We would like to thank reviewer 1 for his/her constructive comments and suggestions.

Major comments:

1. Although not brought by the referees originally, the title should mention Germany, since this study only deals with that country.

Response: Added Germany to the title. New title: "Can TROPOMI-NO2 satellite data be used to track the drop and resurgence of NOx emissions within Germany between 2019 – 2021 using the multi-source plume method (MSPM)?"

2. Concerning the important questions arising from the 4h lifetime, the authors have made a small adjustment in the text to say 2-5 hours, but my impression is that they are trying to avoid a proper discussion. For example, in their reply to Referee #1 they cite evidence of 12-24 hours in winter, and although this estimate was for China I would expect something like that for Germany in the (high-emitting) winter months.

The discussion of footprints in the reply to Ref #2 is also useful, and helpful for readers. Readers of the new manuscript will be unaware of the points made in this reply-to-referees, and will still be surprised I think to see the assumption of 4h without much explanation. The authors should expand this issue in the manuscript, or add much of the reply to referees in the SI.

Response: We added parts on the discussion on lifetime and footprints/plume spread to the manuscripts:

Lifetime, Replaced line 254-259 "The effective … for local and seasonal variations" with: "The lifetime of NO\textsubscript{x} depends on both the chemical decay rate and loss to surfaces (dry deposition). Within our domain of interest, the chemical decay will be the dominant factor. Commonly used lifetimes in literature are typically based on either modelled lifetimes or derived lifetimes from (satellite) observed plumes. Modelled lifetimes are commonly estimated via the availability of OH and production thereof (often including radiation) \citep{ Valin2013, lorente2019quantification}. Several studies have explored this route before and either estimate the availability of OH by some basic assumptions on production, or by using modelled OH fields (with the drawback of a potential bias within the simulated concentrations). Either route is possible and estimates for the effective lifetimes end up around 2-5 hours for spring and summertime values \citep{ Valin2013, lorente2019quantification}. Outer estimates for wintertime lifetimes are 12-24 hours \citep{Shah2020}. Alternatively, lifetimes can be derived from tagging emitted molecules and tracking these within the model domain \citep{Curier2014}. The study reported that for a region representative of Germany (Benelux), approximately 50\% of the modelled satellite signal (Ozone Monitoring Instrument, OMI, \citep{levelt2018}) results from NO\textsubscript{x} emissions in the 3 h prior to OMI overpass.". Assuming a relatively constant source this translates to a lifetime of about 4 hours (at column level, and assuming a basic mass balance). Several other studies report on effective lifetimes derived from fits to observed plumes from cities and large industrial areas. Using the EMG plume functions the studies derived lifetimes between 2-5 hours based on the decay downwind of major sources worldwide\citep{Beirle_2011, deFoy_2015,Goldberg_2021, Lange_2022, Fioletov_2022} with a recent study by \citet{Fioletov_2022} giving a value of 3.3 hours representative for larger emissions within the US and Canada (2018-2022).

Following the modelled and observed lifetimes we assume a mean lifetime of 4 hours $\pm$ 1 hour to account for local and seasonal variations. A potential point of concern remains how representative the lifetime is for the whole year. Most of the estimates are biased towards spring,

summer and autumn as there are typically more observations available within these months. To correct for the representativity bias a seasonal variation factor (1.11) will be included (explained in next section), but additionally by choosing a value of 4.0 hours we remain on the high end of the lifetime estimates. The standard deviation of $\pm$1 hour ensure that common values within 3-5 hours remain within the uncertainty range. Furthermore, \citet{Fioletov_2022} also notes that while lifetime has a large impact on the emission estimates, relative changes do not have a major impact when comparing individual years to one another. They point out that 1h deviations from the 3.3 hour mean only changed the emission estimates between years by about 1\%."

