Answers to the reviewers 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 reviewers are in italics and the answers and the changes made to the paper are in blue.

Anonymous Referee #1

Received and published: 1 May 2016

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 or monthly 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, those for VDA being included below as example. 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 (**lines 290-298**), we have added the following text (**lines 299-325**) :

"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 Fig. 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, each value being within GB and OMI measurement uncertainty respectively. 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 RT models used and also between the other input parameters. Kazadzis et al. (2009b) concluded also to an overestimation due to aerosol variability (in time and space). 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 (2004) 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, which could explain the 1.7 % bias. Note that the denominator of the relative differences (UVI or TOC) is the mean, contrarily to the SB – GB comparisons because, in this study, 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.

Underestimation of OMTO3 and of GOME-2 TOC for cloud-free and cloudy cases, as is found also by Antón and Loyola (2011), can explain part of the observed biases between SB and GB UVI. Aerosol climatology from Kinne et al. (2007, 2013) might also contribute to the biases. Indeed, these aerosol climatologies rely on AERONET data that show an interannual variability, and the gridding is 1°x1° in latitude-longitude. Cloud cover variability within the satellite pixel (Kazadzis et al., 2009b) is expected to contribute to the biases as well as the surface albedo climatology from Tanskanen (2004)."

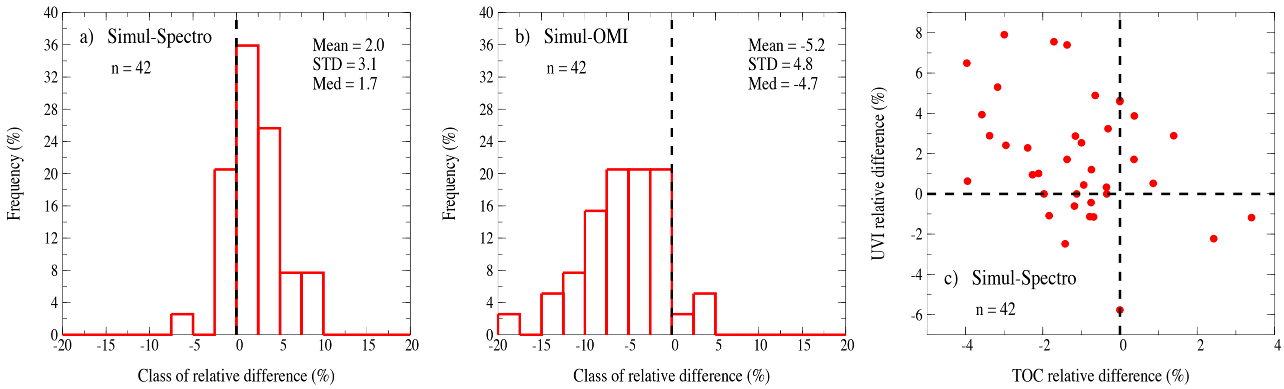

Figure 5.

We have made similar studies for the two other sites (**lines 414-427** for OHP and **521-535** for SDR).

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.

We have added a sentence in the modelling section (see above).

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/modelling and GB measurements.

We have stated that in the modelling section (see above).

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 (**lines 607-616**):

"The 90th percentiles indicate that for all sky conditions, 10 % of the cases correspond to relative differences larger than about 70 % at VDA and OHP for both space-borne instruments. These 10 % cases correspond 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. At SDR, UVI is often large so this strong overestimate is related to the site environment.

Underestimation of UVI by the space-borne instruments is more risky than overestimation for public health. The 10th percentiles indicate that 10 % of the cases have a relative difference lower than -17 % at VDA, -12 % at OHP and -10 % at SDR. For example, a 17 % underestimation for UVI = 6, means that the actual value is 7, and a 10 % underestimation for a high UVI, i.e. UVI = 15, implies an actual value

16.5, which has more important consequences than overestimations. Though, these cases are not very frequent."

And at **lines 637-639**:

"Finally the 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), which, as mentioned above, is less risky for public health than a low bias, and thus OMI and GOME-2 noon-UVI datasets are quite reliable and can be used by the public. "

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 (**lines 44-45**):

"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 (**lines 212-215**):

"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 (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 (**lines 215-216**):

"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 also in section 3 (**lines 221-224**):

"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

90th-percentiles."

Following Reviewer's suggestion we have included the relative differences at the 10th and 90th percentiles in the figure captions and in the tables, and we have added discussions on these values for each site and in the conclusion.

