Answers to the Reviewer #1 comments concerning the manuscript "Validation of satellite-based noontime UVI with NDACC ground-based instruments: influence of topography, environment and satellite overpass time", by Brogniez et al.

In the following the comments of the reviewer are in italics and the answers and the changes made to the paper are in blue.

Anonymous Referee #1

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We thank the Referee for all his/her helpful comments and suggestions.

Review of the work:

This is an interesting work concerning the validation of satellite based UVI at local noon using GB spectroradiometric measurements at three sites. The main conclusions are in agreement with various similar comparisons that have been held and they are mentioned by the authors.

The use of satellite data in important parameters for public health (like UVI) is very important and such studies help in the direction of assessing these results. Well maintained and quality controlled ground based spectroradiometers, like the ones used in this study are the tools to perform such studies. The authors use up to date techniques and results from ground based and satellite measurements and retrievals.

In general the work is interesting and solid and I suggest that can be accepted for publication in ACP. However, there are several points that need clarifications. In addition, what I miss from the paper is the quantification/explanation of the different factors that cause these deviations.

These factors can be grouped as:

Satellite algorithm.

In CS cases after the OMI correction there is still a bias. Where this bias come from ? One factor can be an OMI underestimation of Total column Ozone (TOC). Has this been checked with GB measurements?

We have compared the TOC derived from OMI (OMTO3) with the one derived from the spectroradiometer's spectra for several cloudless sky cases at the three sites. It appears that OMTO3 is often smaller than TOC from the GB instruments. (See below the text we have added).

Even if the TOC is more or less correct, what is the UVI calculated by a simple radiative transfer model using only OMI derived related inputs (solar zenith angle, TOC, AOD, albedo)?

We agree that such a study could help to understand the results, so a modelling study has been added. We have preferred to use ancillary data from various sources because, apart from the differences that would exist between the radiative transfer models, there might be issues with the input parameters.

So, we have used OMTO3, aerosol data from sunphotometers of AERONET/PHOTONS next to the spectroradiometers (daily means), surface albedo from Feister and Grewe (1995) and solar zenith angle at noon. We have compared the simulated UVI to both OMI and GB UVI for several cloudless sky cases. The results are reported in several Figures. We have also performed few modelling with the other TOC product from OMI (OMDOAO3) that is sometimes quite different from OMTO3 (either > or <). TOC from GOME-2 is also sometimes different from OMTO3 but we had not performed simulations with it due to a lack of time.

In the modelling study for VDA, after the description of the radiative transfer computations, we have added the following text:

"We have compared the simulated UVI to both OMI and GB UVI for several cloud-free cases. The histograms of the per cent relative difference between the computed UVI and the measured one are reported in Figures 5a for GB UVI and 5b for OMI. GB UVI are 1.7 % lower and OMI UVI are 4.7 % higher than the simulated UVI. Since the TOC is the same for both modelling and OMI, this overestimation of OMI UVI might be mainly related to aerosol parameters and surface albedo, though this parameter value is small. Of course part of the bias might come from differences between the two radiative transfer models used and also between the other input parameters. Kazadzis et al. (2009) concluded also to an overestimation due to aerosol variability (in time and space). The underestimation of GB UVI is within the GB measurement uncertainty. We of course have to keep in mind that modelling computations are affected by uncertainties.

For this previous modelling we have chosen OMTO3 but other TOC data could be used, such as the TOC derived from the GB spectra following the method described in Houët and Brogniez (2003) relying on a differential absorption technique (Stamnes et al., 1988). The accuracy of this product is about 3 %. We find that this TOC is often larger than OMTO3, which is in agreement with Antón and Loyola (2011) findings for cloud-free conditions (OMTO3 smaller than GB-TOC by 2-3 % on average). Figure 5c shows the UVI relative difference between the computed and the GB UVI versus the TOC relative difference. The computed UVI is often larger than the GB UVI for a negative TOC relative difference. Note that the denominators of the relative differences (UVI or TOC) is the mean, contrarily to the SB – GB comparisons because, here, neither data is considered as a reference).