On the footprints / plumespread: We added more of the discussion from the previous review round, Replaced line 342-345, "The GNFR data is … source and emission methodology" to "The GNFR data is used as a basis and summed and regridded for all the NFR classes, to match the 0.1ºx0.1º grid used in this study. A Gaussian filter (\textit{scipy.ndimage},\cite{scipy_source}) is applied to the data with a sigma of 1 grid cell. The posteriori smoothing is only there to bridge the limitations of the method and instrument. The spatial limit to resolve 2 sources of similar size depends on the effective lifetime, the pixel size and meteorological factors such as typical diffusion etc. Of these the pixel size and lifetime are dominant at the TROPOMI pixel limit (5.5x3.5km^{2}). The pixel size combined with diffusion gives us a typical plume width of around 7 km (e.g. σ^{2}=σ^{2}\textsubscript{plume}+σ^{2}\textsubscript{pixel}+σ^{2}\textsubscript{source}). This value varies depending on typical size of a source but most sources of NO\textsubscript{2} are limited in size (except for large mines, very large cities etc). Based on \citet{McLinden:2022} a plume-width of 7 km combined with a lifetime of 4 hours gives an effective resolvability limit of 15-20km, which for 0.1ºx0.1º source cells (e.g. $\sim$10x10km^{2}) explains the choice for a sigma of 1 grid-cell. More smoothing can produce better results, but also reduces the observable details."

3. p6, L167, and Ref #2 comment. The authors reply that "The MAX-DOAS and PANDORA instruments are not completely free of bias themselves but typically have much lower uncertainties than the TROPOMI-NO2 product as stated in Verhoelst et al., 2021.", but again this information is not passed to the readers of this manuscript. This should be explicit here.

Response: We added "It is important to note that the MAX-DOAS and PANDORA instruments are not completely free of bias themselves, however the ground-based instruments typically have much lower uncertainties than the TROPOMI-NO2 product as stated in Verhoelst et al., 2021." to Line 176.

4. Tools and code availability?

The footnotes on page 3, and the "Code and data availability" section gives web addresses. I thought GMD insisted on zenodo for software?

Response: *In my opinion this is generally a point for the typesetting/final editing phase. We can upload a version of the code to Zenodo or any other sharepoint if needed/required.*

<u>Other points:</u>

p11, L294: +a few % plus/minus maybe?

Response: changed to ±.

p23, L471. The (Kuenen..) ref should be before the comma.

Response: reference moved to before the comma.

We would like to thank reviewer 2 for his/her constructive comments and suggestions.

The paper presents a comparison of TROPOMI-based NOx emission estimates and emission inventory data for Germany over the years 2019, 2020, and 2021, including the COVID-19 period. It provides a method to evaluate NOx emission inventories and the possibility of more recent emission information, which can be interesting for emission inventory compilers. The paper is of scientific significance and with the given information reproducible. In some parts, there is a lack of logic, too little explanation, and some misleading discussions. After addressing the comments raised below, the paper should be considered for publication.

General comments:

For some parts, there is a lack of references. See comments in the specific comment section. Sometimes, statements are given without further or too little explanation. For example, regarding the grid size, the assumption of a 7 km plume spread, the rotation method, the COVID-19 effect, and NOx to NO2 ratios. See comments in the specific comment section. There is quite a lot of discussion (Line 374-399) on the visibility of NOx emissions from highways in the satellite-based maps. However, this is not very well visible to the reader from the mentioned Figures (Fig. 4 and especially not in Fig. 6 showing the differences). I would recommend removing or at least weakening the statements and discussions about the NOx emissions and their changes from highways.

Response: *Thanks for the detailed feedback. We have incorporated these comments in the manuscripts. We think this gives the reader more context behind the statements made and thereby improved scientific soundness. We also added some additional hedging on the statements related to the traffic emissions.*

Specific comments:

Line 9/10: How do you know the reductions are related to the COVID-19 lockdowns, not the results of political emission reduction strategies?

Response: *These reductions were simultaneous with lock down measures and breaking with trend lines of declining emissions due to policy measures. Generally speaking this trends don't show reduction of ~15%/y but are more in the order of several precent. We can however never discern between Covid induced changes and other causes. Hence the text was adapted to reflect this.*

Line 116: Missing reference for: "Globally, the lightning NO constitutes about 3% of the total NOx emission budget."

