

# Author response to review by Anonymous Referee #2 of “TROPOMI tropospheric ozone column data: Geophysical assessment and comparison to ozonesondes, GOME-2B and OMI”

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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 O<sub>3</sub> 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 O<sub>3</sub> and on the deep convective cloud cover. I recommend this paper after the issues listed below are dealt with.

This is an excellent paper, and should be published, essentially “as is”. I have a few minor suggestions, listed below, that the authors may wish to consider.

## Minor comments

Line 62: “... systematic error and an uncertainty...”? Do you mean “systematic and random uncertainty? An error is usually something you can measure, and correct.

This is indeed what we meant. However, we adhere to the VIM/GUM standard terminology by the Joint Committee for Guides in Metrology (<http://www.bipm.org/en/publications/guides/vim.html>). See definitions 2.16, 2.18 and 2.26 on *error*, *bias* and *uncertainty*.

We slightly reworded the original phrasing to “... target a bias and an uncertainty () ...” and added the footnote : “The ESA documentation uses “bias” and “random error” but the latter is not retained here since several non-random components contribute to the uncertainty. Here, we use the VIM/GUM terms bias (estimate of a systematic error) and uncertainty (non-negative parameter that characterises the dispersion of the quantity values).”

Line 93: “Rather THEY ARE...” (Data are plural).

Corrected as suggested.

Line 117: “...since THEY ARE not publicly released”

Corrected as suggested.

Line 137: “of systematic error in TrOC lies in a SYSTEMATIC DIFFERENCE BETWEEN clear-sky and fully cloudy TOC. While IT IS challenging...”

These sentences were rephrased following the withdrawal of our statement about a cloud-dependent bias in TOC data.

Line 158: The terms “ex ante” and “ex post” will not be familiar to most readers and the authors might like to define them.

Good suggestion. Both definitions were added to the manuscript. “In the following, we use the terms ex ante uncertainty and ex post uncertainty to distinguish the reported uncertainty from the uncertainty estimated from comparisons to other observations”

Line 164: “AN effective vertical resolution...”

Corrected as suggested.

Lines 184-185: “In reality...”. This sounds as though the authors feel that the homogenization process is oversold. It would be more positive to say something like “This has reduced residual systematic differences to about 5%...”.

Rephrased as suggested.

Line 239: You might wish to comment on the variability you expect to see within this three-day window, or at least refer to section 4.1.3.

We added a reference to Sections 4.1.2 and 4.1.3.

Line 273: “In general, the estimation of measurement uncertainty is more intricate than the measurement itself.” An odd statement, and I’m not sure how you would justify it.

See our response in the next comment.

Line 275: “It is therefore good practice not to use these uncertainty estimates blindly.” A bit patronizing... unfortunately the blind don’t know what they can’t see.

We rephrased lines 273-275 to “Calculating accurate measurement uncertainties generally remains a challenge. Reported uncertainties (ex ante) are often first order approximations that, at times, fail to include important, poorly understood sources of error. It is therefore good practice to use ex ante uncertainty estimates with care.”

Line 285: Delete “component in the”.

Corrected as suggested.

Line 300: “to THE TROPOMI overpass time”.

Corrected as suggested.

Lines 302-309: Can you give a rough estimate of the range of magnitude of this error? Is it important?

It is a challenge to assess the uncertainty due to differences in vertical smoothing. We reply in more detail to a similar question by Reviewer #1.

Line 319: "THE smallest random uncertainty..."

Corrected as suggested.

Line 320: Is there any other type of footprint than on the ground?

Indeed, that is redundant information. We removed "ground".

Line 352: "...cyclic pattern, OR whether it was..."

Corrected as suggested.

Line 384-387: "More likely" seems a strong statement. While it is possible, it is a speculation, and the Stauffer paper deals with stratospheric differences.

