Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1169-AC3, 2021

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Mobile monitoring of urban air quality at high spatial resolution by low-cost sensors: Impacts of COVID-19 pandemic lockdown" *by* Shibao Wang et al.

Shibao Wang et al.

zhangyx@nju.edu.cn

Received and published: 5 March 2021

General comments This paper reports on the findings of a year of monitoring conducted using a sensor pack on two taxis while they drove the streets of Nanjing, China. Based on the data reported the investigators developed concentration information plotted on the many roadways where data were collected. The opportunity to capture the impacts of major activity patterns associated with Covid-related restrictions is an interesting application of the results.

Re: We appreciate Dr. Westerdahl for her/his effort to comment our manuscript and feedback. Dr. Westerdahl gives an accurate summary of our work and brings forward

constructive questions. We have addressed them below. We hope she/he is satisfied with our answers.

1. I concur with RC1 and will only add a few most concerning points beyond those in that review. The basic problem presented by this paper is that crucial methodological/protocol descriptions regarding data collection activities are totally absent. I was unable to determine the nature of the sensor pack from the paper and a look at the Chinese company website was not useful or clear-beyond the weight and size. There is no clear description of data adjustment which is mentioned. One this is very clear–the outcome of the monitoring data both before and after adjustment is startlingly good, beyond what most other users of sensors have reported. This enforces need to describe the process in detail. Overall, the uncertainty in monitoring and calibration practices makes it quite possible that the overall data set and interpretations might be impacted.

Re: Thanks for your query. We included more details for the methodology of data collection in the revised manuscript. Please refer to our response to the comments of the reviewers. For example: About the sensors, we added a note in line 67-69: "……as well as two small in-built sensors for temperature and relative humidity……". And we also added the following sentences to explain the nature of the sensors after that: "All three sensors are electrochemical-based sensors that can detect gaseous pollutants at levels as low as ppb (Maag et al., 2018). It is continuously powered by an external DC 12V power supply provided by a taxi battery". The sentences of "The monitoring data is automatically uploaded to a database in the cloud via the 4G telecommunications network. ……, and their limit of detection (LOD) are 0.01 μmol/mol, 0.1 nmol/mol, and 0.1 nmol/mol, respectively" was also added in line 75-77.

The reason for the good outcome of the monitoring date both before and after adjustment is that the GBRT was selected for data calibration in this paper. And we added following sentence to describe the method in line 100-102: "Comparing different calibration models, we found that machine learning algorithm can improve sensor/monitor agreement with reference monitors, and many previous studies have used this method

(Qin et al., 2020; Esposito et al., 2018; Vito et al., 2018)". And sentences in line103-106: "GBRT, an ensemble learning method, is a decision tree-based regression model that implements boosting to improve model performance using both parameter selection and k-fold cross validation. GBRT needs to be trained by the dataset with target labels (Yang et al., 2017). It takes input variables including raw signals of sensors, other air pollutants concentrations, temperature and humidity. The stationary instrument data are taken as training targets".

2. Specific comments. Since the sensors are not described and data handling is also only somewhat described it appears possible that the data came from some sort of electro-chemical cells. If this is true, it is quite possible that important Ozone/NO2 interactions occurred in, for example in the ozone data. This could have important implications on data observations and would show variable degrees of impacts depending on the mix of pollutants. The findings of fixed site calibration would differ from those made on road since the sensor experience a differing relative mix of NO2 and ozone.

Re: We clarified this in line 69-70: "All three sensors are electrochemical-based sensors that can detect gaseous pollutants at levels as low as ppb (Maag et al., 2018)". And the following sentence was added in line 121-122: "Owing to the interaction between O3 and NO2, the detection accuracy of these two chemicals are influenced, especially for NO2 (Ivanovskaya et al., 2001)".

3. Sensors seem perhaps only to be calibrated as study started and then once a month by comparison with an outdoor monitoring site (whether the sensors were tested in outdoor air or in some facility is unclear).

Re: We clarified this by adding a sentence in line 92-94: "The sensors are usually trained with co-located data collected by reference methods before being deployed to actual measuring campaigns (Kaivonen and Ngai, 2020; Chatzidiakou et al., 2019; Bossche et al., 2015)". And the word "outdoor" was added in line 94-96: "The instrument is placed at the outdoor Station for Observing Regional Pro-

cesses of the EarthSystem (SORPES) in the Xianlin Campus of Nanjing University (https://as.nju.edu.cn/as_en/obsplatform/list.htm) for at least seven days before the taxi began sampling".

Text in the first sentence of section 2.2 appears to state that sensor packs were placed at the campus supersite monthly for "at least seven days." No data are presented regarding the nature of the data at these monthly calibrations. Maintenance or data review are not described,

Re: We clarified this in line 97-99: "The collected data is calibrated against standard instruments (Thermo Fisher Scientific 48i, 42i, and 49i, USA for CO, NO2, and O3, respectively)". And we also added a sentence in line 99-100: "The instrument precision is $\pm$ 2ppbv for O3, and $\pm$ 1% and $\pm$ 4% for CO and NO2, respectively, which have been used in many other studies and found to perform well for long-term runs (Ding et al., 2013; Herrmann et al., 2013)".

However line 85 states "if the data deviated substantially from the nearest national network stations (shown as red stars in Figure 1), the instrument is also taken offline and re-calibrated." This statement indicates that there was some attention to reviewing the quality of data. This topic should be expanded and data on these calibration events should be included. The use of fixed site data should also be expanded. What was meant by the use of the "nearest" station in data review?

Re: We did not calculate the "substantial deviation" from the national network measurements, so we deleted this sentence in the revised manuscript.

