

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 #2

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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.

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

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

Line 137: “of systematic error in TrOC lies in a SYSTEMATIC DIFFERENCE BE-

TWEEN clear-sky and fully cloudy TOC. While IT IS challenging. . .”

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

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

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%...”.

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.

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.

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.

Line 285: Delete “component in the”.

Line 300: “to THE TROPOMI overpass time”.

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

Line 319: “THE smallest random uncertainty. . .” Line 320: Is there any other type of footprint than on the ground?

Line 352: “. . .cyclic pattern, OR whether it was. . .”

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.

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

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

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

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

Line 55: “cloud-free”.

Line 587: “differ”

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

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

Lines 658, 682: Semicolon, not comma.

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?

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.”

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

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!

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