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.

This paper discusses the comparison of MAX-DOAS and TROPOMI observations of NO_2 and H_2CO at Kinshasa. I found two aspects of the paper particularly interesting: the location of the measurements, in a very data-sparse but interesting and relevant region, and the three cases presented, which nicely show the impact of vertical profiles and line of sight on the quantitative comparisons. The paper is generally well written, with a good set of references and a good introduction.

But, in general I found the comparisons with GEOS-Chem not very revealing/useful. The resolution of the model is very low, 2x2.5 degree, and a direct comparison with the MAX-DOAS is not even presented (maybe because of this). The model-TROPOMI comparison is limited to one location. A real model evaluation with TROPOMI would involve assessments over a larger area, addressing aspects of the emissions (choice inventory) and sectors (fires, transport, industry) contributing emissions, and evaluation of other model processes (transport, chemistry, deposition). Drawing conclusions from a time series at one location is not really possible. A reasonal comparison is shown for Kinshasa, which is pleasing but may be coincidental. One aspect which is of interest for this paper is the NO_2/H_2CO profile from GEOS-Chem, and comparisons with MAX-DOAS and TROPOMI a-priori/a-posteriori profiles. This could be extended and structured differently.

Because of this I would be in favour of publication of this work, after my comments below have been dealt with. In particular, the comparison with GEOS-Chem could be shortened and could be given a different focus.

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.
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).

Detailed comments:

- Abstract: The word "bias" should be used in a more balanced way to my opinion. A "bias" normally points to one of the two datasets, taking the other as reference (assuming it to be more accurate). On line 10 "shows an underestimation of TROPOMI with a median bias of -40% (s=0.26 and R=0.41) for NO2 and -26% (s=0.24 and R=0.28) for H2CO". The reader will conclude from this that TROPOMI is biased low. But the other cases show that the difference is influenced by the way the analysis is done. So, I conclude that the -40%/-26% are not so much to be attributed to TROPOMI, but also reflect the comparison approach. An alternative formulation could be "MAXDOAS is biased high by +40/+26%", which sounds like a very different conclusion. I would suggest to use a more neutral "mean difference between MAXDOAS and TROPOMI" instead of "the bias of TROPOMI" throughout the paper.

The current results (revised version), show that there is indeed a bias between TROPOMI and MAX-DOAS (case 1), and the transformation applied by considering the MAX-DOAS profile as a priori attests to the fact that there is a strong improvement in the bias between the two data sets.

- Abstract, line 16: "We found a bias of 16% (s= 0.42 and R = 0.80) for NO2 and bias of 61% (s= 0.05 and R = 0.24) for H2CO". Is the model or TROPOMI higher in this case?

This part was deleted from the manuscript

- Abstract, line 16: "bias"

This term was deleted as it was part of the comparison with GEOS Chem.

Fig. 1. Do these yellow lines correspond to the MAX-DOAS viewing direction? It may be useful to indicate the viewing (azimuth) direction in Fig. 12/13 as a line or arrow. Maybe Figs 12+13 could be brought to the beginning of the paper, e.g. after Fig. 1. The spatial distribution of NO_2/H_2CO is useful as background information before reading the rest of the paper. Would be nice to see MODIS AOD as well.

The two yellow lines point in a slightly different direction to the MAX-DOAS. This is an estimate of the visibility distance as indicated in the caption to the figure 1 (revised version)

The MAX-DOAS instrument as installed on the roof of the Faculty of Science of the University of Kinshasa (panel c). The yellow lines (panels a and b) point respectively to the Lumumba tower, visible at 5.7 km from the site and the city of Brazzavile, visible at about 16 km on clear sky days.

[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

Figures 11 and 12 have been grouped together in Figure 4 (revised version). We have also visualized MODIS AOD maps over Kinshasa (figure below). Given the spatial resolution of MODIS, the information on Kinshasa is less visible, so we decided not to add this map in the revised version.