Response: *Reference added.*

Line 152/153: Since this is a more general explanation of the different TROPOMI products (NRTI, OFFL, RPRO), the given years (2019-2021 and April 2018-November 2018) are confusing. Since they are also not used/relevant for the study, I suggest to remove them. Also, delete the word "reprocessed" in combination with the offline mode in line 152.

Response: *It is true that another data product was used (PAL). This was however added to stress there are multiple versions of TROPOMI NO2 and this topic of continuous improvement. This is now framed differently and more clearly. Still we think it is useful to mention all options here.*

Line 153: At this point, there is no explanation given why a reprocessed version is needed. Please add something like: "Over time, several modifications in the retrieval lead to processor updates and new product versions. Reprocessed data sets based on the latest offline version are provided at a more irregular interval."

Response: *Thanks for this suggestion. It has been incorporated (with slight adaptations) in the manuscript.*

Line 156-159: Relevant for this study in this detail? Explanation would have been helpful in line 153 (see comment above). Maybe move some parts of it or delete it.

Response: *We think it is good to keep this overview. The reader should be aware which datasets are used in order to guarantee reproducibility. Knowing there are many data products and version is crucial in that regard.*

Line 173: Reference missing for "The negative bias can be explained by the low spatial resolution of the a-priori profiles as well as the treatment of clouds and aerosols in the retrieval." Is this shown in van Geffen et al. (2022), otherwise, add Lange et al. (2023, https://doi.org/10.5194/amt-16-1357-2023).

Response: *Lange et al was added here.*

Line 173: Sentence difficult to understand. What is meant by set? Where does this recommendation of 0.3 come from? I am only familiar with the cloud radiance fraction filter of 0.5, which is also the filter when the qa_value of 0.75 is applied.

Response: *The sentence was rephrased. The requirements came from the ATBD (algorithm theoretical basis document) for this data product this is now more clear. The 0.3 cloud radiance fraction filter requirement comes from earlier assimilation applications.*

Line 177: How do you know that it is COVID-19 impact, not meteorology or reducing strategy (see, for example, Goldberg et al. (2020, https://doi.org/10.1029/2020GL089269)) especially since it is not coming back in 2021? See also comment regarding line 9/10.

Response: *See response to the comment regarding line 9/10. Here also the phrasing in the manuscript was adapted.*

Line 193: I think the word "tooling" is not always used correctly; maybe better to say "Emission estimation tool". See also line 196.

Response: *Thanks for this suggestion. The change is incorporated in the manuscript*

Line 195: The term "space-emissions tool" is misleading. Better would be a "satellite-based emissions tool".

Response: *That is more accurately put. We adapted the text accordingly.*

Line 195-200: Repetition this was already discussed in the introduction. Since it is not very relevant for the paper, it should be deleted in the introduction.

Response: We think this is provides valuable context for the developments presented in the manuscript and hence we decided to keep it. It is however true that this is somewhat repetitive. Hence we shortened the text and removed the repetition.

Line 206: Confusing: With "here" you mean the tool and not the paper? So, the tool uses the mass-balance technique, but the paper is focused on the plume based fitting method.

Response: *Yes, this is now clarified further in the manuscript.*

Figure 2 legend: "Space-emissions tool" see above, Replace "A" with "Panel A".

Response: *Changed accordingly*

Line 222 and 223: The term "total column density" is confusing since it is also the term for the column from the surface to the top of the atmosphere, but I think this is not meant here.

Response: *That is meant here. Or more precisely tropospheric column density. Text was changed accordingly.*

Line 235: "Using this method, observations are rotated around a single point, the emission source, so that each is positioned in a similar upwind-downwind frame" How do you know your emission sources, what about area sources like highways?

Response: This is a good question and it should be seen as a potential source. We don't know the sources yet and that is why a grid of potential emission locations is introduced . The strength of the method is that its independent of the initial emission locations. This is now clarified in the text.

