We sincerely thank the reviewers for their suggestions and comments that helped to improve our manuscript.

Please find below the answers to your concerns. Your comments are in black, our responses in red and the text added to the manuscript is highlighted in cyan.

To date there are few air pollution studies being conducted in African megacities despite the fact that many of these cities suffer from poor air quality. The aim of this paper was to compare ~2 years of ground-based MAX-DOAS observations with satellite column observations over the city of Kinshasa in the DRC. In the study, the authors explore 3 different retrieval methodologies and use linear regression statistics to comment on the robustness of each method explored. A comparison between satellite retrievals (TROPOMI) and model observations (GEOS-Chem) is also presented and discussed in the manuscript.

This manuscript provides measurements of NO_2 and H_2CO in a region where information about these pollutants is lacking. In addition, this manuscript adds to the growing body of literature examining comparisons between satellite and ground-based measurements. I think that this paper is well written and the scientific approach taken by the authors in general is sound. I think that the paper could benefit from both an expanded discussion about the differences between retrieval cases and expanded discussion of the model/measurement comparison results. I would recommend this paper for publication after the following general and specific comments are addressed:

We sincerely appreciate your pertinent feedback. The revised version has incorporated several modifications, including:

1. The GEOS-Chem and TROPOMI comparisons have been removed due to the model low horizontal resolution.
2. A bug was found in the algorithms, and its correction had an impact in terms of comparison results for both molecules.
3. In the revised version, we present the results of monthly comparisons instead of the daily comparisons featured in the previous version.
4. The H_2CO measurements have been reanalyzed due to an identified issue in the previous analysis (see the accompanying explanation letter for the product change). As a result, a new H_2CO product is utilized in the revised manuscript.
5. Consequently, all figures and tables comparing TROPOMI and MAX-DOAS have been modified to accommodate the new products.
6. In the revised version, we employ daily median profiles instead of seasonal median profiles. The use of seasonal median profiles was necessitated by the lack of H_2CO data, particularly during the dry season. Now, with the availability of data for all days using the new product, we have opted to use daily median profiles in accordance with Dimitropoulou et al. 2020[1].
7. Section 3.2 has been moved to section 2.4 (revised version) for improved readability, as suggested by one of the reviewers.

[1] Dimitropoulou et al. 2020:: Validation of TROPOMI tropospheric NO2 columns using dual-scan multi-axis differential optical absorption spectroscopy (MAX-DOAS) measurements in Uccle, Brussels, Atmos. Meas. Tech., 13, 5165–5191, https://doi.org/10.5194/amt-13-5165-2020, 2020

8. Figures 11 and 12 have been merged into a single new figure (Figure 4: revised version). A new figure (Figure 5: revised version) has been included to illustrate the approach of case 3. Additionally, figures A1 and B1, previously located in the appendix, have been integrated into the main text of the revised version (Fig. 9 and Fig. 11: revised version).

General comments:

Section 2: There are some missing details in the methods that should be added to or moved to this section. There should be more instrumental details presented here or Section 2.1 should reference a previous paper where the details can be found. There are also a few details noted later in the manuscript (eg. Filtering techniques/criteria, TM5 model, etc.) that should be described in Section 2 first. Finally I think it would be nice for Figures 12 and 13 to be presented in this section to better understand the NO2 and H2CO distribution, as opposed to later in the manuscript, and the oversampling technique should also be described here.

We have moved the information on comparison approaches to a new section called "Intercomparison methodology" in section 2.3. You'll also find some new information and a new figure illustrating the case 3 approach. Figures 11 and 12 have been moved to the introduction and grouped together in a single figure, currently called Figure 4.

Section 2.5: In general, the statistical results in section 3 are presented well. The statistical analysis and filtering methods should be clarified in more detail here.

Thank you. In the revised version, three distinct paragraphs have been added to each of the following subsections:

2.1 (Lines 98-109): Additional information about the instrument has been included.

2.2 (Lines 143-155): A paragraph detailing the filtering criteria for MAX-DOAS data has been added.

2.3 (Lines 190-204): A paragraph explaining the selection criteria for TROPOMI data and an explanation of the oversampling technique used to produce Figures 11 and 12 (now combined into Figure 4 in the revised version) has been included.

2.5: This section is newly introduced. It stems from the relocation of the former subsection 3.2. Furthermore, explanations regarding Approach 3, coupled with two new figures (Figure 4 and Figure 5), have also been incorporated.

For further specifics, please refer to the specific questions.

Section 3.1: This section would benefit highly from an expanded discussion of seasonal and diurnal trends (see more specific comments below). Or the authors could consider moving some of this discussion to Section 4 instead. I think the diurnal and seasonal trend analysis should also be revisited and further explored.

We chose not to delve further into this section due to the limited information available. Specifically, the daily variations for both molecules are relatively minor, with elevated peaks observed in the early afternoon, although these peaks are scarcely discernible for H_2CO. While

these trends may appear subtle, we opted to include an explanatory paragraph to highlight these nuanced behaviors (lines 314-323).