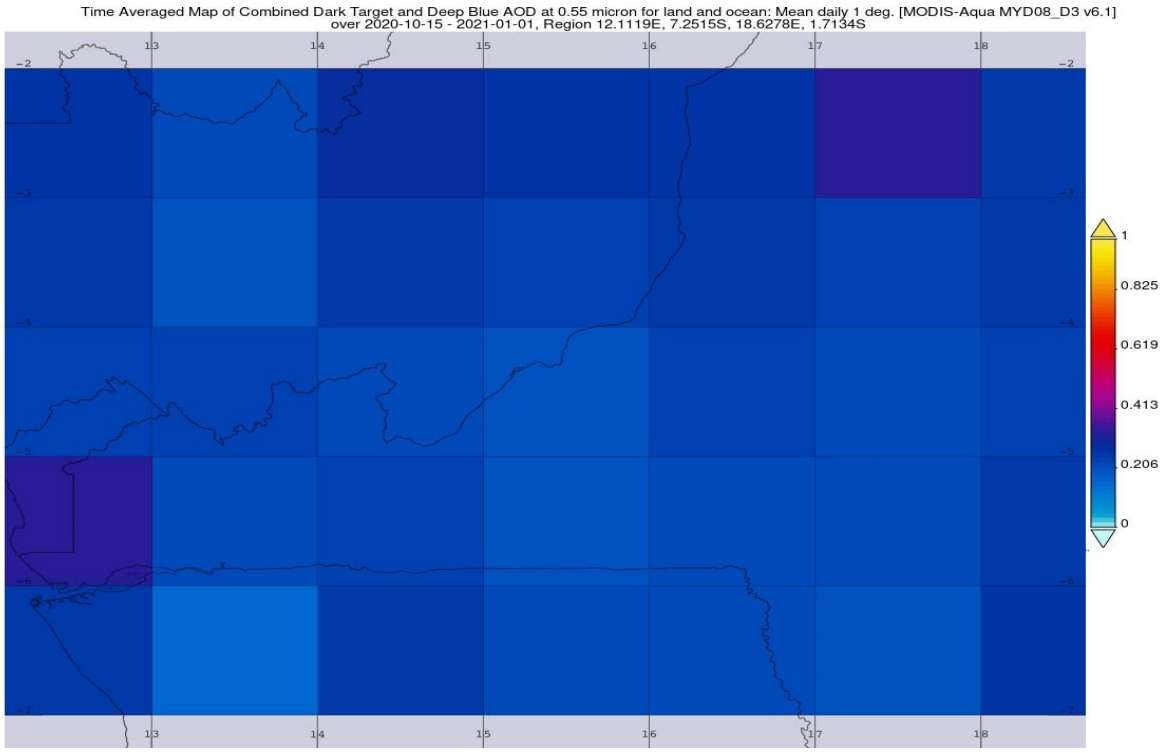

Figure A: AOD MODIS map over the Kinshasa area.

- line 107: "only MMF data selected for their consistency with corresponding MAPA results are retained." What does that mean? Which dataset(s) is (are) submitted?

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 H_2CO 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 one 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.

- line 113: "we only considered MMF due to inconsistencies in the MAPA aerosol retrievals for our Kinshasa spectra." Please explain the "inconsistencies".

Please consult the previous response and Figure B.

- line 113: It would be valuable to see the results for both retrieval approaches and to know how much the MAX-DOAS results (tropospheric columns) differ between the two (e.g summarise the findings of the papers cited in line 112-113). Would it be possible to present MAPA results?

The figure below shows the results of the MMF and MAPA algorithms. As mentioned above, there is only agreement between MAPA and MMF for low-value VCDs (panel d: H_2CO). The use of MMF is also motivated by its good concordance with MODIS AODs (panels a and c), while MAPA AODs remain much higher than both MODIS and MMF AODs.

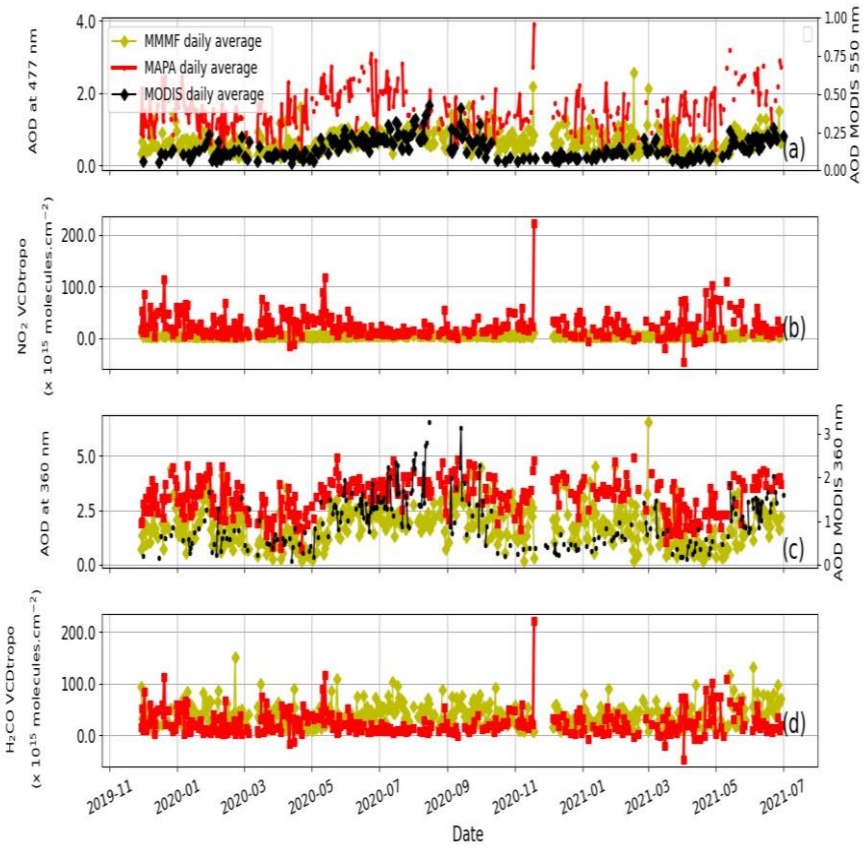

Figure B : MMF and MAPA products overview.

- line 115: "both algorithms." Does this refer to NO_2 vs H_2CO, or MMF vs MAPA? But MAPA is not used?

Indeed, these are two molecules. We replaced the term "algorithms" with "molecules" in the text to enhance clarity.