Highway sources can be seen as a continuous line of point sources instead of an area source, which aggregates down to 0.1x0.1 degree steps. A similar thing can be said for area sources. See the discussion on resolvability as to why the limitation of a 0.1x0.1 degree grid is valid. Alternatively one could describe highways as a more detailed set of single source points with a single enhancement multiplier for segments of the road to increase resolvability. This was however not the goal of the study.

Line 251/252: Change "by dividing the emission enhancement ai by the decay-rate λ;" to "by dividing the emission enhancement ai by tau".

Response: Agreed and incorporated. Thanks for noticing this error.

Line 253: What are the reasons for a grid with a resolution of 0.1 x 0.1 degrees? How does the resolution influence the results? Please provide a short statement.

Response: *The resolution is chosen as a compromise between computational burden, level of detail required, limitations of the instrument (resolvability) and conditioning of the linear system describing the source receptor relations. This is added to the text.*

Line 255: "Within our domain of interest, the chemical decay will be the dominant factor." How do you know?

Response: We have a lot of experience in modelling NOx with chemical transport models in which case you can determine the strength of the loss terms (wet- and dry-deposition, chemical sinks etc) because you can track budgets for the various processes. Chemical processes dominate the loss of NOx within this domain.

Line 261: 7 km of plume spread seems small to me, isn't the plume spreading over a larger area quite quickly due to diffusion? It would be good to add references to other studies using similar values.

Response: Other studies by Griffin et al., and Fioletov use 7 and 8km. In both cases the authors derived the plume width from fits to single sources. The plume width itself has a fairly limited result on the resulting emissions. These references are added.

Line 265/266: Why do you need a correction factor for seasonal variability when you can get seasonal information from satellite observations?

Response: Seasonal was removed. This is indeed not required. When averaging one needs to take into account that there are more observations in summer than in winter generally.

Line 268: Please comment on whether assuming a constant lifetime over the day and for different seasons is valid.

Response: A similar question came from the earlier(other) reviewer(s), see those replies for further explanation. We added to the manuscript the following "commonly used lifetimes in literature are typically based on either modelled lifetimes or derived lifetimes from (satellite) observed plumes. Modelled lifetimes are commonly estimated via the availability of OH and production thereof (often including radiation) \citep{ Valin2013, lorente2019quantification}. Several studies have explored this route before and either estimate the availability of OH by some basic assumptions on production, or by using modelled OH fields (with the drawback of a potential bias within the simulated concentrations). Either route is possible and estimates for the effective lifetimes end up around 2-5 hours for spring and summertime values \citep{ Valin2013, lorente2019quantification}. Outer estimates for wintertime lifetimes are 12-24 hours \citep{Shah2020}. Alternatively, lifetimes can be derived from tagging emitted molecules and tracking these within the model domain \citep{Curier2014}. The study reported that for a region representative of Germany (Benelux), approximately 50\% of the modelled satellite signal (Ozone Monitoring Instrument, OMI, \citep{levelt2018}) results from NO\textsubscript{x} emissions in the 3 h prior to OMI overpass.". Assuming a relatively constant source this translates to a lifetime of about 4 hours (at column level, and assuming a basic mass balance). Several other studies report on effective lifetimes derived from fits to observed plumes from cities and large industrial areas. Using the EMG plume functions the studies derived lifetimes between 2-5 hours based on the decay downwind of major sources worldwide\citep{Beirle_2011, deFoy_2015,Goldberg_2021, Lange_2022, Fioletov_2022} with a recent study by \citet{Fioletov_2022} giving a value of 3.3 hours representative for larger emissions within the US and Canada (2018-2022)." on the discussion of lifetime was added. We think this point is now covered amply.

Line 271: What is the temporal resolution of these profiles? Are there individual profiles for the different sources available, if yes, how large is the variability in the correction factor? Is it valid to use an averaged profile over all sources?

Response: Hourly by default and yes there are different time profiles for different sources. The variability differs per source. Traffic for example has stronger fluctuations and certain industries are simply flat. Using an averaged profile is always an assumption that potentially introduces a misrepresentation at specific locations.

Line 286: The NOx to NO2 ratio is influenced by seasonality and time of the day. At least add something like: The NOx to NO2 concentration ratio depends on the local chemistry, influenced by ozone concentration, photolysis frequency of NO2 (solar zenith angle), and the rate constant of NO+O3 reaction (temperature).

