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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of UV irradiances from Aura/Ozone Monitoring Instrument (OMI) with Brewer measurements at El Arenosillo (Spain) – Part 2: Analysis of site aerosol influence" *by* V. E. Cachorro et al.

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Items-by-items response to Reviewer #1

(* Reviewer comment, ++ Our response)

* P16388 L18. your sentence is not completely true: TOMS UV algorithm has an Aerosol Index-based correction for the absorbing aerosols which is not included in the current version of OMI UV algorithm. Please check it.

++ The sentence has been modified according the reviewer comment.

* P16389 L1. Change the sentence as follows: "However, a positive OMI bias up to 50% was found for polluted sites,...".

++ The sentence has been changed as recommended.

* P16389 L5. A recent paper by Ialongo et al. includes the absorbing aerosol correction also for erythemal dose rates. Please check it and mention this paper in the introduction. Ialongo, I., Buchard, V., Brogniez, C., Casale, G. R., and Siani, A. M.: Aerosol Single Scattering Albedo retrieval in the UV range: an application to OMI satellite validation, Atmos. Chem. Phys., 10, 331-340, doi:10.5194/acp-10-331-2010, 2010.

++ The paper by Ialongo et al. (2010) has been included and mentioned in various lines in the text, in the introduction and in other sections.

* P16391 L14 "we use both indistinctly": please use only one between AOD and AOT (AAOD and AAOT too) for clarity, and change them accordingly in the text.

++ Now we use in the text only AOD and AAOD (AOT and AAOT has been removed).

* P16393 L19-20 Did you checked the SZA dependence of this correction approach?(see again Ialongo et al., 2010 as they showed a SZA dependence of the correction factor). You can use the slant AAOD defined as AAODS=AAOD*cos(SZA).

++ We have not checked this possibility because we work with daily average data for SSA and hence AAOD data, therefore the dependence on cos(SZA) has not sense. As we explain in the text, we take average daily values to get a more reliable SSA data. Otherwise the correction factor should not depend on the SZA because this factor represents the absorbing aerosol properties and obviously SSA (or AAOD) values do not depend on SZA. SSA or AAOD must have the temporal variation which correspondent to aerosol conditions in the atmosphere but not a systematic variation with SZA. The observed dependence of the ratio (OMI-Brewer/OMI) on SZA is reasonable because are experimental measurements. Otherwise we think that the use of AAODS is not justified because a) as mentioned absorbing properties do not depend on SZA b) To

include AAODS adds more noise in the ratio (OMI-Brewer/OMI) to the intrinsic noise of this ratio due to SZA (see figure 4 in Antón et al., 2010) c) If the justification is that the application of AAODS gives a better agreement than AAOD, we think that this justification is not sufficient, because also the value of b=3 given by Krotkov gives a better agreement in Ialongo et al. (2010).

* P16394 L4 change the sentence with "...UV products, the analysis was restricted to lambda(use greek symbol)=324 nm, ..."

++ This recommendation is carried out.

* P16394 L17 You should mention these limitations. Do you maybe refer to the large SSA uncertainty or to the reliability of the methods, in general? Please would you cite some papers about that topic?.

++ We refer to both, because they are linked: the poor reliability of the methods gives rise to the large uncertainty of SSA retrieval. In the case of the methodologies based only in irradiance values, this fact is due to the poor sensitivity of SSA to irradiance values. Anyway, we have modified the original sentence adding “due to the low intrinsic sensitivity of SSA to UV irradiance values”. The authors are not confident with the methods that use only irradiance UV measurements (global and/or diffuse) and a radiative transfer model (RTM) without added any radiance information. The accuracy in the comparison between the experimental UV irradiance and a reliable RTM is about 5%-10% (furthermore we are not capable to measure UV irradiance with an experimental error below 5% in the best conditions). Thus, the uncertainties under we are working can give any SSA values between 0.7 and 1 (or 0.5-1 if this is the interval of work that you put in the algorithm), because the sensibility of SSA is not enough (it is very poor) for a good retrieval. This is our experience with this type of data. Otherwise, this is the current methodology and a lot of papers have been published using this methodology. However, we are not confident with the irradiance methods because of our own experience as mentioned. Nevertheless, we can not put in doubt the results of other

authors, only our results. We do not found in the bibliography a) one paper where two methods, one using only irradiance and another one using radiance to compare the retrieved SSA values; b) or combining radiance and irradiance data as complementary information, and demonstrate congruent results. c) Furthermore, why in Kazadzis and Ialongo papers only use a wavelength to retrieve SSA. We wonder why they do not use other Brewer wavelengths to retrieve SSA and compare both retrievals to assure the confident of the obtained results. The high observed variation of SSA along the day in figure 1 in the work of Ialongo et al. (2010) is not congruent with a given type of aerosol. Our results, taking various wavelengths were also not confident. Therefore, we are working in this way to solve the observed problems. This is an open research topic and we must continue working.

