

## ***Interactive comment on “Ultraviolet Radiation modelling from ground based and satellite measurements at Reunion Island, Southern Tropics” by Kévin Lamy et al.***

### **Anonymous Referee #1**

Received and published: 16 August 2017

The manuscript presents a comparison of spectrally measured UV-index under clear sky conditions with model calculations for a tropical site over the period 2009 to 2016. First a sensitivity study is reported, where the effect of different sources for local ozone column, of two different extraterrestrial spectra and of two different ozone cross-sections on the calculated UV-index are discussed. These are not really new findings (as stated in 4.2.5, ln. 18), because very similar studies have been carried out already so far, but here they are specifically for the atmospheric conditions of a tropical station. Then in section 5 the model calculations are 'validated' against the observations. In general I think this is an interesting approach, as measurements under such conditions are very rare, and they complete and improve our understanding of UV-levels

C1

at the Earth's surface. Therefore I think the manuscript is worthwhile to be published. However, I think that some specific points should be considered prior to publication:

The abstract could be shorter and more concise, there are some sentences appropriate for the introduction but not for the abstract.

ad 2. Dataset: a bit more information could be given about the calibration of the spectroradiometer (traceability, frequency, ...).

ad 5.1 Radiative Amplification Factor (RAF):

- There are not 2 different definitions for the RAF. The linear relationship is simply the derivation of the power law. Therefore it is valid only for small variations of ozone and it becomes more and more erroneous for relative variations greater than 5%. The data presented in the manuscript are in the order of up to about 15%, but the systematic deviation between the linear relation and the correct power law is not obvious due to the scatter of the data points.

- The RAF describes the sensitivity of UV to ozone variations, while all other influencing parameters should be constant. Therefore it does not make sense to calculate the RAF including the varying earth-sun-distance (ESD), because the RAF should be the same if ozone is 300 DU in January or July, whereas the UV is higher in January. This means, not the model calculation should include the varying ESD, but the measurements should be converted to a constant ESD. This is of specific importance, when the number of measurements is not constant over the months of the year, because then it will produce a systematic error. Similarly, also the aerosol amount should be constant in the model calculation, whereas in the measurements its variation will produce a significant scatter in the analysis. So the usage of model RTUV01 is not suited to determine the RAF, it only can be used to compare the measured results with modelling (but this is anyway better done by direct comparison of the UV-index derived from measurements and from modelling, as discussed in the following paragraphs).

C2

- what data are used for the calculation of RAF with the ideal model case i-RTUV concerning extraterrestrial spectrum and ozone cross section – in Tab. 3 there are in both cases two different options mentioned with ‘or’.

- the calculated RAF with the ideal model case i-RTUV for the power relation (1.2 at SZA=25°) is in very good agreement with the value of 1.25 from Herman [2010] at low SZA, so the statement in ln. 11 (‘the higher RAF values found here ...’) is not valid. Furthermore, the argument that the ‘lower value of ozone’ (ln. 12) is responsible for any difference cannot be true, as the RAF is valid for the whole range of ozone values due to its definition.

ad 5.2 Validation against observed clear-sky UVI: for a validation of different results of model calculations against measurements the significance of the comparison between model and observation should be stated. In this case, the uncertainty of the measurements is +-5% (I guess this holds for a coverage factor of 1). Therefore a mean relative difference between model and measurement in the range 0.4% to 1.3% (Table 4) cannot be significant. Only mean relative differences greater 4% might be significant on a certain level.

Quite many technical corrections: (unfortunately, in my copy of the manuscript the page numbers are missing - this makes the commenting more laborious)

ad 1st page of introduction, ln. 20: not ‘during winter’ but ‘during summer’

ad 2nd page of introduction, ln.18: ‘tries’ instead of ‘tires’

ad 3. Clear-sky filtering, ln. 35: ‘with 15 minute intervals’ instead of ‘with at 15 minute intervals’

ad 4.1, ln. 27: ‘by Lacagnina’ instead of ‘by to Lacagnina’

ad 4.1, ln. 30/31: unclear formulation

ad 4.2.1, ln. 17: ‘crossing the atmosphere is longer’ instead of ‘crossing the atmo-

C3

sphere travelled is longer’

ad 4.2.1, ln. 18: ‘processes’ instead of ‘process’

ad 4.2.4, ln. 32: ‘aerosol measurements’ instead of ‘aerosols measurements’

ad 1st page of 5.1, ln. 40: not a linear relation between UVI and TO3, but between  $\Delta(UVI)/UVI$  and  $\Delta(TO3)/TO3$

ad 2nd page of 5.1, ln. 17: ‘RTUV01’ instead of ‘RTUV04’

ad 2nd page of 5.1, ln. 18: ‘to see how’ instead of ‘to seehow’

ad 2nd page of 5.1, ln. 31: ‘RTUV01’ instead of ‘RTUV04’

ad 1st page of 5.2, ln. 11: this sentence is almost a repetition of ln. 5

ad 1st page of 5.2, ln. 13: ‘SAOZ’ instead of ‘SBUV’

ad 1st page of 5.2, ln. 40-43: this paragraph is a duplication of ln. 1 and 2 and does not fit here

ad 1st page of 5.2, ln. 42: duplicate ‘on on’

ad 2nd page of 5.2, ln. 11: ‘which increased’ instead of ‘with increased’

ad 1st page of 6, ln. 47: for which ‘higher SZA’ the given numbers are derived?

ad 2nd page of 6, ln. 11: duplicate ‘to to’

Table 4, last line: ‘Median of the RD’ instead of ‘Median of the Mean RD’

Figure 8: the figures 8a and 8b are by far too small to see the different data points

Figure8: the legend says ‘8f’, but this is not shown in the figure itself. May be ‘8e’ would be appropriate (but not labelled). Anyway, this last part of Fig. 8 could be skipped.

Figure11: in the Figure ‘RTUV01’ is mentioned, in the legend below the figure it says ‘RTUV06’

C4

Overall I think the manuscript is worthwhile to be published in ACP after revision.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-571>, 2017.