Item-by-item response to Reviewer #1

The authors greatly acknowledge the anonymous reviewer for carefully reading the manuscript and providing constructive comments that have led to an improved paper. This document contains the author's response to comments from reviewer #1. Each comment is discussed separately with the following typesetting:

*** Reviewer's comment**

++ Author's response

Changes in the manuscript

Specific Remarks:

*** P21243, L4: delete "human beings because they are involved in" (otherwise it sounds as if photochemical processes throughout the atmosphere have a direct effect on humans and that this effect is the most important one for human well being).**

++ According to the reviewer's suggestion, we have deleted these words.

*** P21243, L4: change "dispersion" to "scattering"**

++ We have made this change.

*** P21244, L29: it should be mentioned here that the measurement of cloud optical depth is an indirect method for the large optical depths (e.g. COD > 10) discussed in this paper because the attenuation of the direct solar beam cannot be determined. (In contract, the measurement of aerosol optical depth is straight forward because the attenuation of direct sun light can be directly measured with sun photometers).**

++ We agree with the reviewer's comment. Thus, we have mentioned this issue in Section 2 "Instrumentation and data" when the sun-photometer's characteristics are detailed.

*** P21246, L20: the sentence "AERONET COD are retrieved from the average of the ratios of the difference to the sum of two zenith radiances at 440 and 870 nm (ten ratios in 1.5 min)." is incomprehensible. Please split into two or three sentences and explain unambiguously.**

++ According to the reviewer's suggestion, we have revised the entire paragraph (see in the manuscript). Thanks.

*** P21246, L22: Please explain what is meant with "standard ground-based flux method". Provide a reference if available.**

++ We have revised the entire paragraph and added a reference for the flux method (see in the manuscript).

*** P21246, L23: Please explain what algorithm is meant with "This algorithm"**

++ We have revised the entire paragraph and removed the ambiguity of "this algorithm" (see in the manuscript).

*** P21246, L27: It is stated that the algorithm requires the presence of "green vegetation". This is a rather relative statement. How much "green vegetation" is required? In the following line, the albedo for 440 nm and 870 nm is specified, but not for a green wavelength (e.g. 555 nm). For example, what would be the minimum albedo at 555 nm required for implementing the method?**

++ The retrieval method for cloud optical depth takes advantage of green vegetation, which typically has a big jump in surface albedo from 0.1 to 0.4 at 700 nm wavelength and thus provides a sufficient surface albedo contrast for retrieval proposes. In principle, any combination of one wavelength below 700 nm and the other beyond will work for this type of retrieval method. For example, we can use 555 and 870 nm, or 670 and 870 nm wavelengths (as shown in Marshak et al., 2004). However, since surface albedo at 440 nm is generally even smaller than that at 555 nm, the combination of 440 and 870 nm is an optimal choice.

Reference: Marshak, A., Y. Knyazikhin, K. D. Evans, and W. J. Wiscombe (2004), The

"RED versus NIR" plane to retrieve broken-cloud optical depth from ground-based measurements, J. Atmos. Sci., 61, 1911–1925.

*** P21251, L12 (and P21242, L14): With respect to the sentence: "... suggests that the radiative transfer code overestimates the clear-sky experimental data." This implies that the reason for the difference is the radiative transfer code. This may not be true. For example, on P21246, L2, the authors state that their instrument was calibrated against a Brewer spectroradiometer that has an uncertainty of 7%. It is therefore quite possible that the code is correct and the measurements are too small. The sentence discussing the discrepancy between measurements and model should be phrased more carefully.**

++ We agree with the reviewer's comments; we have changed the statements that discuss the discrepancy between measured and modeled values for cloud-free conditions (see in the manuscript).

*** I also like to point out that most model input parameters are identical for UVERˆcloudy_mod and UVERˆclear_mod. So if the model values really were to high (for example, because OMI ozone were biased low), these errors would cancel when calculating CMF_mod.**

++ The difference between UVERˆcloudy_mod and UVERˆclear_mod is in the cloud parameters used as input only in the simulations for UVERˆcloudy_mod (cloud optical depth, geometrical depth of the cloud layer, cloud altitude and effective radius). The remaining parameters are identical for both UVERˆcloudy_mod and UVERˆclear_mod. In our work, we have obtained that UVERˆclear_mod are higher than experimental values (~8%) which can be related to uncertainties both in modeled and experimental data as have been pointed in the previous point. In addition, our study has shown that UVERˆcloudy_mod are substantially smaller than measured data (~14%), being the AERONET COD the main reason responsible for that underestimation. All these results explain that CMF_mod are around 22% smaller than the empirical CMF.

*** P21253, L12 - L 23: The authors conclude that variation of the model parameters "effective droplet radius", "geometrical thickness of the cloud" and "altitude of the**

cloud" introduce no "significant systematic error in simulating cloudy UVER data". I disagree. Figure 3 clearly shows that these parameters have a systematic (non-random) effect, which I would consider significant. Rather than saying that the effect is not significant, the authors should quantify the uncertainty (in absolute and relative terms) in the retrieved CMF values (as a function of COD) resulting from using fixed cloud parameters. The use of "significant" in P21253, L12 is also not appropriate. A better word would be "important".

++ We agree with the reviewer with the wrong use of the word "significant". Thus, we have changed this term in the text. The reviewer also suggests quantifying the uncertainty in the retrieved CMF values as a function of COD. In this sense, Figure 3 shows three plots with the ratio of UVERˆcloudy_mod for several scenarios to the UVERˆcloudy_mod for reference scenario as a function of the COD used as input in the simulations. These plots are equivalent to show the ratios for CMF_mod (UVERˆcloudy_mod / UVERˆclear_mod) since the denomitar is common for al scenarios. Therefore, Figure 3 exhibits the relative uncertainties (e.g, ratio=0.95 indicates an uncertainty of 5%) in the retrieved CMF values as a function of COD. These relative uncertainties have been discussed in the text.

*** Comments to language**

++ All the suggestions given by the reviewer have been taken into account.