

Interactive comment on “Community Air Sensor Network (CAIRSENSE) project: Evaluation of low-cost sensor performance in a suburban environment in the southeastern United States” by Wan Jiao et al.

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Author response to interactive comments on “Community Air Sensor Network (CAIRSENSE) project: Evaluation of low-cost sensor performance in a suburban environment in the southeastern United States”

The below comments are the collected response from the three reviewers. Review comments are preceded by "RC:" and author response comments are designated as "AC:". Following the author comment are the changes made to the manuscript.

REVIEWER 1

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RC: General: This paper discusses the performance of a low-cost sensor network deployed in a suburban area of the southeastern United States. Overall the study is well-designed, with results that provide useful contributions to the field. The correlations of the low-cost sensors described here with Federal Equivalent Methods (FEM) will be useful to anyone investigating the usefulness of some of the monitors in other studies. While much remains to be done with understanding the limitations of some of the monitors (for example, testing them in other environments), this represents a significant and well-executed first step. The work is well-explained, and the paper well-written and logically organized. Tables are useful as well. The authors do a good job with presenting enough information about the wide variety of instruments which they incorporate into the study, without overwhelming the reader with unnecessary details.

AC: The reviewer comments are appreciated.

Changes to manuscript: None

RC: Specific Comments: I have a concern with the particle size ranges measured by some of the instruments, which do not match the particle size range of the FEM (i.e., 2.5 μm and below). For example, the Dylos data is shown as being counts either $>1 \mu\text{m}$ or $>0.5 \mu\text{m}$, depending upon the model. It is problematic to try to correlate this to the FEM, because they are measuring two different size ranges of PM (>0.5 or $>1.0 \mu\text{m}$ vs $<2.5 \mu\text{m}$ of the FEM). This can be easily fixed. The Dylos DC1100 should also include a "large" channel that measures PM $>2.5 \mu\text{m}$; thus, if you subtract the "large" ($>2.5 \mu\text{m}$) counts from the "small" ($>0.5 \mu\text{m}$) counts at each time step you will end up with a better approximation of PM_{2.5} (PM $<2.5 \mu\text{m}$). Doing this might improve the correlations and comparison with FEM. I acknowledge that the total number of particles greater than 2.5 μm may be rather small in comparison to those below, but this should still be done for completeness (or at least you should explain that those counts above 2.5 were negligible). [For further details on this and similar conversions of Dylos counts to mass concentration, see "Determining PM_{2.5} calibration curves for a low-cost particle monitor: common indoor residential aerosols," Environ. Sci: Processes Impacts, 2015,

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17, 1959. DOI: 10.1039/c5em00365b].

AC: The authors raise a good point – that one could take advantage of the larger size bin for the Dyls to try to eliminate potentially large particles and this may improve the sensor agreement with the reference monitor. One complexity for our specific testing situation is that two models employed – the DC1100 PRO and the DC1100, have differing lower size detection limits for the larger size bin (one is $\geq 2.5 \mu\text{m}$ and the other is $\geq 5 \mu\text{m}$).

While having this size separation is likely important for indoor applications, where unique emission sources (e.g., vacuuming, cooking) is expected to generate potentially significant number concentration in the coarse fraction, in the suburban ambient environment the contribution of coarse particles to the total number concentration is anticipated to be less of a factor. To determine the potential impact of the contribution of the larger particles for our testing environment, we did a simple ratio of the Large vs. Small size bins for the two model types. On average the Large vs. Small count was a factor of 0.032 for the DC1100 and 0.041-0.044 for the two DC1100 PRO units.

Changes to manuscript: We have added the following statement which appears on page 10, lines 16-22: “It should be noted that one sensor type – the Dyls units – does provide an additional larger particle size channel ($\geq 2.5 \mu\text{m}$ for the DC1100 PRO-PC version, $\geq 5 \mu\text{m}$ for the DC1100-PC version), which one indoor application study utilized to remove the larger particle signal (Dacunto et al., 2015). However, in the suburban ambient environment in this study, the fraction of particle count in the larger size channels appeared to be a small component of the total particle number count, with the ratio of the large vs. small count channels averaging 0.03 and 0.04 for the DC1100-PC and DC1100 PRO-PC, respectively.”

