Authors' response to Referee#1

First of all, we would like to thank the Referee for his/her positive evaluation of our manuscript and would like to thank him/her for the comments and suggestions, which have helped us to improve the manuscript. Below you will find our detailed, point-by-point responses to the comments and suggestions. Responses are given in blue.

Comments to the Author

The manuscript by Haszpra et al. presents a 10-year time series of carbon monoxide (CO) fluxes measured using the eddy covariance (EC) from a tall tower in Hungary. The study presents CO fluxes separately from populated areas and vegetation-dominated quasi-natural regions. The results indicate that CO emissions from populated areas are substantially higher than emissions from quasi-natural areas. Authors suggest that also quasi-natural areas are a small but statistically significant CO source. The manuscript also suggests that the CO emissions measured from populated areas significantly exceed the current estimates reported in the EDGAR inventory.

The long-term dataset of CO fluxes is valuable, and the topic is relevant for improving our understanding of regional CO budgets in central Europe. However, I have several concerns regarding flux processing, data quality control, and the approach used to separate fluxes from populated and quasi-natural areas. Addressing these concerns is needed before the manuscript can be considered for publication in ACP.

Thank you for the positive evaluation. We have done our best to address your concerns and improve the presentation of the results of our study.

Major comments:

1. Flux processing (Section 2.3)

One of my concerns relates to the validity of the EC flux processing and several methodological choices made during data processing. The description of the processing steps is currently unclear. I encourage the authors to clearly describe the full flux calculation procedure, starting from the raw 5 Hz data to the final hourly fluxes, and revise Section 2.3 accordingly.

Thank you very much for your comments, which have helped make our work clearer and easier to follow, and have strengthened the credibility of the findings presented in the manuscript. Section 2.4 of the revised manuscript (the chapter numbering has changed in the revised manuscript!) now presents the eddy covariance data processing workflow in detail. We provide ample information to make the workflow reproducible and transparent. Originally, we planned to provide only a brief description since the methodology has been published in our previous works. However, we now understand that it is better to have everything in one place for improved readability. At the same time, we sought to maintain a balance between the methodology chapter and the chapters presenting the results/discussions, since the novelty of the study primarily lies in the results and conclusions.

In particular, the authors should explicitly address the following points:

- From the text (Lines 185–186), it is not clear which method is used to determine the time lag between the sonic anemometer and the gas analyzer. Please clarify the method used (e.g. covariance maximization or something else) and specify at which stage of the processing the time lag is calculated (before or after coordinate rotation). Please also clarify what is meant by "interactive visualization of lagged covariance data".

The methodology applied for the calculation of the hourly turbulent fluxes of CO is identical to that used for N_2O flux calculation at this site and is described in detail in Haszpra et al. (2018). In the revised manuscript, we describe how the time lag is calculated. Rather than copying the description available in the Supplementary material of Haszpra et al. (2018), we have reformulated it slightly for the main text of the present paper. Figure R1a-c, which are replotted from the aforementioned paper, demonstrate that the daily, interactive lag time calculation is the supported method.

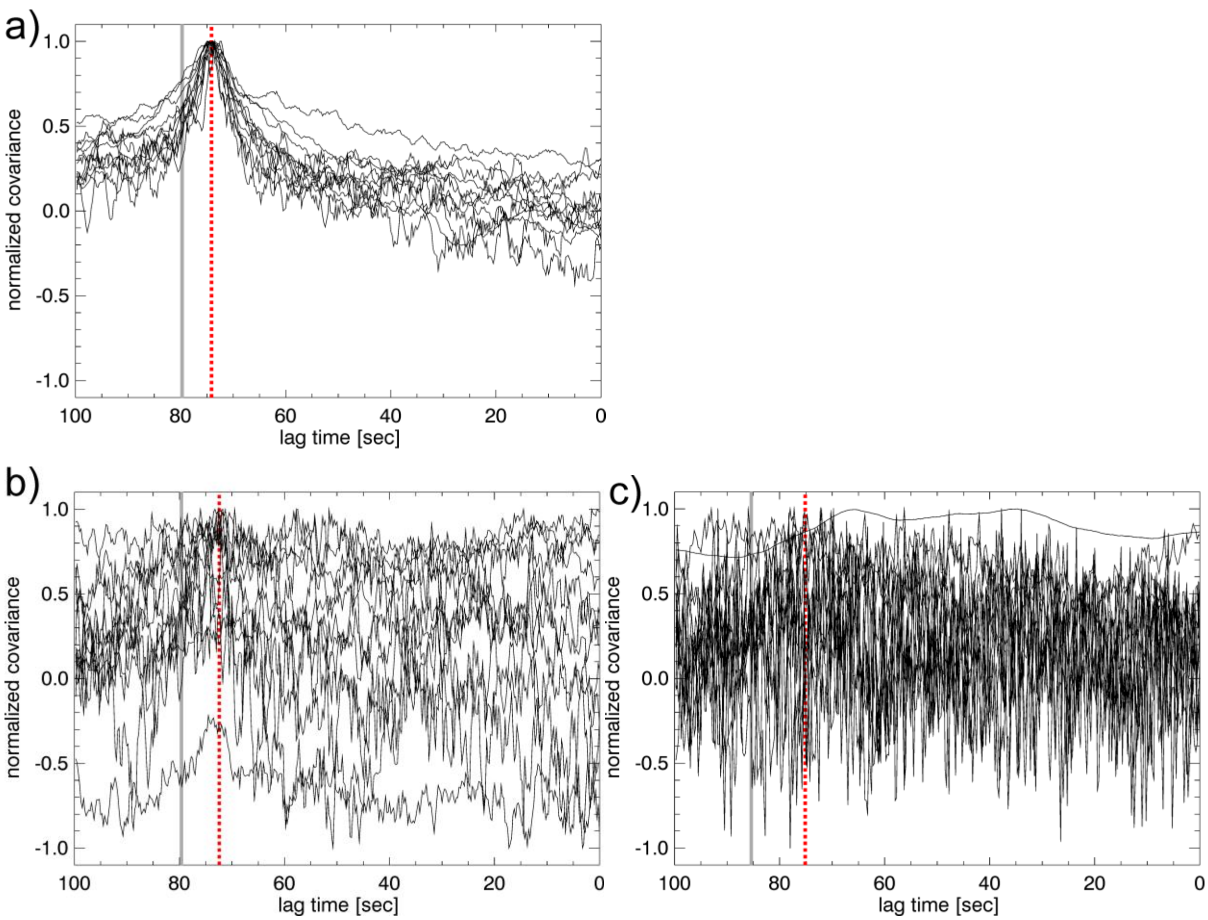

Figure R1 (Fig. S4 in Haszpra et al., 2018). Normalized lagged covariances for N_2O as functions of the time lag for three selected days. a) high flux case (15 July, 2016); b) low flux case (5 August, 2016); c) another low flux case (12 April, 2017). Vertical grey lines show the time lag for H_2O, while dotted red lines show the estimated time lag for N_2O.