Regarding NO2 VCDtropo, we note a weak diurnal increase of similar amplitude during the 3 periods mentioned above. In the case of H_2CO VCDtropo, the diurnal variation (also similar during the 3 periods) seems to be characterised by a maximum around noon. This behavior could be related to the diurnal pattern of biogenic emissions and fires. Isoprene emissions are favored by light and warm conditions (Guenther et al., 2006). Most of the fires occur around noon (70%) and 13h (22%), as reported by Cizungu et al. (2021) at the Luki Biosphere Reserve (5.5◦N, 13.3◦E), close to Kinshasa. The warmer and drier weather from noon onward is favoring the occurrence of fires and their spread. This would affect the H_2CO production with some delay, due to the VOCs oxidation. Oxidation of biogenic VOCs such as isoprene and monoterpenes leads to H_2CO typically after a few hours (Marais et al., 2012). For pyrogenic VOCs, their lifetime is highly variable, from a few hours to several days (Stavrakou et al., 2009).

Section 3.3: I think comparing observations with model outputs always provides some valuable insight. Ideally I would like to see a comparison of the MAX-DOAS measurements with GEOS-Chem here, but I agree because of the spatial resolution differences this wouldn't make sense. Similarly, I think it doesn't make sense to compare only a single (or a few) model points with TROPOMI given the coarse resolution. I think the section could largely benefit from expanding the domain of the comparison between TROPOMI and GEOS-Chem to a larger region across the DRC. Since the distribution of H_2CO and NO_2 are very heterogeneous this could provide insight into some of the model/measurement differences across a broader region and more statistics would be available.

We agree that the interest of the TROPOMI versus GEOS-Chem comparison was limited due to the small investigated area, and that this was not informative enough. The other reviewer also pointed out this weakness of our study and suggested to remove GEOS-Chem from the paper, except the profile. We followed this advice and the comparison between TROPOMI and GEOS-Chem will be the focus of a future study.

Section 4.1: I would like to see an expanded discussion of the differences between Cases 1, 2 and 3 and justification for picking case 3 for NO_2 and why exactly case 3 did not work as well for H_2CO. For example, I would possibly argue that case 2 for NO_2 showed a stronger correlation with TROPOMI and that the monthly averages also followed a more similar trend in case 2 vs case 3. I am not suggesting one method is better than the other, I just think a more detailed discussion here is needed as to why the slopes, correlations, trends may have changed in the way they did in all three cases.

In the revised version of our study, we have indeed found that Case 3 no longer appears to be the most relevant solution. This update to our conclusions stems from the obtained results, which have demonstrated more favorable performance with case 2, particularly in terms of correlation and consistency of monthly median difference, for both studied molecules.

However, it is crucial to note that Case 3, although less effective, should not be definitively dismissed. The limitations identified in this context, primarily related to the reduction in sample size and associated noise, highlight the need to approach with case 3 with caution. This observation underscores the significance of a diligent evaluation of each approach within the specific context of the study.

Section 4.2: This is a very short discussion section mainly about model uncertainties in general. I would suggest either moving some of the points made in this section to Section 3.3 or expanding upon it with the inclusion of additional model/measurement comparisons moving forward.

This part was removed from the revised manuscript.

Figures: Figures should be proofread and make sure the panel labels and legends are all present and readable.

Indeed, all the figures have been reviewed and retouched or amended according to the various changes made.

Specific comments:

Line 20: Formaldehyde has strong biogenic sources and signatures as well (eg. Biomass burning, secondary formation from isoprene emissions). Consider discussing here, as you do later in the introduction, that H_2CO can be a marker of both anthropogenic and biogenic sources.

We added the following sentence to the introduction (line 34-35 : revised version).

These compounds are also strongly emitted by fires and the biosphere; and H_2CO is also considered an excellent marker of biogenic VOC emissions (Stavrakou et al 2009[2] ; Bauwens et al., 2016)[3].

Line 22: This is not true, under high NO_2 conditions ozone production can decrease as ozone gets titrated in the atmosphere. I would suggest referencing more sources here (eg. Seinfeld and Pandis, 1998) and rewrite to say that VOCs and NO_2 react in a non-linear manner to form ozone in the atmosphere.

We made the following changes in the manuscript (line 38-39).

The VOCs and NO_2 react in a non-linear manner to form O_3 in the atmosphere (eg. Seinfeld and Pandis (1998)).

Line 80: Please include more instrumental details here, such as spectrometer characteristics, optical head setup, fiber guide etc. Or reference previous manuscript where these details can be found.

The following paragraph has been added to section 2.1(lines 98-109 : revised version).

The MAX-DOAS is an upgrade of the single-axis DOAS instrument described in more detail in our previous study (Yombo Phaka et al., 2021). The spectrometer is an Avantes ULS2048-XL with a spectral range of 280-550 nm and spectral resolution of 0.7 nm (Full Width at Half Maximum), Light enters the spectrometer through a lens connected to an optical fiber 600 micrometers in diameter. The upgrade first consisted in installing this spectrometer and a single-board computer (PC-104) in a box, which is air-cooled with a fan and where we also installed a temperature sensor. This box is located under the roof of UniKin. Secondly and more importantly, we added an optical head on the roof, to perform elevation scans. This optical head is based on a home-made box of dimensions 22×14×8 cm3 mounted on a pod at 45° and

[2] Stavrakou et al. 2009 : Evaluating the performance of pyrogenic and biogenic emission inventories against one decade of space-based formaldehyde columns, https://doi.org/10.5194/acp-9-1037-2009

