Atmospheric Measurement Techniques Discussions

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Comment on amt-2022-39

Anonymous Referee #1

Referee comment on "Real-world wintertime CO, N2O and CO2 emissions of a Central European village" by László Haszpra et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-39-RC1, 2022

The submitted study uses tall-tower Eddy Covariance measurements combined with flux footprint modeling and attempts to discriminate wintertime CO_2, N_2O and CO emissions of a small village from the background “natural” fluxes. A spatial attribution method is used by linking 2D footprint weighting to land cover classes across the measurement domain. The method and the results are novel and interesting and the topic is very relevant to the ongoing research and challenges related to advancing the national and regional GHG emission inventories.

I have however some main concerns regarding the methodology and the subsequent results:

1. My initial concern relates to the validity of the estimated long-term (top-down) village emissions, which are then compared to the bottom-up estimations and subsequently used to estimate the influence on the concentration measurements. The Authors use the median values from 44 – 64 hourly estimates (across several years) to estimate indicative 3-month period emission totals (Lines 280 – 282). The used sample size is extremely small compared to the aggregated period (2 – 3 %) and it is thus very difficult to extract safe conclusions for a parameter with such intense hourly variability as the household emissions.

The variability of household emissions would follow a diurnal pattern according to local population habits for heating (and cooking) along a temporally aggregated pattern (detectable from daily to monthly steps) which would follow mainly temperature. Hence, the sample used can be strongly biased according to the above patterns, especially when considering the multiple climate-related filters on the observation data, which would favour daytime and clear-weather observations.

The interquartile ranges (Table 1) confirm the high variability of village emissions and the related uncertainties in the approach. I suggest the Authors to perform an analysis on the available dataset to examine the possible biases that could affect the conclusions (e.g. statistics on hour of day, type of day, temperature compared to Dec-Feb period, etc.). Furthermore, to present the results with caution, providing uncertainty ranges where possible, and discussing the data representativity, potential biases and their effects on the conclusions.

2. The second main concern is about the assumption of homogeneity in the "natural fluxes". By keeping Fnatural constant on Eq. 2, temperature variability would induce some bias on the estimation of village emissions. Moreover, directionality could be significant and affect the medians used as representative natural fluxes. I wonder if the conclusion regarding the extremely high N2O emissions is somehow affected by the cropland emissions in the direction of the village. Furthermore, the major road on the south and the west of the station could be a confounding factor on the natural flux estimation (mainly concerning the southern wind directions). I suggest the Authors to provide detailed analyses to examine the validity of temporal and directional homogeneity hypothesis and the potential errors associated with it, as well as to discuss the main effects on the methodology and the results.

3. The third concern goes back to the main question raised by the Associate Editor Prof. Domink Brunner. In my first reading of the article I was left with the impression that Eq. 2 was not finally used in the estimation of village emissions and the weighting factor alpha (α) was only used to filter which Eddy Covariance measurements are affected significantly by the village emissions (Fig. 5). But if this was the case, then the subsequent calculations of total village emissions (lines 280 – 282) would not be valid, so I assumed that the method described in section 2.1 was applied. To avoid confusion, I ask the Authors to further clarify the method used to estimate the village emissions. There is a number of unclear points:

- Statistics from two Fvillage estimates are given, at a = 0.25 and a= 0.3. Does this mean that the indicated value is the lower threshold used to apply Eq. 2?

- Is Eq. 2 applied at the hourly Fmeasured and α values, keeping Fnatural constant, to derive the Table 1 statistics for Fvillage?

- The description of the concept in Lines 85 – 96 is not totally clear, confounding flux densities (Fx) with their theoretical areal attribution, without specifically defining the equation of α which is the tool used for this attribution. I think this part needs to be more detailed, giving the Equation used for estimating α.

Specific comments:

Lines 43 – 45: An appropriate reference regarding household emissions is missing.

Lines 57 – 58: A reference regarding the uncertainties related to residential heating in the emission inventories is missing.

Lines 85 – 96, 214 – 215: The description of the method is not very clear (see main point no. 3). It is important to clarify the equation used for estimating α.

Line 111: Please provide the orientation of the sonic anemometer. Are there wind flow disturbances from the tower structure at some wind directions?

Lines 135 – 137: Is this the only data quality check applied on the EC fluxes? What about other quality flags such as steady state and integral turbulence characteristics tests, technical failures, weather effects?

Lines 222 – 224: Please clarify the input dataset concerning the village emissions and the temporal resolution and the time period of the simulations. Is the mean estimated Fvillage used for hourly simulations?

Lines 227 – 244: This part in more related to the theory or the basic concept descriptions, rather than the results section.

Lines 267 – 270: The method used for the estimation of village emissions is not clear (see main point no. 3).

Lines 300 – 332: The description of the bottom-up emission estimation approach would be better related to the methodology section rather than the results.

Lines 334 – 335: This statement is arbitrary. Uncertainty ranges are not given to enable comparison and similarity conclusions between the datasets.

Lines 401 – 402: This statement is a bit strong and not entirely supported considering the uncertainties related to the results.

Section 3: The discussion of the results is absent. The Authors should develop this part of the manuscript considerably, given that the methodology and the results raise several and substantial issues and questions which should be adequately covered (see also main points 1 and 2).

Finally, two points for potential consideration and discussion:

- It is evident that a considerable area covered by croplands, gardens and other green areas is classified as urban (Lines 161 – 165, Fig. 3). This misattribution of land cover to emissions which is not functionally associated to could have an impact on the spatial weighting approach used to estimate urban emissions (ɑ factor in Eq. 1 and 2). It would be interesting to examine the effects of this theoretical attribution on the final results by restricting the definition of the village territory to the "actual" built-up area compared to the existing definition.

- CO appears to be the trace gas that is most efficiently discriminated between village and background values (Table 1). Past studies have used it as an indicator of fossil fuel CO2 emissions by employing some standard CO:CO2 ratio. Having very little experience on the subject, I wonder if the applied spatial weighting methodology can be complementary to a methodology that estimates village emissions by using CO as an indicator.