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Comment on acp-2022-381

Anonymous Referee #2

Referee comment on "Global tropospheric ozone trends, attributions, and radiative impacts in 1995–2017: an integrated analysis using aircraft (IAGOS) observations, ozonesonde, and multi-decadal chemical model simulations" by Haolin Wang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-381-RC1, 2022

General comments:

The research paper by Wang et al. shows decent work on analyzing multi-platform tropospheric ozone observations along with modelling study. The results of tropospheric ozone trends and emission-driven result via aircraft contributions are important for the research community. The model simulation was done with coarse grids, which could be improved, but the general results are solid. I would recommend publishing this work after addressing the following comments.

Specific comments:

Figure 2: The trend of the aircraft emissions should also be included in the figures, not just CEDS results.

L298-299: The simulation is on very coarse grids, i.e., 4°× 5°. Even for mid-latitude regions, the footprint of the model grid would be an area of 300 km × 500 km. How often is such a process needed (especially for ozonesondes)? A related question is, unlike stratospheric ozone, tropospheric ozone has more fine-scale structures (e.g., surface pollution, lightning, etc.). Could you please provide any comments on why not higher resolution simulation was used for this work?

L309: this seasonal bias cannot be seen in Figure 3. If this is provided in other figures or supplements, please indicate it properly.

L326-329: The previous section mentioned the current work (simulation/analysis) already removed contributions from STE. I.e., L272-275 (remove data points with ozone higher than 125 ppbv at altitudes higher than 500 hPa). Is the same filter been applied to Figure 3 (or just the trend part)? If yes, this argument of STE should be clarified carefully. If not, please provide reasoning why not.

Figure 4: The bias between model and ozonesondes is larger and “uniform” (from the surface up to near tropopause) in Polar Regions, when compared to lower latitudes results. Do you think there could be other systematic bias in the model causing such a feature? The sites for Polar Regions are very sparse (only three Canadian sites in the Arctic, and only two Antarctic sites), but all five sites show consistent feature in term of bias (Fig. S2). I.e., it is not an averaging issue.

L342-343: Please clarify that the numbers provided in this paragraph here are only for IAGOS. I saw a discussion of modelled results later (e.g., L386-391).

L344-346: I am not challenging the authors about this seasonal difference, i.e., “largely driven by boreal winter” or “driven by ozone decreases in the summer”. But, the figures provided here are only annual trends. Please provide evidence to support the argument. If this is using figures in the later part, please give some indications.

L371-372: Two issues here. First, Payerne’s sampling rate is not any close to 4 profiles/month. Table 1 says it is 12 profiles/month. Also, four profiles/month is not something “unusual”, i.e., only 6 out of 27 sites have a sampling rate >= 5. So, with current evidence, I could not support that these negative trends detected at some sites are simply due to their low sampling rate at 4. One must find stronger support.

L424-429: This part along with Figure 9 show key information here. Figure 9 panel (b) shows the “Aircraft-drive trends”, while it is a bit confusing which part it will contribute to panel (a). The Aircraft emission is much lower in terms of the tropospheric ozone burden, as described in the previous section. Some better description is needed.

L467-476 & Figures 8 and 9: In terms of trends, the sharp peak in GEOS-Chem simulation in 1998 looks interesting. Authors attributed this to ENSO. However, such a feature is only captured by only a few CMIP6 models, e.g., CESM2-WACCM. This feature is more prominent for GEOS-Chem than any other modelled results. For GEOS-Chem, the peak value in 1998 is only a few Tg Ozone less than the values in 2017. Without this peak, the trend of GEOS-Chem would be more clear and more significant. Any comments or explanations on this feature? From Figure 9, this 1998 peak is not new “emission-driven” at least.

L484-485: I could not support this description about stratospheric ozone being observed having a recovery. Most recent works only show some level of the signal of possible recovery from observations (e.g., Weber et al. 2022; 10.5194/acp-22-6843-2022). One thing the community agreed on is that the Montreal Protocol levelled off the decreasing trend. But, no solid observational-based stratospheric ozone recovery could be claimed yet (e.g., see Weber's results). Please wording this part carefully.

L495: The STE trends in Figure S7 are global average results, not for high latitudes. Trends for different latitude bands should be generated to show the feature and support this argument.

Figure 12: Well, this figure is confusing. I think panel (a) is only a tropospheric column, but why do panels (b) and (c) show results up to 0 hPa? If so, I would assume they are total columns. Anyway, Figure 12b shows that even for the most significant increasing layer (i.e., tropic above 200 hPa), the amount of increase is only at a level of 0.2 DU. Total column ozone is normally at a level of 300 DU. Any comments on the significance of these changes to radiation via total ozone contribution?

L514-524: The description here is strange to me (i.e., it says its total ozone radiative impacts, while Figure 13 says its tropospheric ozone radiative impacts). I think this paragraph is talking about tropospheric ozone radiative impact, not total. Please be specific. Note that tropospheric ozone is only about 10% of total ozone.

L547-549: Well, 1995-2017 results from GEOS-Chem might look slightly "better" when compared with CMIP6. However, it is more important to show comparison results for the same time window. For 1995-2014, GEOS-Chem's trend is only 0.2 Tg year^{-1}. This key result should be included in the conclusion.

L566-568: Could also include % changes in the description. The absolute number here is important but less informative for the conclusion.

Technical corrections:

L126: the notation of accuracy looks very strange, please double check.

Figure 10: unit of the y-axis is missing.

Figures 12b and 12c: unit of the y-axis is missing.

L458: it is not an ozone trend, but a tropospheric ozone trend. Similar to other parts in this work, please be specific.

L469: STE was defined in the previous section.

Figures S2 and S8: unit of the y-axis is missing.

L539: tropospheric ozone increases.