

Response to the Comments of the Reviewers

Evaluating Vehicle Emission Control Policies using on-Road Mobile Measurements and Continuous Wavelet Transform: a Case Study during the Asia-Pacific Economic Cooperation Forum, China 2014

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We thank the reviewers for their helpful comments. We have revised the manuscript according to their suggestions and respond to their concerns below.

Reviewer #2:

Major Comments:

1. It is not clear what this study adds to the literature on the subject. What critical questions does it answer, what is novel about the study design, methods, etc.? How do the results compare to previous studies? Without properly placing the results of the study in the context of the larger literature it is not possible for the reader (or reviewer) to judge its value.

Response to Reviewer comment No. 1: This study provided a new way to estimate vehicle emission and evaluate the effectiveness of vehicle limit policy implemented during the APEC 2014. We firstly combine on-road mobile measure with the CWT method to estimate vehicle emission.

2. Evaluation of the CWT approach for quantifying instantaneous emissions needs more emphasis. First, briefly describe what kinds of signals have been decomposed using CWT, and indicate the basis for why you think CWT is an appropriate tool for air pollution applications. If previous air pollution studies have used CWT then it would be sufficient to cite that literature. Next, describe how your results (% reductions of traffic-related emissions during APEC) obtained using the CWT methods compare to results obtained with other widely reported methods. Finally, indicate how sensitive your % reductions are to choice of signal processing method and comment on how do we know which method is better?

Response to Reviewer comment No. 2: Agree. First, in our study we using the CWT method to decompose signals of on-road measure pollutants, which were mainly emission from vehicles and measured by fast-response instruments. We chose these signals because they contains “fresh emission” (emission from vehicles for a relative

short time) and the “fresh emission” part were less influenced by the meteorological conditions. Second, we had made a compare with the moving 5-minute-5%-percent method in supplement materials S4. Thirdly, sensitive test result had been given in line 219.

Other Comments:

1. The introduction is too long. The total length of the MS is 23 pages and the introduction is 5.5 pages. The introduction should be cut at least in half. It need only contain a clear statement of the problem, a short summary of the findings and shortcomings of earlier studies, a clear statement of the gaps in the literature and the study objectives to address the gaps.

Response to Reviewer comment No. 1’: Accepted. We cut some unnecessary parts.

2. A study limitation section is missing. This is a challenging study with many variables that could not be fully controlled. The limitations of the study design should be discussed.

Response to Reviewer comment No. 2’: Accepted. We will add limitation section.

3. Use of the term “instantaneous concentration” to describe exhaust plumes from individual vehicles is misleading. Some plumes can linger for several 10s of seconds before dissipating – it all depends on source strength and atmospheric mixing conditions. I think the term “short-term concentration spikes” is more appropriate and suggest its use in place of instantaneous concentrations.

Response to Reviewer comment No. 3’: Accepted. We instead “instantaneous concentration” of “short-term concentration spikes”.

4. Why are O₃ and SO₂ part of the traffic analysis? O₃ is a secondary pollutant, who’s formation is contributed to by traffic emissions, and SO₂ is not a traffic-related pollutant unless vehicle fuels in use in Beijing contains lots of sulfur (but I suspect it is mostly from coal). Inclusion of these two pollutants in the main analysis (Figs 3 and 4) and the methods needs to be justified. Otherwise, the results for these two pollutants should be moved to the supporting information section.

Response to Reviewer comment No. 4’: Agree. We give the measurement results of O₃ and SO₂, their temporal variations and distributions can help analyze other pollutants variation and distribution.

5. Line 171. Why is PNC not reported in the results section? Arguably, it is one of the

best indicators of traffic-related air pollution of the pollutants you have measured. This should be described in the text.

Response to Reviewer comment No. 5’: We only have 6 days (November 4 – 9) PNC measure results and can’t effectively use it now.

6. Line 289-90. It is not clear why O₃ increases during APEC when instantaneous NO_x concentrations decrease. The explanation provided needs greater detail and a reference so that reader can understand why a “NO_x-controlled regime” could lead to greater O₃ when the opposite would be expected.

Response to Reviewer comment No. 6’: Agree. We will list references.

7. Line 309. Need to explain why this ratio is useful and cite references. Also, indicate what threshold values to use to indicate coal combustion.

Response to Reviewer comment No. 7’: The ratio roughly indicates diesel vehicle account of vehicles. It cannot precisely indicate the proportion of diesel vehicle or gasoline vehicle.

8. Line 313. References supporting this statement should follow “concentrations”.

Response to Reviewer comment No. 8’: Agree. We add “concentrations” to support this statement.

9. Lines 317 and 323. Your box plots in fig 4 are not showing averages. The horizontal lines show medians. Please describe the medians not the averages when describing fig 4 results.

Response to Reviewer comment No. 9’: Accepted. We instead “average” of “median”.

10. Line 355. Regarding sources of BC, aren’t diesel vehicles in Beijing a significant source of BC? Many readers will assume this to be the case. Please provide references that specifically indicate otherwise or modify the statement.

Response to Reviewer comment No. 10’: Accepted. We modified the statement.

11. Line 390. On-road vehicle numbers. Would be very interesting to see this. If possible present a time series before/during/after APEC of vehicles on 4th ring road distinguishing gasoline and diesel vehicles and night and day populations of each.

Response to Reviewer comment No. 11’: Sorry for lack of vehicle number data.

12. Line 422-423. “increase in diesel buses”. This needs to be better described (e.g.,

how many buses per hour and on which days – weekdays vs. weekends – etc.?) and it should come much earlier as well as appear in Table 1.

Response to Reviewer comment No. 12’: 400 buses was added, and fleets of buses also added from 3rd November to 12th November.

13. Line 432. Suggestion is made that the decline of instantaneous BC is due to decline of diesel vehicle emission. This seems to contradict lines 354-55. This apparent contradiction should be addressed.

Response to Reviewer comment No. 13’: Accepted.

14. Lines 461-467. These sentences should be deleted as they don’t add materially to the conclusions section.

Response to Reviewer comment No. 14’: Accepted. We deleted it.