

Interactive comment on “Measurement of limb radiance and Trace Gases in UV over Tropical region by Balloon-Borne Instruments – Flight Validation and Initial Results” by A. G. Sreejith et al.

Anonymous Referee #3

Received and published: 6 July 2016

The manuscript presents a technical set up to measure vertical distributions of atmospheric trace gases by a balloon-borne limb-viewing UV spectrometer. Although a technical setup is described sufficiently the manuscript does not report any significant scientific results. From the presented results it is unclear if the technique works reasonably well. Advantages and disadvantages of the technique with respect to the well-established ozone sonde measurements are not discussed. Similar measurements performed by other groups are not explicitly mentioned (although some of them are cited in an indirect way). It is absolutely insufficient to present ozone slant column density without converting them into the profiles as well as without presenting any compar-

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ison with independent data. Although it is difficult to judge from the profile of SCDs, my personal opinion is that the profile does not look as expected. Even in terms of SCDs I'd expect the maximum of ozone to be clearly pronounced. The paper does not discuss possible error sources, e.g. influence of pointing error or possible stray light. Unexpected features seen at the plots are not analyzed, these are e.g. an apparent harmonic oscillation seen in the dependence of the dark current on the temperature (Fig. 2), irregularities in the vertical behavior of the radiance with altitude (Fig. 6) or a funny peak seen in the UVA radiance at about 44 deg SZA which is not seen in UVB range (Fig. 7). The retrieval software seems to be used as a black box without thinking too much about the results coming out. For example, the ozone fit in Fig. 8c just cannot be true if a polynomial of the third order is subtracted as stated in the first paragraph at page 6, the fit for the ozone differential absorption structure in the 320 - 330 nm range often used for ozone retrievals looks just terrible. Furthermore, I doubt that the spectrum shown in Fig. 8d is really the ring spectrum (its source is not specified).

In general the paper is absolutely immature to be published in AMT. I'd like to encourage the authors to reconsider the paper by including a retrieval of ozone profile instead of SCDs, comparing to independent results and providing a comprehensive error analysis and then re-submit it.

Interactive comment on *Atmos. Meas. Tech. Discuss.*, doi:10.5194/amt-2016-98, 2016.

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