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Atmospheric Measurement Techniques Discussions

Interactive comment on "The next generation of low-cost personal air quality sensors for quantitative exposure monitoring" *by* R. Piedrahita et al.

R. Piedrahita et al.

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Referee Discussion Response: We appreciated the comments, suggestions, and criticisms from the referees. We have taken steps to improve the manuscript by incorporating revisions as outlined below:

Anonymous Referee #1 Received and published: 20 March 2014 General comment: This is an important manuscript detailing the procedures by which users of sensors can utilize in better establishing the performance of their devices and therefore the credibility of data they are collecting. The article is worthy of publication following minor

revisions which are documented below:

Abstract: Suggest using the term "collocated" instead of co-location. While both are grammatically similar, the first term is the one most often used in these types of side by side comparisons. I would not suggest you indicate that the collocated comparisons were "better" than the true laboratory calibrations. The laboratory exercise did provide true calibration while the collocated effort provided "normalized" data. Both have their merit. I would suggest you indicate that the collocated option provided a potentially less sophisticated but no less valuable approach to establishing sensor performance. Remove the double use of the phrase "M-pods" in line 24.

Response: We agree with the reviewer's comments on the abstract regarding the terminology of using 'collocated', and have changed the manuscript accordingly. While we acknowledge that the use of 'collocation normalized' may be more technically correct than 'collocation calibration, we wonder if it is not better to use 'calibration' since it is widely used in this context. We do currently mention 'normalization' when describing the collocation calibration process. We agree that both have their merit, and this distinction has been made in the manuscript, while removing the value judgment that one was 'better'.

page 2427. no comments

page 2428. Be sure to differentiate between FRM and FEM in all of your text. Most regulatory agencies to my knowledge are typically using FEMs for most of their required monitoring. Line 13. Provide a reference indicating the recommendation for daily or weekly calibrations.

Response: We did not mean to suggest that the FRMs are more widely used than the FEMs, we only wanted to draw attention to the differences in sensing principles used from instruments on different ends of the cost spectrum, FRMs and low cost sensors. Regarding the calibration frequency recommendation, we have decided to remove that sentence due to its qualitative nature. There are many types of MOx sensors that may have different drift rates, and the frequency of calibration should depend more on the application and desired uncertainty bounds rather than a rule of thumb.

page 2429. Provide references associated with the points you discuss in lines 8, 11, 17.

Response: The requested reference for line 8 was for a price for a specific sensor module, but we have decided to omit the price. We are comfortable listing ranges of prices, but will refrain from doing so for specific sensors and instruments since these can vary with volume, time, and location of purchase. The reference for line 11, the accuracy of the electrochemical sensor, was added. A reference for line 17 was added. The price range listed is a range between the SGX Sensortech sensor used in our study, and a NOx sensor made by Synkera, a company based in Colorado.

page 2430. no comments

page 2431. line 7. I would suggest that you are normalizing response, not calibration. Calibration is an engineering term often referring to a direct challenge and subsequent output of a test device. You are normalizing response of these devices when you discuss the collocated measures. line 13, NAAQS, spell out this acronym.

Response: Again, we are of mixed opinions on this nomenclature. We have stuck with the use of the word calibration but now mention that this is a normalization process. NAAQS is now spelled out.

page 2432. Provide references for the points you are making in lines 18 for the LabView and Labjack devices. Report the version and manufacturer. You indicate in line 21 that you "calibrated the devices for temperature and humidity but offer no insight as to how you did that. What tools and approaches were used. These procedures should be more clearly defined in the text. Provide a reference for line 23 in your discussion about NDIR RH effects. You indicate in line 28 that sensors were warmed up for at least a week to ensure stabilization. Most end users of sensors will not wait that long.

Please define what you believe is an adequate warm up time. If it truely takes a week of stabilization, then the system being described is not one of practical use.

Response: Substantial detail was added to the system information section, including calibration procedures and instrumentation. Added a reference showing some RH dependence in a previous work with a different sensor. We omitted RH in this work due to results from our calibration chamber, and this has been noted. We can include some of this supporting data if it is recommended. We have clarified the warm-up time section. We now distinguish between initial warm-up time when the sensor is powered on for the first time and the heater is stabilizing, with turn-on time, the time to heat up a sensor back to its operating temperature, having been in regular use prior to that.

page 2433. How did you generate known concentrations of test gas in the laboratory? What purety of gas did you employ? What was the flow of test gas through the chamber and how did you validate that the postulated test atmosphere was correct with respect to test gas concentration? Did you employ on-line gas analysis or pull samples for off-line analysis? You do not indicate if you maintained a constant flow rate through the test chamber for all conditions. Was this the case? Greater details need to be provided here concerning how you performed the in-laboratory testing. Provide a reference in support of line 11 with respect to heterogeneousity of the MOx sensors.