- line 116: "monthly climatology" Why not use the actual meteorological variables from for instance the ERA-5 reanalysis? Is the retrieval sensitive to meteorology (temperature)?

Indeed, utilizing ERA-5 is the preferred option, albeit with a marginal anticipated impact on the results. Illustrated below (Figure C) is a test scenario for air mass factor (AMF) calculation, incorporating two distinct temperature profiles: ERA-5 and the current operational profile. A

comparison of the calculated AMFs demonstrates negligible differences between the two datasets (see figure C).

It is important to highlight that previous research conducted at the same site also utilized the same climatological data. Notably, Beirle et al. (2022)[2] and Karagkiozidis et al. (2022)[3] have also adopted the same climatological data as in our study. Furthermore, we would like to underscore that in order to ensure consistency with the original study conducted at the same location, we have maintained the exact same climatological dataset.

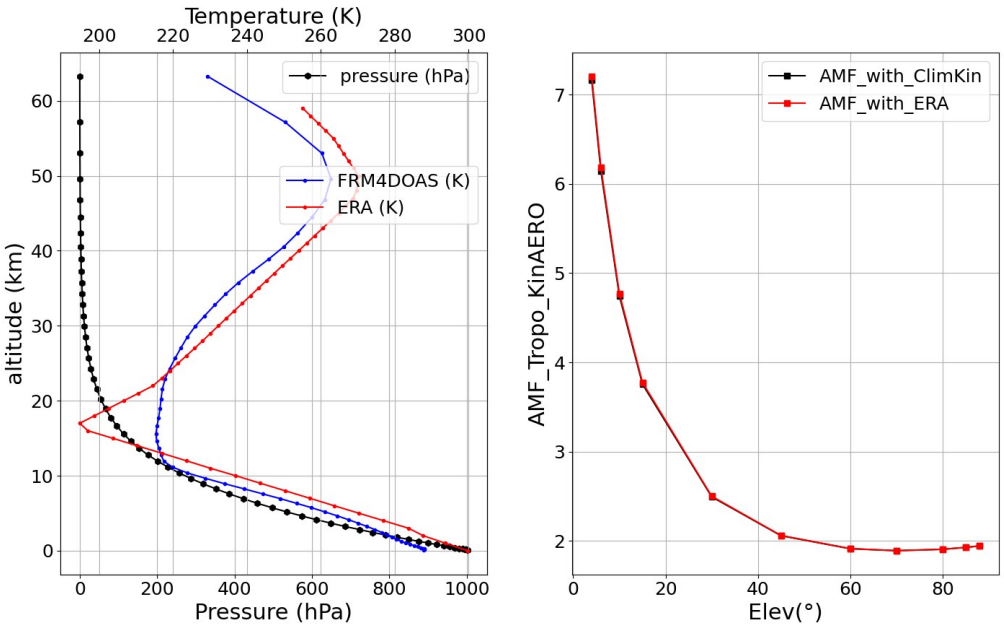

Figure C : Testing the use of different climatologies for calculating AMF.

- line 124: Please add a comment on the a-priori error used: how much is the retrieval constrained by the a-priori?

Figure 3 (revised version) shows that the sensitivity to the true state decreases with altitude, due both to the MAX-DOAS geometry and to the a priori used in the retrieval. We added the information on the a priori (Table 2)

A priori covariance : diagonal elements as x_a^2, correlation length of 0.2 km

- Sec 2.3: For FRM4DOAS you cite the ATBD. Likewise it would be useful to include a reference to he TROPOMI ATBDs.

Thank you, below is the sentence added with suggested reference (line 174-176)

For more technical details on the two products used, the reader is referred to the Algorithm Theoretical Basis Document (ATBD), all available at http://www.tropomi.eu/data-products/ (last access: 25 May 2023)

[2] Beirle et al. (2022) : Calculating the vertical column density of O_4 during daytime from surface values of pressure, temperature, and relative humidity.https://doi.org/10.5194/amt-15-987-2022

[3] Karagkiozidis 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. https://doi.org/10.5194/amt-15-1269-2022

- line 129: What is this "S5P-PAL" product? Please explain in one or two sentences.

Thank you, below is the sentence added with suggested reference (line 171-172)

TROPOMI data used in this work are based on the S5P-PAL, which stands for Sentinel-5P Products Algorithm Laboratory S5P for NO_2 (https://data-portal.s5p-pal.com/) and the reprocessed (RPRO v1.1) and off-line (OFFL: v2.1.3) for H_2CO. The NO_2 product from S5P-PAL is reprocessed with the same processor as version 2.3.1, covering the period from 1 May 2018 to 14 November 2021.

- line 132: "Only pixels within a radius of 20 km around the observation site" Why 20 km?

We added a justification for the 20 km in the manuscript (lines 186-190).

The choice of 20 km was made for three main reasons: (1) consistency with the horizontal sensitivity of the MAX-DOAS instrument, which generally varies between 3 and 20 km depending on visibility conditions, as shown in Fig. 1, (2) reduction of random uncertainty in TROPOMI data, especially for H2CO, as tested by Vigouroux et al. (2020)[4], (3) consistency with Yombo Phaka et al. (2021)[5], a study similar to this one and also other studies such as Pinardi et al. (2020)[6]; Irie et al. (2008)[7], having tested these selection criteria for the case of NO_2.

