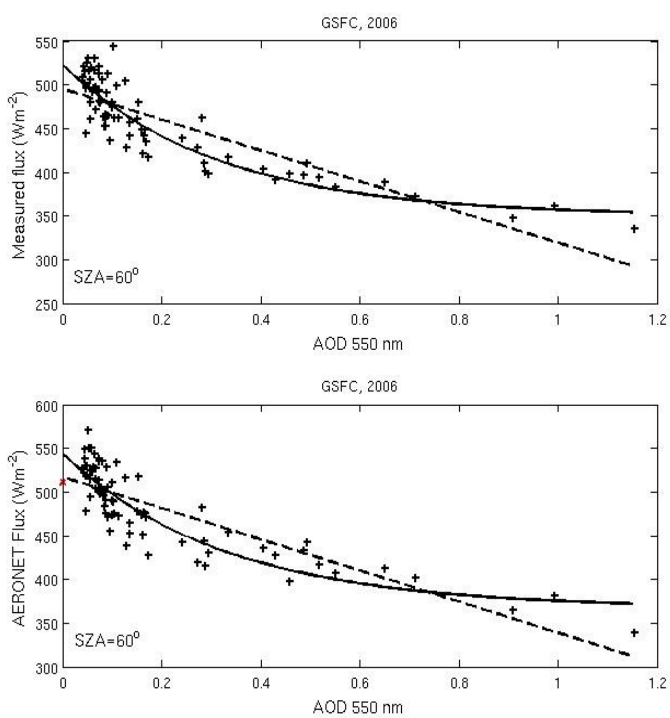

Fig. 1: Measured fluxes (upper subfigure) and the AERONET fluxes (lower subfigure) as a function of AOD with the regressions (nonlinear and linear) in GSFC 2006. There is no significant difference between the measured and the AERONET fluxes. Also the ADRE is close to the same for the both fluxes.

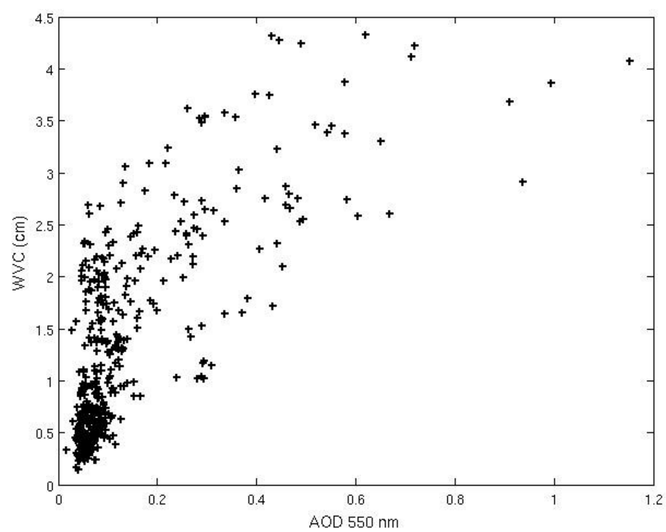

Fig. 2: AOD and WVC observations from Fig. 1. A significant positive correlation between the parameters.

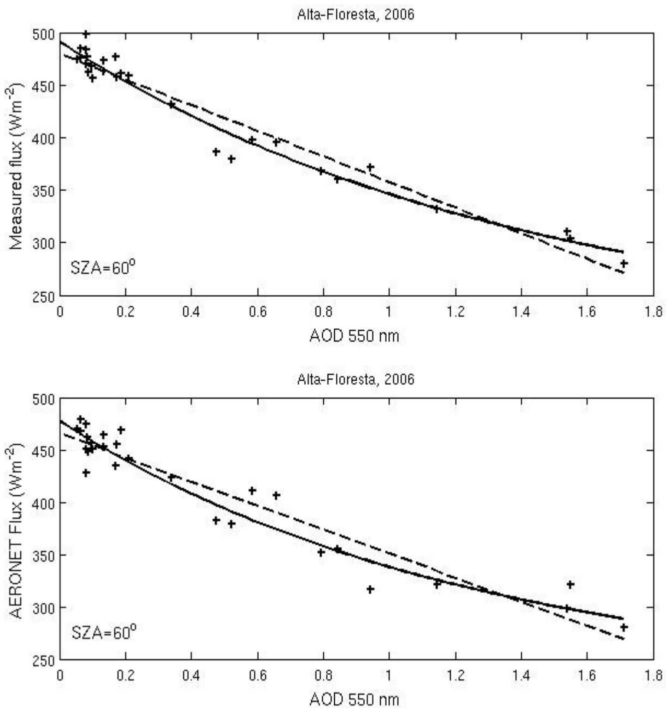

Fig. 3: The same as in Fig. 1, but for Alta-Floresta. The conclusion is the same as in the previous case.

