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Atmospheric Measurement Techniques Discussions

Interactive comment on "Traffic-related air pollution near roadways: discerning local impacts from background" *by* Nathan Hilker et al.

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Response to Anonymous Referee 3

The authors are grateful for the useful comments and technical corrections outlined here. Referee comments are shown in *italicized* text below, with author's replies and actions following each comment.

Major comments

1. In Tables 2-4, the authors should reorganize their presentation to show and directly compare results of all three methods for estimating local contributions (C_L) to

measured concentration at NR-TOR-1 (Table 2), NR-TOR-2 (Table 3), and NR-VAN (Table 4). The current organization of these tables emphasizes comparisons across the measurement sites, whereas the main point of the paper is to compare methods for estimating C_L.

Reply: We agree with this suggestion. Condensing the information into a singular table as you have suggested is likely the best way of presenting relevant information as efficiently as possible. Alternatively, the suggested table could be shown visually in a figure, in which the local concentrations determined by each method are compared between pollutants and sites.

Action: Tables 2-4 will be moved to the supplementary information, while the mean C_L values from each will be agglomerated into a single table or figure to be included in the manuscript.

2. I suggest the authors verify their regression coefficients relating pollutant concentrations to wind speed are consistent via separate analysis of weekday and weekend conditions: traffic conditions and emissions change on weekends, whereas average meteorology should be the same.

Reply: This is a great suggestion and there is no reason not to include it in an updated manuscript version. As you have pointed out, since average meteorology should be similar between weekdays and weekends, regression between these two subsets should yield similar results. The primary difference between weekdays and weekends (aside from the frequency of data) are the volumes of traffic, which would yield greater local concentrations with respect to mean values, so the regression would effectively be modelling higher and lower ranges.

Action: The suggested analysis will be performed and included in the supplementary information.

3. The presentation of NO/NO2 ratios is unconventional. I suggest reporting NO2/NOx instead, where NOx = NO + NO2. The reasons for variations in NO2/NOx among sites should consider differences in background ozone, transit/residence time in near-roadway setting, differences in diesel truck fractions (diesel has higher NO2/NOx ratio in primary emissions). Also it appears the calibration of the chemiluminescent NOx analyzers was only checked regularly for NO. Was there any checking of NO2 converter efficiencies?

Reply: We agree that it is more sensible to instead report the ratio of NO2/NOx and will update the discussion of results in accordance with this.
Thank you for pointing out the converter efficiencies of the NOx analyzers. Indeed, the NO and NOx channels were calibrated using an NO standard located on-site. The manuscript needs to be updated to indicate that each station had either a Thermo 146i gas calibrator or an Environics 6100 multi-gas calibration system (only NR-TOR-2 used the Thermo). In addition to mixing various flow rates of zero and span gasses, these calibrators also have UV lamps, allowing O3 to be generated by a calibrated amount. This was how the O3 analyzers were calibrated. Additionally, following each NO/NOx calibration, a significant amount of O3 was generated (about 50% of NO by mole) to test the efficiency of the molybdenum converters. Generally, the efficiency of these converters was very close to 100%, and the test was only done to ensure a conversion efficiency of > 99.5%. The NO2 coefficients were left at 1.000, and if the instrument's converter looked like it was struggling (i.e. < 99.5%) then it was sent back to Thermo Scientific for calibration/maintenance. The fact that molybdenum converters were used is another important point as they cannot distinguish between NO2 and more oxidized forms of nitrogen: NOy (NOz – NOx). Being that local NO2 was defined

by short-term temporal fluctuations, however, it is doubtful that NOy (which is primarily affected by secondary chemistry) contributed to it substantially.

Action: Greater discussion of local quantities of NOx will be included in the results section, with ratios being reported as NO2/NOx rather than NO/NO2. Also, the methodology section will be updated to mention both calibrator models (Thermo 146i and Environics 6100) and converter efficiency checks for the 42i.

Minor Comments and Technical Corrections

Line 158, 193: minutely should be rewritten as one-minute

Reply: Changed.

Line 242: many such algorithms (omit "of")

Reply: Changed.

Line 302: non-tailpipe PM emissions such as brake and tire wear and road dust are expected to be predominantly in the coarse mode and should not contribute much to fine particle mass (PM2.5).

Reply: The text will be updated to emphasize this fact. While it is true that non-tailpipe emissions are generally greater than 2.5 microns in diameter, these sources still contribute enough to the PM2.5 size range to produce discernible differences between sites, and these differences are generally more heterogeneous than things such as secondary organics, for example (see Jeong et al., 2019).

Lines 319-320: fix wording: the reason these values...is believed to be due the following reason

Reply: This sentence will be reworded for clarity and brevity.

References:

Jeong, C-H., Wang, J. M., Hilker, N., Debosz, J., Sofowote, U., Su, Y., Noble, M., Healy, R. M., Munoz, T., Dabek-Zlotorzynska, E., Celo, V., White, L., Audette, C., Herod, D., and Evans, G. J.: Temporal and spatial variability of traffic-related PM2.5 sources: Comparison of exhaust and non-exhaust emissions, Atmos. Env., 198, 55-69, doi:10.1016/j.atmosenv.2018.10.038, 2019.

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