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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of mobile emissions contributions to Mexico City's emissions inventory using on-road and cross-road emission measurements and ambient data" *by* M. Zavala et al.

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The paper attempts a comparison of the official road transport inventory of Mexico City with the results on emission factors produced by on-road measurements conducted by a mobile lab. The emission factors produced in this way are compared with remote-sensing measurements and ambient pollutant ratios from the air quality monitoring network. This is a useful study in the process of validating and quantifying the uncertainty of urban inventories. I would therefore recommend publication of the paper. However, I have a few points that the authors should consider and resubmit a revised version,

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Discussion Paper

before a final approval.

Major Points

Point 1: There is not a deteiled description of the total measurement sample used in this study. In p.6369, l.16, the authors refer to “28 gasoline vehicle fleet average sampling intervals. . .during the analysis of approximately 6.5 h of on-road data”. They also suggest that “in the fleet average sampling periods hundreds to thousands of individual exhaust plumes are actually sampled”. I don’t find this enough information to judge the quality of the input data. I would therefore suggest that the authors provide more detailed information to respond to the following questions: What is the total duration of the 28 gasoline sampling intervals? How can one separate gasoline intervals only, out of a driving condition involving hundreds to thousands cars? What is the split of the intervals to SAG, TRA, CRU? A SAG condition is defined as a condition of 16 km/h for a period of 5 min or more. In such conditions, measurements are mostly affected by the plume of the cars in the immediate vicinity of the mobile lab (mostly the car directly in front). How many SAG intervals were measured to obtain a representative measurement sample? In general, I feel the authors need to better support the representativity of their sample.

Point 2: A discussion on the effect of ambient conditions might be useful to include in the paper for two reasons. First, as Mexico City is situated at high altitude, the authors should discuss whether effects of the altitude on emissions are expected. In particular when comparing results with US cities (e.g. p.6372, l.15-20). In addition, information on the period and weather conditions between the 2003 and 2006 studies should be given, in particular following the discussion in Table 2.

Point 3: I feel that the conclusions in p.6371, l.13 and p.6372, l.13 on the effect of calculation year to VOC species emissions contradict each other. Have the VOC species emission decreased or increased in the period 2000 to 2006?

Point 4: In general the VOC estimate is prone to a number of uncertainties. The au-

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thors correctly recognize that remote-sensing VOC measurements only approximate total NMVOCs (p.6374, l.8). Therefore use of the approximate VOC/CO ratio on the CO measurement, in order to calculate VOC and compare with the emission inventory, can only provide an approximate result. Just because of its uncertainty, this is not enough evidence to come up to the conclusion that the MCMA underestimates emissions by 1.4-1.7 times (p.6374, l.23). In addition, no big difference was found for the VOC species actually measured (p.6374, l.25). Therefore, it could be that the inferred VOC calculation overestimates emissions and not vice versa. In order to provide a more fair judgment to the difference, I would propose the following two revisions:

1. The blue bar of HC in Fig. 3 should be formatted differently (e.g. blue hashed) and a clarification should be added in the caption to make clear that this is an inferred and not a measured value. 2. The inventory should not be judged as providing an underestimation. Instead, the authors should talk about a difference between the inventory and the inferred value and explain the reasons (as they do) of the difference in a non-biased manner. This also includes the discussion in the abstract.

Point 5: The authors suggest that the inventory significantly underpredicts PM emissions from gasoline cars (p.6375, l.7). As gasoline cars are not prime emitters of exhaust PM, the authors should provide more information on the exhaust vs non-exhaust (tire and brake wear) contribution to total PM. Does the inventory include non-exhaust sources? If yes, what is their share in PM1? What is the (estimated) share of non-exhaust sources in their measurements?

Minor Points 1. What is NOy? (p.6378, l.16).

2. Where does the value 54.1 molC/lt come from? (p.6370, l.4). Is there any reference? This may significantly affect the g/kg fuel emission factors produced.

3. The abstract discusses about NO emissions (p.6364, l.10) while the main body discusses about NOx. What is the CLD analyzer tuned to?

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4. The VOC “underprediction” is quoted in the range of 1.4-1.9 in the abstract (p.6364, l.17) compared to 1.4-1.7 in the main body of the text (p.6373, l.26).

5. The discussion in the abstract on diesel vehicles effects on NOx does not make sense (p.6364, l.28). NOx is found not to have changed but this suggests that the contribution of diesel vehicles has increased.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6363, 2009.