- Sec 2.4: Apart from Marais, it would be useful to add a few key references for this global GEOS-Chem (version 12). Are there other relevant studies done over Africa with the model?

Certainly, there is some research conducted on the model and ground measurements in Africa. We have added the following sentence (lines 209-210).

The GEOS-Chem model has seen multiple applications across various regions of Africa, including the works of authors such a Mark et al. (2016)[8], Eloise et al. (2019)[9], Alfred S. et al. (2020)[10].

The work of Eloise Marais has been cited here explicitly for its use of the DICE AFRICA emission inventory.

- Sec 2: I would expect a section on the intercomparison approach, dealing with aspects like profile shape, horizontal gradients, line of sight, collocation and meaning (use) of the circles in Figs 12 and 13. Instead, the cases are discussed in section 3.2.

Thank you for your comment. We have moved section 3.2 to a new section 2.5 in the methods section. And we have added some additional information and a new figure (Figure 5) explaining case 3.

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[4] Vigouroux, C.,etal. 2020:.: TROPOMI-Sentinel-5 Precursor formaldehyde validation using an extensive network of ground-based Fourier-transform infrared stations, Atmos. Meas. Tech., 13, 3751–3767, https://doi.org/10.5194/amt-13-3751-2020, 2020.

[5] Yombo Phaka, et al. 2021:: First Ground-Based Doas Measurements of No2 At Kinshasa and Comparisons With Satellite Observations, Journal of Atmospheric and Oceanic Technology, pp. 1291–1304, https://doi.org/10.1175/jtech-81d-20-0195.1, 202

[6] Pinardi, G. et al. 2020: Validation of tropospheric NO2 column measurements of GOME-2A and OMI using MAX-DOAS and direct sun network observations, Atmos. Meas. Tech., 13, 6141–6174, https://doi.org/10.5194/amt-13-6141-2020, 2020

[7] Irie, H., et al.2008 : : Validation of OMI tropospheric NO2 column data
using MAX-DOAS measurements deep inside the North China Plain in June 2006: Mount Tai Experiment 2006, Atmospheric Chemistry and Physics, 8, 6577–6586, https://doi.org/10.5194/acp-8-6577-2008, 2008

[8] Mark F. et al., An increase in methane emissions from tropical Africa between 2010
and 2016 inferred from satellite data. https://acp.copernicus.org/preprints/acp-2019-477/acp-2019-477.pdf

[9] Eloise et al 2019 : Air Quality and Health Impact of Future Fossil Fuel Use for Electricity Generation and Transport in Africa *Environ. Sci. Technol.* 2019, 53, 22, 13524–13534

[10] Alfred S. et al 2020 Air Pollution and Climate Forcing of the Charcoal Industry in Africa *Environ. Sci. Technol.* 2020, 54, 21, 13429–13438

- line 168: I was wondering how much the biomass burning season is contributing to AOD in comparison to local (dust, transportation, industry, household) contributions? Could you summarise what is known from e.g. the inventories.

Unfortunately, further exploration in this domain was not feasible within the scope of our current research. However, it is planned to conduct more comprehensive investigations into this particular aspect of the model in forthcoming studies.

- Fig. 5. "The error bars represent the standard deviation." The standard deviation of what? Is it an error bar or a measure of the spread of the values?

We have added the following text (caption figure 8).

The error bars indicate the standard deviation of VCDtropo computed for each hour within the specified period.

- line 184: "with some delay, " What would be a typical delay during daytime?

We have added the following sentence (line320-321), as per your request

Oxidation of these VOCs leads to H_2CO after a few hours, e.g. few hours for isoprene (Marais et al. 2012[11]). For pyrogenic VOCs, their lifetime is highly variable, from a few hours to several days (Stavrakou et al., 2009)[12]

- line 187: Looking at figure 12 this first case does not seem very useful. There is clearly a strong gradient and a lot of clean area is included in the average which is not observed by the MAX-DOAS instrument. I could imagine that for H2CO a larger circle may be needed because of the larger noise level compared to NO2.

We agree that case 1 is not very representative for NO_2 and should give a worse result than the other two cases. We have kept it for 2 reasons: 1) consistency with he H_2CO approach, where this choice is relevant, and for continuity with a first study (Yombo et al., 2021[13]) that was carried out on the same site with this approach but for a less performant instrument . So we thought it would be a good idea to start by presenting this basic approach first, and from this, improve the method to show the major impact of the vertical profiles.

- line 195: Please provide the details. What are the units of the MAX-DOAS profile (molecules/cm^3?). How is the interpolation done? Does the interpolation conserve the column amount? What is the collection of MAX-DOAS observations from which the median is computed? Why a median instead of a mean, and does it matter?

The profiles used in Equation (1) are given in vmr (ppb), while in Figure 6 (Figure C1, revised version), the MAX-DOAS profiles extracted from measurements are presented in molecules/cm³ as indicated in the caption. It is important to note that in the current version of the paper, we utilized the daily median profiles instead of the seasonal median profiles. This change in approach is motivated by two reasons:

[11] Marais et al. 2012: Isoprene emissions in Africa inferred from OMI observations of formaldehyde columns, https://doi.org/10.5194/acp-12-6219-2012

[12] Stavrakou, T.,et al. 2009 : Evaluating the performance of pyrogenic and biogenic emission inventories against one decade of space-based formaldehyde columns, Atmospheric Chemistry and Physics, 9, 1037–1060, https://doi.org/10.5194/acp-9-1037-2009, 2009.