Response: These are important system details that were omitted earlier to reduce length, but we acknowledge that they are important to replicate the work, and have been added. A reference was added with respect to sensor heterogeneity (Romain and Nicolas, 2009).

page 2434. Define "ambient" in terms of relevant concentrations you believe are applicable. Provide references for line 22 with respect to Taylor approximation. Readers might not be familiar with this data treatment.

Response: 'Ambient' is now qualified by specifying that it is for Colorado outdoor air quality measurements, and reference is made to the Colorado Department of Public

Health and Environment annual report for concentration ranges. A reference to the mathematics for Taylor expansion of exponentials is now included (Kate, 2009).

page 2435. No comments

page 2436. Line 23 you indicate a "calibration" was performed prior to the deployment. How and where was this done and was protocol was used. Provide a reference concerning the calibration.

Response: The manual for the LI-6262 instrument was referenced, as it contains the procedure we followed.

page 2437. line 6. How were these calibrated? Was this a laboratory calibration and if so, define it more thoroughly. Lines 10-17. Define the total amount of data censored in either data points, percentage of total or some other metrics. Currently, we have no understanding of how much acceptable data were obtained and used in the statistical treatment.

Response: Calibration procedure for the user study was clarified. The M-Pods were collocation calibrated at the Denver site before and after the deployment for 1 week. The median censored data for each sensor during the user study was 15.5%. This has been included.

page 2438. Provide a reference in support of line 24 in the discussion of S/N. Did you actually calculate S/N for the reference monitors or was this a value you pulled from published findings?

Response: These S/N values were calculated from the daily instrument QA calibrations performed by the CDPHE. 4 days of daily zero and span check data were used in this calculation, and standard error of regression is used as noise. More detail has been added to the text.

page 2439. Drift is a term that often applies to a change in response when the challenge condition has not changed. If that is how you intend for your discussion to be

presented, clearly define that for the reader.

Response: Our use of drift has been clarified, and is as the reviewer suggested. We use it to mean the change in calibration function coefficients due to physical changes of the sensor at a certain set of conditions.

page 2440. Lines 10-14. This text is confusing with respect to what was actually performed and there is insufficient text documenting the procedures employed. What does the term "low" mean in line 21. That is a subjective term which should be replaced with a value.

Response: This passage was clarified. The calibration in question uses reference data from a monitor far from the location of the M-Pods, a shortcoming of that portion of the work. A lab calibration and this collocation calibration were both performed in this case. As per the reviewer's suggestion, a large portion of the user study data has been removed from the main body of the paper. We have retained the discussion and statistics related to the uncertainty estimation from the user-study deployment, but have moved the actual user results to the Supporting Information. The use of the word 'low' has been changed by adding detail on exposures relative to historical air quality in Colorado.

page 2440 and follow on pages. I understand the authors wish to include personal monitoring data in this manuscript. However, the article is better presented if all such data are removed. Currently, there is insufficient discussion about subject population, exposure monitoring compliance, survey instruments used and other key aspects of human observational monitoring to include any such reporting in this article. I believe the article is totally sufficient discussing just the laboratory and in-field exercises. I strongly just removal of these data or adding pages of text to support materials and methods and results/discussion. For example, implying that subjects changed their behavior is totally unsubstantiated with the information you provide the reader. Simplify your article by removing this section of the results and discussion.

Response: This point is well taken, and as it is written, the user study data is not fully addressed. However, we believe that the uncertainty estimates from the paired M-Pods inform the discussion on calibration uncertainty, and would like to retain that section in the manuscript. We have moved user study result information, with additional usage detail and less qualitative discussion, to the Supplemental Information.

page 2441. Personal exposure data is not needed in this article. Strongly suggest it be reported elsewhere

Response: This suggestion is addressed above.

page 2442. How did you balance zero grade air? Is that what you are referring to in lines 6-9. I am uncertain of what your intentions are in that text but it calls into question your test apparatus. Better define your intentions here. Lines 12. Suggest you change the word "worse" to "poorer". Line 14. Why are you suggesting power supply issues? Do you have data to support such a hypothesis. Why might it not be intra-variability of the sensors themselves? Others are reporting batch to batch inconsistencies in MOx sensors and maybe this is what you experienced. Lines 22-25. Need to define the % of data removed and parameters used to censor data.