Another TOC product from OMI (OMDOAO3) exists, which is sometimes quite different from OMTO3 (either larger or smaller) leading to a different modelled UVI and thus to a quite different relative difference. For example a relative difference between GB UVI (4.8) and UVI modelled using OMTO3 (290 DU) equal to 7.6 % has become equal to 4.8 % while using OMDOAO3 (297 DU).

TOC from GOME-2 is also sometimes different from OMTO3, and often smaller than spectroradiometer-TOC but we had not performed simulations with it due to a lack of time.

Underestimation of OMTO3 and of GOME-2 TOC for cloud-free and cloudy cases, as is found also by Antón and Loyola (2011), might explain part of the biases between SB and GB UVI observed at VDA. Aerosol climatology from Kinne et al. (2013) and surface albedo climatology from Tanskanen (2004) might also contribute to the biases."

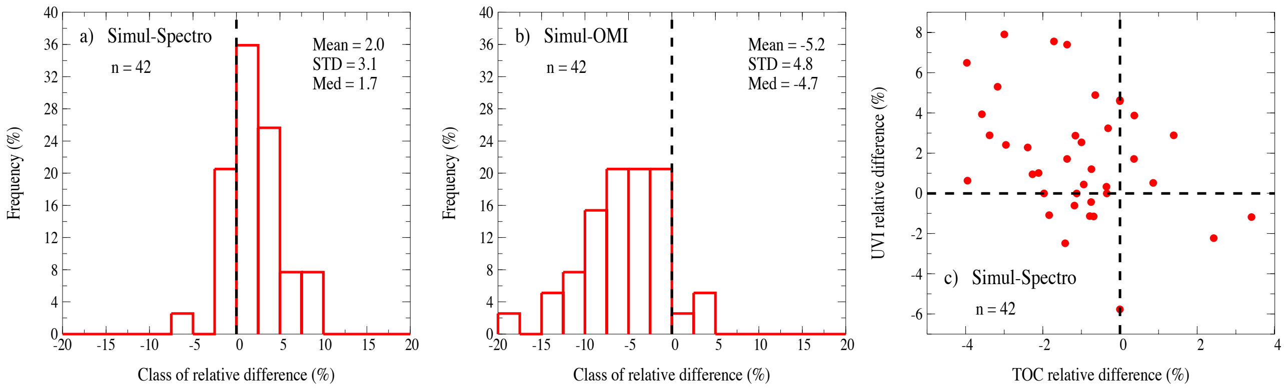

Figure 5.

We have made similar studies for the two other sites.

CS cases OMI correction: Is the AOD and SSA used by Kinne et al. realistic for the particular locations?

Kinne et al. climatology proposes monthly means relying on AERONET data between 1996 and 2008. of AOD and SSA at 315 nm. An interannual variability exists, so Kinne et al. climatology is not perfect. Moreover the gridding is 1°x1° in latitude/longitude, missing small scale spatial variability.

are there any GB measurements of AOD and SSA ?

AERONET/PHOTONS provides daily and monthly means of AOD at 440, 380 and 340 nm and the Ängström exponent (440-870 nm) to derive the spectral AOD in the UV and it provides also SSA at 440 nm. At the three sites a sunphotometer is operating enabling to obtain more suited data.

Finally, it would be interesting to point out a publication that describes in detail how this SSA at the 315nm has been derived. See also comment below.

Kinne et al., 2013, gives details on the derivation of the aerosol parameters. It is already in the references.

Cloudy cases: It seems that there is a constant overestimation of Omi for cloudy cases. This can be a comparison (spatial, or temporal effects) or OMI algorithm problem:

Starting from the temporal comparison problems (satellite local noon calculation using the overpass time cloud conditions). This could be an issue, but other studies using only overpass data for the comparison showed similar results (see also comment below). In addition, it is more or less equally possible to have overestimation or underestimation by OMI as overpass cloud conditions could be either CS or cloudy while during the same day noon conditions could be cloudy or CS, respectively. So statistically, this effect should not have a systematic bias on the GB-satellite differences.

We agree but since OMTO3 is often underestimated compared to TOC from GB instruments (see Figure 5c cited above), and even in cloudy cases (see Antón and Loyola, 2011) it leads to a UVI positive bias between SB-modelling and GB measurements.