[13] Yombo Phaka et al.2021.: First Ground-Based Doas Measurements of No2 At Kinshasa and ComparisonsWith Satellite Observations, Journal of Atmospheric and Oceanic Technology, pp. 1291–1304, https://doi.org/10.1175/jtechd-20-0195.1, 2021.

1. The previous H_2CO MAX-DOAS product had fewer exploitable data points. During the dry season, there were limited usable days, whereas with the new H_2CO product, all days within the study period are included.
2. Daily profiles exhibit significant fluctuations, making the use of daily medians the preferred choice, in accordance with Dimitropoulou et al. 2020[14].

Interpolation : The median profile obtained after smoothing depends on the Averaging Kernel (AVK) of the TROPOMI pixel involved. The figure below shows 9 smoothed median profiles linked to AVKs corresponding to these 9 pixels. Each of them shows a different shape, which is quite close to the MAX-DOAS daily median profile (in blue). The column is not preserved

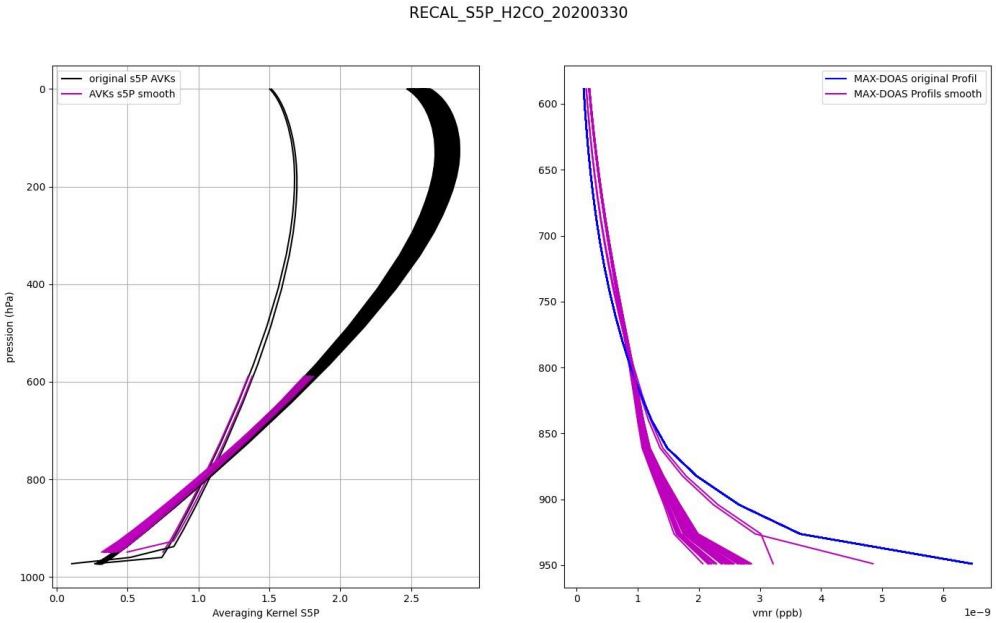

Figure D: Illustrating the smoothing of the daily median MAX-DOAS profile using TROPOMI averaging kernels (AVKs).

- Case 3: Is this done in the same way as described in Dimitropoulou et al? I was wondering if the method could be visualised? For instance for one day / one overpass, showing the region like in Fig 12/13, the azimuthal viewing line and TROPOMI pixels selected. Are weights applied to the TROPOMI observations? Are pixels close to the MAXDOAS more important?

Certainly, the approach is similar, albeit with variations. Dimitropoulou et al (2020) method involves multiple azimuths, considering the horizontal sensitivity fluctuations of MAX-DOAS, and incorporating weighted averages of TROPOMI column data from various pixels intersected by the MAX-DOAS line-of-sight.

In contrast, our approach employs a single azimuth with a fixed effective distance. We do not employ weighted averages of TROPOMI columns from the intersected pixels. For instance, in the case of January 24, 2020, when observing TROPOMI data over Kinshasa, 24 pixels meet our selection criteria. However, under approach 3, only 3 pixels align with the line of sight.

[14] Dimitropoulou et al. 2020 : Validation of TROPOMI tropospheric NO_2 columns using dual-scan multi-axis differential optical absorption spectroscopy (MAX-DOAS) measurements in Uccle, Brussels, https://doi.org/10.5194/amt-13-5165-2020

Consequently, our comparison is confined to these 3 pixels, which are then subjected to averaging (as illustrated in Fig 5 add in revised version).

