Answers to the Reviewer #2 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 #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: "During the QASUME campaigns, held for the three instruments, biases were observed: on average about 10% for VDA and OHP instruments and less than 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, and all the data reprocessed. An intercomparison campaign held in July 2015 in Hanover, and further analysis have shown that the measurements are still 3-4 % lower than the reference measurements.

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: "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).

We have written: "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)."

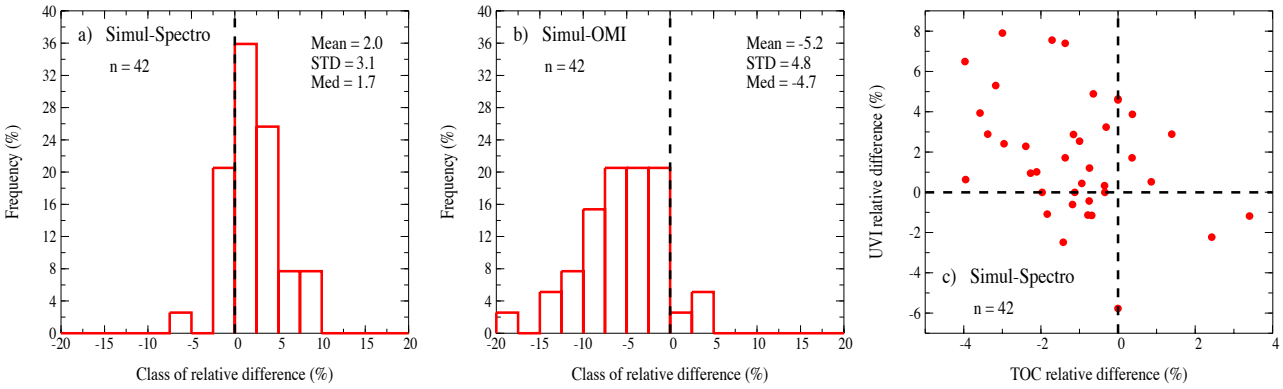

Figure 5.

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 more clear 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.