RC: In addition, the MetOne is reported as providing a wide variety of PM size data. The reader assumes that you are using the PM_{2.5} output to compare to the FEM, but it would be better to state that specifically.

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AC: This is a good clarification point.

Changes to manuscript: We have adjusted Table 1 to make it clear that the PM2.5 output is what is utilized in the analyses, and have also added a footnote to indicate the other size channels available for both the Dylos and MetOne units.

RC: Finally, it may be helpful to have a couple of representative plots of low-cost instruments vs FEM, perhaps one showing an instrument with very good correlation, and one with poor correlation. Doing so would enable the reader to better understand some of the relationships.

AC: A full scatterplot matrix showing the sensor versus the reference, as well as relationships between sensors, is available in the supplemental information.

Changes to manuscript: None.

RC: Technical corrections: Pg 1, line 27: add "the": ". . . over a surrounding _2 km area in

THE Southeastern U.S."

AC: The careful review is appreciated.

Changes to manuscript: Corrected text as suggested.

RC: pg 2, line 29: change "utilizing" to "utilize

AC: This has been corrected.

Changes to manuscript: Corrected typo as suggested.

REVIEWER 2

RC: This article discusses a very important topic, as the widespread use of low-cost sensors by researchers, governmental organizations and citizen scientists will soon reshape the way to measure and regulate air pollution. In my view the real value of this work will become clear once the measurement results from all other CAIRSENSE

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locations will be made available. At the moment, as acknowledged by the authors, the results discussed in this paper may not apply to other locations with different RH, T, wind conditions, and air pollution sources. When commenting on sensor precision, it may be worth mentioning that firmware version may also contribute to the measurement variability between units of the same model. Overall, I think this document is well written, has already been improved from the version that was originally submitted to AMT and deserves publication.

AC: The reviewer comment is appreciated. We have added in some language in the conclusions discussion the multiple design factors – firmware included – that are likely to impact results.

Changes to manuscript: Additional text now states on page 14, lines 9-16: “A variety of factors are anticipated to contribute to sensor performance in the measurement of outdoor air pollution trends. Key design aspects include the sensitivity and stability of the internal pollutant sensing component, design of the device enclosure and mechanism of introducing air to the sensing region, addition of any ancillary sensors used for signal adjustment (e.g., RH sensor), as well as on-board or cloud-based firmware processing raw signals into estimated concentrations. In addition, the pollution mixture, concentration regime, and environmental conditions are anticipated to impact the sensor performance.”

REVIEWER 3

RC: This an important topic with a great deal of significance to the air monitoring community involved either research or community air monitoring projects. And it represents a good start at addressing the many issues one faces when doing such studies with low cost sensors. An overarching concern with this paper is that it too briefly describes sensor systems being tested beyond a define low cost. Several seem unsuitable or not designed for such outdoor use. Further, it would seem that the process for selection of the sensors may not have included important and readily available units (for example

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there is are Aeroqual sensors for gases beyond those selected and AQ mesh also may offer a PM sensor option). A final point is that many readers would like guidance on how to select Aand use these and other sensors. This topic is not included.

AC: These are very important points raised by the reviewer. It should be realized that this is a quickly developing worldwide market, therefore some sensor types that are now available (e.g., AQMesh PM2.5 sensor) were not available at the time of the field study in Atlanta. To help the reader understand the full considerations on sensor selection, and that it was not an exhaustive testing process of every sensor on the market - we have expanded our sensor selection description to include some additional details.

Changes to manuscript, added text on page 5, lines 7-14: “The term “sensor” in this paper refers to the off-the-shelf hardware that was selected for testing, which generally includes one or more pollutant detection components (e.g., an electrochemical cell) combined with a form of on-board microprocessor to convert the signal into a concentration units. The design of the sensor for long-term use in an outdoor environment (e.g., a weatherproof enclosure) was not a selection factor, as the research team was aware of a number of outdoor air quality field studies utilizing sensors designed for indoor application. The field testing was therefore conducted to provide weather protection for all sensor types tested.”