The spatial issue: Satellites provide a cloud optical thickness and cloud coverage in percent for an area that cannot simulate a measurement point. In this case the most important issue is the sun visibility (direct sun component) at the time of the measurement. Statistically there are cases that there are few or more clouds and the sun is not visible (in this case OMI should overestimate) but also cases that there are clouds and the sun is visible. In that case Omi should underestimate.

As mentioned above, there is a bias due partly to OMTO3 underestimate related to subpixel inhomogeneity (Kazadzis et al., 2011). It is hard to know whether the sun is visible or not at the three sites, and the occurrence of each situation. On the other hand, the surface albedo from Tanskanen (2004) seems also overestimated compared to that from Feister and Grewe (1995), and though this parameter value is small at the three sites, it might impact the estimate of the cloud optical depth (Bernhard et al., 2015).

From the analysis it is evident that 90% of the data fall in the first category. So this is not easily explainable quoting only spatial comparison differences.

To be more clear, let's assume that there is a case with 50% cloud coverage. The UVI measured from the ground can vary as much as 200% depending if the sun is visible or not. However, for almost all of these cases satellite based data overestimate UVI meaning that someone's got to have a closer look at the satellite algorithm and especially how this calculates direct and solar irradiance at such conditions, in order to explain this systematic bias.

The sky imager located at VDA could serve to determine whether the sun is visible or not under variable cloud cover. This information will be important for understanding the reason of underestimation/overestimation by the SB sensor. This will be the subject of a future study.

Figures 3, 8 and 12 show a systematic overestimation in the range of 20-40% for cloudy conditions plus a lot of outliers only in the direction of the satellite overestimation.

So in general some more discussion on the quantification of the results based for example partly on the discussion above is needed.

As requested by the reviewer we have added part of his/her explanations (see all the text mentioned above).

Finally, I miss some general conclusion about the quality of the satellite data. In what extend can these data be trusted by the public in order to use their derived UVI?

In the conclusion we had written « UVI estimates derived from satellite sensors OMI and GOME-2 are only weakly biased high (on average less than 0.5 unit of UVI at VDA and OHP and less that 1. at SDR), and thus are quite reliable. » This needs to be detailed (see below).

Probably this is simplified but also it has to be commented that at high UVI cases the results of the comparison are much better than in low UVI's. For public health a 200% satellite overestimation when UVI is in the order of 0-2 could be not as important as a similar one for higher UVI's.

We agree with Reviewer's comment so we have detailed our previous conclusion accounting for Reviewer's suggestion: "The 90-th percentiles indicate that for all sky conditions, 10 % of the cases correspond to relative differences larger than about 65 % at VDA and OHP for both space-borne instruments. These 10 % cases are identified as red and violet crosses in Fig. 3 and 8, i.e. to UVI lower than 3, meaning that the comparisons are much better for high UVI than for low UVI. At SDR, for all sky conditions, 10 % of the relative differences are larger than about 85 % for OMI and 100 % for GOME-2. Underestimation of UVI by the space-borne instruments is more risky than overestimation for public health. The 10-th percentiles indicate that 10 % of the cases have a relative difference lower than -17 % at VDA and -12 % at OHP and SDR. A 12 % underestimation of a high UVI, i.e. UVI = 15, means that the actual value is 17, which has more important consequences than a 17 % underestimation of a low UVI, i.e. UVI=3, corresponding to the actual value 3.5. Though, these cases are not very frequent."

Detailed comments

The sentences: "Observations at northern mid-latitudes help complete geographical coverage. Observations from Reunion Island, close to the tropic of Capricorn, are useful as well." Need some more clear scientific wording

We have reworded the sentence: "Observations at northern mid-latitudes help complete geographical coverage from other sites. Observations from Reunion Island, close to the tropic of Capricorn, are useful as well because only few sites exist in the low latitudes."

Missing paper: http://www.atmos-chem-phys.net/15/7391/2015/ There is a lot of discussion on the above Bernhard publication that falls within the aims of this work.

