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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.

Anonymous Referee #1

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This manuscript describes the deployment of low-cost air pollutant sensors for O3, NO2, and CO on taxis in Nanjing. This work is novel because it combines low-cost sensors with a distributed, quasi-random sampling platform. Overall the manuscript is appropriate for the journal, but it is not ready for publication at this time.

My main criticisms focus on the methods. As detailed in my comments below, the authors need to provide more information on the sensor package that they used. They do not even tell the readers whether these gas sensors were electrochemical, metal oxide, or something else. Additionally, the way that the data are assigned to points in space is not described in sufficient detail.

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Line 26-27 It's unclear what is meant by global air pollution deteriorating by 8%. Is this for a specific pollutant?

Line 45 - you might need to capitalize Street View

Line 83 - What does SORPES stand for? Also, the link in this line returned a 404 error.

Line 92 suggests that the sensors were not calibrated until June 2020, however the measurements started in 2019. I am confused about the calibration schedule - hopefully the sensors were calibrated before the sampling on the taxis started. Please clarify.

I am not familiar with the XHAQSN-508. What kinds of sensors are these? Electrochemical? Metal oxide? More detail on the specific gas sensors is needed. Also, is the sample refreshed by pumping air pash the sensors, or do you rely on the airflow generated by the moving vehicle? If it's the latter, does it impact the performance to have the sensors stationary during calibration experiencing wind during sampling?

As shown in Figure 2, it seems that the calibration approach was to use the "forward" method - e.g., calibration models were built on one week of data, and then that calibration was used going forward. Other low-cost sensor studies use k-fold cross validation. In this approach, the data are divided into k chunks, and models are built on k-1 chunks and tested against the holdout. Does a k-fold cross validation of your data result in different (or perhaps better performing) calibration models?

The authors should specify what parameters were used in the calibration models. Is it just sensor raw signal, or are variables like temperature and humidity also included? Including a humidity term may improve performance of the NO2 model, as the authors note in lines 100-101 that the NO2 model may have a humidity bias.

Section 2.3 needs a better explanation of how the data are assigned to points in space. Data are logged every 10 seconds. Under many driving conditions (speed > 18 km/hr), the vehicle will cover more than 50 m in 10 sec. How is the resulting data assigned in

space? Is it the location of the vehicle when the data point is logged?

Is the final output the mean concentration in each grid? Since grid cells can be sampled unevenly across different days, other studies have first internally averaged the data by day. E.g., Apte et al 2017 compute the grid cell median for each sampling day, and then compute the mean of all daily medians.

How do large concentration spikes impact sensor performance? In our laboratory tests of electrochemical sensors, we observed that concentration spikes can cause the raw signal to remain high for several minutes. Presumably there are many spikes encountered during mobile sampling. Have the authors considered the potential impacts of these spikes? More broadly, have the authors considered that the sensors may not be able to reliably report at 10-sec resolution?

I'm confused by what is shown in Figure 4. I think that the standard error of the mean was calculated for each grid cell, and then averaged over all grid cells, but that is unclear. Were grid cells excluded if they did not meet a data threshold (e.g., if they did not have "enough" data)?

Figure 6 is hard to read. The lines indicating the roadways (or grid) are very thin, and it's hard to see the variation in the color scale with such thin lines.

Section 3.3 - This section is titled uncertainty analysis, but the discussion (especially lines 186-194) are more about spatiotemporal variability than uncertainty. This means that I am unclear on whether Fig 6 shows variability in measurement uncertainty (e.g., because of different sensitivities for different species), or if the variations in the coefficient of variation represent physical phenomena associated with emissions and chemistry.

Are the concentrations shown in Table 1 the mean concentration, or the concentration above background? The latter might be more informative.

Section 3.4.2 - how are the different types of roads defined?

Figure 9 shows diurnal patterns for the different pollutants. Were the data sub-selected in any way? I imagine that the locations sampled might be different across different times of day (e.g., maybe more time on highways at certain hours). It would be best if the data were somehow filtered - e.g., by only showing data collected on a certain road type, or by ensuring that data for each hour have a similar mix of road types sampled.

Figures 8 and 9 do not show a strong weekend effect. In the US, there is a strong weekend effect due to lower commercial diesel traffic (so there is lower NOx on weekends); but gasoline passenger cars have similar activity on weekends as weekdays, so CO is similar. Do your data suggest something about traffic patterns on weekdays versus weekends?

Figure 10, much like Figure 6, is hard to read. Maybe the authors could show a single panel in the main text and put the rest in the Supplement.

Line 293 - is the traffic percentage of O3 even a useful figure? As the authors note, attributing O3 is complicated because of secondary chemistry. I think they should remove the ozone estimate and focus here on CO and NO2.

Figures 11 and 12 are too faint to be readable.

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