And additionally, added text on page 5, lines 15-19: “Finally, it should be noted that the sensors utilized in this study represent a selection of sensors available on the market at the time of the study initiation, and that the sensor development market is quickly changing with time.”

RC: Specific comments Abstract-issues of data quality are raised as being important for low cost sensors. However, this is a weak point in paper. What is acceptable or sufficient data quality? For PM the authors appears to be a defined level of acceptable quality-when agreement between reference monitors and the sensor shows a “moder-

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ate to strong” agreement of $r=0.5$. This reviewer finds that such a level of agreement is arbitrary and perhaps unacceptable for many uses. While text on page 8, line 12 seems to acknowledge that there is no good means to classify agreement, the arbitrary use of terms like weak, moderate, good and fairly strong are still used in the findings section. Reading page 8 one would expect only r or r^2 values to be presented in the abstract, results and summary sections and not the qualitative terminology.

AC: It is common in all air quality monitoring studies to provide associated descriptions that parallel various ranges of correlation, which allows some discussion of the relative scale of agreement between two data sets under comparison. Having qualitative descriptors does not equal a pass or fail designation to the device – it simply provides the reader a sense of where the relationship resides between absolutely no relationship ($r = 0$) and perfect correlation ($r = 1$). While our research team understands the strong desire for this paper to pass a clear judgement on what is acceptable or unacceptable for use, we have to also recognize that there are a variety of potential use cases and data quality objectives that are application-dependent. Additionally, there are design factors beyond the sensor correlation that might be considered in the application, where the user may accept a trade-off on correlation for other factors such as portability or built-in wireless communications.

Changes to manuscript: To avoid any confusion to the reader, we have expanded the sentence on page 9, lines 7-9: “Therefore, the results in this paper are communicated quantitatively by their correlation, or lack thereof, in comparison to regulatory-grade monitors, with common associated descriptors of the strength of agreement (e.g., “moderate”).”

RC: “Overall, this study demonstrates a straightforward methodology for establishing lowcost air quality sensor performance in a real-world setting and demonstrates the feasibility of deploying a local sensor network to measure ambient air quality trends.” This closing statement of the abstract seems only generally based on the paper and does not include a clear summary of data quality findings of the study. The paper

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does not provide detailed protocol/procedures needed for testing of sensors and characterization that users should carry out before they deploy monitors-rather it seems to simply be based on partial success of a small scale deployment of wireless network. This summary of the study should be modified to better reflect the specific findings of the study that includes the findings of sensors used in the field tests (which seem quite negative) not just the protocol. A key issue needs some consideration– whether these sensors/systems are actually suitable as they come from the seller for use as outdoor air monitors in community monitoring. Does the research point out useful things regarding how users should select, evaluate and employ low cost sensors? A cautionary statement such is as found on page 13, line 22 should be pulled into the abstract.

AC: While it is beyond the scope of the paper to go into great detail on recommendations for sensor use in a wide variety of potentially envisioned applications, we agree with the reviewer that the abstract should provide a more clear take-home point and have adjusted the concluding statement.

Changes to manuscript: Revised concluding statement on page 2, lines 9-12: “Overall, this study demonstrates the performance of emerging air quality sensor technologies in a real-world setting; the variable agreement between sensors and reference monitors indicates that in situ testing of sensors against benchmark monitors should be a critical aspect of all field studies.”

RC: Page 2, line 27-in defining low cost it is unclear whether it is proper to set a 2K USD per sensor limit for consideration. Sensor are only useful if part of a complete system so it is important to define what a “sensor device” consists of. The paper should include data on the complete system costs and if web/server based systems such as the AQmesh the system costs should include service costs. Further, this tight cost definition could cut out other sensors that cost a bit more but might produce better data.

AC: While we understand the practical considerations of trying to estimate “full cost”,

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this would require making many poorly substantiated assumptions about how each device would be applied and what additional costs would be added, as well as how the manufacturer may adjust their pricing of factors beyond the hardware cost over time. For example, sensors that do not include wireless data transmission or internal battery power may have those components added on by the user for a specific application, which would add cost that depends on the selection of components, means of wireless data transmission (e.g., cellular data plans, satellite transmission), and so on.