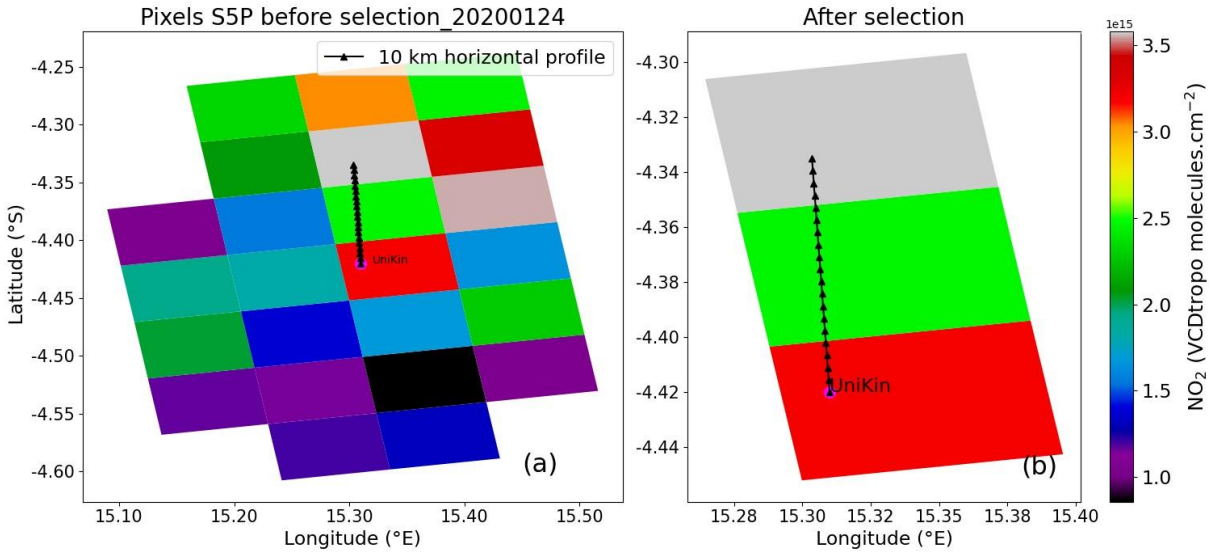

Figure 5. illustration of the approach taking into account the pixels along the MAX-DOAS viewing direction. Panel (a) shows all the pixels selected within a 20 km radius of the UniKin and panel (b) shows the pixels selected along the viewing direction shown as black line.

- line 203: "a coincidence test is performed" What are the criteria? What is the "surface point" of a pixel?

The following text, explaining the algorithm of approach 3 has been added to the revised version (line 239-244).

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.

- line 213: Looking at the figure it seems that the retrieval is producing negative concentrations in several cases. Do you apply a clipping, or are negatives used as is?

No. We exclusively filter out data that do not adhere to the criteria outlined in section 2.1 (for MAX-DOAS, qa<2) and in section 2.3 (for TROPOMI). All other values, irrespective of their negativity, remain within the scope of our study.

- line 216: "motivate the application of the transformation". According to the optimal estimation theory of Rodgers averaging kernels are to be used in profile comparisons. So this would be the main motivation, rather than an observed difference in profile shape. The difference in profile shapes indicates that case 1 and 2 may differ substantially.

Indeed, it is accurate that averaging kernels (AVKs) are applied to profiles for the purpose of facilitating inter-profile comparisons. This methodology is also pertinent to our study. The vertical columns we ultimately employ are derived from profiles measured by MAX-DOAS.

Therefore, to account for the specific sensitivity of the satellite instrument, we apply the averaging kernels to the measured profiles. This approach serves to adjust the measured profiles while considering the instrumental response of the satellite, aligning with Rodgers' optimal estimation theory which advocates for the use of AVKs in such comparisons.

- Fig.6. In May-September the profiles for H_2CO look quite different. Which one of the two would be more realistic? You mentioned before that the sensitivity of the MAX-DOAS rapidly decreases at 2 km and above. This is also indicated by the small spread around 2 km altitude. So maybe the ground-based observation is not sensitive enough to capture elevated layers? Could this explain part of the difference ground-satellite? Please comment.

First, please note that the profile of H_2CO has changed with the new MAX-DOAS product. Indeed, MAX-DOAS profiles exhibit distinct day-to-day variations influenced by seasonal changes. This led us to select the utilization of daily median profiles in the revised manuscript for recalculating TROPOMI columns. Furthermore, it holds true that the contribution from the free troposphere eludes detection by the MAX-DOAS instrument due to its limited sensitivity beyond approximately 3 km.

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.