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.

Anonymous Referee #2

Received and published: 5 May 2016

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

This review is written by someone not directly involved in this field so can be viewed as the opinion of someone involved in other aspects of inter-comparison campaigns, UV spectral radiometry, and atmospheric spectroscopy. I commend the authors on their highly systematic approach to the comparison in three different locations, and the equivalent partitioning of the data into different, but consistent, observing conditions. I do, however, have a few comments that need to be addressed.

One thing that seem to be seriously missing in this document is clear presentation or a clear chain of references detailing the performance of the ground-based instruments used for this study. The authors say, in a single sentence, that the instruments have been compared against the QASUME traceable ground-based spectral radiometer standard and nothing more substantial is discussed beyond that. More information and some assurance of complete validation are definitely required. Was this comparison conducted at each of the three stations? How much dispersion in measured irradiance is there between the instrument(s) under test and traveling standard instrument? Was there a measurement campaign conducted and are those results published? Are there site specific differences in the level of agreement? I consider this to be a very important point. One of the important conclusions in this paper is that the space-based measurements still over estimate the irradiance (slope >1) as previously published. Because of this, some objective evidence is needed to demonstrate that some portion of this bias is not coming from the ground-based instruments. I do recognize that other publications also suggest that the bias is in the space-based instruments and algorithm improvement reduces the apparent bias, but potential ground-based systematic errors still must be ruled out as a potential contribution.

We agree that it is important to assess the quality of the GB measurements. So, as requested by the reviewer we have given the web site address where the QASUME reports can be viewed. In the modified manuscript we state that following the QASUME campaigns the raw measurements were reprocessed to account for the observed biases.

We have added (**lines 118-125**):

"During the QASUME campaigns, held for the three instruments, biases were observed: on average about 10% for VDA and OHP instruments and 3% at SDR (local instrument measurements lower than those of QASUME, reports available at http://www.pmodwrc.ch/wcc_uv/wcc_uv.php?topic=qasume_audit). Following these results, the VDA and OHP lamps have been recalibrated in July 2012 at the World Radiation Center, Davos, Switzerland, and all the data reprocessed. A NDACC intercomparison campaign held in July 2015 in Hanover, Germany, and further analysis have shown that the measurements are 3-4 % lower than the reference measurements, that is within the reference measurement uncertainty.

The SDR lamp irradiance has been adjusted to the QASUME irradiance (May 2013), and all the data reprocessed."

We have also given the uncertainty for each instrument separately (**lines 126-128**):

"The irradiance uncertainty leads to an UVI uncertainty for a coverage factor $k = 2$ of 5.3 % at VDA and OHP and 5% at SDR. The remaining biases observed at VDA and OHP are thus within these uncertainties."

This manuscript would also benefit from a short summary of what new information and advancements are presented in this study relative to other comprehensive studies; for example, Buchard et al., 2008.

As stated in the Introduction (L58-67), Buchard et al., 2008, dealt with OMI v1.2, for data collected at time of overpass, the tropical site (SDR) was not yet operating. Moreover GOME-2 was not considered. The studied periods also are different. Therefore it is difficult to compare the results.

The topic of this paper is not about total ozone, but an indication of the level of agreement on that would be a welcomed addition and would provide helpful information about the extent of other potential space-based biases. This is only a suggestion.

As requested by Reviewer #1 we have included a modelling study. In that study we have used total ozone and state that sometimes OMTO3 is quite different from OMDOAO3 though they are derived from the same OMI instrument. We show that the UVI simulated with these 2 different TOC can be quite different. We have also compared OMTO3 with TOC derived from the GB spectra (Figure 5c above in the answer to Reviewer #1).

We have written (**lines 307-318**):

"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 (2004) 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, in this study, 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)."

We have made similar studies for the two other sites.

Finally, as a person not directly involved in this field of research, I found this manuscript to be acronym-intensive to the point where it disrupted the flow of the narrative. I actually had to make a table of acronyms so I could follow the logic of the discussion. If the authors don't feel that including such a table would benefit the manuscript, then at least spell them out in the conclusion section of the paper. Most readers will look at the abstract and the conclusions first before reading the remainder of the paper, so aiding these readers would be beneficial.

We agree with the Reviewer that the conclusion would be clearer without a lot of acronyms, so as suggested we have removed many of them.

With my best wishes to the instrument team for an excellent research effort.