The modified text in the revised manuscript, lines 191- 199, reads as follows:

"As a first step, the lag time caused by the ~100 m long air intake tube is determined. The H_2O lag time is used as an indicator for the identification of the CO lag time. It is typically 5-10 seconds longer than the lag time for CO due to the adsorption/desorption of water vapor on the inlet tube wall. For each day, the lag time is manually determined by visualizing the lagged covariance data for all 24 hours. This method was chosen because the covariance maximization would frequently lead to erroneous lag time values for days with low turbulent flux (cf. Supplementary material S2 in Haszpra et al. 2018). Assuming a constant lag time for each particular day is supported by the relatively constant performance of the pump and the visual inspection of the dataset over the 10-year-long measurement period. For low flux days, the lag time can be determined from the lag time from previous days with larger fluxes, and with careful visual selection of some of the lagged covariance curves that correctly indicate the true value (e.g. associated with occasional higher fluxes)."

- The manuscript suggests that the time lag is calculated on a daily basis rather than for each flux averaging period. This is potentially problematic, as the time lag is likely to vary throughout the day due to changes in temperature, pressure, and flow conditions. Could the authors justify this choice? Why not determine the time lag separately for each averaging period (e.g. hourly) by maximizing the covariance, which is common practice in EC flux processing?

See the previous answer.

- I also have concerns regarding the choice of coordinate rotation (Line 187). The use of 3-D wind rotation is not well justified, as the determination of the lateral component (v′w′) is often uncertain and can introduce additional noise into the flux estimates (e.g., Finnigan, 2004). Why not use 2-D rotation or planar fit?

As for the 3D wind rotation, we also extended the text to provide a rationale for the method. Figure R2 (replotted from Barcza (2001)) shows the essence of the method: the wind direction-dependent estimation of the wind vector deflection that was derived using the methodology provided by Lee (1998) and long-term measurement results.

The modified text in the revised manuscript (lines 200- 210) reads as follows:

"Angle-of-attack dependent calibration is applied to the raw sonic anemometer data (van der Molen et al., 2004). Since the EC system is located at a height of 82 m, the slight tilt of the sonic anemometer, the tower body structure, and the surrounding terrain can affect the mean airflow that is sampled by the anemometer. We hence chose the three-dimensional wind vector rotation to align the coordinate system with the prevailing streamlines (Lee, 1998). This method performed better for our site than the popular planar fit method and the simple 2D rotation (Aubinet et al., 2012). Using the technique detailed in Barcza (2001), rotation coefficients are calculated for each 3° wind direction sector using data from long time periods. The length of the time periods is determined by the timing of interventions when the tilt of the anemometer might have changed due to maintenance work. These coefficients are used to determine the rotation angle of the 3D wind for a given wind direction (see Fig. 2.12 in Barcza, 2001), which means that the rotated coordinate system matches the mean streamlines (but allows non-zero mean vertical wind speed – Lee, 1998). Linear detrending is applied to the wind and CO mole fraction time series."