4. There is mention of a data calibration mentioned about "A supervised machine learning methodology based on the Gradient Boost Decision Tree (GBDT) is used for data calibration" with a reference. But this should be fully described.

Re: We clarified this by adding the following sentences in line 103-106: "GBRT, an ensemble learning method, is a decision tree-based regression model that implements boosting to improve model performance using both parameter selection and k-fold cross validation. GBRT needs to be trained by the dataset with target labels (Yang et al., 2017). It takes input variables including raw signals of sensors, other air pollutants concentrations, temperature and humidity. The stationary instrument data are taken as training targets".

5. Line 187 about consideration of observations vs. "life times" of each pollutant is incomplete and it is not clear how it applies to a near roadway urban environments where there is an impact of complex emissions/conversion and new emissions are present.

Re: To clarify this, we added the following sentences in line 227-229: "Lifetime (or residence time) is the average time for a chemical compound that is transported in the atmosphere before it is deposited or consumed by chemical reactions. It is associated with its spatial scale of variability. The longer the lifetime, the more uniform the concentrations are distributed".

This especially the case for the pollutant "NOx" mentioned on line 190 about a pollutant that is not reported on in this study about the pollutant reported is NO2. The authors should provide a complete and careful consideration of these issues and they should be careful in the use of "NOx" vs the pollutant they measured. It seems to be used interchangeably in several places.

Re: We measure NO2. But here we use NOx (=NO+NO2) as NO and NO2 are in a fast chemical cycle. It is thus more meaningful to use the lifetime of NOx instead of NO2. In other part of the manuscript, we also replace NOx with NO2 if we are referring to the chemicals we measure (e.g. line 260).

6. Figure 2 about confidence in NO2 is not high seeing the good agreement with the fixed site dropped to R2=0.67. The authors suggest that this may be due to humidity impacts. NO2 and NO are probably the most important gaseous pollutant today in many urban near-roadway locations, but the authors have failed to follow up on the

observations of possible poor model performance by repeating the calibration procedures.

Re: We added a sentence to clarify this in line 121-122: "Owing to the interaction between O3 and NO2, the detection accuracy of these two chemicals are influenced, especially for NO2 (Ivanovskaya et al., 2001)".

Further, for this pollutant, in this situation, it might be beneficial to see how the two sensor packs performed at each calibration.

Re: We tried so but there was no significant difference between the two sensors packs performed at each calibration.

Current text only says they were in 'good agreement'. Authors should discuss the contributors to the mismatch between agreement at cal vs validation for NO2. Is it clear that this fitting is successful as the sensors aged over the year?

Re: We included more explanations for the mismatch between sensors and reference method for NO2 (see responses to comments above). We further clarified the "aging" issue by adding a sentence in line 123-126: "The accuracy of the sensor generally decreases with time (aka aging) due to the evaporation of the electrolyte (Ribet et al., 2018). However, we find no significant decrease in the R2 values for the three pollutants during our campaign. It seems that the machine-learning algorithm could successfully compensate the aging of the sensors."

7. What was the data capture completeness in this study?

Re: Figure 5 shows the completeness of data capture.

Were there any sensor re-placements?

Re: It happens a lot. We explained it in line 201-205: "As shown in Figure 5b, the median number of repeated frequency in each grid is 66 (18, 286), with the highest value of 15449 in Nanjing South Railway Station and the lowest in some residential roads (1). The repeated frequencies in each 50-m grid along the arterial roads and Neihuan line are higher than other types of roads, i.e. Zhongyang road, Huju road, Neihuandong and Neihuanxi lines (Figure 5b)".

Pollution observation examples would be helpful about provide specific time series examples.

Re: That's a good suggestion. We added a sentence in line 206-208: "By comparing the time series of the air pollutant concentrations with that from nearby state-operated air quality stations (A' and E', with repeated frequencies > 500), we find that the results are consistent (Figure S1), which shows the stability and reliability of our data". And we also provide the original dataset for other researchers, so they can analyze the results in similar ways.

8. Para beginning on line 205 about where attribution of sources to observations is made. The actual basis for these is only general and not closely linked to the study. It appears to be conjecture.

Re: The observed concentrations are only a part of the basis for our source contribution. We also comprehensively analyze the pollution sources in hotspots through field surveys. We clarified this by adding a sentence in line 246-249: "To identify the main sources contributing to these hotspots, we use the different relative concentrations of the measured pollutants (Zhao et al., 2015). We also use field information around hotspots area, such as the existence of subway stations, construction sites, factories, and restaurants nearby".

9. Line 245 about states that VOC control is necessary to control ozone at this site. This may be true but is not studied or established by the investigators. It should be rewritten to reflect the basis for this statement.

Re: We agree with Dr. Westerdahl. So we deleted this sentence in the revised manuscript.

10. The statement that lack of sunlight in the tunnel is the reason for low ozone may or may not be correct. A more complete consideration of emissions, ambient air ozone and reactions is called for here.

Re: We thank Dr. Westerdahl for this suggestion. We modified the sentence in line 292-293 as: "The O3 concentrations are lowest in tunnels, which is associated with the weak sunlight in the tunnel (Awang et al., 2015)". We also added a sentence: "Furthermore, due to the unfavorable diffusion conditions in the tunnel, NO2 concentrations are accumulated to a relatively high levels (40.7$\pm$29.7 μg/m3), which titrates O3. The tunnel also blocks the replenish of surrounding O3-rich air, resulting in lower O3 concentrations than other roads (Kirchstetter et al., 1996)".

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1169, 2020.