Anonymous Referee #3

Received and published: 18 May 2016

We thank the Referee for all his/her helpful comments and suggestions. The reviewer is also thanked for the proposed English language corrections.

General comments

The manuscript by Brogniez is a straightforward paper, describing the difference of satellite- and ground-based UVI measurements at three sites using various statistical quantities. As already pointed out by Referee #2, one important shortcoming of the paper is that the uncertainty of the ground-based measurements is not well quantified. I urge the authors to include a comprehensive uncertainty budget of their measurements when submitted a revised version of the manuscript.

As recommended by both referees we have given a reference for the estimate of the uncertainty budget, **line 115** (Bernhard and Seckmeyer, 1999).

We have also given the web site address where the QASUME reports can be viewed. In the modified manuscript we state that following the QASUME campaigns the raw measurements were reprocessed to account for the observed biases.

We have written (**lines 118-125**):

"During the QASUME campaigns, held for the three instruments, biases were observed: on average about 10% for VDA and OHP instruments and 3% at SDR (local instrument measurements lower than those of QASUME, reports available at http://www.pmodwrc.ch/wcc_uv/wcc_uv.php?topic=qasume_audit). Following these results, the VDA and OHP lamps have been recalibrated in July 2012 at the World Radiation Center, Davos, Switzerland, and all the data reprocessed. A NDACC intercomparison campaign held in July 2015 in Hanover, Germany, and further analysis have shown that the measurements are 3-4 % lower than the reference measurements, that is within the reference measurement uncertainty.

The SDR lamp irradiance has been adjusted to the QASUME irradiance (May 2013), and all the data reprocessed."

We have also given the uncertainty for each instrument separately (**lines 126-128**):

"The irradiance uncertainty leads to an UVI uncertainty for a coverage factor $k = 2$ of 5.3 % at VDA and OHP and 5% at SDR. The remaining biases observed at VDA and OHP are thus within these uncertainties."

At least OMI provides the UVI also at the time of the satellite overpass (other data products include the

UVI at local solar noon and the daily UV dose). Satellite- and ground-based measurements performed at the time of the overpass should agree better than the respective datasets for solar noon that are discussed in the manuscript. For example, changes in cloud cover between the time of the overpass and solar noon would not contribute to differences between the ground- and satellite datasets if the comparison had been based on overpass data. I suggest that the authors also consider a comparison of the difference of satellite- and ground-based observations at the time of the satellite overpass. This does not have to be lengthy.

We agree that a comparison would be interesting but, as stated in the answer to Referee #1, GOME 2 does not provide data at satellite OP time.

Moreover many other works do exist for OMI data at OP time and not for noon, though noon UVI is important for public health.

We have written now in the introduction (**lines 79-80**):

"OMI and GOME-2 websites make available UVI data and maps at solar noon, when values are generally close to the maximum and more risky for health, therefore comparison with ground-based UVI is carried out in this study at noontime."

The manuscript should be thoroughly copy-edited before publication in ACP is considered. For example, the article "the" is frequently missing and many other grammatical errors should also be addressed.

Specific comments

L22: The main difference between versions v1.2 and v1.3 (i.e., treatment of aerosols) should be mentioned here.

We agree, it has been mentioned (**lines 22-25**):

"The present study concerns the period 2009-September 2012, date of the implementation of a new OMI processing. The new version (v1.3) introduces a correction for absorbing aerosols that were not considered in the old version (v1.2). Both versions of the OMI UVI products are available before September 2012 and are used to assess the improvement of the new processing."

L33: "that did not account for absorbing aerosols." should be mentioned earlier (i.e.., L22)

Done (see above)

L65: It should also be discussed here that the ground-based UVI measurement and SDR may not be representative for the UVI of the satellite pixel because the majority of the area contributing to the satellite measurement is the UVI over the ocean. Cloud cover over the ocean may be quite different from that over the mountainous island of La Reunion!

We thank the reviewer for his/her suggestion because indeed this should be specified. We have included the following sentence in the revised manuscript (**lines 71-73**):

¨This site may be not representative of satellite pixel because a large part of the area contributing to the satellite measurement is over the ocean where the cloud cover is likely different from that over the mountainous island."

L50: Delete "Thus, GB measurements are essential for validation of finer scale satellite measurements". (I don't understand what's meant with "finer scale satellite measurements". At least such measurements are not discussed in the manuscript.)

We agree that since we did not study finer scale, this sentence is not necessary, so we have removed it.