Changes to manuscript: We have added in the following sentence to the sensor cost description on page 5, lines 4-7: “It should be noted that the cost break-point was set by the estimated hardware pricepoint at the time of the device selection, and does not incorporate other possible other costs that may vary by application (e.g., maintenance, data-hosting fees, modification of power input).”

RC: Page 3, line 11-there is mention of two PM sensors and how they perform. However, the text only mentions performance acceptability in very general terms in undefined moderate, high or very high concentration situations. However, these concentration and performance characteristics are not defined or described. This issue is important and should be expanded with details added beyond the simple reference in the text.

AC: This comment is appreciated and we have added in some further details on the previous studies.

Changes to manuscript: Page 3, Lines 17-26: “For particulate sensors, PPD42NS sensor comparison at low to moderate ambient concentrations revealed good correlation (e.g., $R^2 = 0.72$ for 24 hour averages, $PM_{2.5}$ ranging $\sim 3\text{--}20 \mu\text{g m}^{-3}$) with a reference monitor (Holstius et al., 2014), however the same particle sensor at very high concentrations (hourly average $PM_{2.5}$ ranging $\sim 77\text{--}889 \mu\text{g m}^{-3}$) had nonlinear response and authors used high-order model fits to correct their data (Gao et al., 2015). Additionally, a modified commercially available particle sensing device (Dylos) was shown to match

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diurnal ambient PM_{2.5} trends with a research grade monitor (DustTrak) under ambient concentrations (hourly average PM_{2.5} ranging \sim 5-50 $\mu\text{g m}^{-3}$), after adjustment with 24-hour averages derived by a beta-attenuation regulatory-grade monitor (Northcross et al., 2013).”

RC: The “ad-hoc” testing of the candidate sensor systems does not appear to include comparisons to reference monitors. This would have increased the value of observations. Why was this not done? Page 4, line22-sensor devices are not adequately described and details regarding their suitability for measurement of outside air is not included. One clear example is the MetOne 831. It is not a sensor, rather it is a complete monitor and there are several others that could have been included. It is not designed for the application in continuous outdoor air monitoring, instead it might be more properly be considered a hand held PM survey instrument. The air egg devices also don’t seem to be designed for use in outdoor air. The authors should expand discussions of the selection process and suitability of the sensors selected.

AC: All sensors were compared against reference monitors – what may have caused confusion was the use of the term “FEM” in Tables 3-4.

Changes to manuscript: We have replaced that term with the “Reference” to make it explicit, as well as updated the Table 5 description to say “Reference” as well. Please see earlier response regarding the sensor type selection process.

RC: Page 4, line 27–There is an enumeration of ‘sensor types’ for the various pollutants especially for gases. However, it is likely to be unclear to many readers what is meant by sensor types. Are any of the actual sensors the same in any of the units since only Aeroqual, and perhaps Caripol appear to make some of their own gas sensors (this reviewer understands that they also may use sensors made by others-especially Alfasense. Perhaps the table and text would be improved by a more clear definition of sensors in each system tested. The list of pollutants assessed is not complete-the AQ mesh appears to have reported NO and CO₂ in addition to those on the list. Aeroqual

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50 sensors and sensor testing could have included CO and SO₂. Why were these not included in tests? A final point on sensor descriptions is that the sensors used in the Air Quality Egg units are described as being electrochemical-It would appear they are not. Rather they may be metal oxide units. The authors should clarify this and include any corrections in Table 1.

AC: The review comments are appreciated. We have included the specific internal sensor type and model to the extent that level of detail was provided by the manufacturer. Please refer to earlier discussion on the sensor selection process – we are noting that not all sensors on the market were tested, but a subset.

Changes to manuscript: We have made the correction as noted to Table 1 regarding the Air Quality Egg; additionally, the revised sensor selection text on page 5, lines 9-12 reads: “The term “sensor” in this paper refers to the off-the-shelf hardware that was selected for testing, which generally includes one or more pollutant detection components (e.g., an electrochemical cell) combined with a form of on-board microprocessor to convert the signal into a concentration units.”

