



EGUsphere, referee comment RC1
<https://doi.org/10.5194/egusphere-2022-407-RC1>, 2022
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Comment on egusphere-2022-407

Anonymous Referee #1

Referee comment on "Are dense networks of low-cost nodes really useful for monitoring air pollution? A case study in Staffordshire" by Louise Bøge Frederickson et al.,
EGUsphere, <https://doi.org/10.5194/egusphere-2022-407-RC1>, 2022

- **Does the paper address relevant scientific questions within the scope of ACP?**
yes. the paper deals with a very interesting and important aspect: analyzing, calibrating and utilizing low cost sensor network data for air quality assessment
- **Does the paper present novel concepts, ideas, tools, or data?**
yes. The paper presents a concrete methodology for calibrating the sensor network results utilizing reference level measurement stations and temperature data. While there are some practical limitations eg. really evaluating the result against independent reference data, the suggested methodology itself is promising, and even without a real evaluation (which the paper correctly does not really claim to perform) , the results obtained in applying the temperature correction; are certainly interesting and worth of publishing. Also the discussion and results related to the spectral analyses (periodograms/fourier analyses) of the sensor network time series is certainly interesting, and while , once again does not necessarily provide exact/evaluated source contributions, gives a lot of useful information on the type of sources contributing to the measured concentrations.
- **Are substantial conclusions reached?**
yes. The study supports the findings of similar studies: even the low cost sensors could be useful, if there is enough supporting data (reference stations/meteorology) to calibrate the sensors
- **Are the scientific methods and assumptions valid and clearly outlined?**
Yes., but, Some suggestions for clarifying some method descriptions given below + requirement: a clear statement on the fact, that the current study does not try to scientifically evaluate the presented methods (yet) + it does not prove that the sensors used pass any concrete quality criteria . These statements should be added/corrected
- **Are the results sufficient to support the interpretations and conclusions?**
Partly , yes. Some detailed comments on this below.
- **Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?**
Some details should be added (see below)
- **Do the authors give proper credit to related work and clearly indicate their own new/original contribution?**
Yes.

- **Does the title clearly reflect the contents of the paper?**
I would probably change the wording slightly .. e.g. "better at"-> "really useful for"/
but this maybe just matter of taste ?
- **Does the abstract provide a concise and complete summary?**
ok.
- **Is the overall presentation well structured and clear?** ok
- **Is the language fluent and precise?**
for a non-native reader completely fine.
- **Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?**
mostly yes.
- **Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?**
Yes. see below
- **Are the number and quality of references appropriate?**
ok.

ADDITIONAL NOTES / COMMENTS/SUGGESTIONS:

All in all, the paper presents results and partly innovative analyses from a very interesting measurement campaign, and as such is a valuable contribution supporting for a further work related to understanding , interpreting and calibrating sensor network results in the future.

Some specific notes/suggestions which would make the paper even stronger.

- it should be clearly stated if **ALL** data from the reference station is used for calculating the temperature , scale and offset correction; or is some of the data left for evaluation ?
- is there any reference or justification for the choice of the specific quantiles (20,80,25) used for scale & offset correction ?
- as the temperature correction /regression fit is obviously quite crucial for the whole

study, it would be very important to see at least some summary statistics related to the "goodness" of the fit: naturally, if all the available data was already used for correcting the concentrations no real evaluation is possible, but e.g. adding the reference station data/correlations to figure 4 in addition to the sensors would at least give some rough idea how well the temperature correction works

- the correction coefficients: some short explanation/discussion e.g. on the value of a_3 (~ 1000)..this seems to mean that the sensor raw-"concentration" is given in completely different units ? (which should reflect on fig 3 ?)

+ a short comment also on the meaning of a_0 value, which is higher than the average concentration? ,

+ some "order of magnitude" estimate on the importance of the 4 terms of the equation - just to get an idea what is important and what is not

- this statement should be removed (CH 3.1.3) " In addition, this indicates that the AirNodes meet the specifications of the Class 1 device standard specifying quality objectives for indicative measurements (AQ, 2021). Class 1 dictates that measurement uncertainty should be below 25% for NO₂ and 50% for PM_{2.5}. "
.. **or** very concrete/new data supporting this should be represented: currently there is no evidence on this and the real evidence for this can most probably be achieved only using co-located reference sensors.
- (minor) did not exactly like too much this part.." Using the Einstein-Smoluchowski relation $K = d^2/(2t)$ (Einstein, 1905; Smoluchowski, 1906), we can solve for the characteristic distance as a function of time, At wind speed of 5 m s⁻¹, after a day, a spike of pollution will take a minimum of 15 minutes to pass.

My concrete suggestion: this could be safely removed, without losing too much:

to start with: referring to "Einstein et al." ,would also require explaining how this is related to dispersion of pollutants (the analogy may not be so obvious to everyone) ..and even after that the final statement is just trivial, "plumes will broaden with time..":. putting any concrete numbers in that statement (which seems to be the goal), is dangerous as everything is very much dependent on met-conditions/terrain/local environment, so any values "guessed" for K maybe order-of magnitude wrong, and for this study these numerical values are not too relevant anyway

- the spectral analyses part was very interesting; actually it was so interesting that it

would be very useful to give a short, concrete description of the exact method of "integrating the peaks" (as a set of equations, with exact integration limits, or something similar)

- in the conclusions it should be stressed, that this study (like many others) indicates clearly that (LC) sensors CAN be useful, but the calibration/correction of the results is far from trivial and requires supporting data and reference level station(s)
- in the conclusions: more clear statement on the fact, that the corrected NO₂ concentrations have a very strong connection to the one reference stations used for "calibration" .. so whatever is seen/analyzed from the corrected data, reflects **not only the sensors**, but also strongly the reference station data
- (very minor) eq. 9.. also, vk should be explained