Response: Added.

Line 287: How is the value of +/- 0.26 determined, is it really the standard deviation, how do you know the distribution of the NOx to NO2 ratios?

Response: This can be estimated using CTM output or measurements. Both of these approaches come with uncertainties. Generally values on the standard deviation of ~20% come out of these studies. This value was also used in Beirles 2021 paper.

Line 300: Please provide a reference, especially for the large part regarding the missing variations in the stratospheric NO2 concentration; the first part was mentioned before.

Response: Reference is added.

Line 325: You only discuss the wind speed, what is about wind direction, which is essential for the rotation?

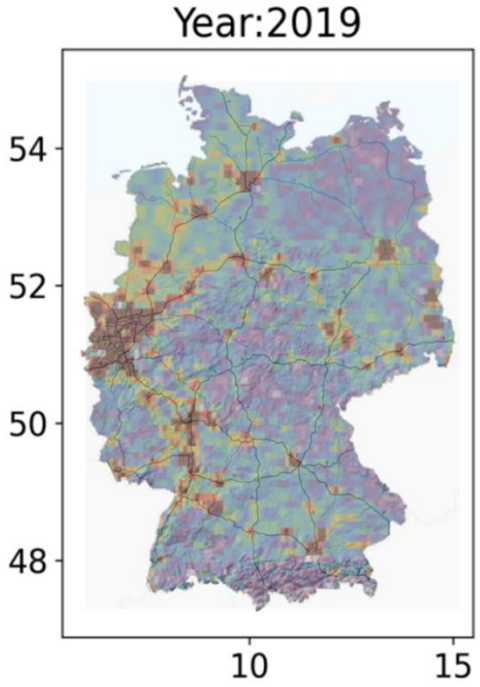
Response: Yes. +-1 ms-1 in eastward and northward directions, its implicitly included. The text is now reformulated to stress this more.

Line 374: I only see elevated NOx emissions from the Rhine-Ruhr region to Hannover but not between Hannover and Magdeburg and not towards Berlin. At least not in the satellite-based maps. There are also several large power plants close to highway A2 near the Ruhr area, which is visible in your Fig. 6, so I probably don't see the NOx emissions from the highway but from the power plants located nearby. I would recommend removing this part about the highways.

Response: We have to disagree with this statement. It might require a keen eye but we are confident the road network (an particularly the busies highways) are visible in the emission estimate. We do appreciate that one needs to have a thorough look and hence some hedging of the statements concerning road networks was added.

Line 394: You mention a noticeable drop in emissions around highways. Where I think this is not visible. There is probably a reduced signal in the region of the A2 highway close to the Ruhr area, but this isn't easy to disentangle since there are several power plants along the highway in this part. See comment regarding line 374. I would recommend removing the part about the highways here.

Response: See the response to the previous comment. We agree that at the location of the A2 highway we see a clear drop but also we are confident the signals from the A1 and A3 are visible. Actually plotting the road network on the emission estimates might show this more clearly to the reviewer:



Line 396: You mention a rise in emissions from 2020 to 2021, most noticeable in large urban areas. I think this is very hard to see...maybe Berlin shows higher emissions in 2021 compared to 2020, but I don't see any clear rise in signal. Please provide examples, including values for the rising emissions.

Response: We added a refence to figure 6 in the text here that shows this more clearly.

Line 398/399: The Hannover-Rhine-Ruhr part is still visible in 2020, but especially the part with large emitters (see Fig. 6) along the highway. All this highway discussion is on very small differences and not really visible in the provided maps.

Response: We agree that the plots are small as presented in the text. However they have a high resolution and remain of decent quality when one zooms in to be able to appreciate these changes that are indeed quite nuanced. It is however required to do this because at first glance differences are not always apparent.

Line 415: "The Weisweiler power-plant reduces into 2019 while reducing further into 2021." I cannot follow this statement; do you mean 2020 instead of 2019?

Response: Yes! Thanks for pointing this out.