* P16398 L2-3 "Note that...": this sentence is not clear at this point in the manuscript, please mention it later in text to better clarify this comment.

++ We are sorry, but we do not agree with this recommendation. A paper is also an open discussion and the mind must be open. We are talking about a correlation already published in by Antón et al. (2007) and we also prevent the reader with the sentence that follows..... “as we will discuss later in the paper”. Therefore, if the reader can not capture the importance of this sentence in a first lecture, it may enrich the discussion of the paper when making a second lecture.

* P16401 L15 Replace "at level 1.5" with "(level 1.5)": do it in the whole manuscript. Would you please also better clarify the difference between level 1.5 and 2 aeronet data in the text?

++ We have replaced “at level 1.5” by “(level 1.5)” in most of the places, and we have clarified the differences between level 1.5 and 2 of SSA of AERONET in the paragraph of page 11(16399), adding more information and improving some sentences. The first three paragraphs of this page have been broadly modified (now the three earlier are two). Now, this is the second paragraph: “Al-

though AERONET provide SSA (or AAOD) values at two level of quality (level 1.5 is cloud screening pre-calibrated data and level 2 requires post-calibrated data together with manual inspection) and at the four aerosol wavelengths (440, 670, 870, 1020 nm) according to AERONET inversion algorithm (Dubovik and King, 2000; Dubovik et al., 2002), a climatology is not usually feasible at most AERONET sites. Level 2 for SSA requires a set of restrictions for inversion: post-calibrated data, SZA greater than 50°; 21 azimuth angles, high AOD values, etc., (see for detail, http://aeronet.gsfc.nasa.gov/new_web/Documents/AERONETcriteria_final1.pdf, and Prats et al., (2008)), which reduce considerably the data and hence it is difficult to have a representative climatology. The restriction of AOD(440nm)$\geq$0.4 eliminates most of the aerosol data, as in the cases of our station."

* P16403 L11-13 You showed very low correlation coefficients between OMI/Brewer ration and AAOD, even lower than those related to extinction AOD. If the bias is explained mainly by the effect of absorbing aerosol, the correlation coefficient should be at list slightly higher for AAOD than for AOD. Could you comment on that? Could you also mention in the conclusions which are in your opinion the major reasons of the OMI overestimation?(there is a positive bias left also after the absorbing aerosol correction). (It seems that the AAOD is not the proper optical parameter to estimate this aerosol effect in El-Arenosillo site)

++ Part of the people working in this area may agree with your comment (also part of the authors that sign the article). It seems that absorbing properties and hence AAOD seems not to be the proper optical parameter to estimate this aerosol effect, at least in El Arenosillo. Perhaps this approach only works properly in urban or polluted areas as was demonstrated in Kazadzis et al. (2009) and Ialongo et al. (2010), but this is not our case. Kazadzis et al. (2009) do not gives the values of the correlation coefficients (r or r2) to obtain b and Ialongo et al. (2010) obtained better correlations that our work for the urban atmosphere of Rome, but not so high (see also our table 1). For final results, it is observed that Kazdazis et al. (2009) works with the ratio OMI/Brewer, Ialongo

et al. (2010) with (OMI-Brewer)/Brewer and we work with (OMI-Brewer)/OMI, hence the values are not directly comparables. However the results are comparable taking into account the characteristics of the sites and the results of Taskanen et al. (2007). Certainly it is difficult know exactly if the bias of OMI is due to not account for the absorbing aerosols as it appears in all publications, mainly if we are working in clean areas. Perhaps the solution is simpler and the model used to predict the OMI irradiance is not so perfect to give the correct effect of aerosols. This is an open research topic and perhaps we must also work in new approaches. About to express our opinion on conclusion we think that the last two sentences are sufficient (also it must takes into account that different teams signed the paper) but we have enlarged the last sentence expressing the opinion that new approaches must be investigated.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 16385, 2010.