[3] Bauwens et al 2026 : Nine years of global hydrocarbon emissions based on source inversion of OMI formaldehyde observations, Atmospheric Chemistry and Physics, 16, 10 133–10 158, https://doi.org/10.5194/acp-16-10133-2016, 2016.

pointing 5° West of the North, i.e. towards the city. Light enters the box through a fused silica window and hits a flat elliptical mirror of minor axis 26.97 mm coated with enhanced aluminum. This mirror is attached to a HITEC servomotor (HS-7985MG) and scans between the horizon and zenith at multiple angles above the horizon (0°, 1°, 2°, 3°, 4°, 5°, 6°, 7°, 8°, 15°, 30°, 45°, 88°). The mirror reflects the light to a fused silica plano-convex lens of diameter 25 mm and focal length 50 mm, which focuses the light on the optical fiber. In each mirror position, we accumulate light for 50 seconds leading to a total scan time of about 10 minutes

Line 113: Please explain the inconsistencies here and why only MMF retrievals were considered.

The following paragraph has been added to the revised version (lines 145-155).

Currently, in FRM4DOAS, MAPA is mainly used as a quality check, but it does not provide averaging kernels. Due to a sampling effect, using MAPA as a quality check for H2CO introduces a bias in the statistics. Higher VCDs are more likely to be flagged out, leading to discrepancies between MAPA and MMF. When assessing Aerosol Optical Depths (AODs), it becomes evident that MMF-produced AODs closely align with MODIS AODs, while MAPA-derived AODs consistently surpass both MMF and MODIS. We therefore opted to exclude MAPA from this study. Consistency is maintained by applying the same flagging criteria to NO_2. Only MMF values for which the quality assurance (QA) is lower than 2 were used. Three conditions should be met to establish this flagging (QA < 2). Firstly, scans with a degree of freedom (dof) below 1.3 are excluded. Secondly, all scans with an average root-mean-square (RMS) (between measured and simulated dSCDs) larger than 4 times the QDOAS estimated dSCD error are excluded. Furthermore, due to lack of good a priori knowledge for the aerosols, two aerosol retrievals are performed (differing by a factor 10 in AOD). If the retrieved aerosol profile agrees well, only 1 trace gas retrieval is performed and no extra test is applied. If however the retrieved aerosol profile differs more than 10% (as average partial AOD in each layer), the trace gas profile is performed with both aerosol profiles and all scans for which the retrieved VCD differs more than 10% are flagged as invalid.

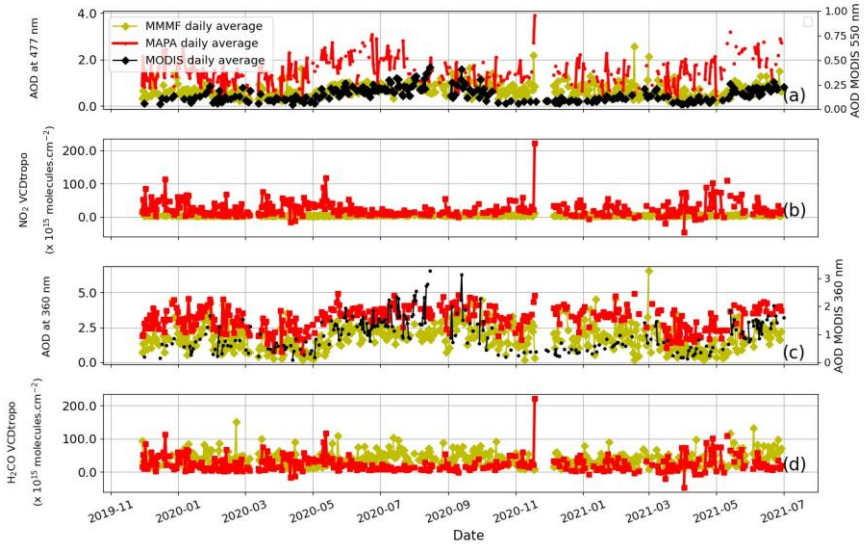


Figure A : MMF and MAPA products overview

Table 2: Was there a reason a scale height of 1 km was initially used in the MMF retrieval for NO_2 and H_2CO or was this the default setting?

1 km is the default setting. 1 km scale height is common to use for OEM (optimal estimation method) retrieval codes (Karagkiozidis et al.(2022)[4]; Tirpitz et al.(2021)[5] ; Frieß et al.(2019)[6]).

Line 132: Why was a radius of 20 km used to select TROPOMI and GEOS-Chem for comparison? And why wasn't an expanded range used for the satellite/model comparisons?

