The authors introduced one continuous wavelet transform (CWT) method to improve the data analysis of on mobile measurement, which I think could be a novel enhancement of the mobile measurement techniques. The 2014 APEC summit also provided an interesting chance as a case study to present the useful features of the CWT method. I feel the methodology and information are useful in the measurement field. However, several technical issues remain in the current version, which the authors should in detail address with substantial revisions. Although the authors noted this manuscript had been reviewed by two native speaker editors, however, the current version is far away from a basic standard of academic journals in English. Numerous language errors do harm the readability, and a further round of language editing is necessary. Please find the major concerns and specific comments listed below.

1. The article title clearly reflects the authors' focus on evaluating the efficacy of vehicle emission controls by using the mobile measurement results. They reported the emission reductions of CO and NOx (i.e., 28% and 16% for the APEC period compared with those before the event) in the abstract and conclusion sections. I doubt the accuracy of the results for several major technical issues. First, as the author noted in L 358 to 361, a considerable fraction of direct emissions from on-road vehicles attribute to the accumulative concentrations (e.g., on-road background, which is derived by using the CWT method). Thus, the variations in instantaneous concentrations may not represent the variations in vehicle emissions. Second, this manuscript has not addressed the link between vehicle emissions and concentrations of air pollutants, which may be affected by a series of complex conditions (e.g., meteorological conditions, traffic conditions that impact the turbulence in micro environment). Even including the fraction of vehicle emissions owing to the on-road background concentration, I am afraid that the change of concentrations may be not strictly equal to the change of vehicle emissions. Third, the authors conducted the on-road sampling only on one road. Therefore, it is not sufficiently representative of all roads in the city of Beijing. For example, the local roads in the downtown area should be more influenced by the traffic variations of passenger vehicles, while the traffic corridors in the outskirts (e.g., highways outside the 4^{th} Ring Rd) would be relatively more associated with the traffic variations of diesel vehicle fleets. If without the results for other types of roads in Beijing, I suggest the authors restrict the within the scope of 4^{th} Ring Rd and amend the current statements in abstract and conclusion sections.

2. Black carbon (BC) is one key pollutant that well reflects the emission contribution from diesel vehicles. I disagree with the authors on the traffic contribution to BC concentrations (e.g., L 333 to 358). For example, previous studies have found higher BC levels in traffic environment than background. One source apportionment study using one site near 4 Ring Rd indicated that the traffic contribution of elementary carbon concentrations was higher than other sources like coal and biomass burning (ES&T, 2015, 49, 8408-8415). Instead, I do feel the coarse time resolution of BC measurement (1-min sampling) might be an important cause of the less significant pikes of BC than CO and NO_X. Therefore, I suggest the relevant statement be revised, and the limitation should be noted.

3. The authors present the meteorological data during three periods in the manuscript. However, this section may be not so close to the main objective of this study. I wonder whether the CWT method can derive instantaneous concentrations by eliminating or reducing the short-term effects from various metrological conditions. If so, the manuscript can be enhanced by providing more links between the meteorological data and the results derived by using the CWT method.

Specific comments

L 21 to 23: Why the authors consider vehicle emission controls implemented during the APEC summit week were the strictest ever in China, just because the implementation included regions outside Beijing? Using the odd-even policy as an example, the implementation period during the 2008 Olympic Games was throughout 24 hours; however, this restrictive policy was implemented from 3 am to 0 am during the 2014 APEC summit. (similar comments on L 148)

L 23: The mobile measurement is not recent technique. Modification is suggested.

L 25: 4th Ring Road (capital "R"s). Please make the corrections throughout the manuscript.

L 30-36: please understand the difference between concentrations and emissions.

L 34: 56.0% instead of 56.0 (similar errors are seen in many other places)

L 37: Avoid the use of "extremely", which is non-scientific.

L 38: the 2014 APEC summit

L 43: replace trace gases with gaseous

L 44: replace numbers with population

L 45 the phrase "lack of control policies" is not proper.

L 51: replace polluted days with pollution episodes

L 53: Please add the reference of source apportionment results in Beijing concerning vehicle emissions.

L 54: replace near with approaching

L 55-59: I don't think the illustration specifically for the APEC period is appropriate here. A later place (e.g., discussion) may be better.

L 62: please use plural nouns of policy

L 63-67: More concrete descriptions with references would be helpful.

L 70: Please use other phrase instead of inconsistent, e.g., the discrepancy between various studies. (similar comment on L 84)

L 73-85: The illustration of previous studies should be improved. First, the reductions reported Zhou et al. were derived by using emission inventory data, not the observed changes of curbside concentration levels. If focused on the concentration changes reported by Zhou et al., they would be close to Zhang et al, which is unlike what the authors understand (e.g., L 79 to 80). Similarly, the reductions reported by Wang et al. were also derived from emission inventory data, not the simulation results by using air quality model (CMAQ).

