

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2022-282

Anonymous Referee #1

Referee comment on "Long-term detection, mapping, and interpretation of the trend of ozone in China (1978–2020) by constructing long-term consistent ozone datasets" by Rongqi Tang et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2022-282-RC1>, 2022

General comments:

The paper by Tang et al. performed some analysis of satellite and in situ ozone data. The basic design or idea of the work is valid and could be interesting. The results mainly rely on SBUVs, AIRS, and in situ data. Some basic analyses were done and some comparison and data harmonization work were been performed. However, there are many major issues with the results and conclusions. The editing of the work itself also needs more effort. I only provided some of the issues I found throughout my reading. I feel the work needs more improvement before it can be published. Unfortunately, I would not recommend a publication.

Major comments:

L24-26. This claim is very strange to me. Please provide how this "more significant" is defined.

L26-28. I think the author wants to say this spatiotemporal variation of stratospheric ozone has a strong altitude dependency, which might even have opposite features. Anyway, this is ambiguous to me, and I am not sure I fully understand the meaning of the sentence.

L28. Please clarify what is “the ozone”, i.e., tropospheric column ozone or surface ozone?

L64-69. There are many papers that studied the trends of ozone partial columns and even surface ozone. I would suggest the author include at least several here. E.g., Tarasick et al., 2016; Weber et al., 2022.

L81. AERONET cannot provide any total ozone information, CIMEL sunphotometer is for observations of aerosol optical depth (AOD) and related properties. This is a wrong or at least misleading claim. Please rewrite this.

L104. Well, it’s a strong claim and might not be true. I.e., similar studies have been performed. Please rewrite this.

L123-125. Please clarify which portion of the ozone is described here, total column, stratospheric, tropospheric, or surface ozone (or maybe all?)?

L135. Change “high levels of ozone” to “high surface ozone levels”.

L138-141. Well, please be aware that the satellite has very limited sensitivity to surface ozone. This is determined by the fact that most nadir-viewing instrument samples the total column, in which 90% is from stratospheric ozone. It is very difficult to separate the surface ozone signals. Anyway, the point is, with current techniques, the satellite still cannot replace in situ to provide reliable and/or high-accuracy surface ozone trends.

L163. A proper reference for SBUV/TOMS V8 algorithm is needed.

L186-187. It is logical to me that the author would provide some information on the data resolution here (the one author generated).

L213. Give the coverages of the 28 layers. Also, proper references are needed for this entire paragraph.

L266. Well, after Fig. 2, here the figure number jumps to 7. Some basic editing is missing in this work and some proofreading is needed.

Figure. 3. For any year, with 200+ sites, even using monthly averaged data I would expect more co-incident observations. Why there are only 104–140 points per year? Have any filters been applied? This looks strange.

Figure 4. What is the unit? What is the colour bar stand for? These must be included in the figure and its caption. The regression results also need to be provided, not just RMSE and R.

L453-460. I can not see how the authors can see the larger variation trend throughout the troposphere using Fig. 7. To detect a trend, one must do some calculations. Or, at least, plot something better than Figure 7. I only can see jammed lines.

L512-520. The interpretation of the seasonal variation has too many issues. The author must understand the meaning of whisker plots. Just for example, for 31 km results, we see much more dynamics in wintertime (i.e., see the box and whiskers, not just the mean).

Figure 10. Units for the first two columns are missing. For the 3rd column, there is no description of this "slope of ozone" anywhere. In the caption (L609-610), there are two descriptions for the left columns.

L719-723. I do not want to be harsh. But, this is nothing new. Note that all SBUV series are pretty similar instruments, while AIRS are something very different in terms of observation techniques. Also, even if this is a "new" finding, one should at least provide some quantified numbers here to support the claim.

Technical issues:

L17: Define AIRS.

L45. Use proper notation for wavelength.

L58. Define TOMS and OMI.

L60. Change N to °N, same for S.

L81. Define AERONET.

L83. Capitalize "Gosat" to GOSAT, and define it.

L86. Not that, even if you defined AIRS in your abstract, in the body part of the paper, you must redefine it.

L97. SBUV. Also, definition is needed.

L131. Define NO_x and VOC.

L152. SBUV.

L188. Change NOAA_16 to NOAA-16.

L208. Change "Initial" to "initial".

L218. Use proper notation for the unit. It's not ug, but µg. Same for other places.

L226. Change "hp" to "hPa".

L251. Change "hp" to "hPa". Same for other places.

L271. CDF has already been defined.

L337. Figure 3. Y label is wrong. Change "ddetected" to "detected".

L463. Please do not use * for the multiplication sign. Same for other places.

Reference

Tarasick, D. W., Davies, J., Smit, H. G. J., and Oltmans, S. J.: A re-evaluated Canadian ozonesonde record: measurements of the vertical distribution of ozone over Canada from 1966 to 2013, *Atmos. Meas. Tech.*, 9, 195–214, <https://doi.org/10.5194/amt-9-195-2016>, 2016.

Weber, M., Arosio, C., Coldewey-Egbers, M., Fioletov, V. E., Frith, S. M., Wild, J. D., Tourpali, K., Burrows, J. P., and Loyola, D.: Global total ozone recovery trends attributed to ozone-depleting substance (ODS) changes derived from five merged ozone datasets, *Atmos. Chem. Phys.*, 22, 6843–6859, <https://doi.org/10.5194/acp-22-6843-2022>, 2022.