Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-123-RC1, 2020

Atmospheric Measurement Techniques Discussions

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Interactive comment

Interactive comment on "TROPOMI tropospheric ozone column data: Geophysical assessment and comparison to ozonesondes, GOME-2B and OMI" *by* Daan Hubert et al.

Anonymous Referee #1

Received and published: 24 August 2020

This paper presents the evaluation of TROPOMI tropical tropospheric ozone columns by comparison with ground based (SHADOZ) and other satellite (OMI and GOME2) data. The subject is suitable for publication in AMT and the results are of interest for the users of these data. The paper is well organised and provides valuable information about TROPOMI O3 data. The analysis of error sources is interesting. It clearly highlights the limitations of the CCD retrieval method for UV sensors based on strong assumptions about the variability of the stratospheric column of O3 and on the deep convective cloud cover. I recommend this paper after the issues listed below are dealt with.

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Discussion paper

General comments:

The manuscript is a bit long and could be shortened to improve its readability and efficiency. For instance, section 4.1.1 about the validation of data is too general and too long and does not provide practical and useful information and figures. Section 6 concerning "geophysical information" is providing useful results (e.g. Wave-One, Biomass Burning) but also contains less convincing information about the MJO and Kelvin waves impacts on tropospheric O3 that should be improved or removed (see details below). The conclusion (3 pages) is rather long and detailed. It should be shortened keeping the most prominent and strong results and skipping more hypothetical ones as much as possible. The authors state that "TROPOMI . . . retrievals have reduced sensitivity close to the surface and increased sensitivity above clouds". There is therefore probably a large sensitivity gradient within the troposphere itself with high sensitivity in the upper troposphere and almost none in the boundary layer. Such gradient is probably a large source of error and responsible for large discrepancies between sonde and retrieved data. But the smoothing error is not evaluated and its impact is not taken into account for the sonde versus TROPOMI comparisons. I find it ashamed because a large effort is made to quantify the other sources of uncertainty. It could interesting that the authors provide some more information about this issue and discuss the possible impact of the smoothing error on their results.

Detailed comments:

- p2l38:"some O3 is released by soils and plants". Do the authors mean that O3 is a primary pollutant directly produced by soils and plants? Could you provide references?

- section 4.4:TROPOMI is compared to OMI and GOME-2 and the biases are in good agreement with the OMI versus GOME-2 biases reported by Heu et al. (2016). The authors also document different spatial structure of the TROPOMI versus OMI and GOME-2 biases. Are there some previous studies reporting the spatial structure of the GOME-2 versus OMI bias which would support this result? Do they show a better agreement between sondes and GOME-2 than between sondes and OMI as suggested by the similar biases from TROPOMI with sondes and GOME2?

-p10L296:"assuming that the diurnal cycle in the tropics ressemble the one over Frankfurt". Is this assumption important? Could you justify it with some references?

-p11l314:"assuming these hold for tropical conditions". Could you justify this strong assumption?

-p12l1:"r ranges 45- 75%" with SHADOZ sonde data. This values seem rather weak compared to validation studies of other satellite sensors. Could you compare your results with other tropospehric ozone validation studies for e.g. OMI/GOME-2/IASI and TES in the tropical band (low O3 variability) to put them into some perspective?

- p16l491:could you explain what is a "non-geophysical" outlier and how you identify it?

-p21l644 and Fig. 13: "at Kuala Lumpur four significant peaks. . . in the 30- 60 days". From Fig. 13 we see some significant peaks in the 30- 60 days at other locations as well. This does not corroborate th MJO impact at KL.

- p21l648 and Fig. 14:"the periodic . . . dips in TROPOMI TrOC coincide reasonably well...(Fig. 2, bottom left and 14)". Looking at Fig. 14 the coincidence between TrOC dips and the MJO Index is not very clear. On the contrary,the first and second arrows of deep convection eastward propagation do not coincide with really depleted TrOC and the third one starting on 15 April 2019 even coincide with enhanced TrOC! Furthermore, the large region of enhanced TrOc extending from 180 to 330E from 15 August to 1st November is not related to the MJO Index. The authors should strenghten their analysis of MJO-TroC relationship.

- p21l651 to l662:this part concerning Kelvin waves is not convincing at all. The relation between TrOC high frequency variability and Kelvin waves is only supported by the statement "Period, amplitude. . . are all reminiscent of Kelvin waves". These are rather light arguments! The authors suggest "Further analysis...nedded". I suggest to remove this part and to keep it for another publication.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-123, 2020.