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.

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. 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 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?

- 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?

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?

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.

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.

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.

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.

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?

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.

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.

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.

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.

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.

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.

Line 40: Please use the abbreviation CO2, as it has already been introduced earlier in the text. Please ensure consistent usage throughout the manuscript.

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

Line 60: Bruhl -> Bruhn

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.

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.

Line 64 and 67: Lassonen –> Laasonen

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.

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.

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

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

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

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

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

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

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

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

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?

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

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.

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)?

Reference:

Finnigan, J.J. A Re-Evaluation of Long-Term Flux Measurement Techniques Part II: Coordinate Systems. Boundary-Layer Meteorology 113, 1–41 (2004). https://doi.org/10.1023/B:BOUN.0000037348.64252.45