Response: We are not familiar with the terminology 'balance zero grade air', but add some more detail here. Zero grade air cylinders from AirGas were used as the balance gas for our target pollutants in all of the lab calibrations. The power supply issue we refer to was observed in some cases when the CO2 sensor momentarily drew more current than the power supply could provide. The issue is discussed in more detail in the supplementary information. The inconsistency in sensor behavior may still be due to sensor heterogeneity, as the reviewer suggests, and this has been noted.

page 2443 Lines 1-18. You provide no supporting data on the personal monitoring performed or the procedures used to ensure adequate data collection rates. Encourage its removal from this article. The term "very good" is not informative and suggest it be replaced with a more definitive qualifier.

Response: We have removed the bulk of this section and placed it in the Supplemental Information. Data collection rates and usage procedures were also added in that section. We acknowledge that the exposure results are not intended to be compared with the highest quality of exposure data, and were more for the purpose of instrument validation. The term 'very good' was replaced with information about the exposures in relation to local air quality from ambient monitoring stations.

page 2444. What are the power issues you refer to here? Might the curious NO2 concentrations you observed be associated with in-home gas appliances? Such observations have been previously made by others investigating such events. Lines 18-19. It would be worthwhile for you to define the cost of the in laboratory exercises in contrast to the field normalization events. There should be quite a cost savings here.

Response: The power issues are again related to the power supply providing sufficient current for proper operation, and this is detailed in the SI. The possibility of indoor NO2 sources is definitely a possibility, and was noted. The study took place during the winter months, and Colorado homes are often heated with natural gas forced-air heating systems. The cost comparison is noted in the conclusion, as a way to highlight the differences in methods, as suggested.

Citations; Sufficient except with the needed references defined above.

Table 3. Define S/N as how many folds above baseline (2X, 3X)? That is never reported in the article. Define the "N" term as minutes, seconds? The bold and italic text is confusing. I suggest you use * and ** superscripts to differentiate these lines

Response: We have better defined S/N in the methods section now, as the median concentration values over the standard error of regression. The typographic and clarity suggestions will be heeded as we work with the editors to type set the next version of the manuscript.

Table 2. The values listed in the co-location column are of course not realistic. Should

they be included at all? Make a case for their inclusion or simply excise them from the article. Table 4. Align columns for presentation quality. Maintain significant decimal places in the columns. Supplemental materials. No comment

Response: We agree, and have removed the values, noting the inability to find an optimal solution with the computational method used.

Anonymous Referee #3 Received and published: 27 June 2014 The authors present an interesting application based on low cost gas sensors. They developed a system that integrates 4 MOX gas sensors and one optical gas sensor, and temperature, humidity and light sensors. All the components of the system were off the shelf. The system is intended to quantify low levels of CO2, O3, NO2, and CO to monitor air quality. Although the explored application is interesting, the authors give very few details on the developed system and overlooked several issues that are relevant to come up with a robust and reliable system. Hence, the authors need to discuss the actual limitations of their system and present their system in the state of the art of gas sensing. In particular, the authors need to address the following points: a) Monitoring air quality on-field has already been investigated. De Vito (2009) is only one example of a system aimed to detect CO, NO2 and NOx for air quality monitoring. The authors have to present the advances/differences of their system respect to previous systems.

Response: Additional text has been added to draw the distinctions between our work and those of De Vito et al. and the other systems that were previously referenced. The primary distinction between our works are the selected sensor suite and the approach for quantification and uncertainty estimation. Without trying a machine learning approach as in De Vito et al., we cannot claim that our system has better performance, and we acknowledge that both methods are valuable in different contexts. This has been included.

b) The authors did not consider humidity in their models claiming that 'absolute humidity has a lesser effect on signal response'. The authors do not provide any evidence for

that claim. Actually, it is well accepted that humidity causes dramatic decrease of sensors' resistance due to the dissociation of the water molecule and the creation of lattice vacancies (Romain 1997). Sohn (2008) shows that the sensor resistance can change a factor of 2 when changing the humidity levels. A lot of effort has been done to reduce the effect of cross-sensitivity to humidity. Humidity correction is considered to be a must, especially for uncontrolled sampling systems (Marco 2014). Different authors have explored algorithms to correct humidity (Di Natale, 2008; Romain 2010). The authors need to discuss the effects of humidity or provide evidence that the sensors are not sensitive to humidity.

