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Interactive comment

Interactive comment on "Evaluation of traffic exhaust contributions to ambient carbonaceous submicron particulate matter in an urban roadside environment in Hong Kong" *by* Berto P. Lee et al.

Anonymous Referee #1

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GENERAL

This paper reports measurements of carbonaceous particles at a busy street canyon. The authors focus on primary hydrocarbon like (HOA) and elemental carbon (EC) fraction of the aerosol. They combine the concentration data with engine-type specific vehicle counts to obtain the contribution of diesel, gasoline, and LPG vehicles. The changes in emission regulations and the rapid development in engine and after-treatment techniques make efforts such as this necessary in evaluating the effect of the policies and development on the air quality. The measurement site is ideal for this kind of study because of the high contribution of traffic and the local measures taken

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Discussion paper

for decreasing the traffic emissions. The paper brings new information on an important question and merits publication, but there are some issues that should be treasted, as discussed below.

I understand that there are so many aspect on carbonaceous aerosols that the discussion needs to be limited somehow. However, I find the approach especially in the introduction too limited to the Hong Kong primary aerosol case. The introduction should shortly discuss the relative importance of the primary and secondary aerosol in urban settings and not just mention the secondary aspect in the last sentence of the paper. The authors should make a case why it is still important to study the primary components. In presenting and discussing the results, the authors should clearly, preferably in a table, report the primary and overall concentrations. The effect of renewing fuels and vehicle fleet on the urban concentrations has been studied also outside Hong Kong. These studies should be discussed, starting with the Harrison and Beddows, Nature 2017 paper.

There are recent papers on the same measurement cite, partly by the same authors. The authors should clearly state what is new here, especially compared to the Lee at al. 2015 JGRA and Huang et al ACP 2014 papers. There is an important methodological difference to the Huang paper in treating the traffic related OC. The difference in methodology and results, as well as the meaning of those should be discussed more explicitly.

Finally, the methodology and the analyses appear solid but they are treated so concisely that the reader needs to deduce what data was used. The methods are partly the same as used in the papers mentioned above, but this paper should also be readable separately. As this media allows some volume, the methodology should be described in more detail.

SPECIFIC

Abstract:

The abstract should highlight the new findings of this paper and report them quantitatively.

Methodology:

The methodology part should be rewritten to work on its own for this paper. Reading just this paper, it remains unclear what instrument data was used for, e.g., the engine specific contributions. Was this done on hourly basis as is indirectly indicated on the vehicle flow chapter? Obviously AMS data was used for the HOA, but what about EC: Aethalometer or the hourly EC measurement? And how did the latter two compare? Huang et al., 2014 should be cited already in the methodology part for the hourly EC/OC. An obvious measurement missing from the study is CO2. This inhibits the calculation of emission factors and should be clearly stated.

Vehicle identification method to gain engine-type specific emissions seems to have been used at least in tunnels, although not necessarily documented very well. The authors should cite those, such as Cui et al., STOTEN 2016. They might also discuss the individual-vehicle specific approach of Wang et al., AtmEnv 2016.

Results:

The paper reports the contributions of the vehicle types, but also PM2.5, PM1, EC and OC concentrations should be given, maybe in a table gathering the results. Those reported earlier with a citation.

At least Zhang et al., ACP 2005 should be cited for using HOA as a surrogate for combustion POA. In discussing the seasonal variations and gaseous/particulate phase partitioning, Robinson et al., 2007 should be cited, maybe together with comparison to some other studies, such as Budisulistrioni et al., ACP 2016; Huffmann et al., ACP 2009.

The paper reports exceptionally high EC fractions (EC/HOA ratios) for gasoline vehicles. This is an interesting and potentially important finding, but also subject to controversy. While the site-specific driving patterns could explain some of this, other aspects should also be discussed. A high fraction of gasoline direct injection (GDI, DISI) vehicles could possibly affect this, as they have been found to exhibit high primary EC fractions (e.g. Karjalainen et al., ACP 2016; Fushini et al., AtmEnv 2016). On the other hand, the HOA concentration of this study comes from AMS, while the EC does not. The low sampling efficiency of the AMS for sub-50 nm particles could cause low HOA concentration as a measurement artefact, especially if a high mass fraction of the POA is within the nucleation mode.

Details

In section 3.3.2. it would be good to explicitly state that the reconstructed mass does not include SOA.

The Environmental Protection Department (?) is differently named in the reference list.

Kirchtetter et al. is misplaced in the alphabetical reference list.

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