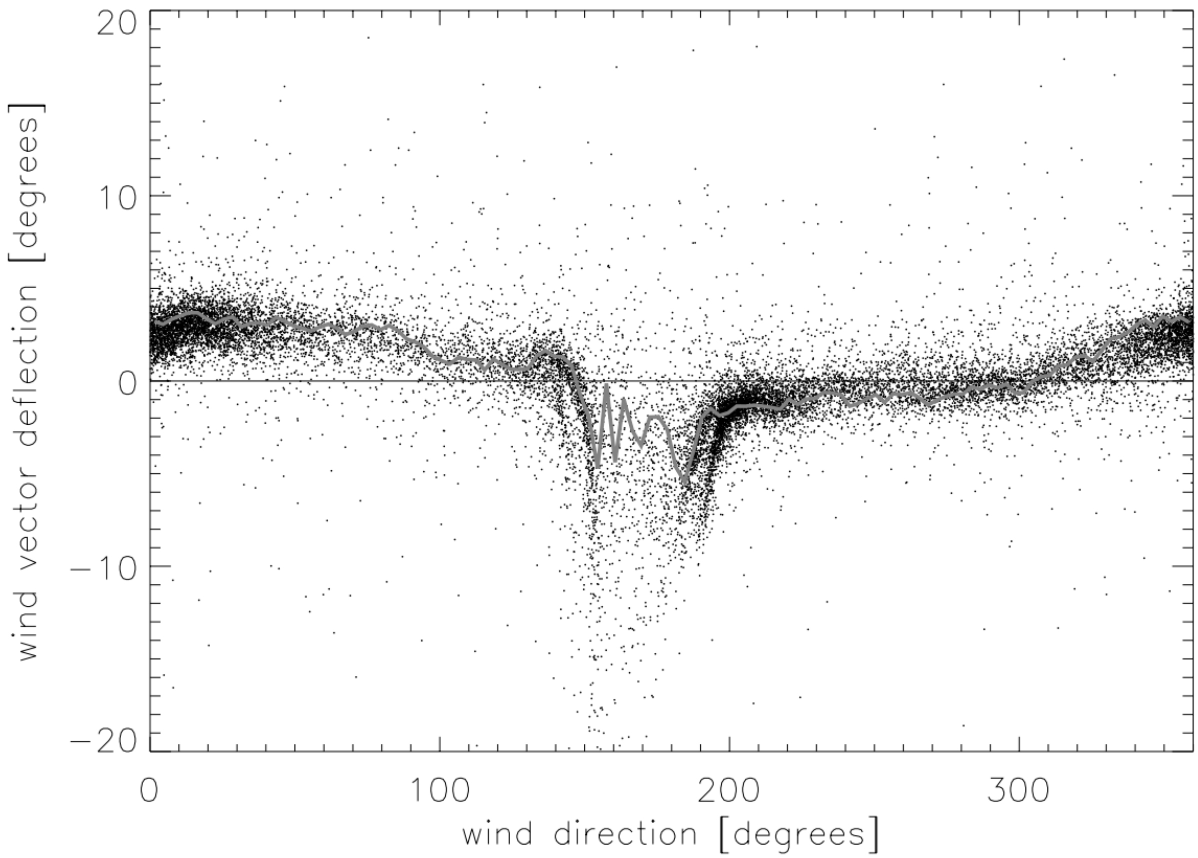

Figure R2. The figure demonstrates the wind vector rotation procedure (Fig. 2.12 in (Barcza, 2001)).

2. Data filtering and quality control (Section 2.5)

The description of data filtering and quality control procedures requires further clarification. While I understand the authors' choice not to remove outliers (Line 205), some form of quality control of turbulent conditions is essential in eddy covariance analysis. Please clarify what types of quality control procedures, if any, have been applied to the data. For example, were fluxes filtered based on stationarity or other turbulence criteria?

During the eddy covariance data processing workflow, we calculate several quality control (QC) flags. Different flags are calculated for CO_2, N_2O, and CO fluxes. Some flags are only calculated for certain components.
According to the authors' experience (note that CO_2 flux measurements have been performed since 1997 – Barcza et al. (2020)), the application of the QC flags in the data analysis is not always the best choice. We think that the flags do not provide enough information for interpretation and further application (e.g. aggregation, gap filling, flux partitioning). The flags are not indicative of the outliers either, as outliers are typically associated with non-informative quality flags. In contrast, most of the fluxes associated with questionable quality fit well with the other fluxes. Specifically, for our study, the issue with the flags might arise from the fact that the eddy covariance system is installed at 82 m height, where larger eddy sizes dominate. Other error sources are also present (i.e., flow distortion by the tower body, advection, convection, etc.).

Hence, we think that in this case, a visual quality control to filter ecologically unlikely fluxes for CO_2 outperforms standard QC flag filtering.
Nevertheless, we tested the applicability of the QC flags to see if there was any detectable relationship between the outliers of CO flux and the flags themselves.
Within quality control, uncertainty estimation was done according to Kroon et al. (2010), who proposed that the majority of the random flux error is caused by the one-point sampling of turbulence. We approximated the uncertainty of the hourly flux values using Eq. (16) in Kroon et al. (2010), which estimates one-point sampling uncertainty. We also used three quality control indicators focusing on the non-stationarity of the horizontal wind (Vickers and Mahrt, 1997). Namely, alongwind relative non-stationarity, crosswind relative non-stationarity, and vector wind relative non-stationarity were calculated based on the raw eddy covariance data. Those four quantities were plotted against the observed CO flux to detect any relationships that could be used to filter data (Fig. R3).

