Atmospheric Chemistry and Physics Discussions

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Comment on acp-2022-205

Anonymous Referee #1

Referee comment on "Insights on estimating urban CO2 emissions using eddy-covariance flux measurements" by Kyung-Eun Min et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-205-RC1, 2022

Dear Authors,

This study conducted the eddy covariance measurement at an urban area in Korea during a year for evaluating contributions from different emission sources, such as traffic, heating, industrial, and vegetation and validating an emission inventory. Since direct measurements of CO2 emissions are scarce in urban areas, the topic is interesting to potential readers and thus is suitable to the journal. However, due to several limitations in the presented work, I recommend substantial revisions.

Main comments:

- First, the authors did not state the objective of the study in the introduction. Consequently, I hardly justified the main conclusion, new finding, or hypothesis with the data. Furthermore, structure of the manuscript is not well organized. I strongly recommend that the results and discussion sections must be separated for deeper discussions.

- As we know that urban flux measurements were not ideal especially for the instrumentation, I have concerns for the data quality of the eddy covariance measurements. The authors conducted the general quality controls according to Mauder and Foken (2004), but did not provide detailed information. In addition, authors used measured fluxes for all wind directions (Figs. 2, 4) although the flow distortion was expected (Fig. S1). Please show the flow statistics, such as sigma u, v, w per friction velocity for assuring the quality control.

- Traffic CO2 emissions based on the two methods contained substantial uncertainties. First, the traffic counts were measured at a few points within (or outside?) the flux footprint; thus, the estimated emission factor (flux per car) should contain biases. The term emission factor should be inappropriate in this study. Second, more seriously, human activity (e.g., commercial and business activities) could be closely related to traffic count at the diurnal and weekday/weekend scales. The simple regression or weekday/weekend statistics included not only traffic activity but also other human activity. This could overestimate the traffic CO2 emissions.

- For comparison to the inventory, many differences were shown in Fig. 8, but not were well discussed about the concrete reasons. Measured CO2 emissions were several times higher than those by the inventory. Such large discrepancy should be caused by fundamental problems with measurements and/or inventory. The authors must carefully discuss potential problems in the inventory with their calculation methods (e.g., how the inventory estimated emissions in detail). Furthermore, measured CO2 flux also contained missing values, but there was no description how gap-filling was conducted.

Specific comments:

Line 77: "thus easily covers a city scale". The statement is incorrect. Eddy covariance measurements even using a tall tower typically could not cover the entire city. Furthermore, given the heterogeneous nature of the city, spatial representativeness often hampered interpreting measured CO2 emissions.

Table 1: Range of CO2 flux was vague in terms of their temporal coverage. Such information should be described with annual CO2 emissions or mean flux with specified period (e.g., daytime mean at the annual peak month). The unit of car seems to be strange, because the unit of traffic count should be car per period (e.g., car per sec, car per hour, or car per day).

Line 144: The equation of the covariance is too general and should be removed.

Lines 175-189: As mentioned in the above major comment, DOW method contained uncertainties, because weekday/weekend differences in flux could be associated with differences in traffic as well as whether commercial sectors were open or not. Thus, DOW is influenced by CO2 emissions by commercial sectors.

Fig S3. Please change the color scale for easily distinguishing vegetation and non-vegetation areas. Currently, almost all sectors are colored green or yellow.

Lines 217-218: Please explain more detailed information how the authors correct the traffic density.

Lines 226-233: I could not understand how HDD was used for the analysis, because temperature sensitivity was estimated with air temperature (Fig. 7). Add more information in details.

Lines 240-244: For upscaling CO_2 emission to city scale, floor number of buildings must be considered for commercial and residential sectors in addition to aerial coverage.

Fig. 1b: Please show actual photos rather than deformed schematics because readers more easily understand the instrumentation based on the photo rather than the image.

Lines 279-280. I am not sure how the authors wanted to explain using this statement. If the authors wanted to mention air storage or underestimates in turbulent fluxes, discuss more details in a quantitative manner.

Line 292-293. Polar plots in Fig. 4 are interesting, but were not described how is was conducted in the method section. Add detailed methods with relevant citations.

Fig. 5 and lines 309-310. Weekday/weekend difference in cars seems to be marginal. Please explain how differences were statistically significant. The unit of Fig. 5a should be [number per hour]. Furthermore, how are weekday/weekend differences in traffic count consistent among the traffic count sites? How did the range of inconsistency among the sites affect the estimates in the traffic CO_2 emission?

Line 308: Be quantitative manners.

Line 364: I cannot understand how HDD was used.

Line 371: As mentioned above, further quantitative discussion is required.

Line 376: I cannot understand the rationale of this statement “were able to estimate from … strategies”. Is this supported by the current data analysis?

Line 392: The CO2 uptake by vegetation is too high. Please see Fig. 2 in Baldocchi (2014) which showed that range of annual CO2 uptake by natural or disturbed ecosystems.

Line 420: Based on the current environment for open data science, “author upon request” seems to be insufficient. Please use public databases, such as KoFlux, FLUXNET, or other open databases.

Reference

Baldocchi, D. 2014. Glob. Change Biol., 20, 3600-3609.