

Haszpra et al., present a valuable 10-year record of CO fluxes from a tall tower in rural Central Europe, a rare dataset given the scarcity of long-term CO flux monitoring. The authors present CO fluxes for quasi-natural land and populated sectors, attributing fluxes to these categories using a directional flux distribution based on footprint-weighted flux maps. They find that the vegetation-dominated sector acts as a weak net CO source during the growing season (April–September), with a midday peak and no statistically significant nighttime exchange. Solar radiation is identified as the primary flux driver. Populated areas emit CO at rates substantially higher than activity-based inventories suggest, with residential heating identified as the likely underestimated source. These findings are interesting and well presented, however, several methodological aspects require clarification or strengthening before the results can be accepted for publication. The main concerns are 1) general clarity in Methods section, 2) PBL height from ERA5 and its interaction with the nighttime filtering approach and 3) storage flux assumption. These points are elaborated below.

Major comments

- 1. The Methods Section needs more clarity and better structure.** There are several things that need better explanation. For example, it is not clear if the meteorological sensors cited on L116 include a temperature profile, which actually could be used to get a rough idea of the nighttime PBL height. Then in Section 2.2 the reader learns that Air temperature is only measured at 10 m, so what are those meteorological measurements on several heights mentioned earlier? Another example is when the authors state (L186) that “lag time estimation was done interactively by visualizing the lagged covariance data for predefined time windows.” What does “interactively” mean, what was the day to day variability of the lag? One last thing that could improve readability, is that the authors could describe the tower site first and then separately, describe the instrumentation in another (sub)section.
- 2. The PBL height from ERA5, input to the footprint model and nighttime filtering.** I think the authors should make a more thorough assessment of the uncertainties associated with the use of nighttime PBL height from ERA5. For stable boundary layer conditions, ERA5 has been shown to overestimate the PBL height (Sinclair et al 2021) and has also difficulties in the timing of the stable period (Sinclair et al 2021). The study cited is for Finland, but I wonder how would this be for the presented site in Hungary, perhaps worth checking. These aspects could influence the footprint calculation during nighttime and the nighttime filtering at low PBL height, defined by the authors at 100 m based on ERA5. I wonder how is the seasonality in CO fluxes (Fig 6, 7) affected by this filtering? Do you select more/less data points in winter/summer nights? The exclusion of hours with PBL height < 100 m systematically removes the most stable nocturnal conditions when ERA5 has more difficulties in resolving the nocturnal PBL. The finding that nighttime fluxes are not significantly different from zero may partly reflect this filtering rather than the absence of nighttime exchange? A more comprehensive assessment on these things should be presented. For example, nighttime radiosondes from airports could be used to try to bias-correct the values from ERA5 or at least assess the magnitude of the PBL bias.
- 3. Storage flux omission.** The assumption that CO neither accumulates nor depletes in the surface layer during decoupling is physically reasonable and could be partly handled by the filtering of data points at PBLH<100m. However, the omission of the storage term deserves more attention, particularly given that at the same tower some of the co-authors explicitly included a CO₂ storage term in the long-term CO₂ flux methodology (Barcza et al., 2020). The storage correction was deemed necessary for CO₂ at 82 m, yet it is omitted for CO without quantitative justification. An analysis of the potential magnitude of the storage term would substantially strengthen the paper. Two relatively straightforward approaches are available using existing data: 1) a single-point concentration-based storage estimate using the continuous CO concentration record at 82 m, following the approach discussed in Greco and Baldocchi (1996) and 2) an assessment of morning flush peaks in the hourly flux record, which would provide empirical evidence for or against nocturnal CO accumulation below the measurement level. If neither approach shows a significant storage term, that result itself would constitute a valuable justification for the omission.

Specific Comments:

L68: I suggest to rephrase:

“was overcompensated by the mainly radiation-driven emission”

To

“was offset by emissions mainly driven by radiation”

L71-72: What is meant by: “seasonally variable net flux direction”? Do you mean a seasonally variable net sink or source? Perhaps it is worth rephrasing this.

L116-117: It is not clear if these air intake tubes air for CO. I realize that they aren't afterwards because the authors are not calculating storage fluxes. It is not clear for what are these air intakes then, CO₂ profiling system? If so, why adding this here? I think is rather confusing.

L278-284: This reads more like Methods. The authors should consider adding this to the Methods section.

L311-322: Also reads like methods. I also recommend to add a scheme of this back-and-forth and possibly circular methodology.

L402-404: Given this possibly reason (biomass burning), did the authors check fire counts? A quick check on the VIIRS fire counts could strengthen this argument.

L431: With “The longer-term EC measurements recorded diurnal CO flux variations similar to our measurements” did you mean previous studies? Rephrasing needed to better introduce this paragraph.

References

Barcza, Z., Kern, A., Davis, K. J., and Haszpra, L.: Analysis of the 21-years long carbon dioxide flux dataset from a Central European tall tower site, *Agricultural and Forest Meteorology*, 290, 108027, <https://doi.org/10.1016/j.agrformet.2020.108027>, 2020.

Greco, S. and Baldocchi, D.D. (1996), Seasonal variations of CO₂ and water vapour exchange rates over a temperate deciduous forest. *Global Change Biology*, 2: 183-197. <https://doi.org/10.1111/j.1365-2486.1996.tb00071.x>

Sinclair, V. A., Ritvanen, J., Urbancic, G., Erner, I., Batrak, Y., Moisseev, D., and Kurppa, M.: Boundary-layer height and surface stability at Hyytiälä, Finland, in ERA5 and observations, *Atmos. Meas. Tech.*, 15, 3075–3103, <https://doi.org/10.5194/amt-15-3075-2022>, 2022.