Response: Although the laboratory model development for the MOx sensors did not use humidity, we did not mean to discount its importance. In fact, a correction was added for all of the presented results. We simply meant to suggest that for the sensors tested, the error due to a swing in temperature was larger than that due to absolute humidity for reasonable ambient changes. Covariance of error with humidity swings can indicate when humidity is driving the sensor signal; the lesser error due to humidity during these swings (even large ones, e.g. precip events), support our claim for these particular sensors in their current configuration. We have clarified this point, and added the suggested references for drift management using machine learning algorithms.

c) Similarly, the authors need to discuss the limitations of the system due to temperature variations. A change of the gas flow or of the surrounding atmosphere temperature can disturb the temperature of the semiconductor surface and hence the conductance values.

Response: As the reviewer suggests, changes in sensor temperature due to airflow can result in substantial error. Specifically, ambient temperature can only be used to correct for sensor temperature changes when the device is calibrated under steady flow conditions. Otherwise, a change in the convective heat transfer coefficient due to increased flow velocity will cool the sensor surface with no change in ambient temperature. We have now added text speaking to this limitation. We have clarified that although collocation calibration should incorporate error due to temperature fluctuation, transient effects may be more substantial while users carry the M-Pod than during a collocation. The uncertainty assessment using paired M-Pods does not necessarily incorporate this either since they will generally have very similar airflow effects in both M-Pods, making this a limitation that should be studied further in this system.

d) The authors made a big effort to compare different calibrations procedures, but more details need to be provided to compare the calibrations. For example, gas flow, number of calibration points, range of calibration points, etc are not detailed. In particular, it is important that the authors provide the calibration ranges. From the results it seems that the calibration ranges are different for different calibrations. Hence, the direct comparison between calibration errors is not possible.

Response: Procedures and methods have been added to remedy this deficiency. The calibration ranges chosen for lab calibration were comparable with the ranges seen in the collocation calibrations (details in manuscript), so we believe the comparison between calibration errors is a fair one. The errors were both calculated in the same way using propagation of uncertainty principles.

e) The system needs to be detailed too. Dimensions, weight and volume of the gas chamber need to be specified. How are the sensors exposed to the gas samples? Is there any gas chamber?

Response: These changes have been made. During lab calibrations, the M-Pods were placed in a custom made carousel that could accommodate 12 M-Pods. This was then placed inside a Teflon coated chamber. Gases are fed in via Teflon tubing and a mass flow controller system. A photograph of the setup has been included in SI. Flow rates and mixing times are listed as well.

f) The authors made an effort to increase the robustness of the system. They reduced the effects of temporal drift by adding a simple linear term in the calibration function. However, correlation between sensors can be used to address drift and sensor failures in more efficient ways. The authors should include reference to other works to show that the robustness of the system can be improved by means of data processing techniques (Ziyatdinov 2010, Fonollosa 2013, Vergara 2012)

Response: The authors were not familiar with this drift correction approach. We have now read the suggested references and cited the work. We believe the methodology presented in these references, while powerful, is not suitable for this device, and should be considered in a different context. The device developed for this publication does not include a large array of intra-sensitive gas sensors with the intent of discriminating a large group of analytes (e.g. e-nose), thus does not lend itself well to a classical machine learning technique. We have redefined and expanded on our definition of drift.

g) A figure showing the sensors' signals from a calibration measurement would be very informative. Also, an example of the signals acquired while a user was carrying the sensor would show the complexity of the task. The authors should discuss the difficulty of gas discrimination in open sampling systems due to turbulences and environment variations (Vergara 2013). In short, the developed system showed some promising results, but the authors need to provide better the limitations for integrating gas sensing in wearable devices.

Response: We agree that additional details such as this should be included. An example calibration function and time series for CO was added to the SI. A snapshot of the full data stream (multiple sensors) for a short period has been added to the manuscript as the reviewer suggests. The full suite of sensor data collected in each M-Pod is certainly ripe for further analysis using methods such as those described in the suggested reference

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