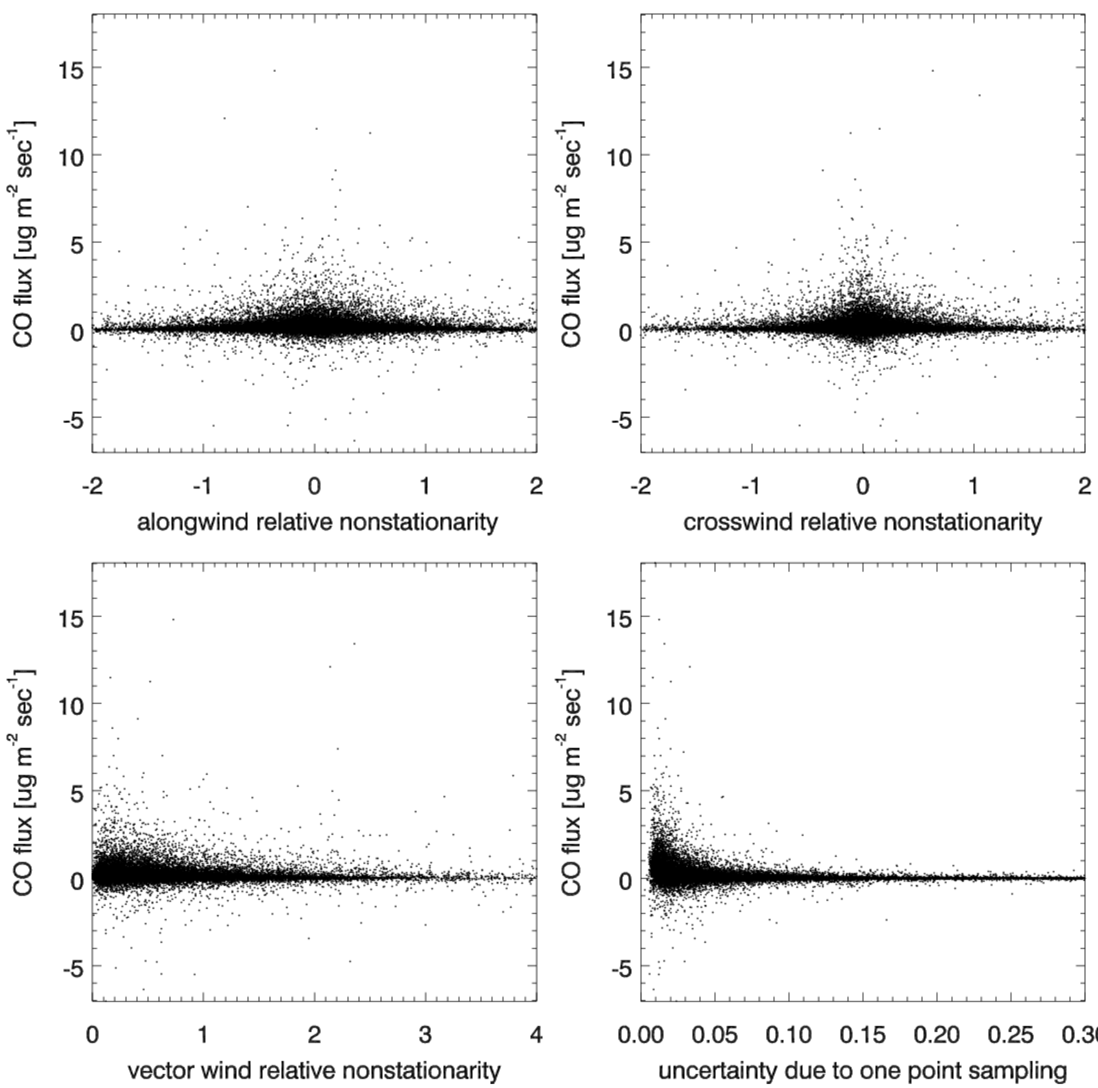

Figure R3. Relationship between the quality indicators and the CO flux for the entire observation period. As can be seen, there is no detectable relationship between the outliers and the larger values of the quality indicators (typically indicating questionable data).

As we did not find any evidence showing that the flux quality indicators could be used for deriving data filtering thresholds or criteria, we decided not to apply them. At our measurement height, random errors are clearly the most important factors affecting the flux. The revised manuscript has been supplemented by the following text (lines 237-245):

"Considering quality control and data flagging based on the calculated quality indicators (Vickers and Mahrt, 1997; Kroon et al., 2010), an attempt was made to check their applicability to filter outliers. However, as there was no detectable relationship between the magnitude of the CO fluxes and the indicators, this filtering was not applied. Another approach would have been to filter the data based on statistical measures (e.g. removing data outside the +/- 3 standard deviation threshold or using percentile-based filtering, etc.). We rejected this approach because we expect significantly different fluxes from the different land cover types (arable lands, forests, villages, etc.) in the heterogeneous landscape surrounding the tower. Hence, no outlier filtering was applied during the flux calculation. It is important to note that the prerequisites for the footprint calculations inherently filter out low wind speed conditions (i.e. strong inversion during nighttime), which represents an alternative quality control."

In addition, it is unclear whether any wind direction filtering has been applied. Are there wind sectors affected by tower structure or platform disturbances, and if so, were these sectors excluded from the analysis? This is not mentioned in the manuscript.

The ultrasonic anemometer is mounted at the end of a 4.4 m long instrument arm at 82 m above the ground, where the diameter of the steel-tube tower is 1.8 m. As Fig. R2 shows, the tower structure affects the flow around the tower, causing a shadow effect. Despite the wind flow modification, wind direction filtering is not performed. We developed a wind direction correction method based on a short-tower (3 m) eddy covariance system installed in the immediate surroundings of the tall tower (cf. Barcza et al., 2003). The wind direction measured at 82 m height is empirically adjusted based on the wind direction climatology at 3 m height, taking into account the changes in wind direction with height. This correction enables the correct localization of the footprint functions (see Barcza et al., 2009). We now mention this correction in the revised manuscript (lines 255-259):

"As wind direction is affected by the tower body (see Barcza, 2001), we developed a wind direction correction method based on the co-located short-tower (3 m) EC system documented in Barcza et al. (2003). The wind direction measured at the height of 82 m is empirically adjusted based on the wind direction climatology of the short tower (3 m), taking into account the changes in wind direction with height. This method was successfully used in earlier studies (Barcza et al., 2009; Gelybó et al., 2013) focusing on footprint analysis based on the tower data."

3. Derivation of populated and quasi-natural fluxes

One of my major concerns relates to the methodology used to separate fluxes from populated and quasi-natural areas, as several key results rely on this approach. The method appears to assume constant fluxes for all populated grid cells and, similarly, constant fluxes for all quasi-natural areas. This is a strong assumption that requires further justification. Could the authors provide additional support for this assumption? For example, if the flux from populated areas is assumed to be spatially uniform, one might expect a linear relationship between the parameter alpha and the measured flux f (as introduced in Equation 1). Demonstrating such a relationship would help evaluate the validity of the approach.

In our model, we assumed that emissions are spatially homogeneous both within the populated areas and in the vegetation-covered, quasi-natural areas. This seems a bold assumption, but despite the long time series, there was not sufficient data to account for spatial differences. Most of the quasi-natural areas are small plots, where owners may change the crop type on a yearly basis, making it essentially impossible to break down emissions into further categories. In populated areas, residential heating is the major source of CO emissions. Natural gas is available in all settlements surrounding the tower, but there is no information available on whether there are significant local differences in the use of natural gas and biomass - or in the burning of waste, if any.

Nevertheless, we examined the relationship between the proportion of built-up areas and the calculated flux, regardless of direction, as suggested by the Referee. Indeed, if emissions from populated areas are spatially homogeneous, then there should be a linear relationship between the proportion of the area and the measured flux. Based on the data shown in Fig. R4, there is a statistically highly significant linear relationship ($p<0.001$) during both winter and summer. We hence conclude that emissions from populated areas may be approximately spatially homogeneous.

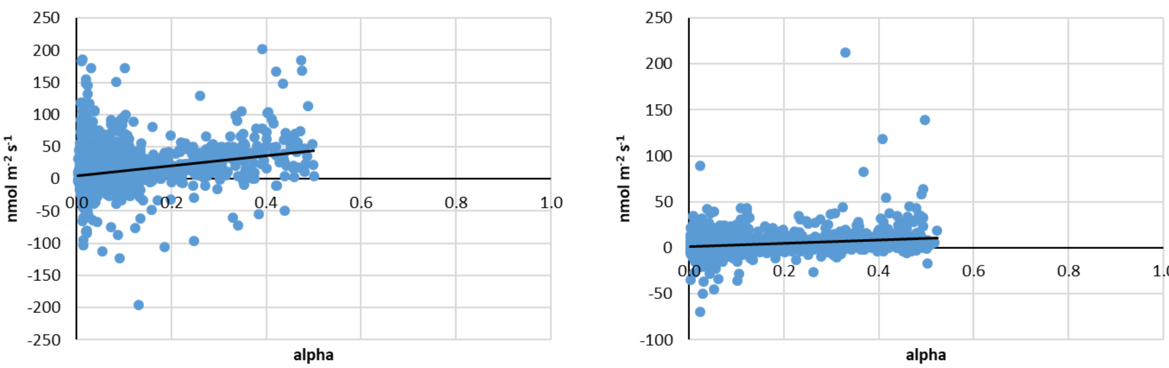

Figure R4. The measured CO flux as a function of the share of the populated area within the footprint in winter (Dec-Feb, N=4890, R^2=0.075, left panel) and summer (Jun-Aug, N=7492, R^2=0.067, right panel)

