Response to Reviewer's Comment

(Manuscript No. acp-2014-359)

Reviewer Dr. Cebrunis

1st comment

The paper by Lin et al. is a comprehensive study characterizing size-segregated particulate matter and trace metal emissions in the tunnel. Tunnels are indeed excellent natural laboratories, but I wonder if the authors did a strategic mistake in the sampling set-up greatly diminishing the value of their study. The Equation 2 of the paper can only be applied to a closed system, i.e. applying a box model which implies that an air parcel enters the tunnel at the inlet and exits at the outlet accumulating emissions along the length of the tunnel. The "box" should be "airtight" - no exchange of air is allowed with the clean outside air inside the box. Based on the description of the experimental set-up there was automatically activated air exchange system (triggered by CO exceedances and/or randomly) in-between the inlet and outlet sampling points, thereby invariably diluting accumulated emissions in the air. Considering the dilution effect, Equation 2 can only be used in sections where no active air exchange has happened (not the 8.9 km length of the tunnel) and/or taking into account the dilution effect between those sections which complicates calculations significantly (and only if dilution was constant which was not probably the case). Consequently, I conclude that the emission factors were greatly underestimated in this paper while comparison with other studies reporting similarly low values were only valid providing no mistakes were done in any of the studies. For example, Valiulis et al. (2002 Atmos.Environ.) reported emission factors for Zn, Ba, Mn, Cu and Pb an order of magnitude higher than in this study with similar traffic flow and composition albeit in a much shorter tunnel with only natural ventilation. It fact, Table 6 reports PM emission values in other studies also greatly higher than in this study pointing to the problem above. I argue against any advantage of long tunnels because of mandatory elaborate ventilation systems absent in short tunnels.

Author's response:

Thanks for the reviewer's comment. The ventilation system inside the tunnel is triggered when CO concentration and temperature exceed their threshold values (ex. CO $\geqq$ 75 ppm and temperature $\geqq$ 40°C) . As we check the CO and temperature data monitored by Taiwan Area National Freeway Bureau, it is found that temperature was frequently higher than 40°C at the outlet site, especially, during the July and August sampling periods, suggesting that ventilation system was operated. We also agree that the approach for estimation of EmF used in this work can only be applied in a close system. Since the exchange of inside-air and outside-air was occurred, the EmF

should be much underestimated in this work. Thus, we have followed the reviewer's comment and deleted all the descriptions related to emission factor in the revised manuscript.

2nd comment

In addition, why only PM1 emission value in Hsuehshan tunnel is presented in Table 6 when comparative PM10 and PM1.8 could also be calculated from all three fractions and meaningfully compared to other studies?

Author's response:

Thanks for the reviewer's comment. As the response to 1st comment, all the descriptions related to emission factor have been removed from this manuscript; however, Table 6 has been also deleted in the revised manuscript.

3rd comment

I also wonder why the authors assume that emission factors should be same or similar among different size fractions taking 4.4 - inlet/outlet ratio of PM1 - as a reference? I would argue against the correction of PM1-1.8 emission factor supposedly taking into account dry deposition - a notoriously difficult parameter to estimate, particularly in the tunnel. Different processes (combustion (PM1) versus abrasive wear (PM1-1.8) versus re-suspension (PM1.8-10)) were contributing to different size fractions, so why expect similarity? Dry deposition cannot account for 43% losses of PM1-1.8 particles based on aerosol fundamentals - my estimate is at most 10-15%. However, I agree that the larger the size fraction the larger the underestimation of the emission factors due to deposition. In summary, the experimental set-up clearly suffered from neglecting the dilution effect preventing any meaningful estimation of dry deposition which can be safely neglected by acknowledgement. I believe that the authors will carefully revisit their experimental set-up and calculations of the emission factors and possibly find the way of correcting the emission factors. It is imperative to consider geometry of the tunnel finding unperturbed sections; air exchange rate at all stations estimating a dilution effect; and activation pattern of the ventilation system (and any differences in the pattern between weekdays and weekends) to see which data can be reliably used in Eq.2 (if any). If that is not possible, the sections reporting emission factors should be removed which would be pity because of the otherwise valuable dataset obtained. The remainder of the paper - pollution patterns from air quality point of view, corresponding ratios of metals, size-distributions, etc. - is all fine.

Author's response:

In this work, we used a correction factor of 4.4 as a reference ratio to correct the underestimated EmF of different sized PM. Nevertheless, we make a very serious

mistake that different process contributes different sized PM and thereby the correction factor should be overestimated as suggested by the reviewer. Consequently, we have re-organized this part in the revised manuscript. (lines 4-13 on page 10). On the other hand, the approach (Equation 2) for calculation of EmF is unsuitable in a "open system". Thus, we have also deleted the descriptions related to EmF.