Authors' response to Referee#2

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. The point-by-point responses given to the comments and suggestions are printed in **blue**.

The paper proposes an original analysis of CO, CO2 and N2O eddy flux measurements made from a tall tower, in order to identify surface emissions at the scale of a village. The presentation of the measurement site, the eddy-fluxes and concentration measurements as well the tools used for the footprint calculations are short but rather clear. The approach is really interesting, and represents an interesting valorization of flux measurements. Very clearly, the main question concerns the uncertainties in the emission estimates, both by the bottom-up method and derived from the eddy flux measurements. Consequently, the main general comment would be to give as much as possible an estimate of the ranges of possible values considering the main identified uncertainties.

Our study was intended to shed some light on a hardly studied question (emission of small settlements) offering a possible methodological solution. Although raw data series was available for six winter seasons, the number of hourly data useable for the estimation of the emission from the village was low (only 44-64 data points depending on the criteria used to filter the data). This low number practically prevented the detailed, quantitative statistical analyses of the results and the scientifically solid uncertainty calculations rightly required by the Referees. Nevertheless, in the revised version of the manuscript, we do our best to present and discuss the uncertainties as much as possible. We add a completely new Supplementary material to present uncertainty-related results that do not fit directly into the main manuscript but seem important to support our assumptions. Below you will find our detailed, point-by-point response to the comments and suggestions.

Few specific comments:

Introduction: could you provide an estimate of the percentage of CO2 emissions related to villages less than few thousands inhabitants either at the scale of Hungary or Europe ?

Unfortunately, we cannot. According to the recent IPCC Report urban areas are associated with approximately two-thirds of the total anthropogenic CO_2 emissions, however, the definition of "urban area" may be different in different publications. In the case of most countries, urban-scale emission data are available only for the (mega)cities. Emission data for small settlements are not available for Hungary and the neighboring countries under similar conditions, at least according to our best knowledge.

"we removed all flux values for the periods when the top of the boundary layer was below 100 m": Given that you have vertical profile measurements of trace gases and meteorological data, wouldn't it be possible to derive the periods with low boundary layer height from observations rather than from ERA5 ?

In an MSc thesis, we tried to use the tall-tower vertical profile measurements for the determination of the height of the planetary boundary layer (PBL). (Unfortunately, the thesis is available only in Hungarian.) Due to methodological issues, the maximum stable boundary layer height that could be reliably derived from the vertical profile data was only 65 m. The eddy covariance system used in this study is mounted above this elevation, at 82 m. Identification of the cases, when the PBL height was at least 100 m, was only possible using the ERA5 database. The flux measurements may be questionable in very stable conditions, like when the top of the PBL is close to the measurement elevation or below that.

"The emission densities of CO, N2O, and CO2 obtained for the natural landscape are 139 ng m-2 s-1, 5.9 ng m-2 s-1, and 12 µg m-2 s-1, respectively": according to the criteria used for the PBL development I guess your methods favors daytime events, compared to nighttime. Still do you have cases for the night ? If so do you see a difference between day and nighttime fluxes ? I do not expect strong diurnal variability for the natural fluxes in wintertime, but for the anthropogenic emissions it must very significant.

Concerning the "natural" emission, there are more daytime data than nighttime ones, however, the difference is not big as it will be presented in the new Supplementary material added to the main paper, and can be seen here in Fig. 1. The daytime-nighttime emission differences are not negligible but do not seem significant at the general uncertainty of the flux data (Fig. 2 below, figures for all three gases will be available in the Supplementary material). For climatic reasons (specific wind direction occurring under specific weather conditions), emission data for the village are available only during daytime hours (Fig. 3). Although there are no quantitative data on the diurnal heating/cooking behavior of the populations, it can be assumed that our data avoid both the nighttime low emission period and the morning/evening high emission periods, and hence should be representative of the diurnal average emission values. This hypothesis cannot be tested with the available data, and the resulted uncertainty must be taken into account in the interpretation of the results. This reasoning is inserted into Section 3.2 (Emission from the village) of the revised manuscript.

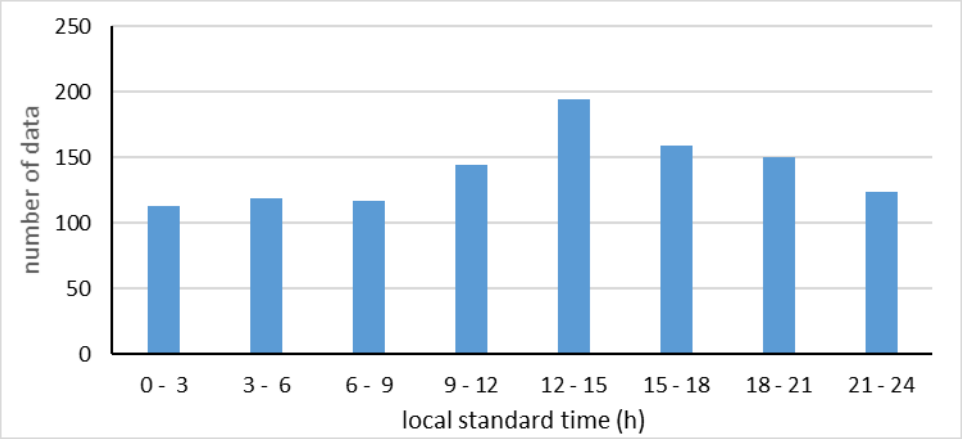

Fig. 1: Temporal distribution of the number of hourly flux data available for the determination of the emission density of the „natural" landscape

