

Interactive comment on “Validation of satellite-based noontime UVI with NDACC ground-based instruments: influence of topography, environment and overpass time” by Colette Brogniez et al.

Anonymous Referee #2

Received and published: 5 May 2016

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 instru-

[Printer-friendly version](#)

[Discussion paper](#)



ments 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.

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.

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.

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.

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

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-262, 2016.

ACPD

Interactive
comment

Printer-friendly version

Discussion paper