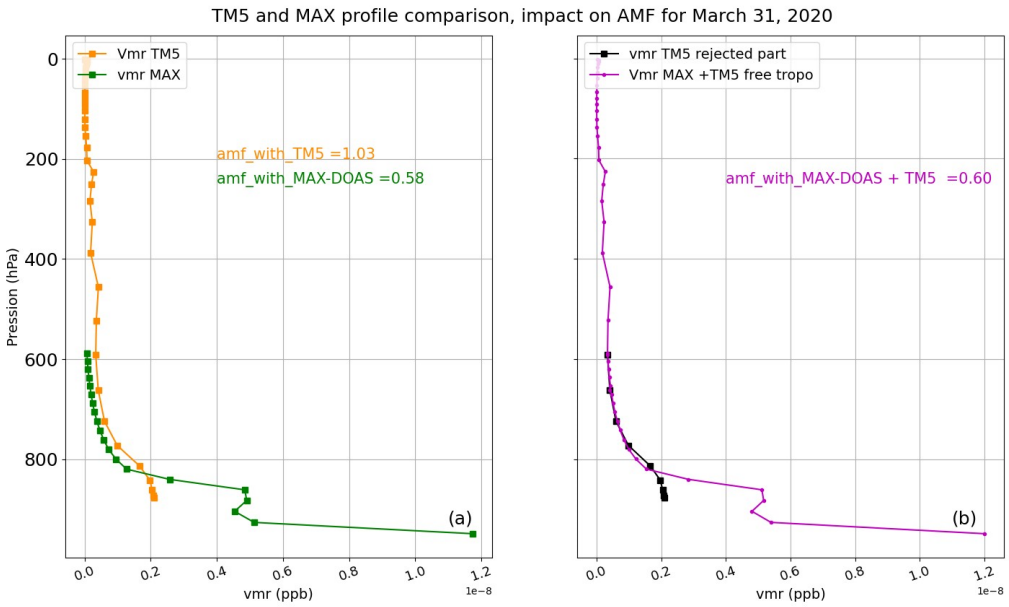

Figure E. H_2CO Profile TM5 and MAX-DOAS of March 31, 2020. Illustration of the impact of the profile change on TROPOMI air mass factor calculation.

- Fig.7. What kind of regression method is used? Does it account for satellite and ground-based retrieval errors? Please indicate in the caption that this is a case-3 comparison.

This is a least-squares linear regression that does not take errors into account.. We also tested the Theil-Sen method (T-S) and the results are close to the classical regression we used. see in

the figure below the slopes calculated with the Theil-Sen method is 1.08, approaching our own slope of 1.20. For the intercept, the T-S method gives 9.70 and L-S gives 1.60).

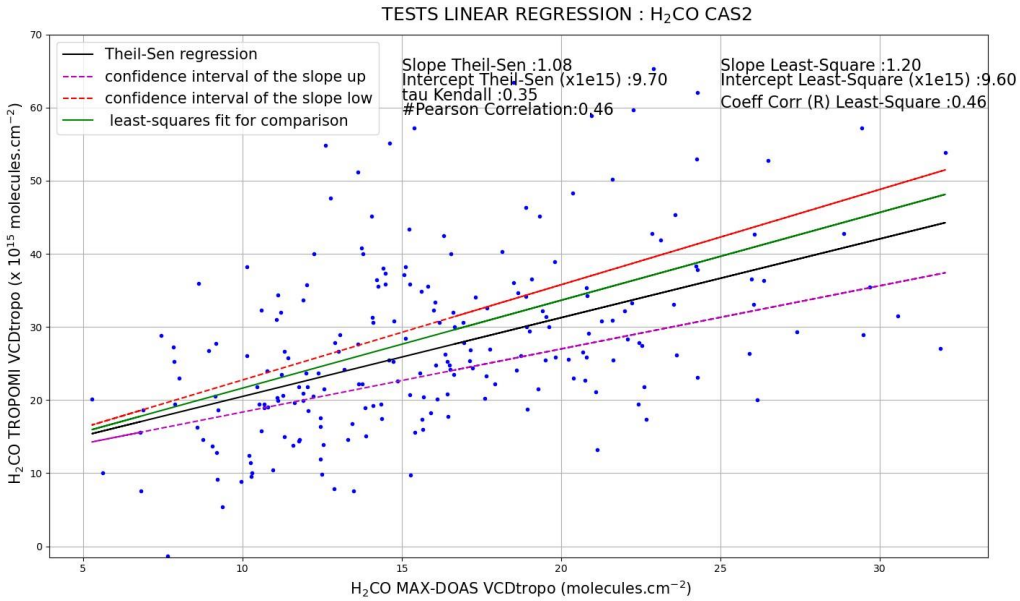

Figure F. Test linear regression.

- Fig.7: "TROPOMI error bars are standard deviations. " For the monthly values I assume this is the spread in the individual column amounts used in the average. But what is shown for the daily points? Is it now the retrieval error, or still a spread in values?

For TROPOMI measurements, we end up with 2 to 10 measurements per day at the overpass. We then average these values and calculate a standard deviation, so the vertical bars are again an estimation of the spread around the mean value of the day around the site.

- line 277: "As for NO_2, the results of the third case are shown in Figure 8" .. for H_2CO.

Deleted in the revised version manuscript.

- line 278: "The dynamic range of MAX-DOAS measurements is small compared to that of NO_2 ". Does this refer to the blue-green error bars? "..because of the different points filtered .." this is unclear to me.

Deleted sentence the revised version manuscript.

- line 282: "reduced number of TROPOMI measurements" How many measurements are used on average for NO_2 and H_2CO?

From case 1 and 2 to case 3, we move from an average number of pixels of around 30 to 4.

We have added the following sentence (lines 353-354).

The number of TROPOMI data used for each co-location with MAX-DOAS measurements is reduced by about a factor of 0.15 on average (see Fig. 5), in comparison with case 2. The number of days with valid data is also reduced from 198 to 90.

- Fig. 9: It would be nice if the a-priori profiles from TROPOMI and MAX-DOAS could be added in this figure as well. Like in Fig. 6 it would be good to see the season-averaged profile shape. Maybe Fig 6 and Fig 9 could be combined?

