Huber and co-workers present a global scale analysis of NO_2 trends over large cities based on satellite TROPOMI column data. The study covers 6 years of observations, from 2019 to 2024, and includes the NO_2 sudden drop and subsequent rebound in many cities due to the COVID-19 disruptions, as well as air pollution changes in response to policy regulations, economic growth, and armed conflicts. Based on population-weighted annual columns, cities of Asia, Europe and North America show a NO_2 decline, whereas African cities show a moderate increase. Although not strictly comparable, the observed trends and the anthropogenic NOx emission trends from the EDGARv8.1 bottom-up inventory give an indication that emission trends are underestimated in inventories, in particular in the Southern Hemisphere. The manuscript is well written but too lengthy, and the figures seem to support the drawn conclusions.

Thank you for this synopsis, and for taking the time to review our manuscript. We have made substantial changes to both the content and structure of the revised manuscript, and we feel that these changes greatly improve the clarity and quality of our work.

Although I appreciate the importance of the topic, there are several important issues with the paper. First, the trend analysis lacks an estimate of the statistical significance. I have serious doubts that the calculated trends in most cities are different from zero given the uncertainty in the satellite measurement and the limited number of available observations per city. The findings are therefore not sound. It is crucial to include a robust uncertainty analysis, and I argue that part of the conclusions will considerably change.

Second, a major limitation is that the period (6 years) of the analysis is short for attributing the observed trends to anthropogenic changes only, as short-term meteorological variability (not accounted here) might partly explain the observed trends. Furthermore, the fact that the study period includes the COVID-19 lockdowns makes the derivation of trends even more uncertain. I recommend extending the analysis to a longer period, by combining OMI and TROPOMI datasets, both having the same overpass (e.g. Glissenaar et al. https://doi.org/10.5194/essd-2024-616).

Third, the seasonal variation of NO_2 data is not adequately discussed. It is unclear whether a threshold of available observations per month was used and why the May-to-September trends are not compatible with the November-to-March trends in some regions, e.g. Europe. Overall, the discussion is poor, and the scientific content is limited. To my opinion, the main problem of the manuscript is the lack of innovation and inadequate analysis. Therefore, I cannot accept this manuscript for publication in ACP.

Thank you for providing this thoughtful feedback. Below, we list the ways in which the above concerns have been addressed. We are confident that the value and quality added to the manuscript in including these substantial changes will be apparent.

1. In this revision, we have performed an uncertainty analysis, being sure to provide statistical significance where relevant. We have made sure to note clearly if a trend is or is not statistically significant. After performing this analysis, our original findings have been strengthened.

2. We do not claim that the observed changes are only anthropogenic in nature; we are simply reporting observed concentrations. Previous work has shown that meteorological variability has lesser influence when aggregating to the country or continental level as opposed to at the local level (Petetin et al., 2020), and our presented evaluation of NO_2 VCD urban enhancements against anthropogenic emissions inventories is only presented at the urban continental level. Despite this, even if hypothetical changes in NO_2 over a six-year period were to be driven by meteorological as opposed to anthropogenic factors, we still find that such a trend would be worth reporting. We have added the following text to line 510 in the conclusions of the revised manuscript to acknowledge these limitations:

   "Additionally, while many of the trends presented here reflect changes in anthropogenic NOx emissions, it is important to recognize that atmospheric chemistry also influences the observed NO_2 variability. Seasonal differences in photochemical lifetimes (i.e., longest in winter), boundary layer mixing (i.e., more vertical mixing in summer), chemical partitioning between NO and NO_2 (i.e., the fraction of NO_2 is largest in winter) and meteorological variability can all modulate the magnitude and timing of observed trends. These processes likely contribute to some of the regional and seasonal differences highlighted in this study".

3. We appreciate the suggestion to include a longer time series by including an evaluation of OMI trends, however, there are instrument (i.e., coarser resolution) and algorithm differences that make a stitching of these two datasets complex. The inherent advantage of this work is that we are using a single instrument with a consistent algorithm throughout, to calculate the trends. There has been prior work documenting NO_2 trends between 2005 – 2019 using OMI and it would be repetitive to repeat that work. We instead cite the OMI NO_2 trends papers to give the reader references to refer to.

4. The novelty in this paper is the timeframe and global expansiveness (11,000 cities) of the analysis that we are conducting. To our knowledge, no published paper reports satellite-based NO_2 trends through the year 2024. We are documenting urban and country-level NO_2 trends that, before this paper, have not been documented in the literature. For example, it has not been documented that Seoul has the largest NO_2 drop between 2019 and 2024 and that Tehran has the largest NO_2 burden in 2024; this scratches the surface of newly documented NO_2 trends that we report herein. This is an important scientific advancement because we are documenting which urban areas appear to be implementing effective control strategies and which are not. This has implications for future control strategies.

5. Thank you for the questions regarding seasonality. We have expanded our discussion in the revised manuscript to more clearly discuss those results. The noted difference between May-September and November-March in Europe is driven by Western Russian cities, which caused the initial spike in Winter 2022. We have added the same figure to the supplement with Russian cities removed (Fig. S17), to show that seasonal trends match more closely when excluding Russian cities.

Specific comments:

- L.112-116: Any data filtering used?

