Response to the Reviewer #2 comments for the manuscript "Erythemal ultraviolet irradiation trends in the Iberian Peninsula from 1950 to 2011" By R. Román et al. in ACPD

We acknowledge Reviewer #2 for her/his review in detail (of this manuscript and the other mentioned in the review). Reviewer comments are in black font (RC), and author comments (AC) in red font.

Author's answer to Reviewer

RC: 1. To begin with, I want to acknowledge the editor and the authors that I am currently also reviewing another manuscript by the same authors submitted to Atmospheric Research (title: "Comparison of nine different models to reconstruct erythemal ultraviolet radiation").

These two manuscripts are connected in the following way: the manuscript submitted to Atmospheric Research (denoted #1) presents and compares models for reconstructing UV radiation, while the present manuscript (denoted #2) presents an analysis of the resulting UV time series, calculated using two UV reconstruction methods included in #1, over the Iberian peninsula since 1950.

My main concern, which I already mention in my initial quick review, is that there may be overlap between these papers by the same authors. In principle, it could be possible to keep these two manuscripts apart and publish them as separate papers. However, if they are to be published separately, the authors need to make it perfectly clear what has been done in each paper and how the papers connect to each other, why each of the paper is needed and what their respective scientific contribution is.

Manuscript #2 (this review) is still not clearly explaining the differences and connections to manuscript #1. Sections 2 and 3 include lengthy passages which are more or less the same in both manuscripts. This is not acceptable.

AC: Our initial intention was develop a paper (manuscript #1) focused on the reconstruction model development and a depth comparison of reconstructed series with quality measurements. This was motivated by the intention to publish a second paper (manuscript #2, this one) exclusively focused on the analysis of the reconstructed series (trends, influence of uncertainty, etc.). We thought that it could avoid the need of a really extensive paper. However, it had some problems as reviewer said. Some parts of

the papers were similar even repeated, and the connection between them was not enough clear.

As a solution we decided remove the manuscript #1 (Atmospheric Research; title: "Comparison of nine different models to reconstruct erythemal ultraviolet radiation"), and we include the model development and a quick comparison with measurements in the new manuscript at Section 3.2. It avoids the problems about overlap between both papers and the not clear connection between them.

However, rejecting the first paper about reconstruction models, the depth analysis about models vs. measurements has been strongly reduced (the paper will be too much extensive). Anyway, the mentioned problems have been solved by this way.

RC: 2. Another important concern with this manuscript (#2) is the use of the open body fraction. I agree with the other reviewer that it is not clear how to interpret the open body erythemal UV series. If the open body UV is to be included in the study, it would require much more motivation, background, and discussion on why it is a useful quantity, for example, for epidemiological studies.

AC: We are agreed with both reviewers, this variable $UVER_{ob}$, should be more explained and discussed. In this sense, the paragraph has been changed by:

"*UVER irradiation quantifies the toxicity of solar radiation over human skin. However, if the human body is totally covered by clothes or anything else, the skin will not be affected by sun exposure even for high UVER irradiation values. Therefore, in order to find a new variable which quantifies the UVER dose received by human skin, the UVER over open body ($UVER_{ob}$) is defined as the UVER radiation multiplied by the open body fraction. $UVER_{ob}$ irradiation, measured in Jm^{-2} per open body unit, physically means the daily UVER irradiation received over the naked skin of a human who is exposed to sun the whole day. The open body fraction is usually multiplied by the UV radiation weighted by the vitamin D synthesis action spectrum (e.g., Chubarova and Zhdanova, 2013), but not by UVER radiation. The damage of UV over human skin is cumulative (WHO, 2002), and $UVER_{ob}$ can be used to evidences how much portion of skin is damaged or how much bigger is the redness skin after an overexposure to sun.*"

The changes and motivations are included in the response to the reviewer #1, but we included the same text for the reviewer #2 convenience.

WHO (2002) indicated that the risk of adverse health effects from UV radiation exposure is cumulative, hence we don't try to quantify the risk to produce sunburn (like UV-Index) but we want to quantify the UVER dose received by human skin. The portion of damaged skin will be higher when the open body fraction will be higher. As an example, we know the case of a 69-year-old man presented with a 25-year history of gradual, asymptomatic thickening and wrinkling of the skin on the left side of his face (Gordon and Brieva, 2012); his left side was exposed to UV radiation causing skin damage due to the cumulative UV effect, but if a more portion of his skin was also exposed to the same radiation, then more skin will be damaged. $UVER_{ob}$ evidences in a certain way how much portion of the skin is damaged.

We also assume that the size of sunburn is important; for example, two persons in a beach, one with a t-shirt but the other without t-shirt; both irresponsibly decided take a nap leaving their backs exposed to the same UV radiation; after the nap both had sunburn but the redness skin of the person with t-shirt was the arms while the person without t-shirt had redness skin in the arms and his full back. We considered that the damage of UV radiation over the person without t-shirt (more redness area) was higher, and it happened because the $UVER_{ob}$ received by the person without t-shirt was also higher.