We have combined Fig 6 and 9 to add also GEOS-Chem (Figure C1 in revised version).

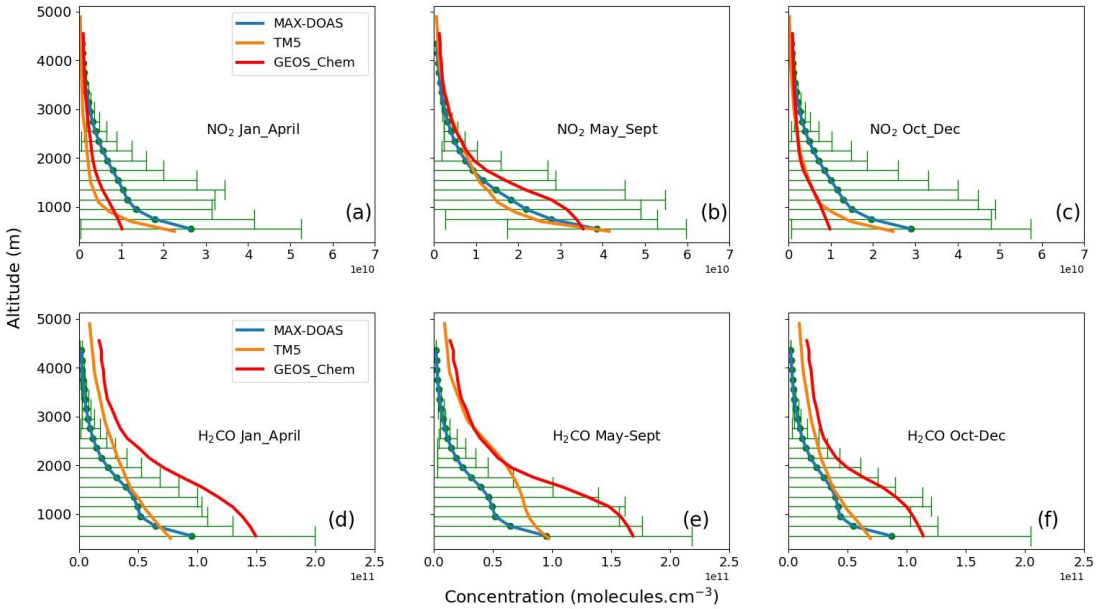

Figure C1. MAX-DOAS, TM5 and GEOS-Chem median profiles of NO2 (panels: a, b, c) and H_2CO (panels : d, e, f). The error bars represent the standard deviation.

- Why is GEOS-Chem only compared with TROPOMI and not with the MAX-DOAS? Of course the resolution of the model is very low compared to TROPOMI, leading to large mismatches in the air masses probed.

We did not see fit to compare MAX-DOAS with GEOS-Chem because of the resolution coarse model.

- line 331: "The general underestimation of TROPOMI compared to MAX-DOAS ". It would be good to mention that you refer to the difference between case 2 and case 1 here. The "best" comparison, case3, does not show a prominent underestimation.

In fact, we have added the term (case 1) : line 463.

The general underestimation of TROPOMI compared to MAX-DOAS observations (case 1)

- line 344: "Additional uncertainties comes from clouds and aerosols". How are clouds treated/filtered in the retrieval of the MAX-DOAS? This information is not provided in the paper.

We added this text in the revised version (line 148-155).

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 retrieva 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.

- line 363: Why refer to the raw comparison here? The best comparison is presented in Figs 10 and 11.

This part was deleted from the manuscript.

- Section 4.2. See my general comment above. The discussion cites a few studies and possible shortcomings. But there is not enough data to draw conclusions concerning GEOS-Chem (or TROPOMI) apart from a general reasonable agreement and similar seasonality.

This part was deleted from the manuscript.

- line 404: I have the feeling that the noise in individual TROPOMI H_2CO retrievals is an important reason for a poor correlation with GEOS-Chem and/or MAX-DOAS. This role of the retrieval noise could be discussed in more detail.

This part was deleted from the manuscript.

- line 406: "The present comparisons have shown the importance of correcting the initial TROPOMI products with the profile measured over the observation site and taking into account the horizontal variability of the studied molecules." Could you generalise this finding and formulate recommendations for other sites and satellite-ground remote sensing comparisons in general? Large gradients near cities are common, and the case 3 comparison approach could be a general recommendation for future validation work. How should previous comparisons (e.g. Verhoelst et al, Vigouroux et al) be interpreted?

We have added the following sentence(lines 565-571).

Our study demonstrates and confirms the impact of using MAX-DOAS profiles as a priori in the retrieval of TROPOMI columns. Indeed, due to the satellite's low sensitivity near the surface, biases can manifest significantly in conditions of highly polluted large cities like Kinshasa, potentially resulting in an underestimation of satellite observations. However, this tendency is markedly mitigated when correction is applied by considering profiles actually measured by the ground-based instrument.

Consequently, our recommendation is to implement this transformation, particularly in settings of highly polluted urban areas like Kinshasa. Nonetheless, caution should be exercised in the incorporation of the MAX-DOAS line of sight due to the introduced noise during downsampling, as observed in this study.