In theory, if we were to assume a spatially homogeneous flux either for populated areas or for vegetation-covered areas, we could infer spatial inhomogeneity within the other land-use category; however, we think that this approach would be too speculative for a scientific publication.

A second concern relates to the derivation of quasi-natural fluxes. It is not entirely clear how the fluxes shown in Figures 6 and 7 are calculated. Do these figures include all flux data, or only data filtered by a threshold (alpha <2.5%)? Please clarify this in the figure captions and in the main text. Furthermore, the terminology is somewhat inconsistent. In the Abstract, the authors refer to a "vegetation-dominated sector." It is unclear whether this corresponds directly to the quasi-natural grid cells used elsewhere in the manuscript. Please ensure consistent terminology and clearly define these terms throughout the manuscript.

Both Figures 6 and 7 in the manuscript show emissions from (quasi)natural grid cells based on the data obtained from the iteration procedure described in the text. We have modified the figure captions and the related text of the paper to make this clearer to the reader:

"Figure 6: Mean diurnal variation of net CO exchange in the (quasi)natural grid cells (2015-2025) based on the converged data (α < 2.5 %). Whiskers indicate the lower and upper quartiles, while the symbols give the probability level of the deviation of the median from zero. Note the different y-axes."

"Figure 7: Seasonal variation of the monthly mean CO flux from the (quasi)natural grid cells (α < 2.5 %)."

For stylistic reasons, we used the terms "vegetation-dominated" and "(quasi)natural" interchangeably in the study. We reviewed the entire manuscript and made changes wherever it was unclear or misleading.

The authors themselves note that quasi-natural fluxes are likely affected by anthropogenic emissions, which complicates their interpretation. Given this limitation, further analysis would be helpful. I suggest including an analysis of quasi-natural fluxes as a function of wind direction. Such a plot would help assess whether fluxes are consistent across directions or whether certain sectors are more strongly influenced by anthropogenic sources. This is particularly important because the manuscript presents relatively detailed interpretations of quasi-natural fluxes and attributes them to specific drivers. In order to support such conclusions, it would be important to better isolate (or at least quantify) the influence of anthropogenic CO sources.

As shown in Figure 3 in the original/revised manuscript, anthropogenic influence is significant across a wide sector, while the amount of available data from areas predominantly covered by vegetation varies greatly due to the prevailing wind directions. Therefore, a wind sector attribution of the fluxes may result in misinterpretation of the fluxes. Instead, we chose a different method to assess the spatial homogeneity of emissions from (quasi-)natural grid cells.
Using Equation (2) and the monthly average emission data for populated areas, we estimated the flux proportion originating from (quasi)natural grid cells for each measured flux value. This is, of course, only an approximation, since we only have data on emissions from populated areas at a monthly temporal resolution, and we suppose uniform spatial distribution. From the resulting "virtual" (quasi)natural fluxes, we created footprint-weighted maps (Fig. R5) similar to Figure 3 in the manuscript, which thus should exclude anthropogenic fluxes. For this reason, the maps do not contain any data for populated cells (grey pixels = no data).

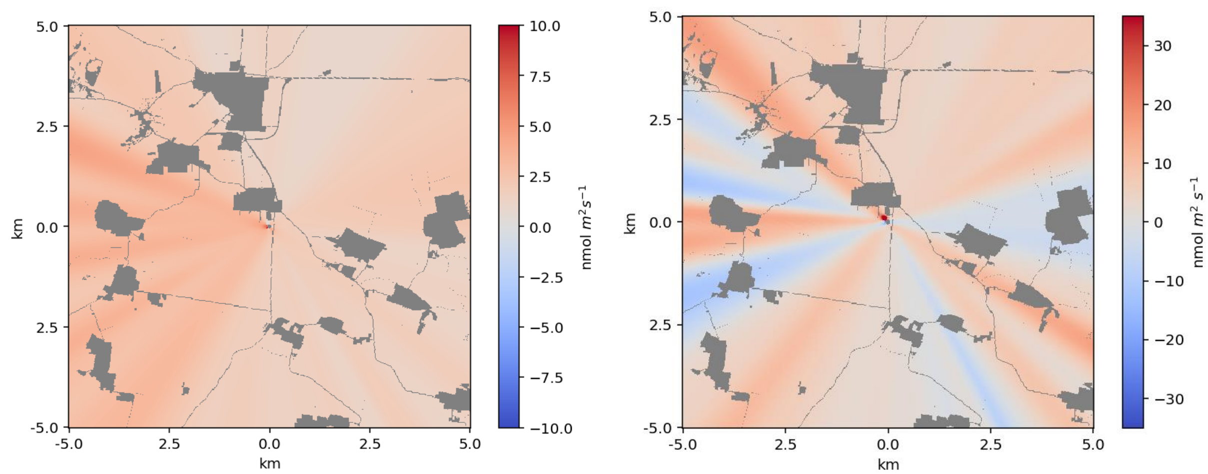

Figure R5. Footprint-weighted flux maps for the summer (Jun–Aug, 2015-2025, left panel) and winter months (Dec-Feb, 2015-2025, right panel) based exclusively on the emissions over the (quasi)natural grid cells. The maps are based on cases in which the footprint contribution over a radius of 5 km from the tower is at least 80 % to the measured fluxes. The tower is at the origin (0, 0). Note the different scales of the panels.

