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Atmospheric Measurement Techniques Discussions

Interactive comment on "A new method for the absolute radiance calibration for UV/vis measurements of scattered sun light" *by* T. Wagner et al.

Anonymous Referee #3

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General comments In general the topic of this study is within the scope of AMT. In my opinion, this is an interesting study with useful results. In summary, I believe that it merits publication in AMT provided that authors incorporate into the following:

Specific comments:

In the abstract, lines 17-19: "For wavelengths below about 330nm it is essential. . .". In my opinion it is important that below 330nm the calibration results, using a constant ozone column density, have to be interpreted with caution, as also mentioned in Section 2.2 and in the "Appendix". And thus it should be mentioned in the abstract and in the

conclusions sections.

In page 5333, line 23: Why the authors choose a 10 nm interval and not a smaller interval? A brief explanation should be inserted at this point.

Page 5335, lines 7-13: The authors mention "the solar cycle" and then discuss for the 2004 - 2007 period. Sun was declining in activity during this period? They should make it more clear to the reader.

In page 5336, lines 11-20 and also in the "Appendix" that I believe it should be incorporated in the main text, the authors discuss about the spatial and temporal differences in ozone profile and column in the area under study using measurements from satellite observations (SCIAMACHY) and from the US standard atmosphere. It would be useful for the reader to add a brief discussion with appropriate references on the comparison-differences between the ozone column and profile measurements derived from satellite observations and US standard atmosphere mentioned here with other most reliable instruments and methods (like Dobson spectrophotometer and ozonesondes) in other places e.g. in Europe. Are there any additional uncertainties from these differences?

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 5329, 2015.