L60: "All these OMI validations were conducted using data collected at the time of the satellite overpass." Please explain why your study is based on comparisons for local noon instead.

We choose to validate noon UVI because OMI and GOME-2 websites provide maps for noon and as mentioned few lines later, the UVI is generally maximum. We have rephrased as following (**lines 67-68**):

"In the present study validations are conducted using data at noon, when the UVI is maximum for cloud-free conditions, over a more recent period at three French sites, including a new southern site."

And after (**lines 79-80**):

"OMI and GOME-2 websites make available UVI data and maps at solar noon, when values are generally close to the maximum and more risky for health, therefore comparison with ground-based UVI is carried out in this study at noontime. "

L72: Mention the websites here or point to the appendix where those are provided.

The websites are indicated in the section "Data availability".

L74: Regarding ". . .during about four years (January 2009-September 2012, date of the replacement of OMI version 1.2 by version 1.3). Both versions of OMI data are used to assess the effect of the absorbing aerosol correction that has been recently introduced (v1.3 available since end of March 2014)." The two sentences contradict each other. The first suggests that v1.2. was available up to September 2012 and was then replaced by v1.3. The second sentence suggests that v1.3. is only available from March 2014 onward. Please clarify. Ideally, comparisons of OMI versions v1.2. and v1.3 with ground-based data should be based on the same time period. Was this the case?

Yes, it was the case. We have rewritten the text to clarify (**lines 82-85**):

"… during about four years, January 2009-September 2012, date of the implementation of a new OMI processing. The new version (v1.3) introduces a correction for absorbing aerosols that were not considered in the old version (v1.2). All the archive has been reprocessed with OMI v1.3, so both versions of the OMI UVI products are available before September 2012 and are used in this work to assess the effect of the absorbing aerosol correction."

L104: The period (full stop) in "25.10-3 Wm-2." is confusing. It should be 25 x 10-3 Wm-2.

Done

L105: MAJOR POINT: The uncertainty of the ground-based measurements needs to be discussed in much greater detail. What is the basis of the conclusion that "The irradiance uncertainty leads to an UVI uncertainty for a coverage factor k = 2 of about 5 %?" Is there a paper that could be cited? The results of the intercomparision with the QASUME instrument should also be discussed in detail. I assume that there is a QASUME report that could be cited.

As stated in the answer to the first General comment or the Reviewer above, we give now more information (we give a reference for the estimate of the uncertainty budget (Bernhard and Seckmeyer, 1999), the web site address where the QASUME reports can be viewed and details on the QASUME results).

L194: Can the qualitative statement "variations around noon must be smooth" be quantified?

We cannot quantify because the examination of the diurnal variation plot is made subjectively by a visual inspection. It is the first criterion to detect cloudless sky. The second criterion based on the relative dispersion around the hourly mean is quantitative and checked automatically.

We have made few changes in the text (**lines 212-215**):

"the shape of the curve of the UVI diurnal variations around noon must be smooth (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."

L196: Explain "SEVIRI/MSG"

We have now written **lines 215-216**:

"the SEVIRI sensor on the MSG satellite".

L198: The sentence "One has considered two limits for the distance between the GB station and the cross track position (CTP) for OMI and the grid cell centre point for GOME-2." comes out of the blue. Do you mean "We have considered . . ." If so, what are the limits?

Yes, "one" was for "we", we have replaced it. We have also given here the distance limits (100 and 10 km) that were given later. (**line 218**)

L206: The following recent paper could also be cited here: http://www.atmos-chem-phys.net/15/7391/2015/

It is added: "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".

L220 and all figures with the exception of Figure 5: I don't see a need to use different colors for OMI-v1.3 and GOME-2 in the upper and lower panels of those figures!

It sounds to us practical to distinguish both instruments, so we keep the different colours.

L 455: Regarding "Due to the mountainous topography of. . ." Not only that. A good fraction of the satellite pixel covers the ocean rather than the land where the instrument is located. This must have some effect. For example, cloud cover over the ocean is likely different from that over land.

We agree with the reviewer, so we have written (**lines 483-485**):

"This site may be not representative of satellite pixel because a large part of the area contributing to the satellite measurement is over the ocean where the cloud cover is likely different from that over the mountainous island."

And in the conclusion (**lines 589-591**):

"SDR is difficult for spatial UV estimates because of (i) the mountainous topography of Reunion Island, and thus to the frequent formation of clouds at around midday and (ii) the satellite pixel covers both land and ocean, for which the cloud cover are likely different."