During summer, the spatial distribution of natural flux is fairly homogeneous. Slightly higher fluxes are only detected from the southwest-westerly sector. This relatively slight excess is not linked to the location of settlements (e.g., no such effect is observed in the northern direction) or anthropogenic emissions. It may be due to a higher proportion of forests in that direction, but this cannot be substantiated with the available data. As discussed in the manuscript, the natural emissions calculated for the winter months are distorted by anthropogenic emissions that are two orders of magnitude greater; it is not possible to completely filter out the latter using such a simple model. Accordingly, the winter footprint-weighted map also shows a more heterogeneous picture than that of the summer. Since no systematic directional pattern was evident, the observed heterogeneity can likely be attributed to uncertainty in the estimated anthropogenic emissions. Even a small relative uncertainty in a large anthropogenic flux can propagate into a substantially larger relative uncertainty in the inferred natural emissions, as these are considerably smaller in magnitude. With the caveats mentioned in the manuscript regarding the winter period, we consider the approximate spatial homogeneity of natural emissions to be an acceptable assumption in our simple model. Since we do not observe any signs of the influence of anthropogenic emissions during the summer months, we can assume that our data on natural emissions during the summer correspond to the actual values.

Another important issue relates to the treatment of diurnal variability in anthropogenic emissions. As stated in Line 335, the derivation of quasi-natural fluxes does not account for the diurnal variation of anthropogenic emissions. The manuscript argues that this limitation does not significantly affect the diurnal shape of the derived fluxes. However, this assumption is not convincing. If anthropogenic emissions are lower at night than during the day, applying a constant correction would likely lead to an overcorrection during nighttime, resulting in an underestimation of the quasi-natural flux. This raises the question of whether the diurnal variation shown in Figure 6 could partly reflect biases introduced by this assumption rather than real ecosystem processes. Could the authors evaluate the potential magnitude of this bias?

This point is entirely valid. The diurnal variation in anthropogenic emissions can influence the calculated daily pattern of natural emissions. Unfortunately, there are no data on the diurnal variation in anthropogenic emissions; at best, we can only make assumptions. During the summer (non-heating) months, on which the manuscript primarily focuses, the effect may be relatively small, since, on the one hand, the anthropogenic influence on the calculated natural fluxes is low to begin with, and, on the other hand, the difference between daytime and nighttime values for the relatively low emissions from transportation is not significant. The situation is different during the winter months, when residential heating is the dominant source of CO. Here, it is not so much the day-night difference but rather the intraday variations that may be of interest (e.g., the evening heating of residential buildings). Unfortunately, no data are available on this either. Our paper primarily focuses on wintertime anthropogenic and summertime natural CO emissions. In our assessment, our summer data approximately correspond to the actual situation, while the influence of anthropogenic emissions on the calculated fluxes over the (quasi)natural grid cells is mentioned in the manuscript.

Lines 112-113:

"We focused primarily on analyzing anthropogenic surface-atmosphere CO fluxes during winter and natural CO fluxes during summer."

Lines 481-488:

"The area influencing our measurements performed at 82 m elevation can be about two orders of magnitude larger, and may cover different distant anthropogenic sources. This is true even when the "clean air" sector is carefully selected and constrained to footprints for which the contribution of the distant sources (outside of the 5 km radius of the tower) to the flux measured at the tower is less than 20 %. The wintertime emission in the populated grid cells can be two to three orders of magnitude larger than in the (quasi)natural grid cells. Even if the weighted footprint contribution from a distant grid cell is minor, the potentially huge emission from that cell may substantially influence the measured fluxes. This effect cannot be completely eliminated with our rather simple correction algorithm."

Finally, regarding the interpretation of soil water content (SWC) effects (Lines 366–369): It is not clear that the current approach is well suited to investigate SWC effects. Given that the analysis relies on tall-tower measurements and assumes spatially uniform fluxes across different land-use types, it is difficult to attribute observed signals to SWC with confidence. Previous studies have shown that soil CO uptake can vary between land-use types (e.g. agricultural vs forest ecosystems), which complicates such interpretations.

The spatial resolution of the ERA5 database is 0.25x0.25 degrees. For this study, we used data from the two grid points closest to the tower. According to the literature, natural flux may depend on soil moisture; however, the variability in soil moisture and soil structure, as well as the vegetation type within a single ERA5 pixel (approximately 28×20 km², larger than the whole domain studied in this work), can be so great that it cannot be analyzed using our method. Therefore, in our study, we could only conclude that it is not possible to derive a SWC effect based on the data used. We emphasized this fact in the revised manuscript to avoid any misinterpretation. The dependence of the flux on soil moisture content can only be investigated using shorter EC towers with a smaller footprint and local in situ soil moisture measurements within the footprint area. Lines 429-434:

“However, in our study, the correlation between CO flux and local soil water content (SWC) in the top 0-7 cm layer is not statistically significant (Fig. 8). This suggests that SWC may not be an important driver, at least with the given soil/vegetation characteristics. It should be emphasized, however, that we used data from the ERA5 database with a resolution of 0.25×0.25 degrees for the calculation, while there may be very significant variations in soil moisture content, soil types, and structure within an area of this size. Therefore, the fact that we do not observe a correlation between flux and soil moisture content in the present study does not mean that such a correlation cannot exist.“

4. Manuscript structure

I suggest that the authors revise the manuscript structure to improve clarity and better focus on the most relevant aspects of the study. At present, the Introduction lacks a clear statement of the research objectives. The Methods section includes information (e.g. CO_2 flux and concentration measurements) that is not directly used in the analysis and does not appear essential for the reader.

Thank you for the comments, which help improve the manuscript and make it easier to follow. In the revised manuscript, we specify the research objective in the Introduction. The reference to the CO_2 measurements not relevant to this study has been removed from the revised manuscript.

In addition, some methodological details are presented in the Results section and would be more appropriately placed in the Methods, while certain results are described in the Discussion. There is also some repetition between the Introduction and Discussion; these sections could be more carefully structured to reduce redundancy and improve the overall flow of the manuscript.