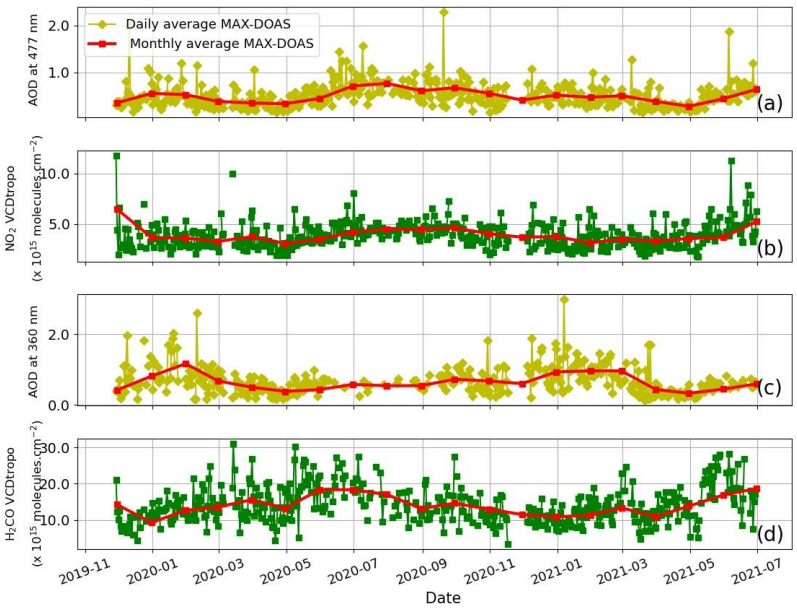
We removed the comparisons between TROPOMI and GEOS-Chem from the revised manuscript.

Line 149: The model is currently initiated with coarse (2 x 2.5 degree) resolution and is run for the entire globe. Given the fine resolution of TROPOMI I think it would be more suitable to run GEOS-Chem with a finer resolution and for a subset of the global domain. A finer run model setup could potentially also be used to compare with the MAX-DOAS measurements.

We agree, see previous comment.

Figure 4: I would also like to see the monthly average measured by MODIS in this figure. And the MODIS measurements should be represented with their own y-axis since AOD is measured at 550 nm. Consider moving MODIS measurements to the supplement and referring to the figure there since it is not discussed in detail in the body of the manuscript.

We agree, see new figure below.



[4] Karagkiozidis, D. et al.2022 : Retrieval of tropospheric aerosol, NO_2, and HCHO vertical profiles from MAX-DOAS observations over Thessaloniki, Greece: intercomparison and validation of two inversion algorithms, Atmos. Meas. Tech., 15, 1269–1301, https://doi.org/10.5194/amt-15-1269-2022, 2022.

[5] Tirpitz, J.-L. et al. 2019.: Intercomparison of MAX-DOAS vertical profile retrieval algorithms: studies on field data from the CINDI-2 campaign, Atmos. Meas. Tech., 14, 1–35, https://doi.org/10.5194/amt-14-1-2021, 2021.

[6] Frieß, U. et al. 2019 : Intercomparison of MAX-DOAS vertical profile retrieval algorithms: studies using synthetic data, Atmos. Meas. Tech., 12, 2155–2181, https://doi.org/10.5194/amt-12-2155-2019, 2019.

Figure 7. MAX-DOAS aerosol optical depth (AOD) measured at 477 nm (panel a) and 360 nm (panel c) and VCDtropo of NO_2 (panel b) and H_2CO (panel d) measured between November 2019 and July 2021. In each panel, both daily and monthly averages are displayed.

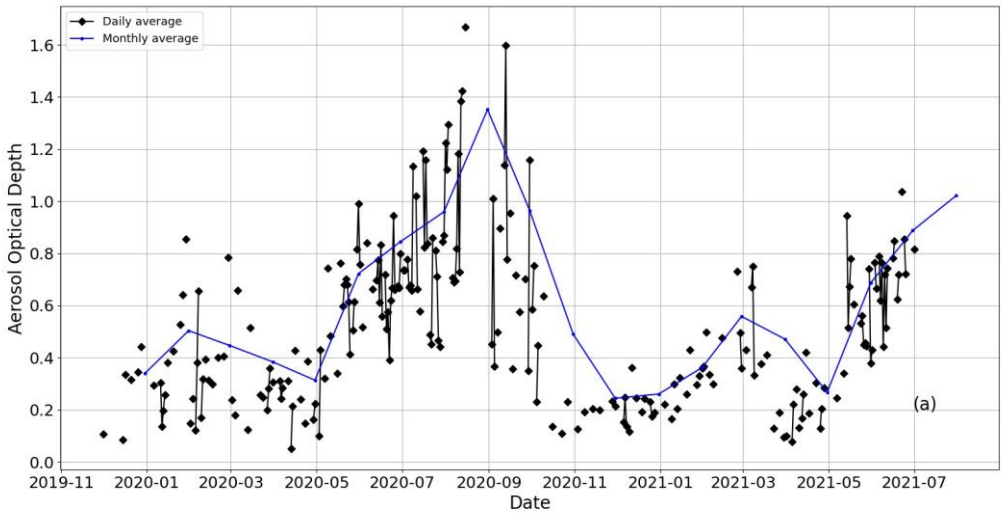


Figure D1. Time series of the monthly aerosol optical depth (AOD) observed at 550 nm wavelength by the MODIS Terra instrument downloaded from https://giovanni.gsfc.nasa.gov/giovanni/ for an area covering the city of Kinshasa (3–5°S, 14–16°E).

Line 168: Is there any contribution from biomass burning in the dry season to the increase in AOD? Or is it only from accumulation of dust?

Certainly, the dry season is influenced by the dual contribution from two sources: biomass burning and dust. Our firsthand observations as residents of this region unequivocally confirm the presence of both fires and dust. However, the accurate quantification of these sources warrants a distinct investigation, potentially involving the application of chemistry and transport models. We intend to conduct such a study in the future.