We still think that the UVER over open body is an interesting, novel and relevant magnitude, but it can be controversial. Anyway, we would like remark that if the reviewers still think that $UVER_{ob}$ is not useful or necessary in this paper, we can remove this part (Section 4.2) in a second review process.

WHO (World Health Organization): Global Solar UV Index: A Practical Guide, 28 pp., ISBN 92-4-159007-6, Geneva, Switzerland, 2002.

Gordon, J. R. S. and Brieva, J. C.: Unilateral Dermatoheliosis, N. Engl. J. Med., 366:e25, doi:10.1056/NEJMicm1104059, 2012.

RC: 3. As mentioned in my previous review, there is also a need to consider more carefully what really can be concluded based on the work presented. Example: how much can be concluded about the role of aerosols and clouds from the reconstructed UV series which is based on climatological aerosols as input?

AC: We try to consider more carefully the conclusions of the paper. In fact there is a section named “Factors not taken into account” which evidences the considered approximations and how they could influence on the obtained results.

Regarding the use of a climatological aerosol, there is a sentence in the manuscript which tries to justify it:

“*The lack of AOD data earlier 2000 led to use a climatological table which does not contain the aerosol changes in the 1950-2011 period. However the aerosol effect is included the SW and F measurements (like clouds) and, as a first approximation, the reconstruction models transfer this effect to the UVER radiation.*”

First, the UV series are based on climatological aerosols, because, as expected, there are not measurements of aerosols from 1950. Hence we need use this approximation. But the UV series are also based on SW (or F) measurements, which are affected by aerosols and clouds. The effect of aerosol and clouds into the SW and F measurements is transferred to the reconstructed UVER series. Therefore we suppose that reconstructed UVER series include the aerosol effect.

In addition, the uncertainty in the cloudless simulations (given by aerosol uncertainty and monthly variability) is taken into account, and this uncertainty is around 50% (Román et al., 2014c) for every month, which is probably higher than the changes in AOD along the last 6 decades. This uncertainty is included in the reconstructed UVER series, and section 4.1.5 (Effect of UVER uncertainty on trends) gives the trends which are significant even taking into account the uncertainty. It means that the aerosol variability has not been considered directly in the cloudless simulations but this variability is taken into account through uncertainty and SW and F measurements.

About the role of aerosol and clouds, we assume that UVER changes are mainly caused by TOC, aerosol, and clouds. We have TOC data from 1950, then we can quantify the ozone effect; but we have not data for clouds and aerosols before 2000, then we cannot discern between the role of aerosols and clouds, but we can assume that the role of clouds and aerosols together is the role of all components minus the role of ozone. Therefore we talk about the clouds and aerosol effect together, but we don’t discern between aerosols and clouds.

Román, R., Bilbao, J., and de Miguel, A.: Uncertainty and variability in satellite-based water vapor column, aerosol optical depth and Angström Exponent, and its effect on radiative transfer simulations in the Iberian Peninsula, Atmos. Environ., 89, 556-569, 2014c.

RC: 4. Finally, I find that some parts of the manuscript are difficult to read and would therefore benefit from language checking and additional checks on the preciseness and logic of the expression (one example: section 2.2 introduces the data used, but it is often not clear whether the values have been used to create a monthly climatology or as a more realistically varying time series).

AC: We take this comment into account and we have tried to improve the new version for an easy reading with better preciseness and logic. Regards the language, the first version was revised in detail by a professional and native speaker (we can provide the invoice if it will be necessary). Regarding the example of the reviewer, we would like point out that section 2.2 only introduces the data used, but this section is not for the explanation about how they are used. In fact, Section 3.1 indicates how the data are used for the simulations:

"*These vertical profiles were rescaled with monthly climatological tables of water vapour, AOD at 443 nm, Angström Exponent, and SSA (at 354 nm for UVER simulations and at 500 nm for SW ones). These climatology tables (one per location and variable) comprised 12 monthly averaged (using all available data) values for each variable, said climatological tables being available in Román et al. (2014c). The daily TOC at each location was included in the inputs, changing the value for each location on each day. Finally, the spectral surface albedo values were also monthly averaged, these monthly values being linearly interpolated to obtain surface albedo at each wavelength to be then used as input in the radiative transfer code*"

The ozone is used as daily input and the remaining as monthly climatology tables; but the climatology tables are calculated using the data mentioned in Section 2.2.

RC: 5. Because of the overlap between #1 and #2 it is difficult to give a standard recommendation on the scale minor / major revisions / rejection. In any case, the manuscript(s) require more work before publication.

AC: This problem has been solved removing the paper #1 as we indicate in the first point of this review.