3.) *The third comment with which I agree is the fact that it is not clear, after having read the paper, how your results could be extrapolated to be used by other researchers. In order to improve the significance of the paper, I consider this aspect of great importance.*

3.) We added in the conclusions-section more text about this, how the method can be extended for other users.

- Figure 2 is not at all readable; I would suggest to split it in two, based on the discussion you do in the text.

We updated Figure 2. Now it contains four subplots and seems to be clearer.

4.) *I have also another comment. You state in page 753 that in your analysis you cover different aerosol types and reflectance conditions. However, by reading your Results section, I have the impression this* *part should be extended a little. In particular, the linear and non-linear fits are expected to be very sensitive to changes in aerosol properties (associated also to aerosol types), as well as to reflectance conditions. For example, if you consider a sea salt aerosol or biomass burning you will have significantly different slopes of the curves, as well as Fzero estimates from AERONET (see also as an example Stone et al., 2011 for the non-linear effect of biomass burning). How the different optical properties of aerosols may affect the calculations is not very well investigated. This point could be useful also associated to a possible more general extrapolation of the results. The surface reflectivity, also, has been shown to affect the radiative impact of aerosols, and the radiative transfer of solar radiation, especially at high latitude sites (e.g., Shine 1984, Grenfell and Perovich 2008, Di Biagio 2012). As you also include Arctic sites in your work (Fig. 4), I guess this is an aspect you should take into consideration.*

4.) We agree. We added into the results-section more text about this. Also in this response we have some material to show.

First, we want to clarify that our Figures 3 and 4 do not cover the same sites; Figure 3 includes the sites in our analysis, while the Figure 4 shows the WVC and AOD correlation and we created it by including all the AERONET sites. By applying our criteria, the high latitude sites were not included, likely due to our requirement to have AOD range of 0.1 – 0.3. So unfortunately we could not look in more detail at the sites with high surface reflectance in the selected data set.

Nevertheless, we did look at the possible AOD and albedo correlation of all the sites, by relaxing our requirement of 0.1-0.3 AOD range and found that albedo does not change significantly as a function of AOD. From the extreme cases (out of entire AERONET data set) we estimated the largest albedo change to be from 0.10 to 0.25 as a function of AOD, while WVC can vary from 5 to 25 mm as a function of AOD. Then we calculated fluxes without aerosols with varying albedo (0.10-0.25) by a radiative transfer code and got a change of 5 Wm^{-2}. At the same, varying WVC (5-25 mm), and calculating fluxes without aerosols, the change was 40 Wm^{-2} (we did this test for SZA=60 degrees). So we argue that in addition for being rare, the cases of AOD vs albedo changes would not induce a significant bias in ADRE estimation.

We want to also stress and clarify that our focus was to look at possible correlations between AOD and any other variable that could influence the ADRE estimation, which assumes that the conditions stay stable and only AOD is changing. We did find significant correlations only between AOD and WVC, not between AOD and surface reflectance, for instance. We did estimates the correlations themselves, but the following more simple analysis illustrates this more clearly.

Figs. 4 and 5 show the paper's results divided into the parameters SSA, ASYM, AOD and albedo (they are shown in the colorbars, otherwise the same as in the paper), for the nonlinear and the linear method. It is evident that non of the other parameters separate the results as clearly as WVC in the Figure 3 of the manuscript.