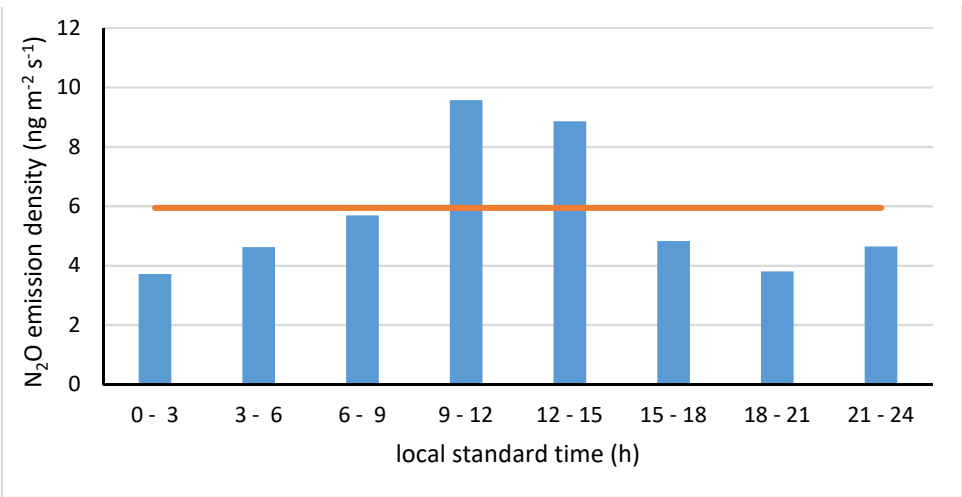

Fig. 2: Temporal distribution of the median emission density of the „natural" landscape, and the calculated overall median emission density (horizontal red line) for N_2O.

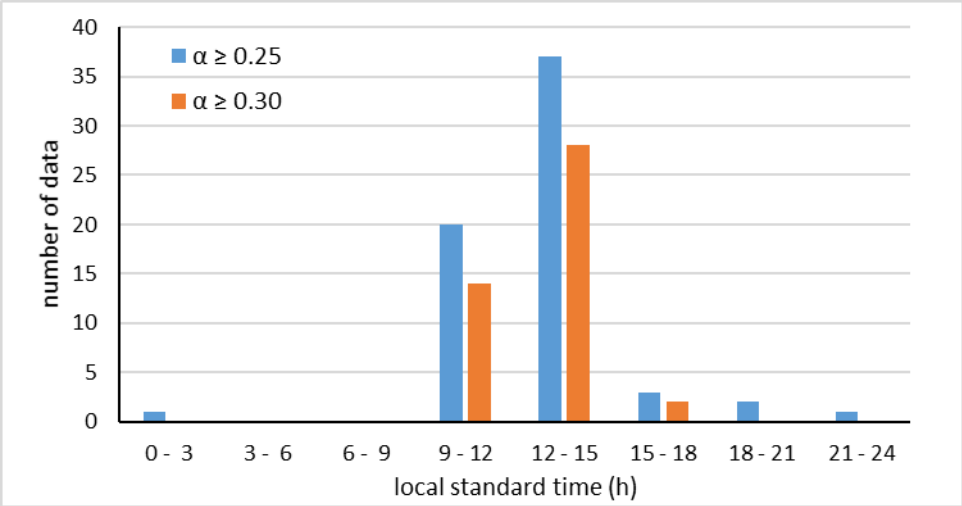

Fig. 3: Temporal distribution of the number of hourly emission density data available for the village at ≥25 % and ≥30 % footprint coverage

"Our concept assumes that both the "natural" landscape … are homogeneous from the point of view of emission density": would it be possible to elaborate on this assumption especially for CO2 ?