L 86-87: The authors should clarify the "advantage" by comparing with various on-road measurement methods, e.g., on-board emission measurement, fixed remote sensing, tunnel measurement. The useful features of mobile measurement vary according to the comparison with various counterparts.

L 88-144: These texts have to be streamlined. Please do not list and describe the previous studies one by one. I suggest the authors should be more focused on the previous methods and their limitations to decompose the vehicle contribution (e.g., in context of instantaneous concentration or pikes) and the background. By contrast, I suggest the authors enhance the novelty of the CWT method, since I found previous adoptions of the CWT method were not in the mobile measurement field.

L 146 during November 5 to 11

L 149-153: The later section 2.2 repeated the sentences here. I suggest the section 2.2 can be skipped. More descriptions related to vehicle emissions controls would be helpful, since the comprehensive controls are not only the odd-even policy.

L 151: This sentence needs revising, since 50% of vehicles restricted does not equal to a removal of half on-road vehicles.

L 157-158: CWT; Further, the CWT method is not used to separate instantaneous "emissions" (here should be concentration) from vehicles (should be on-road background).
L 163: please include ozone
L 166-167: Have the authors estimate the effect of self-pollution from the platform vehicle (IVECO Turin V)? How to avoid or alleviate such a self-pollution issue?
L 169: on-board
L 171: Since PN is included in the section, I wonder why PN is not included in the results. Have the real-time measurements of BC and PM have been calibrated with reference methods? For example, in Fig. S2, the PM2.5 concentrations obtained by using the mobile platform were in general lower than the stationary site, which is quite surprising to me. The authors carefully need to address this issue.
L 177: How and why to maintain the traveling speed at 60 kph? As far as I am concerned it would be quite incredible, since traffic congestion is serious in Beijing. Vehicle speed during the daytime should be much lower than 60 kph, and very transient in the flow.
L 178: the running title should be revised, since the texts from L 187 to L 202 are about the experimental section. The authors may consider to merge sections 2.1 and 2.2.
L 187-202: The absent of nighttime measurement before the APEC period should be noted here. I suggest a summary table including number of trips, duration and pollutants measured.
L 207-208: It appeals that the CWT method has been applied in the geophysical and atmospheric (e.g., Kang et al.; Tian et al.), but not used for data analysis of mobile measurement results. Several sentences in the later section (L 328 to 333) might be moved to this section.
L 239: northwesterly (later: southerly winds)
L 257-268: Please use a table to present the results, not list them in the manuscript.
L 297: after the APEC summit (similar comments in other places)
L 298: please explain "the shift in background concentrations".
L 308: The reference Bond et al., 2013 is not appropriate to address the local source apportionment in Beijing. See my second major comment above.
L 309-313: The sentences here may be better to be moved to later sections. In addition, the discussion here is not solid. The increase in BC/CO or BC/NOx may be also caused by the flood of freight trucks from other provinces, which have higher BC emissions.
L 314-316: please note the date with unfavorable conditions (e.g., southerly winds).
L 335: Correlations
L 338: How about the comparison using other pollutants like CO, PM and BC? Particularly for BC, since the measurement time-resolution is different, I suggest the comparison results be presented in the supplementary information. Please note the units in Fig. S4.
L 342: replace vehicle with on-road
L 344: (Fig. 7) Please do not use the ten-fold data of instantaneous concentrations in this figure, which are not direct to the readers. I suggest change the chart format in total concentrations decomposed to two aspects.
L 356: Both references are not appropriate to address the pollutant patterns in traffic environment.
L 354-371: This section should be improved in a more logical way. Since some contents are the scientific facts (e.g., L 358-361), some are research limitations (e.g., L 361-367). The authors have clearly noted the instantaneous concentration do not necessarily represent the instantaneous emissions. Unfortunately, the reductions of instantaneous concentrations are simply used in the abstract and conclusion sections.
L 390-396: The illustration about the change of traffic volume is not correct. The actual change of traffic

volume could not be estimated by using the controlled range of vehicles. For example, many residents didn't need to commute during the APEC week. Further, the odd-even policy was not implemented throughout 24 hours. (similar comment on L 430)

L 412: Please understand the spatial difference of emission patterns. For example, the diesel trucks might be mainly used outside the urban area, although one of major contributor fleets, which could be different with the emission patterns on the 4th Ring Rd.

L 422: The explanation using the increased use of public buses needs more clarifications. Because, the emission controls on buses are more stringent than those on freight trucks in Beijing, and many buses are powered by natural gas. Is the increase of public bus usage significant?

L 455: The statement needs a modification. Typically, pre-Standard III diesel vehicle have been restricted in Beijing for a long while even during the normal days. The inspection and regulation enforcement after the APEC week might be not as strict as those during the event.

L 467 to 468: see my first major concern.