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Atmospheric Measurement Techniques Discussions

Interactive comment on "Gaussian Process regression model for dynamically calibrating a wireless low-cost particulate matter sensor network in Delhi" *by* Tongshu Zheng et al.

Anonymous Referee #2

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General Comments

This manuscript presents a methodology for performing a dynamical calibration for a network of low-cost sensors (LCS) in a polluted urban area. The work is based on taking 10 Plantower PMS7003 sensors in Delhi alongside 22 reference sites and using a combination of linear regression and kriging to attempt to calibrate the individual LCS without the need for any previous calibration. While this would be a huge boost to the field of LCS (requiring much less work to build out a network), the author's make it clear this method is not really a "calibration" method at all, but works better as a tool for the quality assurance and quality control of large networks. The main takeaway from

this manuscript (to me) was that with a network of 10 LCS, you can somewhat reliably (∼30% error) predict the 24h average PM2.5 concentration in a major urban area. I don't intend for these comments to sound harsh – I think this is a very interesting and important result that others should know about – it is very hard to calibrate sensors in this way due to the spatial and temporal variation for certain pollutants (NO2, PM) while it may work for others that are more regional in nature (O3). . .

Major and minor comments are outlined below.

Major Comments

1. I am unconvinced by the authors claims that they reduce the uncertainty in the spatial interpolation of reference networks – this needs a better description and error analysis to show the 2% improvement is statistically significant and/or applicable across different parts of the network in both space and time. 2. The authors interpolate data for both reference stations and LCS without any evidence this is a valid assumption. If this is going to stand, the authors should spend some time convincing the readers this is an appropriate methodology. The authors could quite easily show this by taking periods where the data are complete and comparing the linear interpolation results to the known concentrations. There is also no mention of the length of time covered by the temporal interpolations – are they just interpolating an hour? 12 hours? 3. The paper covers 24h averaged data – why not 1h data? Does the ability of the calibration model significantly decay? The authors note in the introduction that one of the key advantages of LCS is that they provide high temporal availability. From their results, we can only conclude the 30% accuracy applies to 24h measurements which are less resolved than most real-time reference monitors. 4. The author's base a large chunk of their model on the linear regression for calibrating LCS despite the fact most recent literature on the topic suggests this in invalid – relative humidity causes the growth of hygroscopic aerosols through water uptake which causes the linear regression approach to fall apart at humidities > ∼50% depending on the kappa value of the aerosol. See work by Birmingham or CMU. This should be discussed at some point since linearity is a

major assumption in your model as I understand it. 5. Delhi has extremely complex air quality (as noted by the authors), but there was very little discussion on how these specific complexities contribute to the ability or inability of the model to perform. This is especially important for LCS as they don't suffer from random error, but error caused specifically by their inability to account for changes in aerosol composition and the underlying particle size distribution. There is a paper out by Gani et al (2019) that contains data in Delhi during this time period I think would be useful to this discussion.

Minor Comments (format is Page No., Line No.)

P. 1, L. 1: I don't think it's necessary to create unneeded acronyms – simply call them "sensor networks"

P. 2, L. 14: Are LCS really suffering from calibration issues? Or fundamental issues associated with using light scattering to determine the mass of particles?

P. 8, L. 13: The word "global" could be switched for something more specific. Maybe "Delhi-wide" or something more descriptive.

P. 10, L. 14: I disagree that IITD qualifies to be a background site when it is close to major roadways – see Gani et al (2019). They show a huge amount of local influence, especially during certain periods throughout the winter months.

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