RC: The cost limit for sensor devices was stated as \$2000 per sensor. What is the cost/sensor for this device, including data processing that a user would encounter if they used this sensor system for the period they were used? The AQMesh unit cost should include the support/data fee for the user since its data are not available to the user without this fee.

AC: Please see earlier response regarding sensor selection process and cost considerations.

RC: One further point regarding selection and use of sensors is that PM sensors rely on photometry. Some report particle counts while others convert and report data as mass. Conversions to mass are not straightforward and should be more completely described. Also, it is not clear that the PM sensors are equipped with size selective mechanisms to assure they measure only PM 2.5 for comparisons with reference monitors. Humidity is

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known to strongly impact photometer based results and this issue should be discussed. Together these factors make it quite complicated to understand and compare the performance of the various PM sensors and especially why one should expect agreement with regulatory grade PM 2.5 monitors. Finally, of the sensor systems included, only the AQMesh has a commercial system for measurement of the target gaseous pollutants. There have been several versions of the AQMesh and version 3 was used here. What is the current version (as Aug 2016) and how does it differ?

AC: We have added in some additional description to the introduction on the numerous possible measurement issues that could be anticipated with low cost sensor. Secondly, it is a good question about the potential changes in sensor technology – not just the AQMesh but also other devices that are presented here – since the time of the field study. Since this is a continuously evolving market, we would recommend contacting the manufacturer to have the most up-to-date information on any design changes and whether new testing results are available.

Changes to manuscript: Page 3, Lines 4-9 now reads: “The pollutant detection methods utilized in miniaturized sensors are potentially prone to measurement artifacts. For gas-phase sensors, these artifacts may include cross-sensitivity to other gases as well as impacts by varying humidity or temperature. The optical-based detection of particles is anticipated to be affected by humidity during high relative humidity conditions, as the uptake of water by hygroscopic particles can lead to an enhancement in the light signal. Finally, both lower and upper detection limits are also an expected factor in sensor performance.”

RC: Page 5, Line 4- This text states that the investigators followed manufacturers’ recommendations for operation. Do the various sensors provide operational directions for use in measuring outdoor air and where there any considerations made for humidity corrections performed or recommended by manufacturers?

AC: All sensor types had some level of instructions that were either provided along with

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the hardware or provided through direct communication. As the study was intentionally using the direct sensor readout after any internal onboard or cloud-based processing, any initial adjustment for measurement artifacts was not directly accounted for but is reflected in the results. Additionally, our analyses evaluate whether measurement artifacts exist in the output signal.

RC: Page 5, line 11- It is appears that the researchers were provided adjusted processed sensor data for the units from AQ mesh. If the data were processed in any way by the vendor this process should be completely described. Such adjustments could influence the values reported in this paper. This is an important issue since the readers need to know they do not have access to sensor data from this system instead they only receive processed data so they can't independently perform comprehensive tests of this system.

AC: It is currently stated: "The AQMesh data analyzed were already post-processed by manufacturer proprietary algorithms prior to analysis." As the algorithms are proprietary, we are not privy to the exact algorithms converting the raw signal to the final concentration output. It is an interesting discussion to have on the evolving state of sensors and data handling – for example, firmware on more traditional instruments also has proprietary algorithms that are not fully made available to the user and one could argue that a true "open source" air monitoring instrument is a rare thing. However, the firmware residing on the hardware that is not online-accessible by the manufacturer means that the instrument owner is more aware of when firmware updates happen. Meanwhile, online-access by the manufacturer and web-based transmission of data could lead to what is effectively firmware updates that impact sensor performance without the owner's knowledge. This general trend is one that is worthy of discussion, but we would suggest is beyond the scope of this paper and would require also going into further detail on evolving strategies for sensor network virtual calibration.