Our method assumes homogeneous and isotropic spatial distribution of GHG fluxes from the "natural" landscape. To evaluate whether this condition was met, the measured flux values were grouped into wind sectors of 22.5-degrees, and the median value was calculated for each sector. (See the example figure below (Fig. 4). Figures for all the three gases studied will be presented in the new Supplementary material.) The directional medians were compared with the overall median of the data set using the asymptotic K-sample Brown-Mood median test. Only the median of the southwest direction deviated significantly ($p<0.05$) from the overall median, possibly due to a flux signal from the major road in that direction. Discrimination of a specific direction would have been inconsistent with the otherwise applied α-based filtering, therefore we calculated how much the somewhat higher fluxes from this direction contributed to the overall median. Rejection of the data from the southwest direction would reduce the median CO emission density from 139 ng m^{-2} s^{-1} to 128 ng m^{-2} s^{-1}. The corresponding values for N_2O are 5.9 ng m^{-2} s^{-1} and 5.8 ng m^{-2} s^{-1}, while for CO_2 12 µg m^{-2} s^{-1} and 11 µg m^{-2} s^{-1}, respectively. These small deviations hardly influence the calculation of the emission from the village and are negligible relative to the calculated emissions from the village. Therefore, for keeping the consistency of the filtering method, all available data were used for the calculation of the median emission densities of the "natural" landscape. The sector containing the village could not be tested because of the contribution of the village itself. We hence assumed that the overall median of the "natural" GHG fluxes was also applicable for this sector.

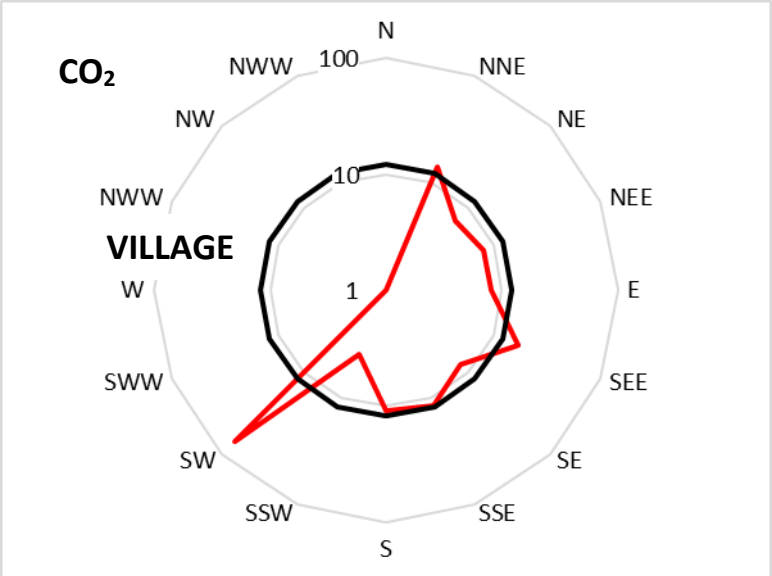

Fig. 4: Directional distribution of the median CO2 flux (µg m^{-2} s^{-1}) of the „natural" landscape (red) and the median flux calculated using all available data (black). There is no data from the direction of the village. Similar figures will be presented in the Supplementary material for N_2O and CO. Logarithmic scale is applied for the better visualisation.

Table 2: The estimation of the emissions by the so-called bottom-up method is well explained in the text, but given the assumptions (at some point in the text an uncertainty of a factor of 2 is considered) , it would be preferable to give the range of possible emissions â â rather than a precise value.

The bottom-up estimations given in Table 2 are the results of several weakly supported assumptions, therefore, only the two extreme cases (everybody uses only natural gas where available/nobody uses natural gas at all) could define the ranges. We do not think that these non-realistic extreme values would orient the readers concerning the uncertainty of the data. Instead, the uncertainties are discussed in the text

Concerning the N2O, it would be interesting to develop the discussion to develop if it is realistic to have an order of magnitude of difference because of the uncertainties on the burning of waste?

According to our best knowledge, waste burning studies focus only on particulate matter and organics emissions because of their health risk. We do not know about any study measuring or estimating N_2O emission from household waste burning, therefore, we cannot answer the question of whether waste burning explains the high N_2O emission. Nevertheless, our previous study, Haszpra et al. (2019) referred to in the original manuscript and using a different methodology, also indicated that the N_2O emission may be much higher than what would be expected from the activity data and the used emission factors. The magnitude difference between the calculated bottom-up and top-down emissions seems too high to think it is only an uncertainty issue of the methods. Taking into account that nitrous oxide is a potent greenhouse gas, studies should be initiated to identify the unknown or underestimated source(s).

The section on the representativeness of the tall tower as regard of atmospheric mole fractions measurements appears decoupled from the main discussion of the paper, but still this is an interesting contribution, but it might make more sense to have this discussion first in the paper.

The Hegyhátsál tall-tower GHG monitoring station was established as a regional background monitoring site, as free from direct anthropogenic influence as it is possible in the highly industrialized, densely populated Central Europe. The selection of the location was based on expert considerations. Later, the diurnal variation of SF6 concentration, the lack of accumulation in the nighttime boundary layer, indicated that the human influence at the site is low indeed. In the present study, the site is explicitly used for the estimation of anthropogenic emissions, which seems to contradict the “background” status of the monitoring site. For resolving this contradiction it seemed necessary to discuss the significantly different footprints of the flux and concentration measurements. It seemed logical to go into these details only after showing up that the site can give information on the anthropogenic emission. We think if this topic were discussed at the beginning of the paper the readers would not understand why this topic is important at all.