Figure 8 legend: "the red error bars show the uncertainty in the satellite derived" The -50 to +30% given in Table 1? But the error bars look similar in + and - directions? This national inventory shows higher emissions in 2021 than in 2020, especially from road transport. In line 407, you stated an "upsurge in the usage of coal-fired power stations for power generation in 2021 compared to the COVID-19 year 2020". I think this is not visible here, do you have an explanation?

Response: You do see a slightly higher Public power contribution in 2021 compared to 2020. Maybe upsurge is a bit of an exaggeration. We have changed the text accordingly.

Line 435: What is meant by power generation, public power, or industry? Probably public power, but also for public power, your Fig 9. upper left panel shows an increase in 2021 (0.925) compared to 2020 (0.85), this is not almost back to 2019, it is in the middle between the two years.

Response: Correct. The is now altered in the text

Line 438/439: "While Road transport emissions were expected to show a recovery this is not matched by patterns in the satellite derived emissions." Your Fig. 9 shows that road transport emissions decreased from 2019 to 2020, but satellite data show no change from 2020 to 2021. However, this reduction from 2019 to 2020 is not visible from Fig. 6, as discussed earlier. How can you explain the apparent difference between the discussed maps and Fig. 9?

Response: This might have to do with the other sources present in the regions that are labelled as road transport dominated (above 50% of the total emissions from road transport). This means that up to maximally half of the emission can originate from another sector that can challenge drawing conclusion. By the way, in figure 6 we do see a reduction in emissions from road traffic as light blue patches co-located with the road network.

Line 440/441: Not so easy to say for shipping emissions: The inventory shows a recovery. Why the difference here? However, industry emissions have continued their downward trend with no sign of recovery but are not mentioned here. In general, it is difficult to speak of a trend when it is only two years.

Response: That is a valid point. We rephrased this. Concerning the recovery in the emission inventory we know how challenging it is to accurately incorporate shipping emission into an emission inventory. How the post covid recovery is taking into account requires detailed knowledge from the compilers of these emission inventories hence we cannot answer this question confidently.

Figure 9 legend: "A clear downward trend is visible for most sectors." Not really true, maybe for industry, and in general, it is difficult to speak of a trend when it is only two years. I would suggest deleting the sentence.

Response: Agreed. Text has been adjusted.

Line 471: This is not clear to me: emissions 2018, based on the in 2020 reported emissions

Response: For emissions there are always two date to take into account. One the reporting year and one is the year of the emissions. So this means 2018 emission as reported in 2020. The text has been adjusted.

Line 552/553: How will this work with the worse resolution of OMI compared to TROPOMI?

Response: Since we are aggregating to 0.1x0.1 degree (~6x11km) the OMI resolution (13 km × 24 km) will start hampering the level of detail available in the map. It might be more optimal to switch to a 0.2x0.2 grid for OMI as the limited footprint will probably not provide enough information to prevent overfitting within a 0.1x0.1 degree grid. This is now also mentioned in the text.

Technical corrections:

Line 30: Change NO2 emissions to NOx emissions

Response: changed to NOx

Line 39/40: Two times "for example", please rephrase the sentence

Response: Change “For example, emissions from road transport for example depend on several factors such as:” to “For example, emissions from road transport depend on several factors such as”

Line 45/46: Change “For example air quality applications such as forecasts” to “For example, air quality forecasts”

Response: changed as requested.

Line 55: x of NOx is not subscript

Response: Changed to subscript.

Line 55/56: Change “Timely verification of the inventories could potentially more rapidly identify such discrepancies.” to “Timely verification of the inventories could potentially identify such discrepancies more rapidly.”

Response: Changed as requested.

Line 58/59: Change to “Furthermore, due to increased instrument sensitivity and spatial and temporal resolution, these satellite-based measurements ...”

Response: Changed as requested.

Line 88: Change ”space-borne measurement” to “space-borne emission estimate”

Response: Changed as requested.

Line 89/90: Change “In this study we focus on the plume based fitting method.” to “In this study, we focus on the results of the plume-based fitting method.”