Figure 5. Missing letter labels for the panels and it is hard to see the letters/time periods within the figures. Please describe all panels clearly in the figure description.

Agreed. Here is the modified figure.

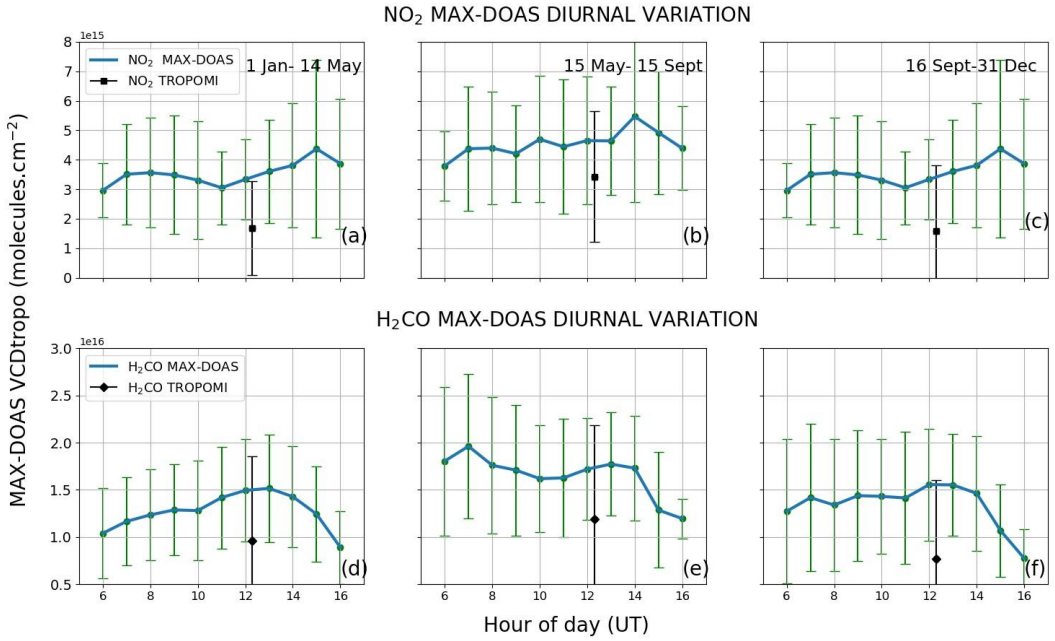


Figure 8. Mean diurnal variations of NO_2 VCDtropo (panels a, b, c) and H_2CO (panels d, e, f) observed by the MAX-DOAS instrument (blue dots) and by TROPOMI (black dots) over the city of Kinshasa between November 2019 and July 2021. The error bars represent the (1-σ) standard deviation of VCDtropo computed for each hour within the specified period.

Lines 179 – 184: I would like a more detailed discussion here of the diurnal patterns observed or else these lines should be removed or moved to the discussion section. To me it looks like there is no diurnal pattern in H_2CO in the dry season and that there is more of a clear pattern during the wet seasons. The clear increase and decrease throughout the day in the wet seasons looks like it could be either a biogenic or anthropogenic signature. The higher H_2CO concentrations in the dry season seem more consistent with biomass burning dominating. In addition, the diurnal pattern in NO_2 is not explored in this section and it follows a different pattern than H_2CO with a maximum later in the day.

NO_2 shows a similar behavior in all 3 periods, with relatively low variations. For this reason, we have focused on the H_2CO section, which peaks around midday, early afternoon.

We have added the following text (line 317-318), based on the new figure 8 (revised manuscript), taking into account the new product H_2CO.

Regarding NO_2 VCDtropo, we note a weak diurnal increase of similar amplitude during the 3 periods mentioned above. In the case of H_2CO VCDtropo, the diurnal variation (also similar during the 3 periods) seems to be characterised by a maximum around noon. This behavior could be related to the diurnal pattern of biogenic emissions and fires. Isoprene emissions are favored by light and warm conditions (Guenther et al., 2006). Most of the fires occur around noon (70%) and 13h (22%), as reported by (Cizungu et al., 2021) at the Luki Biosphere Reserve (5.5N, 13.3E), close to Kinshasa. The warmer and drier weather from noon onward is favoring the occurrence of fires and their spread. This would affect the H_2CO production with some delay, due to the VOCs oxidation. Oxidation of biogenic VOCs such as isoprene and monoterpenes leads to H_2CO typically after a few hours ((Marais et al., 2012)). For pyrogenic VOCs, their lifetime is highly variable, from a few hours to several days (Stavrakou et al., 2009).

Line 203: Please provide more details on how the coincidence test was performed.

We have added the following text (line 229-234).

The selection of TROPOMI pixels in the MAX-DOAS viewing direction is performed in three steps illustrated on Fig. 5. First, a horizontal profile (0 to 10 km) is created, consisting of 20 equally spaced points (distance 0.5 km), starting from UniKin (4.42° S, 15.31° E) and oriented in the viewing direction of the instrument (355°). Second, geographical coordinates are assigned to each of the points. Finally, among the pixels lying within 20 km of the observation site (24 in Fig. 5 a), only a few pixels cross the created line (3 pixels in Fig. 5b). Those are the pixels selected for the test within the MAX-DOAS line of sight.