We agree. This reference "Bernhard, G., Arola, A., Dahlback, A., Fioletov, V., Heikkilä, A, Johnsen, B., Koskela, T., Lakkala, K., Svendby, T., and Tamminen, J.: Comparison of OMI UV observations with ground-based measurements at high northern latitudes, www.atmos-chem-phys.net/15/7391/2015/ doi:10.5194/acp-15-7391-2015", has been added.

QASUME instrument reference needed: https://www.osapublishing.org/ao/abstract.cfm?uri=ao- 44-25-5321

This reference has been added : Gröbner, J., Schreder, J., Kazadzis, S., Alkiviadis F. Bais, Blumthaler, M., Görts, P., Tax, R., Koskela, T., Seckmeyer, G., Webb, A.R., and Rembges, D.: Traveling reference spectroradiometer for routine quality assurance of spectral solar ultraviolet irradiance measurements, Appl. Opt., doi: 10.1364/AO.44.005321, 2005.

The cosine correction: it needs more discussion or a reference publication. As it is written does not help a reader who is not into spectroradiometer measurement uncertainties to understand this.

The reference : "Bernhard., G. and Seckmeyer, G.: Uncertainty of measurements of spectral solar UV irradiance, J. Geophys. Res., vol. 104, 14321–14345, 1999." has been added.

Uncertainties for local noon satellite "extrapolation'. Since ground based data exist for both overpass and local noon. You could make an accurate assessment on the satellite uncertainties due to the satellite local noon time extrapolation. This by comparing overpass and local noon differences at a station with/without clouds e.t.c.

We did not consider a comparison between satellite and ground-based observations at the time of the satellite overpass because GOME-2 does not provide UV data at that time.

You mention that CS data are judged according to ground based measurements. How is this done?

As explained in the paper (L195-198) we examine the shape of the UVI diurnal variations and the relative dispersion around the hourly mean.

Since it sounds not clear, we have made few changes in the text: "Two criteria are set up to declare the sky as "cloudless": (i) the shape of the curve of the UVI diurnal variations around noon must be smooth (made by a visual inspection), and (ii) the UVI relative dispersion around the hourly mean must be less than 5 %, this value being an estimate of the UVI variation due to SZA variation around noontime (estimation derived from modelling). This second criterion is checked automatically."

SEVIRI/MSG comes out of the blue here. Is this used in some part of this work and how ?

As written in the paper (L199) we look at images from the SEVIRI sensor to estimate if the sky is cloud free.

We have now written "In addition, images from the SEVIRI sensor on the MSG satellite must show cloud free conditions close to the measurement time".

In general mean values for non-normal distributions (as are the Gb/satellite differences clearly here) has a limited value. I would suggest to use only median and percentiles (10%-90% for example) in figure captions and in tables. Distributions here are clearly skewed due to satellite (systematic plus outliers) overestimation for cloudy conditions.

We agree with the reviewer that median values are more adapted when the data distributions are skewed, that is why we have used them and we had stated that in the appendix. In the revised manuscript we state that in section 3 rather than in the appendix:

"The following statistics parameters are used to quantify the agreement: mean and root-mean square of the difference, mean, root-mean square and standard deviation of the relative difference. Since the difference/relative difference distributions are skewed we have also used the median and the 10^{th} and 90^{th}-percentiles."

Following Reviewer's suggestion we have included the relative differences at the 10^{th} and 90^{th} percentiles in the figure captions and in the tables.

OMI correction. Practically the OMI methodology for the AOD and SSA correction will lead to an improvement anyway. This is because a correction factor is applied for all data based on a (smaller or larger) aerosol absorption optical depth. So an additional input of this work could be a discussion on why this is not enough? As mentioned in the introductory comments: is the AOD and SSA used realistic? Having a look at the AERONET data I can see that AOD at the VDA for 2010-2012 at 340nm is in the order of 0.23 to 0.26 as a yearly average. On the contrary Kinne et al AOD shown in the figures, is almost double.

Kinne et al. provides AOD at 315 nm instead of 340 nm, though, an Angström exponent of about 1.3-1.5 cannot explain such a difference.

So in a first glance, probably this correction factor is already overestimated.

We agree that several issues remain, a future work is planned to explore them.