

Interactive comment on “Observing local CO₂ sources using low-cost, near-surface urban monitors” by Alexis A. Shusterman et al.

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This paper uses a set of observations from low-cost, near-surface sensors to examine relationships between CO₂ mole fraction and traffic counts, showing strong relationships at individual sites. They also investigate correlation length scales between different sites, and develop a multiple linear regression method to establish relationships between CO₂ mole fraction and the factors that influence it. The methods described appear to give exciting results showing that traffic fuel efficiency can be monitored by combining these methods with traffic count/flow information.

The data and results in this paper are interesting and entirely appropriate for publication in ACP. The major flaw in this paper is that insufficient detail of the methods is given,

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and mostly only higher level data products (correlation coefficients, MLR coefficients) are given in most places. There are several instances where methods first described in other papers are here described too briefly to be understood without detailed reading of the previous publications. In other cases, figures that are key to understanding the methodology are given only in the supplementary material, and details of the “raw” information that goes into the main figures are lacking. Thus it is difficult to evaluate the robustness of the methods, and readers will have difficulty repeating the analysis or trying it out themselves. ACP doesn’t have major length limitations, so the main text should be expanded so that the methodology can be followed. Specific instances are noted in my further comments.

The concepts, data and written language are all good, but I recommend major revisions to expand the explanations of the methodology and show more of the CO₂ measurements and comparison to the MLR model. This will allow the reviewers and readers to better evaluate the robustness of the methodology.

Specific comments:

The effect of low-precision measurements is not detailed anywhere in this paper. How does the 0.5 ppm precision impact the results discussed? How is drift in the sensors accounted for, and how will this impact the results, particularly the concept that changes in traffic fuel efficiency could be monitored over time?

Although the authors acknowledge that traffic contributes only 40% of CO₂ emissions, they then focus on only traffic emissions in the analysis. While sites very close to major roads will indeed be strongly influenced by the proximal road, sites further from roads will be influenced by multiple roads as well as the other anthropogenic sources, AND by biogenic CO₂ sources and sinks. How are these other sources considered? If they are ignored in this analysis, please justify why.

Pg 1 line 25. The % of global CO₂ emissions from urban areas varies depending on how it is determined . 70 to 80 % is probably a better estimate.

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Section 2.2. Traffic counts. For the sites very close to a particular highway, traffic monitor data for that nearby highway makes sense. For sites that are further from any particular highway, even if traffic is the dominant proximal CO₂ source, surely more than one highway (and local roads as well) will contribute to the signal observed at that site. How are multiple sources accounted for?

Pg 3 lines 31 -32, pg 4 lines 1-3. Please expand to explain the methodology used here in enough detail to be followed without requiring the reader to refer to the McKain paper. Are these correlation lengths determined using CO₂ mole fraction, or the enhancements in CO₂ relative to background? Where is the raw data that is used to derive these correlation lengths? Plots of the CO₂ time series should be included (these could go in supplementary material).

Pg 4 lines 6 – 14. Please explain what the correlation lengths should be interpreted to mean. I take it that a shorter correlation length implies more influence of sources close to the sites. Longer length scales would imply more influence of sources further away? The preferred studies are all about pollutant gases, not CO₂ – it might be reasonable to expect higher correlations and longer length scales for a long-lived gas like CO₂ with large and varying background.

Pg 4 lines 18 -19. I don't follow why the daytime correlations imply this information. Please clarify.

Pg 4 line 33. Figure 4, not figure 2?

Pg 5 lines 1 – 10. It is curious that the amplitude of the diurnal cycle is larger in winter than in summer. The timing of the diurnal pattern shown in figure 4 doesn't quite gel with the argument that this is due to lower/stronger daytime boundary layers. Have the authors considered the influence of biogenic CO₂ fluxes, which may be more important in the rainy winter season in San Francisco than in the summer?

Pg 5 lines 26 – 35. I pity those commuters who are contributing to high traffic flows at

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4 am!

“An alternative analysis using traffic density...” I think I understand that this is an attempt to examine how congested vs free-flowing traffic might change the results? Please clarify.

“We observe a factor of 2 difference in local CO₂ between congested vs free-flowing conditions” – which is higher?

How is the regression slope determined, and how is the uncertainty determined? This needs further detail, particularly because the regression slope is determined not from the full dataset, but from a fit to the median values. The idea that trends in fuel efficiency can be tracked by this method is tantalizing, but the statistics must be demonstrated to be robust.

Pg 6 lines 5-17. Not enough information is given to understand how these MLRs are constructed and therefore how they can be interpreted. Please expand on the method, and provide further details on which factors were most important. On the following page (lines 5-12), there is discussion about how improved resolution of the meteorological datasets would help, but nowhere is the current resolution and limitations of the data explained!

It isn't clear how the “modelled” CO₂ values are determined by this method. Figure S5 is the only place where the MLR and CO₂ values are compared – please include in the main manuscript and expand the discussion of the quality of the model results. Figure S5 is a little misleading – it is easy to make it appear that there is good agreement when the model gets the diurnal cycle roughly right. But it does appear that there are large differences hour by hour. It is hard to tell at the scale shown, but it looks like the model is not capturing the morning rush hour peak very well at all.

