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Atmospheric Measurement Techniques Discussions

Interactive comment on "Practical field calibration of electrochemical NO_2 sensors for urban air quality applications" *by* Bas Mijling et al.

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First I want to say that I appreciate the hard work that goes into this. You've selected a good sensor with a good reputation, and you're methodology for a neighborhood study is at a high-level the right approach– colocation calibration, a few weeks in the field, and then colocation calibration. I think this kind of work in the citizen sensing community is important, and I'm glad that your methodology incorporates good sensor technology and recent best practice.

That said, I'm not sure what the precise contribution of this paper is.

In the realm of calibration technique and design, this is not state-of-the-art, nor is the methodology the right one if the point is the verification of a calibration algorithm. See

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this paper [http://www.atmos-meas-tech-discuss.net/amt-2017-138/amt-2017-138.pdf] for an example of the latest techniques and best practice– here HDMR takes into account more complex relationships than linear dependence and more complex variable interactions. In the linked submission, superior techniques with a longer co-location periods are applied to the Alphasense NO2 sensor. Their methodology is also strong– instead of fitting their calibrations to their entire colocation dataset, they train a calibration on part of it and validate it on a holdout set. This is the proper methodology if your contribution is about multilinear calibration for electrochemical sensors. You cite many good sources that have done work of similar complexity/characterization for calibration and colocation analysis of these sensors.

I presume the intended contribution has more to do with the installation/campaign and data collection *between* co-located calibration, but I have some reservations here as well. While I do believe your data is likely reasonable given the calibration process/sensor selection/hour averaging, you haven't provided strong evidence to substantiate this belief, other than anecdotal evidence about one sensor located near another reference device. You yourself only make weak claims that it is 'good enough to detect unexpected hot spots between stations'. You also allude to the fact that (1) your colocation measurement has a lower normal ambient NO2 level than your campaign area, and (2) you don't measure O3 in your campaign area though it more strongly affects your measurement signal than NO2. This combination of facts leaves me quite concerned– the ratio of NO2/O3 might be consistent in your calibration area, and slightly different in your campaign area, and leave you with a systematic bias that you haven't properly accounted for. I don't think assuming the relative contribution of these two components is constant when you know that NO2 levels are different in the campaign area is a safe/fair assumption. The 'sudden and unexplained' offset in the only sensor you kept colocated with your reference is also slightly concerning, and deserves more explanation/treatment than your paper provides.

There are many papers published that look at citizen science installations like this,

and present novel work in other regards– things like spatio-temporal models that are validated against slightly better reference devices ('AirCloud', Sensys 2014), interesting UI for citizen interaction ('HazeWatch', Sensys 2013), etc. They are generally explicit about their contribution as a user interaction or have a slightly more compelling story around validation of their campaign data. They are also typically in human-interaction focused conferences.

I'm not convinced that having a citizen campaign by itself warrants a publication, though it forms a strong foundation to experiment/build work on top of. I do commend you on the open-sourcing of your data, and I think perhaps there is a case to be made that this aspect of it is worth publishing, but I'm still a little wary that validation of your data and key assumptions should be a little tighter (that NO2/O3 in your calibration/measurement region are similar, that your calibration technique is the proper one in the location of your measurement, etc). The lack of quantification of error in the locations you are measuring and the weak/qualitative claims about usefulness of the data are also a little disconcerting in this regard.

Finally, there are several grammatical issues floating around the paper. To me it is noticeably written by someone with English as their second language (it's quite a good job for a second language, but it's still noticeably makes it difficult to read in sections). I'd recommend going over it with a native speaker. A few obvious phrases from the paper that are not properly formed:

'Since a few years new' 'reaching more frequently higher NO2 values' 'Instead of taking from ambient air measurements' 'the corresponding correlation with true NO2 signal' 'The Korte Koningsstraat characterizes as a side street' 'sensor are more closely located' - verb tense 'most sensors have been drifting in the intermediate two-month period' - verb tense 'Alphasense NO2-B series, are known' - weird comma

This is by no means a comprehensive list, just a few I just skimmed over. More in depth grammatical review is definitely required.

I hope this work serves as a jumping off point for you to dig a little deeper into calibration technique, network validation, and/or user-facing design of air quality systems. I'm sure you'll be able to find some very interesting work to do with the data you've collected. Best of luck!

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