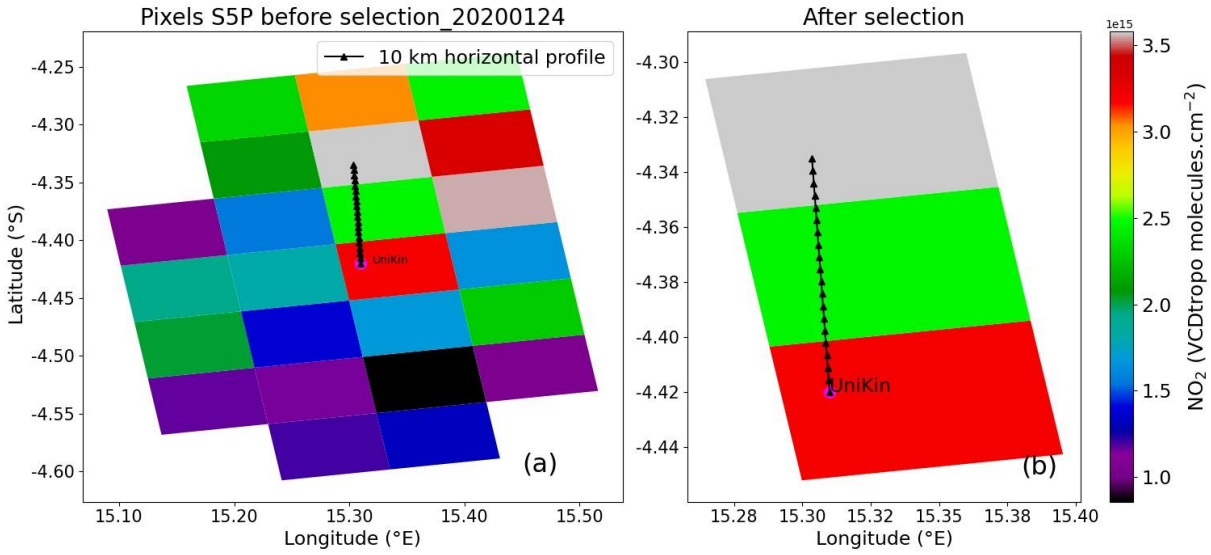


Figure 5. Illustration of the approach taking into account the pixels along the MAX-DOAS viewing direction. Panel (a) shows all the pixels selected within a 20 km radius of the UniKin and panel (b) shows the pixels selected along the viewing direction shown as black line.

Line 206: Why were MAX-DOAS measurements averaged for a time interval of 1 hour around the TROPOMI overpass time?

In the initial version of the study, we chose to average measurements over a time window of approximately one hour around the TROPOMI satellite overpass time. This approach was driven by the variability in the lifetimes of the studied molecules, which spans around a few hours. In the revised version, we have opted to perform the averaging specifically at the time of the satellite overpass, aiming to enhance the alignment of our measurements with satellite observations.

Line 216: I like the discussion of Figure 6 and how it is presented as a motivation for constraining the comparison using different cases that take into account vertical and horizontal sensitivity. Do you know what might be causing the TM5 vertical profiles to be very different from what you are measuring?

First note that this figure was updated with the new H_2CO MAX-DOAS product. Aside from the errors due to emission inventories, there are three possible explanations for the differences between the TM5 profiles and what we measure.

1. The TM5 profiles come from a model with a coarse spatial resolution of 1° x 1°, whereas our profiles are inverted using observations from an instrument with a high sensitivity close to the surface.

2. In a model like TM5, input parameters such as weather and emission inventories play an important role in the simulations. However, in African areas such as Kinshasa, these parameters are less studied and therefore less realistic than in other parts of the world such as Europe. This could lead to less realistic profiles compared to the ground-based measured profiles.

3. The values in the MAX-DOAS profile stop at 4.5 km, i.e. only in the first layers close to the ground, whereas in the TM5 model there is also a contribution from the free troposphere (see figure below: Figure C1 version revised). The difference between the two can have major consequences for comparisons, especially with H_2CO.