Language:

L13: "in very" > "at very"

Done

L21: Delete "date of the change of OMI data processing. UVI" (the phrase is more confusing than helpful considering that the differences in the processing method implemented after September 2012 are not discussed here).

We study the differences in the derived UVI from the two versions, so we have rewritten the text to clarify (**lines 22-25**):

"The present study concerns the period 2009-September 2012, date of the implementation of a new OMI processing. The new version (v1.3) introduces a correction for absorbing aerosols that were not considered in the old version (v1.2). Both versions of the OMI UVI products are available before September 2012 and are used to assess the improvement of the new processing."

L26: "Correlation" > The correlation"

Done

L27: "for both spatial instruments" > "for both space-borne instruments"

Done

L37: "as is a goal" > "which is the goal"

Done

L73: "confrontation"? Do you mean comparison?

Yes, we have made the change

L81: "are listed" > "are provided"

Done

L109: "to NDACC" > "with NDACC"

Done

L116: "on aura" > "on the AURA" ; "on July 2004" > "in July 2004"

Done

L120: "Thanks to Aura orbit and large OMI swath width" > "Thanks to the AURA orbit and the large OMI swath width"

Following the Reviewer suggestion we have added "the" in 2 places but we have kept Aura (according to the literature).

L134: "the high positive bias between OMI" > "a large portion of the high positive bias between OMI"L150: "on Metop-A platform" > "on the Metop-A platform" L167: "in the same grid." > "on the same grid."L189: "has been made." > "was calculated"

Done

L225 (and similar for OHP and SDR): "means (STD nearly 40., means nearly 21)" > "means (STD nearly 40%, means nearly 21%)" Please add "%" also to similar phrases in the OHP and SDR sections.

Done

L237: "in CS conditions." > "for CS conditions."

Done

L237 (and similar for OHP and SDR): "weak (STD<10., means<8.)" > "weak (STD<10%, means<8%)"

Done

L246: "are reliable." > "statistically robust".

Done

L250 (and other places): "weak UVI" > "small UVI"

Done

L255: Please rephrase. The sentence as it stands is confusing.

We have rephrased the sentence (**lines 281-283**):

"On the other hand, GOME-2 UVI relative differences exhibit seasonal variations, that is due to negative values related to small UVI and large SZA occurring mostly in winter rather than in other seasons."

L291: "As expected, v1.3 product is more reliable than v1.2 one." > "As expected, version v1.3 data are more accurate than version v1.2 data."

Done

L315: "in CS" > "for CS"

Done

L324, L361, L400: "The statistics results" > "Statistics of the results"

Done

L332: What do you mean with "previous study"?

We have specified it is the study for 100 km distance

L345: "than the site one" > " compared to the actual altitude of the site"

Done

L364: "that v1.3 product is more reliable than v1.2 one" > "that the v1.3 dataset if more accurate than the v1.2. dataset.

Done

L365: "black plots" > "datasets indicated by black broken lines"

We have written "black dashed lines"

L373: "on average much far from noon than OMI." could be deleted. also: "SDR site" > "SDR"

Done

L396: "One has checked". Do you mean "We have checked"?

Yes, we have made the change

L416: "weakly reliable" > " not statistically significant"

Done

L442: "Overall, these changes are weak and not significant. This could be understood because, as can be seen in Fig. 5 (blue plots). The aerosol content is small (small AOD (Fig. 5a)) and the aerosols are weakly absorbing (large SSA (Fig. 5b)). Thus the correction factor is close to unity (Fig. 5c), leading *v1.3 products to be little improved compared to v1.2 at SDR." > "The small difference between the v1.2 and v1.3 datasets is due to the small AOD (Fig. 5a) and large SSA (Fig. 5b) at SDR. Correction factors are therefore close to unity (Fig. 5c), resulting in only small difference between the two versions."*

We agree with the reviewer that this formulation is shorter and clearer. We have adopted it (**lines 576-578**):

"The small difference between the v1.2 and v1.3 datasets is due to the small AOD (Fig. 5a, blue dashed line) and large SSA (Fig. 5b). Thus the correction factor at SDR is close to unity (Fig. 5c). "

L454: "to NDACC" > "with NDACC"

Done

L457: "these two latter sites are hindered by aerosols of pollutant origin" > "VDA and OHP are affected by aerosols caused by air pollution".

Done

L463: "is worth" > "is worth considering

Done