Thank you for allowing us to clarify. We are using a level 3 data product (Goldberg, 2024), which applies a filter to remove all pixels with a qa_value filter < 0.75. We have added the following text to line 122 of the revised manuscript to make this clear:

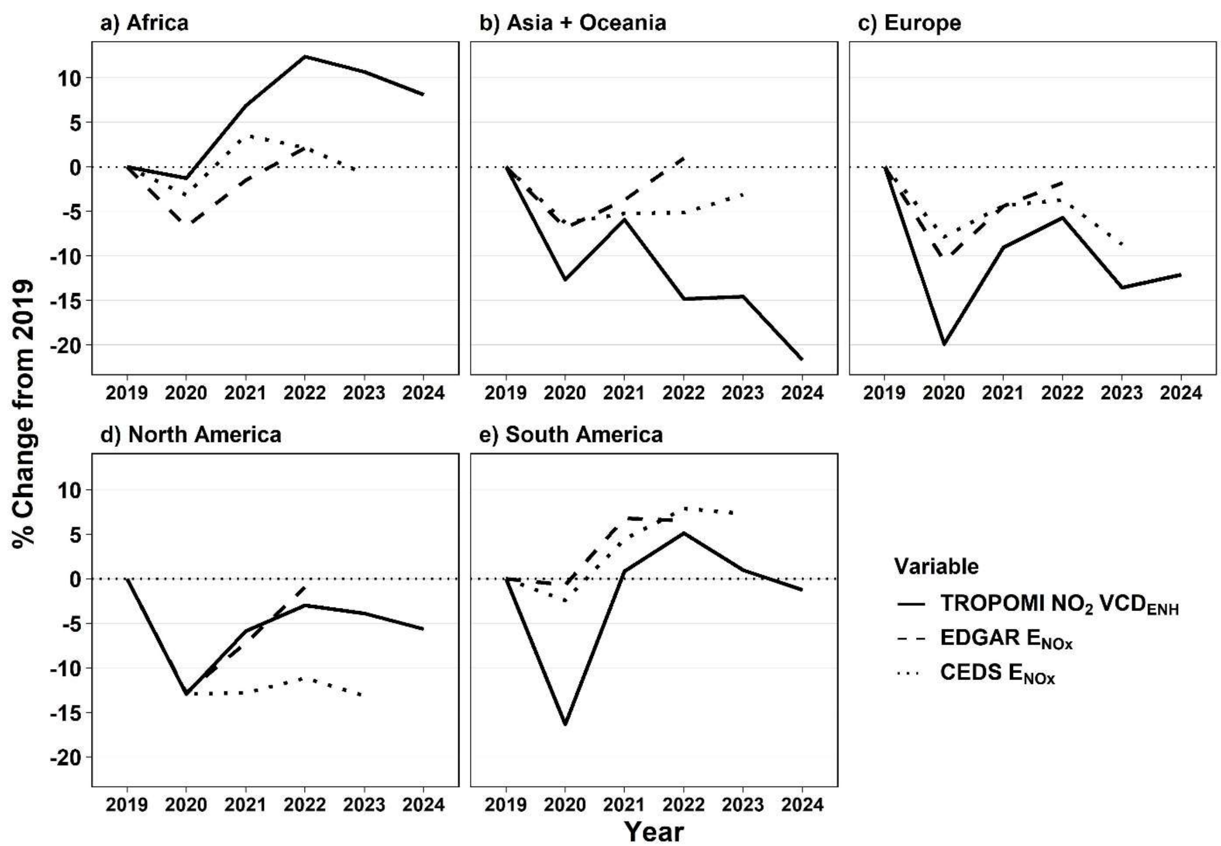
"Data were quality controlled to remove Level 2 pixels with a qa_value < 0.75 before oversampling, which removes data with quality issues related to clouds, surface reflectivity (e.g. snow and ice) or other retrieval errors".

- L. 57: Read SCIAMACHY

Thank you for pointing this out. We have corrected the error.

- Why not using CEDS (instead of EDGAR)? The latest CEDS version covers 2023.

Thank you for this suggestion. We have now expanded the analysis to include an evaluation of EDGAR as well as CEDS NOx emissions. Results can be found in Section 5 of the revised manuscript.



- A table summarizing the percentage changes would be useful

We appreciate this suggestion, however given the large number of cities (11,000+), we find including a table of all percent changes would not be realistic.

- Figs 2, 4, 5, 6 are similar and could be grouped in 2

Based on this suggestion, we re-grouped some of the figures from the original submission to have one main figure per continent, which more effectively groups results based on geographical location.

- Unclear whether the strong column changes in Quito and Santos are due to changes in anthropogenic activities, or to data issues.

The level 2 TROPOMI data used to produce the level 3 product used in this paper (Goldberg, 2024) are quality controlled to remove all pixels with a QA_flag value < 0.75, which remove pixels with data issues. There is no evidence for the changes in these cities being attributed to "data issues". We do not claim that these changes are a direct result of anthropogenic activities; we are simply highlighting cities that exhibited notable changes in NO_2 VCD.

- For which among the cities of Fig.7 are the trends significant?

Thank you for this question. For the labeled cities in each panel, we have noted which represent a statistically significant trend with an asterisk. These results are now spread across Figures 6-11.

References

Goldberg, D.L..: HAQAST Sentinel-5P TROPOMI Nitrogen Dioxide (NO2) GLOBAL Monthly Level 3 0.1 x 0.1 Degree Gridded Data Version 2.4 (HAQ_TROPOMI_NO2_GLOBAL_M_L3) at GES DISC, Goddard Earth Sciences Data and Information Services Center (GES DISC), https://doi.org/10.5067/KKPPL39PEIGE, 2024.

Petetin, H., Bowdalo, D., Soret, A., Guevara, M., Jorba, O., Serradell, K., and Pérez García-Pando, C.: Meteorology-normalized impact of the COVID-19 lockdown upon NO2 pollution in Spain, Atmos Chem Phys, 20, 11119–11141, https://doi.org/10.5194/ACP-20-11119-2020, 2020.