Pg 6 lines 18-24. How is the intercept of the MLR calculated? Where is the data that shows this? I don't understand how the value of 426 ppm is determined. These MLR

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coefficients are key the rest of the interpretation but never clearly explained.

Pg 6 lines 21-24. Why it should be expected that the background is the same in this winter analysis as for the summertime?

Pg 6 lines 32-34. Where do these enhancement percentages come from? I can't see where they are calculated, nor is the CO₂ data for these sites ever shown. I understand that the MLR coefficients are useful for interpreting the data, but the CO₂ mole fractions need to be shown for each site as well.

Pg 7 lines 3-4. As in a previous comment – how are the correlation uncertainties determined? This is an exciting result but the uncertainties must be shown to be robust to make it believable.

Pg 7 lines 13-18. The two papers cited only discuss NO_x and particulates, and I suggest that the authors also refer to the literature on CO₂, CO₂f, or CO ratios, for example: Miller, J.B., Lehman, S.J., Montzka, S.A., Sweeney, C., Miller, B.R., Wolak, C., Dlugokencky, E.J., Southon, J.R., Turnbull, J.C., Tans, P.P., 2012. Linking emissions of fossil fuel CO₂ and other anthropogenic trace gases using atmospheric ¹⁴CO₂. *Journal of Geophysical Research* 117, D08302. Turnbull, J.C., Sweeney, C., Karion, A., Newberger, T., Lehman, S.J., Tans, P.P., Davis, K.J., Lauvaux, T., Miles, N.L., Richardson, S.J., Cambaliza, M.O., Shepson, P.B., Gurney, K., Patarasuk, R., Razlivanov, I., 2015. Toward quantification and source sector identification of fossil fuel CO₂ emissions from an urban area: Results from the INFLUX experiment. *Journal of Geophysical Research: Atmospheres*. Lopez, M., Schmidt, M., Delmotte, M., Colomb, A., Gros, V., Janssen, C., Lehman, S.J., Mondelain, D., Perrussel, O., Ramonet, M., Xueref-Remy, I., Bousquet, P., 2013. CO, NO_x and ¹³CO₂ as tracers for fossil fuel CO₂: results from a pilot study in Paris during winter 2010. *Atmospheric Chemistry and Physics* 13, 7343-7358. Baker, A.K., Beyersdorf, A.J., Doezema, L.A., Katzenstein, A., Meinardi, S., Simpson, I.J., Blake, D.R., Sherwood Rowland, F., 2008. Measurements of nonmethane hydrocarbons in 28 United States cities. *Atmospheric Environment* 42,

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170-182. Warneke, C., McKeen, S.A., de Gouw, J.A., Goldan, P.D., Kuster, W.C., Holloway, J.S., Williams, E.J., Lerner, B.M., Parrish, D.D., Trainer, M., Fehsenfeld, F.C., Kato, S., Atlas, E.L., Baker, A., Blake, D.R., 2007. Determination of urban volatile organic compound emission ratios and comparison with an emissions database. *Journal of Geophysical Research* 112. Nathan, B., Lauvaux, T., Turnbull, J.C., Gurney, K.R., 2018. Investigations into the use of multi-species measurements for source apportionment of the Indianapolis fossil fuel CO₂ signal. *Elementa: Science of the Anthropocene* 6. Barnes, D.H., 2003. Urban/industrial pollution for the New York City–Washington, D. C., corridor, 1996–1998: 1. Providing independent verification of CO and PCE emissions inventories. *Journal of Geophysical Research* 108.

Pg 8 lines 1-5. The work in this and previous papers by this group has made huge inroads using low-cost, high-density CO₂ sensors to examine urban emissions, and have shown some clear pathways to where this method is useful. Yet the high-quality, low-density systems favoured by other researchers have also provided exciting results. Indeed, this paper uses high-quality measurements to calibrate and validate the low-cost sensor array. It really isn't helpful to pit the two methods against each other as if only one method is valid. Rather, I suggest that the authors reword to emphasize where the methods are complementary.

Pg 8 lines 5-9. “. . .we do so without the use of computationally intense and heavily parameterized atmospheric transport models.” The implication here is that this is the first CO₂-based study to achieve this, whereas in fact a number of studies have gone in this direction. See for example the references given above, and: LaFranchi, B.W., Pétron, G., Miller, J.B., Lehman, S.J., Andrews, A.E., Dlugokencky, E.J., Miller, B.R., Montzka, S.A., Hall, B., Neff, W., Sweeney, C., Turnbull, J.C., Wolfe, D.E., Tans, P.P., Gurney, K.R., Guilderson, T.P., 2013. Constraints on emissions of carbon monoxide, methane, and a suite of hydrocarbons in the Colorado Front Range using observations of ¹⁴CO₂. *Atmospheric Chemistry and Physics* 13, 11101-11120. Turnbull, J.C., Tans, P.P., Lehman, S.J., Baker, D., Chung, Y., Gregg, J.S., Miller, J.B., Southon, J.R., Zhao,

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Pg 8 lines 8-10. The interpretation also requires high quality traffic count information, which is not available everywhere and will be a significant limiting factor.

Figure 7. Suggest showing all data rather than just the median values – figure 5 shows that there is a lot of scatter in the individual measurements that shouldn't be ignored.

Jocelyn Turnbull, GNS Science, June 20, 2018

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