

Interactive comment on “Intra-urban spatial variability of surface ozone and carbon dioxide in Riverside, CA: viability and validation of low-cost sensors” by Kira Sadighi et al.

Anonymous Referee #3

Received and published: 29 October 2017

In Sadighi et al., the authors describe the calibration and deployment of the UC Boulder U-Pod for measuring ozone and CO₂ in the Riverside and San Bernardino counties for a period of three weeks. The dataset is meaningful as it discusses not just validation of the low-cost sensor but also an actual deployment.

GENERAL COMMENTS

I believe the contribution of the authors is valuable and should be published in AMT if major revisions are provided. Overall, I found the structure of the manuscript to be confusing at times to follow – the authors make specific references to various units as DO, DA, DB etc without many reminders to the significance of the deployment locations.

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Sensor performance should be put in the context of the expected nearby sources (e.g., it makes sense that the sensor near Highway 91 would be different). I also think the manuscript is lacking in synthesis of the findings – many of the findings are stated without much interpretation.

I also think both the calibration approach, pre- and post-treatment of the data and the calibration models should be discussed in significantly more detail. You mention the significance of a time variable in the calibration, but the magnitude and direction of this coefficient isn't discussed – would this not be critical for other uses to decide on an appropriate sensor? I am also a little concerned on how data was screened to be included in the manuscript. I don't think it's very clear what data were omitted – you make some reference to only looking at data where the deployment exactly matched the calibration range. Why did you do this? Why would you expect your model could not extrapolate? I think given the structure of the mode extrapolation should be discussed.

Lastly, I would suggest you comment on how this study might translate to other areas – Riverside has some of the highest ozone in the U.S.; I would imagine that the sensors would have the least difficulty accurately measuring ozone in this area. What about other areas of the US that have occasionally observe high ozone concentrations but where it is more erratic (e.g., Pittsburgh can be in non-attainment, but average ozone is generally « 30 ppb.) Some generalizations on the findings would strengthen the paper. Lastly, I echo other reviewers when they say the CO₂ discussion as written does not seem to add much value to the paper. I am also not clear on the value of the results or the impact of this auto-calibration which is underdiscussed in the manuscript. Specific comments follow.

SPECIFIC COMMENTS

P3 Line 6-7: I am not sure why you mention the CyberSEES project or what it is. Delete?

P3 Line 17: What is this site C?

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P3 Lines 13-16: In this paragraph you list several findings, but there is not much synthesis of the results or general conclusions. The narrative should be improved.

P3, Line 33: Is it really an ideal test bed? Wouldn't this be the best case scenario where you a) have lots of ozone and b) and lots of sources? I feel like to truly test this question, you would need to try the sensors somewhere more representative of federal average ozone concentrations.

P5, Section 2.1: I don't think enough is said here about the long term drift or cross-sensitivity of the sensors. What is the expected life span? You should be explicit about the tradeoffs.

P6 Line 6: What do you mean by relationships? Linear regressions? You should be specific early on.

P6 Lines 20-28: What are the coefficients? What are their directions? Can you discuss the physical meaning behind the calibrations? How significant was the drift over three weeks (i.e., what is the size of p5?). If it is large, then are these monitors really suitable to replace EPA reference monitors? These sorts of details are critical to this paper.

P7, Lines 10-20: Again, I think some of these model calibrations are critically important and should not be in the SI. The coefficients and their interpretation should be front and center in the paper.

P8, Lines 1-9: I am confused by what this means? Did you only use data where there was exact overlap between calibration and deployment for T and RH? i.e., you did not try to extrapolate from the model? It's not clear to me what you mean in this paragraph. Also how many RH sensors failed? A number or percentage would be helpful to assess whether deploying these low cost sensors is a feasible alternative to EPA monitoring.

P9 Line 1-2: How did you deal with the data from the two ozone sensors? Did you average? Or choose one? This should be briefly mentioned.

P9 Line 5-10: What is this auto-calibration setting? I think more detail is needed here

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about what the auto-calibration does, and how you corrected for it.

P9 Line 19: "We thought that" is informal. Why do you think that? Describe, and also use more formal scientific language.

P9 Line 26: Not sure what "Values that are less than around 5 ppb different" means.

P9 Table 2: I think you need to do a better job describing how to interpret the results of Table 2. I am not clear on the takeaway.

P13 Lines 11-14: You should provide some rationale for these differences and similarities by time of day. Is the 9AM difference due to rush hour? Why not at 8AM? Please expand.

P15, Lines 1-8: So is the statement here that the disagreement between the R2 metric of spatial variability and the absolute differences is real? Does it not then follow that lower R2 between two sites is not a good predictor of spatial variability? You should be clear on the implications of the findings. From my perspective, if two sites are different based on absolute concentrations, that is the better marker of spatial variability. Especially because R2 can be disproportionately affected by high leverage points in a linear regression.

P16, Lines 7-10: Are you sure this is really spatial variability and not some confounding factor with the sensor? I am not convinced the data you have collected here is sufficient to claim that the interesting features of D0, DA, DB and DE are related to spatial variability.

P16, Line 24: Proximity to a major highway would be a critical factor in differences between two sensors, even if they are <2 km apart. There is a very near-road effect of NO+O3 forming NO2 – is the O3 at site DC consistently lower? The direction of these differences should be included.

P18 Line 11-12: Please rephrase the question as a statement – inserting a question like this seems gimmicky. It is up to you as the researcher to provide a hypothesis for

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the observation and inform the reader succinctly what you observed and whether this matches expectations.

P18: Lines 15-17: Again, I am not convinced this isn't just an artifact of the sensor. You should be clear on the potential uncertainty.

P21, Line 11: Some hypotheses should be provided. The discussion should be more than statements of observations but also include some scientific assessment of what was observed. What kind of future investigation is warranted? What would that future study look like?

P22, Line 1: This is the first time, to my knowledge, that you directly state that the MOx sensors cannot replace EPA monitors. Some space should be devoted to discussing the suitability of the U-Pods for monitoring and what is a reasonable expectation from the units.

P22 Line 3-6: Can you expand on the computation time, people demand, etc to give a sense of the rigor involved in the deployment as a guide to others?

P22 Line 19: What is "frequently"?

TECHNICAL CORRECTIONS

P1, Line 11: I am not sure it's grammatically correct to say the tool provides low-cost sensors. This just didn't make sense to me.

I would replace every instance of "Author and coworkers" with "Author et al." – e.g. on P2 Line 29, and elsewhere in the manuscript.

P3 Line 4 and 5 "the Riverside-San Bernardino counties"

P12 Line 6: Some word missing here in "Figure 4 gives some context temporal variability"...

P22-23, Line 31/Line 1: You should just delete the sentence about it being a lot of work.

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P23, Line 3: "undergo" vs "undergone"

P24: Why is there an appendix that is separate from the Supporting Information? I would just put in the SI?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-183, 2017.

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