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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Comparison of total ozone and erythemal UV data from OMI with ground-based measurements at Rome station" *by* I. Ialongo et al.

**Anonymous Referee #2**

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The paper reports on the comparison between OMI ozone and erythemal UV products against ground measurements in the centre of Rome, from 2000 to 2006. The ground data consist in total column ozone measurements performed with a single-monochromator Brewer MKIV, in UV erythemal dose rates at noon measured both with the Brewer instrument and a broadband radiometer (YES UVB-1) and in erythemal daily doses obtained by integration of the YES dose rates. These are compared with the OMI total column ozone obtained with both the TOMS and DOAS algorithms and the OMI erythemal dose rate at noon and daily dose products. While the ground and satellite-derived total column ozone values agree very well, the erythemal radiation intensities estimated with OMI show a substantial overestimation with respect to



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the measurements. These findings confirm previous similar studies in different locations. The authors attribute the bias in erythemal radiation to the presence of absorbing aerosols in the urban environment of Rome, not taken into account in the UV retrieval procedure from OMI data.

A validation study in a new location is valuable and should indeed be published. Given the careful calibration of the instruments, the ground measurements are reliable and so are the results of the comparison with the OMI retrieved estimates. The interpretation of the OMI bias in erythemal radiation intensity as caused by absorbing aerosols is however not supported by any experimental or modelling evidence. The study would be more interesting and informative if this hypothesis was further investigated. If experimental aerosol information is available for the site, it could be examined if the OMI/ground difference is correlated with the aerosol absorption optical depth. It could also be checked with a radiative transfer code whether the attenuation by absorbing aerosols can explain the observed bias. Even without data on aerosols, one could estimate the aerosol load that would be needed to explain the discrepancy and judge whether it is realistic.

A fundamental difference between a ground measurement and a satellite- derived irradiance is that the former measures the irradiance at a point while the latter is by nature an average over a certain area (the satellite product pixel). Because of this, other factors than aerosols can lead to systematic differences such as cloudiness, altitude or albedo for which the conditions at the measurement site may not be representative of the pixel area. For instance, a systematic difference between the cloudiness over land and sea can a priori lead to a bias if the pixel containing Rome also includes a fraction of sea. A detailed analysis would probably be complex but I think this point should be at least briefly discussed. In this regard, more information on the spatial resolution at which the OMI UV products are generated would be desirable. Possibly related to this aspect, there is a sentence at page 2389: “Otherwise, the comparison does not show any significant dependence on the distance pixel-GB”. This should



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be explained. Does the variability of the “distance pixel-GB” result from the swath drift? How much does it vary?

I regret that all figures on the UV comparisons are restricted to clear sky days. My opinion is that figures that would show the results for all days would advantageously replace figures 1 and 2, which do not bring essential information to the study topic. This would also better document the scatter between satellite-derived values and ground measurements. This scatter is also an important factor for some uses of satellite-derived UV, e.g. UV index or impact studies based on daily doses and I think it should be commented.

The bias dependence on the solar zenith angle would support the absorbing aerosols attenuation hypothesis. It is apparent with respect to the Brewer dose rates but practically absent when comparing with the YES data. Is there a difference in the two instruments characteristics that would explain this? In this situation, I think that the SZA dependency of the OMI bias can only be ascertained if there is a reason to believe that the Brewer measurements are more reliable.

At page 2389, the authors state “Furthermore, the difference can be related to the fact that the time of overpass and conditions during overpass do not correspond to time and conditions at solar noon (Weihs et al., 2006).”. In order to contribute to the bias, the difference in the “conditions” between noon and the overpass time must have a systematic nature. Can the authors comment on what it could be, apart from the direct effect of solar zenith angle, which I imagine is corrected for in the OMI product.

To summarise, I think that the paper should be published but with a revised, further developed analysis of the results.

In addition to those of referee #1, I have the following minor remarks:

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“Absorbing aerosols (e.g. organic carbon, smoke and dust) or trace gases (e.g. NO2, SO2) are known to lead an overestimation of the surface UV irradiance (Krotkov et al., 1998; Arola et al., 2005).”

It is neglecting the effects of absorbing aerosols and trace gases that leads to the overestimation.

p 2382 ecosistem → ecosystems p 2383 continous → continuous p 2383 european→ European p 2384 Antartic →Antarctic p 2387 tecnique→ technique p 2389 sistematically →systematically p 2389 slighly →slightly

If the publishing language is UK English

p 2386 behavior →behaviour

If the publishing language is US English

p 2385 travelling → traveling p 2388 analyse → analyze

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2381, 2008.