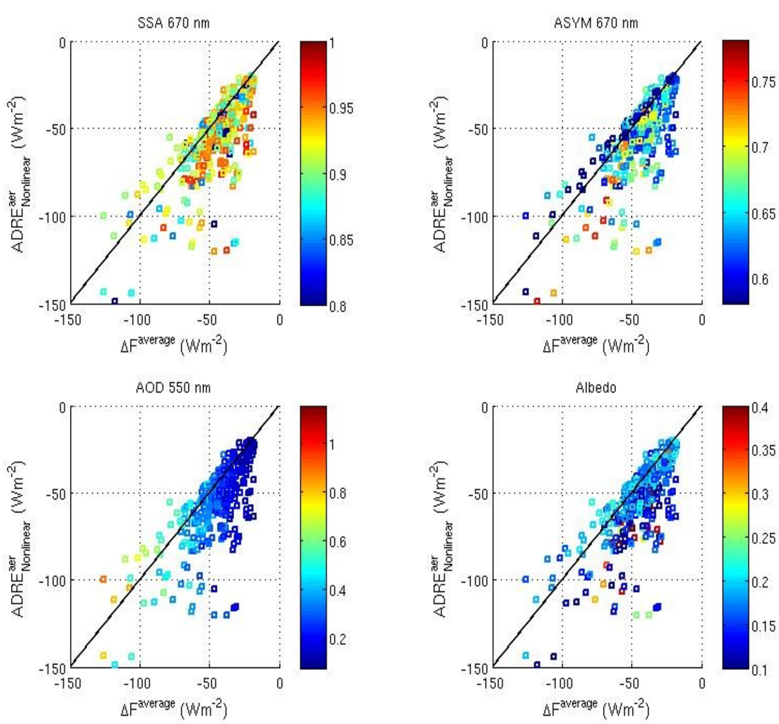

Fig. 4: ADRE estimated with the nonlinear decay compared with the baseline for the main optical properties (AOD, SSA and ASYM) and surface albedo indicated with colorbar. These are the results from the paper, only varying with the different parameters.

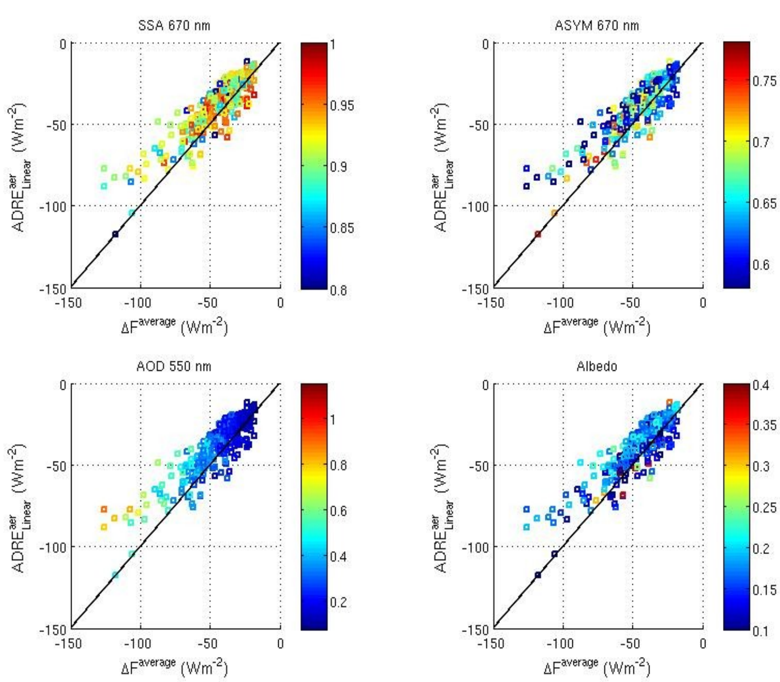

Fig. 5: The same as in Fig. 4, but for the linear regression.

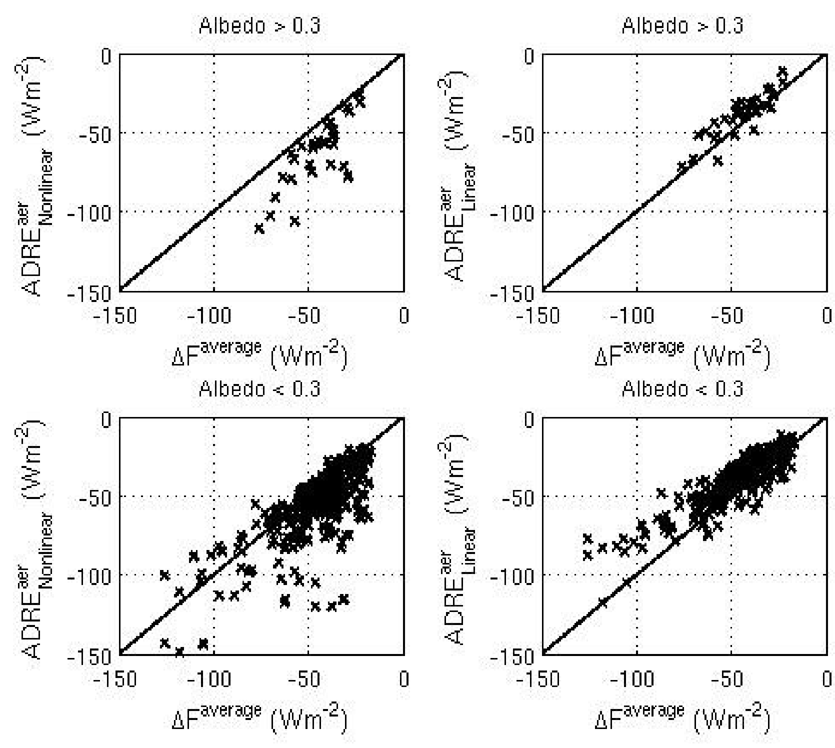

Fig. 6: The results of the paper divided with albedo; above 0.3 and below 0.3.