Response: Changed as requested.

Line 106: Change “Agricultural Soils” to “agricultural soils”

Response: Changed as requested.

Line 107: delete “inventory”, it is doubled with the following “reported emission data sets” and “inventories” in line 108

Response: Changed as requested.

Line 117/118: Why twice reference to EEA (2019)? The first one is enough.

Response: Changed as requested.

Line 125: Change “as kt (NOx)” to “as kt NOx”

Response: Changed as requested.

Line 128: Sentence not complete.

..., mostly based on the anthropogenic emissions Yienger and Levy II (1995) reported, ...

or

..., mostly based on the anthropogenic emissions (Yienger and Levy II, 1995), ...

Response: changed to “*mostly based on the anthropogenic emissions Yienger and Levy (1995) reported”*

Line 152: Space missing between offline and (OFFL)

Response: Changed as requested.

Line 153: Delete "Finally"

Response: Changed as requested.

Line 183: Chance section from "Meteorology" to "Wind data"

Response: Changed as requested. Line 185 also adjusted "the required meteorology" to 'the required wind data"

Equations 4 and 6: Functions (exp, erfc) should not be written in italics

Response: Changed as requested.

Line 261: Change "equation" to "Equation"

Response: Changed as requested.

Line 307: Change "Divergence" to "divergence"

Response: Changed as requested.

Line 320: Change "seems to be" to "is"

Response: Changed as requested.

Line 358: Change "2d" to "2D" as in line 356.

Response: Changed as requested.

Line 369: Repetition of "emissions", remove "emissions" after inventory.

Response: Changed as requested.

Line 371: Change "or" to "respectively"

Response: Changed as requested.

Line 382: Add: …such as power plants, visible in the top row without the Gaussian filter.

Response: Changed as requested.

Line 382: "with a large spatial footprints": remove "a"

Response: Changed as requested.

Line 383: Change "satellite instrument" to "TROPOMI's spatial resolution"

Response: Changed as requested.

Figure 6 legend: Change "2020-2019 and 2021-2019" to "*2020 and 2019, respectively 2021 and 2019****"***

Response: Changed as requested.

You write Power-Plants and powerplants in the legend of Fig. 6. It's power plants. Please check throughout your text. Sometimes you have power-plant or powerplant.

Response: Changed as requested.

Figure 7 legend: Change "2020-2019 and 2021-2019" to "2020 and 2019, respectively 2021 and 2019" There are only 2 panels, change 3 to 2.

Response: Changed as requested.

Line 423: Change "2019-2020" to "2019 to 2020"

Response: Changed as requested.

Line 429: Somewhere, name the 5 sectors, maybe just in brackets: Only 5 sectors (public power, industry, ...) have locations ...

Response: added as requested, "Only 5 sectors (public power, industry, road transport, shipping and agriculture other than livestock)…"

Line 430, 431, 432, 433: Change PublicPower, Shipping, … to public power and shipping

Response: changed as requested

Line 438: Change "Road" to "road"

Response: changed as requested

Line 440: 3500 km instead of kg?

Response: no correct as stated, it means trucks above 3500kg. Relatively standardized weight above which a different driver license is needed throughout most of the EU. Added "(vehicles with a weight above >3500kg)" for clarification.

Line 444: Change "in inventory emissions" to "the analyzed inventory emissions"

Response: changed as requested.

Line 445: Unit missing 75-100 kt NOx

Response: changed to "kt NOx"

Line 447: What is meant by series and sets? Maybe better both emission estimates

Response: changed as requested.

Line 447: Move reference to Fig. 8 forward by one sentence

Response: shifted forward by one sentence

Line 454: Add: Additionally, this approach…

Response: changed as requested.

Figure 9 legend: Change other line to solid line

Response: changed as requested

Line 465: What is meant by inversions here? I think this is not the right word.

Response: changed "inversions " to " satellite derived emissions"

Line 466: Change "The stronger the source is, the better is the resolvability." to "The stronger the source, the better the resolvability"

Response: changed as requested

Line 552: Change "In future" to "In the future" and delete "also"

Response: changed as requested