The original intention was to write the study in a narrative style, in which the methods and results follow one another in chronological order as they build upon each other, without being strictly separated from one another. Following the Referee's suggestion, we have restructured the manuscript to follow the more conventional "methods, results, discussion" format. The major change was moving the model descriptions from Results (Section 3.2 and 3.3) to Measurements and methods (Section 2.6). The numerical results of our measurements have been entirely transferred from the Discussion into the Results section. In our opinion, the comparison with the

EDGAR database is more appropriate for the “Discussion” section than for the “Results” section; therefore, we have left the results of this analysis in their original location (Discussion).

Furthermore, the Results section would benefit from a more quantitative presentation of the findings. In several cases, including numerical values would improve clarity and strengthen the conclusions.

Thank you for your suggestion. We now present numerical values of all main findings in addition to the previously presented figures.

Finally, while the Abstract emphasizes quasi-natural fluxes, I recommend placing greater emphasis on emissions from populated areas, which appear to be one of the key findings of the study.

The paper focuses on both anthropogenic and (quasi)natural CO emissions. The previously unrecognized underestimation of anthropogenic emissions — due to the role of carbon monoxide in the atmosphere — is indeed a more significant finding than the presentation of data on (quasi)natural emissions, which do not differ significantly from the results of other studies. Therefore, we have moved the data on populated areas to the forefront of the Abstract, highlighting some numerical data, and placed greater emphasis on them than in the original version of the manuscript. However, we had to take into account that the length of the Abstract cannot exceed 250 words.

“**Abstract.** The atmospheric trace gas carbon monoxide (CO) significantly influences the atmosphere's oxidative capacity and indirectly affects climate. However, the net land-atmosphere exchange of CO remains highly uncertain due to the scarcity of long-term monitoring. This study presents and analyses a 10-year-long CO flux data series from a tall-tower eddy covariance (EC) system operated in a rural Central European region. The measured fluxes were evaluated separately for vegetation-covered, (quasi)natural areas and for populated areas. The estimated monthly mean emissions from the populated areas varied between 12 and 110 nmol m^2 s^1, with the lowest value observed in July and the highest in January, respectively. The measured wintertime emissions from the populated areas significantly exceed the activity-based estimations, presumably due to underestimated emissions factors for residential heating. The vegetation-dominated sector (agricultural fields and forests) acted as a weak net CO source from April to September with an average emission of 0.58 nmol m^2 s^1 (monthly range 0.31–0.91 nmol m^2 s^1), with the highest value occurring in July. The diurnal peaks of the median hourly emissions can be observed around noon, and they range from 1.59 to 2.66 nmol m^2 s^1. The nighttime (2004 h LST) hourly median values range from 1.01 to +1.17 nmol m^2 s^1. Their deviations from zero are not statistically significant at a $p<0.05$ probability level. Solar radiation was identified as the primary driver of CO exchange. This study also demonstrates the advantages and challenges of tall-tower EC flux measurements.”

Minor comments

Line 30: Please use the abbreviation CO, as it has already been introduced earlier in the text. Please ensure consistent usage throughout the manuscript.

In most places, we have replaced the term “carbon monoxide” with “CO.” We have retained the full term only in the Section titles and, to some extent, in the Introduction, where it seemed necessary for stylistic reasons.

Line 40: Please use the abbreviation CO_2, as it has already been introduced earlier in the text. Please ensure consistent usage throughout the manuscript.

Corrected.

Line 57-58: The sentence appears incomplete. Please revise and provide appropriate references to support the statement.

The sentence has been reformulated for clarity. Reference is provided. Lines 57-60:

"The CO-consuming microbial activity may depend on the organic fraction of the soil, porosity, temperature, water content, etc., while the production rate of carbon monoxide via thermal- or photodegradation of the living or dead organic matter may depend on plant species (Bruhn et al., 2013)."

Line 60: Bruhl -> Bruhn

Thank you, corrected.

Line 60–61: If referring to Bruhn et al. (2013), it should be clarified that their results indicate that grassland acts as a net CO sink primarily under dark conditions.

We have added this additional clarification in the revised text. Lines 60-62:

"Bruhn et al. (2013) conducted a 2-month survey over a grassland in Denmark using UV-transparent Plexiglas chambers and found that the grassland was a net sink in darkness and a net source in natural sunshine."

Line 63–64: The statement "Cowan et al. (2018) over a grazed grassland in Scotland indicated net emission from the soil-vegetation system, not supporting the net soil sink hypothesis" may be misleading. Even if the ecosystem shows net CO emission, the soil itself can still act as a net sink, with emissions originating from vegetation or litter.

We have reworded and expanded the sentence to better reflect the statement from the referenced article. Lines 63-66:

"However, the 20-month-long survey of Cowan et al. (2018) over a grazed grassland in Scotland indicated net emission from the soil-vegetation system. The nighttime fluxes were around zero. The measurements did not reflect a net soil sink, but the EC system used did not allow the separation of the fluxes from the vegetation and from the soil."

Line 64 and 67: Lassonen –> Laasonen

Corrected.

Line 105-107: I suggest reconsidering the wording here. Emphasizing the limitations of the measurement setup at the beginning of the manuscript weakens the overall presentation. I recommend focusing first on the study objectives and discussing these limitations later in the Methods or Discussion.

We agree with the Referee, and now mention the limitations of the study, the lack of certain measurements in the Auxiliary data section.

Line 130: Please include a legend in Figure 1 to describe the different land cover types, as this would improve the readability and interpretation of the figure.

The NÖSZTÉP land cover map distinguishes between 56 different types of land cover. Adding a detailed legend to the figure seems hardly feasible. As a compromise, we list the typical colors used for the land cover type mentioned in the paper. For interested readers, we have provided a reference to the publication where a detailed legend for the map can be found.

Lines 171–172: This statement would fit better in the Introduction than in the Methods section.

The paragraph has been reformulated to better fit into the auxiliary data section. Lines 175-178:

"UV radiation and soil water content (SWC) measurements are not available at the tower. These variables are mentioned in the literature as potential drivers of the CO exchange between the surface (vegetation+soil) and the atmosphere (Bruhn et al., 2013; Murphy et al., 2023; Laasonen et al., 2025); therefore, we turned to the ERA5 reanalysis dataset of the European Centre for Medium-Range Weather Forecasts (Copernicus Climate Change Service, 2022)."