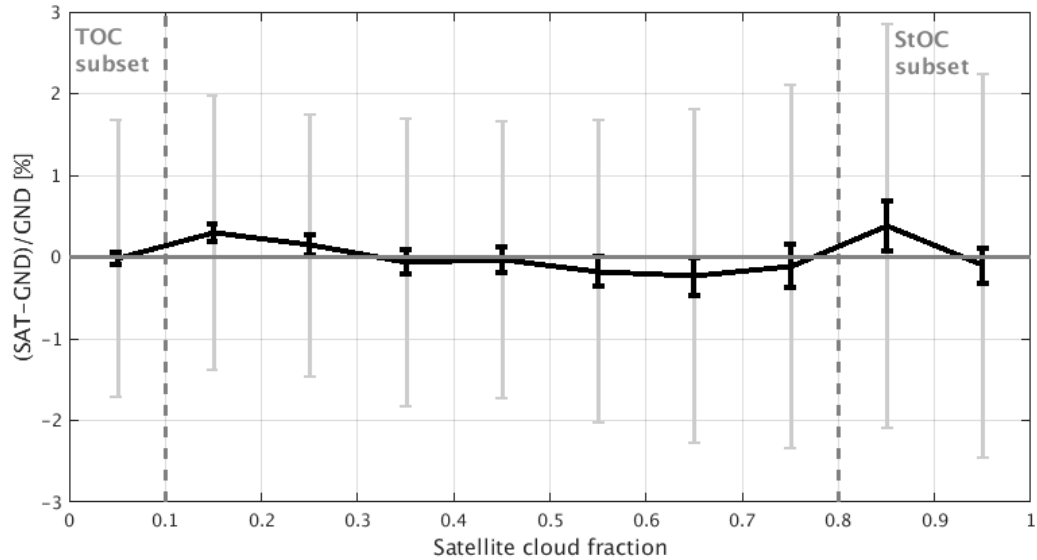
This statement was toned down to "Instead, we suspect that residual instrument-related biases exist between the ozonesonde stations".

The reference to Ryan Stauffer's findings was rephrased to "A similar grouping in the bias between ozonesonde sites has also been noted in total ozone column comparisons between sonde and satellite (OMI and Suomi-NPP OMPS-LP) (R. Stauffer, private communication, 2021)."

Lines 414-429: This seems like an important result that should be noted in the abstract.

The statement was withdrawn since we discovered that zenith-sky observations by Brewer had unintentionally slipped into the analysis. These Brewer data led to a CF-dependence which corroborated the SAOZ results. When restricting the Brewer data to direct-sun measurements only, we find no CF-dependence in S5P TOC data (see figure below). The CF-dependence in SAOZ comparisons are therefore believed to be mainly caused by a (known) cloud dependence in the SAOZ data itself.

Using the updated Brewer data, we estimate the TROPOMI TOC bias CF-dependence (i.e., the difference  $CF < 0.1$  and  $CF > 0.8$ ) at less than  $0.2 \pm 0.2$  % ( $1\sigma$ , standard error of the mean). This translates into at most a negative  $0.4 \pm 0.4$  DU ( $1\sigma$ , standard error of the mean) systematic error in tropospheric ozone. The relevant figure and paragraphs in the manuscript have been modified accordingly.



Line 448: Not sure what a “meridian dependence” is. Do you mean “latitudinal dependence”?

Yes, indeed.

Line 464: “...none of these should have superior calibration...” Is this a condition or assumption for the validity of the method?

We removed the statement as it misleads the reader into thinking it is a necessary condition, which is not the case.

Lines 222, 224 555: “between THE surface”.

Corrected as suggested.

Line 55: “cloud-free”.

Corrected as suggested.

Line 587: “differ”

Is this the correct line number? We couldn't find where this change is needed.

Line 586: “THE location... two-year...”

Corrected as suggested.

Line 589: “...lack of ozonesonde stations...”

Corrected as suggested.

Lines 658, 682: Semicolon, not comma.

Corrected as suggested.

Lines 670-671: This is different from the vertical integrations used by GOME and OMI, which integrate from the surface to 10 km. I didn't see any discussion of these differences. Did I miss something?

Section 3.2 states that the GOME-2B and OMI TrOC data used here are calculated using the same top level as for TROPOMI: 270 hPa. This level lies, on average, around 10.5 km in the tropics (now mentioned in Sect. 2.2 in the revised manuscript). The confusion is perhaps caused by our statement in Sect. 3.2 that the CCI tropospheric ozone data records are generated for a 10 km top level. However, we also stated there that this data version is not used for this analysis.

FYI: a quick analysis based on integrating tropical ozonesonde profiles indicates about 0.5 DU difference between the surface-270 hPa O<sub>3</sub> column and the surface-10 km O<sub>3</sub> column.

Figure 4: I think the caption should define Q50 and IP68. "Blue line and area show median (Q50) and 68% interpercentile (IP68) over the entire period."

Added as suggested.

Figure 5: The yellow dashed line is hard to see.

The yellow dashed line is indeed a challenge to see in print. We will improve its visibility in the revised manuscript.

Figure 14: What does "3-pentad running mean" describe exactly? I presume it's 15 of something... but it mostly tells me that the authors wish to display their knowledge of esoteric English!

This paragraph is completely rewritten after additional analysis and now avoids the use of the *pentad* terminology which is used by NOAA. It is a commonly used term in the meteorology community (<https://glossary.ametsoc.org/wiki/Pentad>).