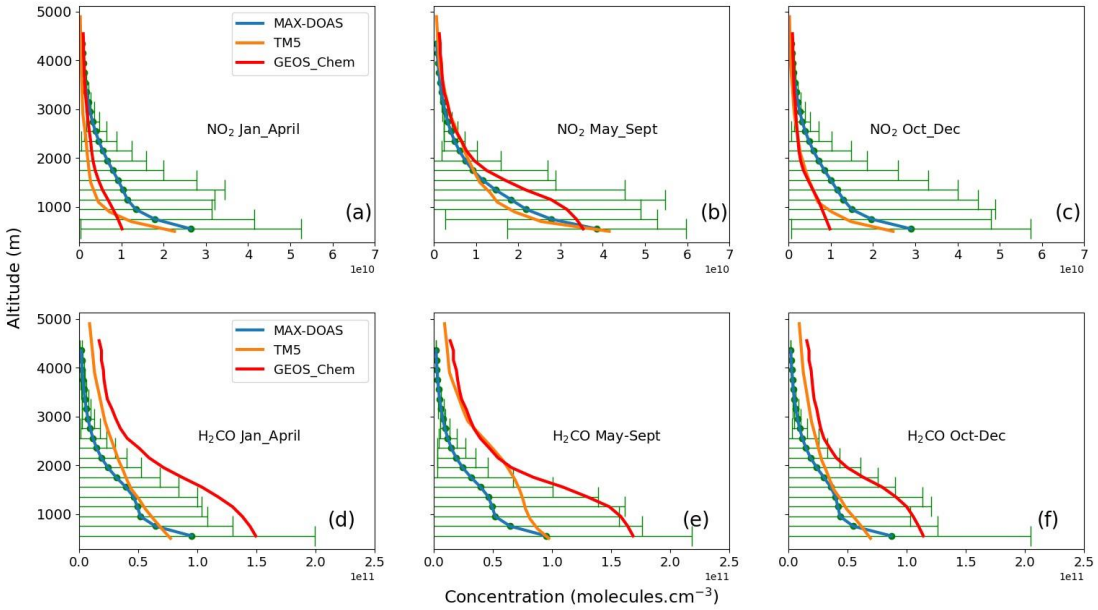


Figure C1. MAX-DOAS, TM5 and GEOS-Chem median profiles of NO_2 (panels: a, b, c) and H_2CO (panels: d, e, f). Error bars represent the standard deviation for MAX-DOAS measurements.

Lines 210 – 217: This paragraph is redundant with the next paragraph that describes Figure 6 in much more detail. I would suggest removing these lines.

Thank you for your comment. The paragraph has been removed.

Lines 221 – 232: Here to me it looks like NO2 profiles match each other very well at 500m in all three cases, and I agree there are large differences between 500 and 3000m. However, I also notice that H2CO is highly underestimated at 500m in TM5 compared with MAX-DOAS, and then it is overestimated at higher altitudes. I think an expanded discussion here would be useful.

We conducted tests to assess the impact of this free tropospheric contribution. To accomplish this, we recalculated air mass factors (AMFs) based on a reconstructed profile that incorporates both the lower segment (MAX-DOAS) and the free troposphere component (TM5 part). This process is depicted in the figure below. Our observations indicate that the influence of this

contribution remains minimal. Indeed, the variations in calculated AMFs within this context prove exceedingly restricted.

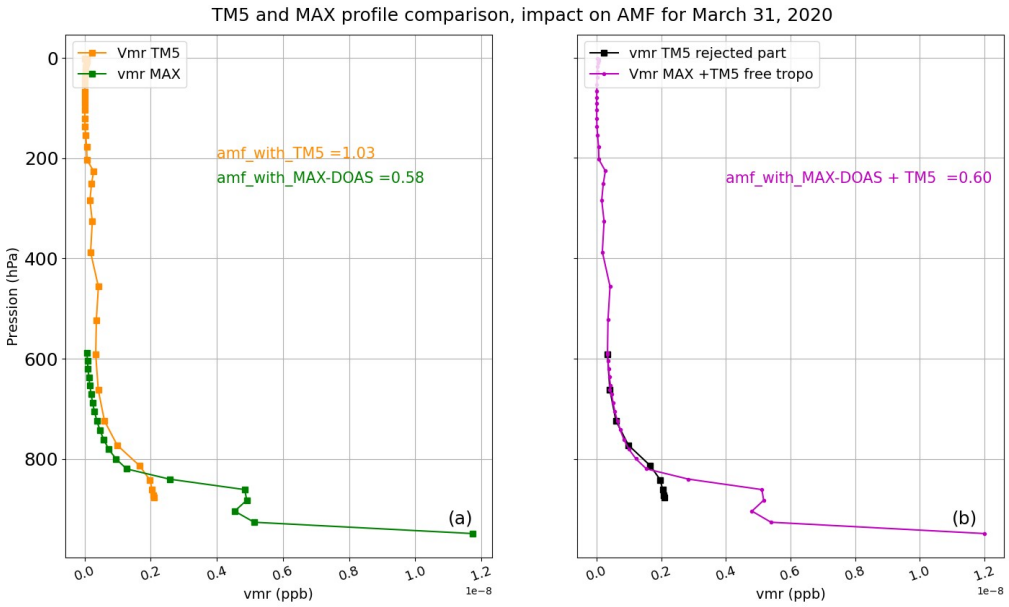


Figure B. H_2CO Profile TM5 and MAX-DOAS of March 31, 2020. Illustration of the impact of the profile change on TROPOMI air mass factor calculation.

Table 3: Please describe the linear regression method used. Since there are many ways to statistically treat this data and given the large standard deviation (error) in both measurements, an appropriate regression method should be used to analyze the data.

We also tested the Theil-Sen method and the results are close to the classical regression we used. The slopes calculated with the Theil-Sen method is 1.08, very close to the slope of 1.20, obtained with the least-squares method, see figure below. For the intercept, the Theil-Sen method gives 9.70 and least-squares gives 1.60.