Line 220: Please clarify how and why these criteria were selected.

These criteria are recommended for the applicability of the footprint model by the developers, and we applied them without revision (cf. Kljun et al. 2015). The sentence is reformulated to make it clearer (lines 264-266):

"In this study, following the recommendations for the application of the FFP model (Kljun et al., 2015), the model was used with the following restrictions: $20z_0 < z_m <$ he, $-15.5 \leq z_m/L$, where he is the height of the entrainment layer. h_e is typically around 0.8 h (Holtslag and Nieuwstadt, 1986). We also applied $u_* \geq 0.2$ m s^{-1} to ensure turbulent forcing."

Line 234: Please clarify what is meant by "technical reasons."

"Technical reasons" cover instrument malfunction, data acquisition gaps, maintenance, etc. We now explain the term in the revised manuscript. Lines 278-280:

"Due to the potential decoupling of the EC system from the surface, we rejected these data points for surface flux estimation. In the case of an additional 16,499 hours, fluxes could not be calculated for technical reasons (power outage, instrument malfunction, data acquisition gaps, maintenance, etc.)."

Line 245: Please use the abbreviation EC for eddy covariance, as it has already been defined. Ensure consistent usage throughout the manuscript.

Corrected and consistently used in the text.

Line 260: Please quantify how much higher the fluxes are from the village direction compared to agricultural/forest areas.

Because each pixel of the footprint-weighted flux map has a different value, a quantitative comparison of the village and the vegetation-covered sectors is not possible. Generally, in winter the fluxes from a grid cell (pixel) in the village sector are an order of magnitude higher than those in the vegetation-dominated sector. In summer, there are regions where the footprint-weighted flux is a small negative value reflecting the overall low fluxes and their uncertainty.

Lines 278–287: Please move this content to the Methods section

This paragraph has been moved to the Measurements and methods section (Section 2.7 Flux map, and separation of populated and (quasi)natural area).

Line 328: Please clarify that this sentence refers to fluxes from quasi-natural areas

The revised manuscript clearly states that Fig. 6 shows the emissions over the (quasi)natural grid cells. Lines 385-386:

"Figure 6 shows the mean diurnal variation of net CO exchange over the (quasi)natural grid cells for each month based on the converged data."

Line 348: Please add some numbers to make results more quantitative.

Fluxes from the (quasi)natural grid cells are 1–8 % of those from the populated grid cells. The numerical values have been added to the text of the revised manuscript. Lines 397-398:

"Fluxes from the (quasi)natural grid cells are only a few percent (1-8 %) of those from the populated grid cells (Fig. 5)."

Line 360: The correlation between CO flux and solar radiation varies seasonally (stronger in July–December than in April–May). Could the authors provide an explanation for this pattern?

This is correct, but at the moment, we have no explanation for this phenomenon.

Line 380: You could compare your results with CO fluxes reported from other tall-tower studies. Are the observed emissions relatively higher or lower?

To the best of our knowledge, our measurements are unique. No one else has yet conducted similar measurements with which we could compare our results. The tall-tower carbon monoxide measurements cited in the Introduction of our paper were conducted specifically in urban environments (London, Paris, Zurich, Munich), and the emission origin differs for these environments. The emissions are hence not directly comparable.

Lines 393–394: "Our measurement recorded 15.2 nmol m^{-2} s^{-1} CO emissions for May-September on average, of which 13.7 nmol m^{-2} s^{-1} may be attributed to the transport sector." It is not clear how these values are derived from the measurements. Please clarify the methodology and assumptions used to obtain these numbers.

According to our measurements, the average emission over the populated grid cells in the period of May-September is 15.2 nmol m^{-2} s^{-1} (Fig. 5). According to the EDGAR database, the residential emissions in summer are only about 10 %, i.e., ~1.5 nmol m^{-2} s^{-1}. Deducting the estimated residential emissions from the measured emissions, we obtained a value (13.7 nmol m^{-2} s^{-1}) for the transport sector, assuming that in summer, these two sectors account for the total emissions. Forest fires and open biomass burning in agricultural areas did not occur during the measurement period in the footprint area according to satellite observations and personal field experience. Lines 457-466:

"Our measurement recorded 15.2 nmol m^{-2} s^{-1} CO emissions for May-September (non-heating season) on average, of which 13.7 nmol m^{-2} s^{-1} may be attributed to the transport sector (90 % of the total). The populated grid cells for which this value was calculated cover 12.8 % of the EDGAR grid cell (84.3 km^2) including the tower. Considering the summer value to be valid for the entire year, the total CO emissions of transport origin are 131 Mg yr^{-1} from the EDGAR grid cell. This value is twice as high as the EDGAR v8.1 average for the years 2015–2022 (60 Mg yr^{-1}).

The annual average flux over a populated grid cell is 48.1 nmol m^{-2} s^{-1} (see Fig. 5). Taking into account the total area of the populated grid cells within the EDGAR grid cell (84.3 km^2 × 0.128 = 10.8 km^2), and summing up the emissions for a year, we obtain that the total measured anthropogenic emissions expressed in mass of CO are around 460 Mg yr^{-1}, from which 330 Mg yr^{-1} can be attributed to residential activity, primarily heating."

Lines 398: Could you clarify how the estimate of total anthropogenic emissions (approximately 460 Mg yr^{-1}) was derived? Does "Mg yr^{-1}" refer to CO mass or to carbon (C)?

The average annual flux is 48.1 nmol m^{-2} s^{-1} over the populated grid cells (data in Fig. 5 of the manuscript). These cells cover 12.8 % of the EDGAR grid cell of 84.3 km^2, i. e., their area within the EDGAR grid cell is 10.8 km^2. Of this, the total anthropogenic emissions falling within this EDGAR grid cell are 48.1*10.8=519.5 mmol s^{-1}. This equates to $16.4 \cdot 10^6$ mol yr^{-1}. As EDGAR provides the emissions in mass of CO over the grid cell, $16.4 \cdot 10^6$ mol yr^{-1} corresponds to $459 \cdot 10^6$ g yr^{-1}. See the corrected text (lines 457-466) above.

Reference:

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