

The article by Vaskuri et al presents a novel method for the determination of the uncertainty of total ozone measurements. Although the findings of the study are interesting, there are parts of the manuscript that have to be improved prior to its publication. In the following there is a list of more specific comments.

### Abstract

P1,L1: replace “calculate” with “estimate”.

### Introduction

P1, L13-14: At this point the authors should make clear that they are talking for correlations in spectral measurements. According to the authors this is the main problem solved when the new methodology is applied. I also suggest adding more information here to help the reader understand what they mean when they refer to “correlations”.

P2, L4-5: Do you mean here that the field of view of the spectroradiometers is equal to exactly one solar diameter? If not, then some scattered irradiance also enters the spectrometer.

### Section 2

The tables 1, 2 and 3 are presented here without any discussion regarding the presented quantities. I suggest that they should be moved to the uncertainty estimation section (section 4). Furthermore, some discussion (e.g. explaining the presented correlation types, description of how the different uncertainty types were estimated) would be useful.

### Section 3

P7, L2: Gröbner and Kerr (2001) did not assume that the air mass factors for aerosols and Rayleigh scattering are equal.

### Section 4

P14, L5: Again, the reference of Gröbner and Kerr (2001) is not correct here.

### Section 5

P17: Please add more information regarding the linear model used for AOD. E.g., why using the particular model for AOD? Are a and b the same with those of Ångström (1964)? If not, how they are estimated? What happens if the TOC is derived by QASUME and BTS using this linear AOD model?

### Conclusions

P18, L27: The results from AVODOR deviate up to 10 DU (and not 10 DU) depending on the SZA. Is the stray light effect enough to explain these discrepancies?

The last paragraph of the conclusions section is now written leads to the conclusion that the main outcome of the study is that the AVODOR is not suitable for TOC measurements, while QASUME and BTS are. In my opinion the main outcome of this study is that the presented method provides more accurate estimations of the uncertainty budget compared to the traditionally used methods. However, it is not adequate for properly estimating uncertainties if the instruments are not characterized for systematic measurement errors. I suggest re-writing the conclusions section in a way that the main conclusions of the study are highlighted.