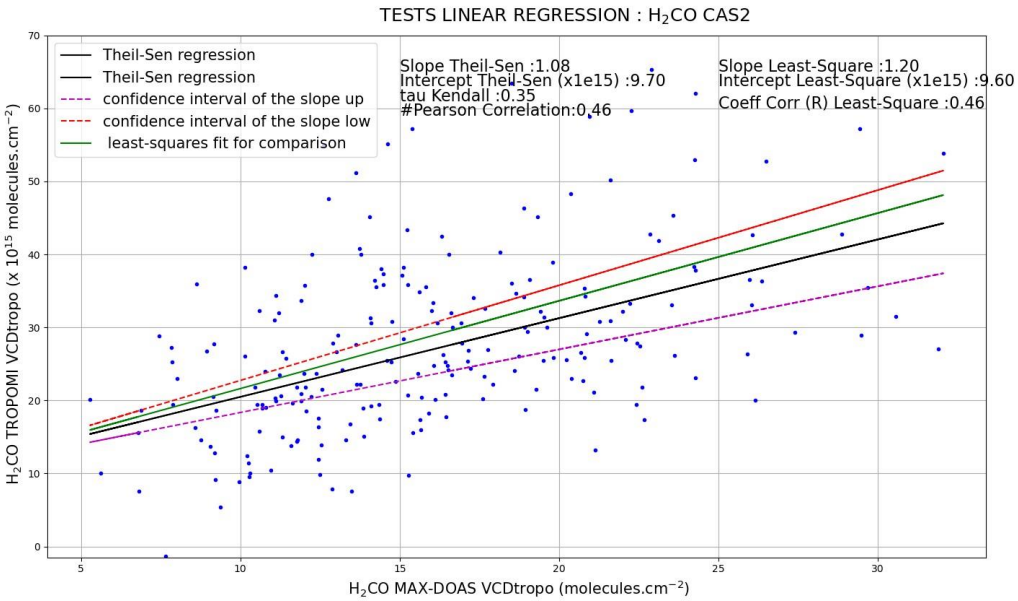


Figure C: Linear regression tests.

Line 279: It seems here like you have filtered more points out for H_2CO versus NO_2, but it seems like you describe the same filtering criteria for both pollutants earlier. Can you please explain why this is the case? And why you make the conclusion later that case 3 was worse due to this, but that wasn't the case for NO_2. In general I think the differences need more discussion and I think these lines should be moved to the discussion section and only results presented in Section 3.2.

There appear to be differences in the dynamic range between H_2CO and NO_2, even though the filtering criteria are the same for both molecules, as explained earlier. This observation has been revisited and addressed in the revised version of the manuscript, largely due to the use of the new H_2CO product, which provides more daily data, particularly during the dry season (weak signal). The results of case 3 should now be interpreted cautiously, due to the reduction in the number of sampling points and the presence of statistical noise. On the other hand, case 2 appears to be significantly more effective than the other two, as it substantially reduces the median differences between TROPOMI and MAX-DOAS.

Figure 11: It seems like there is a huge daily trend in H2CO but GEOS-Chem is not capturing this at all, do you have any idea why that is the case?

This part was deleted from the revised manuscript.

Line 327: "Clear improvements of the results when considering only TROPOMI pixel along the line of sight" – I'm not sure I am fully convinced by this conclusion. I agree that there is less overall bias with case 3 (and this assumes MAX-DOAS are the 'true' values), but I would argue the correlations, slopes and intercepts as well as NO_2 trends may be better captured in Case 2. For example, comparing Case 2 and Case 3 for NO_2 daily averages in Table 3 it seems like there is stronger correlation and less offset with Case 2. Looking at monthly averages the slope and offset also look better for case 2, and in this case TROPOMI becomes biased high at large concentrations, and very biased low at low concentrations (very negative intercept). I would like to see more justification and discussion between picking case 2 vs case 3.

We appreciate your insightful observation. Indeed, upon a more thorough analysis in the revised version of the study, Case 2 remains the most favorable among the three options. The median bias between TROPOMI and MAX-DOAS is significantly reduced in this case. However, it is crucial to acknowledge that the outcomes of Case 3, as previously discussed, should be considered with caution due to limitations stemming from sampling and statistical noise.

Line 328: Similarly here – is there any way to probe why case 3 did not produce as good results for H_2CO? The statistics may be worse but there should still be some improvement in case 3 if line of sight was a major influence on the retrieval comparisons. My initial thought here was that H_2CO is a bit more homogeneously spread around Kinshasa (and in general across most locations) so line of sight may not be as influential as with NO_2 which has more distinct emission sources.

Please refer to our previous answer.

Line 347: Could you try a stricter filter for clouds and aerosols and see if this shows improvement in the comparison at all?

We appreciate your suggestion to test a stricter filtering approach for clouds and aerosols in order to assess its impact on our results. However, it is important to note that our current results were obtained following the standard filtering procedure with QA>0.5, in accordance with the prevailing recommendations (van Geffen et al. (2022)[7]; De Smedt et al. (2021)[8]).

Unfortunately, practical limitations prevented us from conducting further tests. For instance, we were unable to perform a test based on cloud fraction as this information is not available in the files we utilize for our analyses.

[7] van Geffen et al. 2022.: Sentinel-5P TROPOMI NO2 retrieval: impact of version v2.2 improvements and comparisons with OMI and ground-based data, Atmospheric Measurement Techniques, 15, 2037–2060, https://doi.org/10.5194/amt-15-2037-2022, 2022.

[8] De Smedt, I. et al.2021.: Comparative assessment of TROPOMI and OMI formaldehyde observations and validation against MAX-DOAS network column measurements,Atmos. Chem. Phys., 21, 12 561–12 593,https://doi.org/10.5194/acp-21-12561-2021, 2021.