RC: What were the actual dates of performance of the two protocols? Further information is needed to describe attempts to consider response by season or temperature

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regimes Page 8, line 30 and following-it is unclear how the decision to perform FEM comparisons with 12 hour time resolution was based on performance of the BAM. Perhaps this decision impacted the comparisons with the other sensors, since they too must have undergone data averaging. It seems that one hour data is of great value in this comparison study. Data should be explored to consider hourly comparability and observations. This is because users are not going to restrict their use to 12 hour averages; rather they are likely to want hourly or even sub hourly data. The choice seems to be based on imprecision of the BAM. This is illogical. Regulatory monitoring programs all over the world report hourly data from BAMs. Data for hourly agreement between the BAM and various monitors REALLY MUST BE PRESENTED!

AC: Although hourly BAM data is often available in many places, the noise in the data are a function of the concentration level, as the detection is based upon the amount of particle mass loaded to the internal filter on the instrument. One simple way to determine if the BAM hourly data is useful is to see how frequently the data are negative or have significant deviation in an hourly signal, which in an ambient environment isolated from sources would be most likely explained by instrument noise. For the fairly low concentration environment, approximately 20% of the BAM hourly data in this study would fall into a category of an hourly change of concentration exceeding 50%. Or, in absolute terms, 19% of the hourly data would have negative values below what would be expected for the detection limit ($<-1 \mu\text{g}/\text{m}^3$) or fairly significant hourly shift in concentration ($|\Delta C, \text{hr}^2 - \text{hr}^1| > 5 \mu\text{g}/\text{m}^3$, whereas the study mean $\text{PM}_{2.5}$ is $\sim 11 \mu\text{g}/\text{m}^3$). Therefore, while we agree that evaluation of PM sensors at a high time resolution is desirable, increasing the averaging time span was determined to be necessary to provide a fair comparison between the reference and the sensor. Finally, the overall time span of the study is noted (page 4, lines 22-23) and we also note that the WSN network was deployed for 7 months - however in our sensor summary table we make it clear that not all sensors continuously operated (sampling days noted per sensor on Table 3) due to malfunction or other factors.

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Changes to manuscript: None.

RC: Page 9, line 9 and following-comparisons of the raw (or internally processed data) from the various instruments is too simply reviewed. One should expect the various instruments to report quite different values that may or may not be related to PM_{2.5} mass. None are mass (gravimetric) monitors and none appear to have physical size cuts to assure PM_{2.5} is being reported. Carefully conducted calibration vs. reference studies have been performed on some of these units and should have been considered as a useful output of this study as well.

AC: The study methods describe that the PM sensor data are converted to mass concentration units – effectively locally calibrated against the reference, with the current methods text describing the variable data output units and states: “In addition, to enable basic comparison of PM values with a reference monitor, data from PM sensors that had at least moderate correlation ($r > 0.5$) were converted to $\mu\text{g}/\text{m}^3$ units based on upon an OLS regression equation.”

Changes to manuscript: None.

RC: What were the overall summary findings for the PM sensors? Are they suitable for use and under what constraints? It would appear that after reviewing the data presented on tables 3 and 5 that none of the PM sensors tested under the rather careful protocol employed represent good choices for application in community/citizen based air monitoring. If the authors agree with this summary they need to clearly say so. If they do not agree they should clearly present a rationale for their use. It appears that the Cariclip values for ozone and NO₂ are presented after adjustment by AQ monitor data. This may improve the data, but it should be clearly pointed out in the discussion of results and evaluations of the data. The community/citizen user of this sensor is not likely to have the luxury of operations next to a FEM site. The unadjusted data should be reported-perhaps as “oxidant” and the utility of this sensor should be made clear. It appears it does not report ozone and it may not report NO₂. However, this

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is somewhat complicated by the summary of NO₂ sensors on page 10, line 10, where it is mentioned that NO₂ data from the Cariclip was evaluated. Was there a separate NO₂ sensor for the Cariclip? Please clarify cariclip sensor and data handling.

AC: The paper describes in section 3.2: “Of all the sensors discussed, the CairClip NO₂/O₃ sensor is unique in having a single data value output that nominally represents the addition of NO₂ plus O₃. Therefore, CairClip NO₂ or O₃ values discussed represents the initial summation minus a FEM reading (i.e., $\text{CairClipNO}_2 = \text{CairClipNO}_2/\text{O}_3 - \text{FEMO}_3$; $\text{CairClipO}_3 = \text{CairClipNO}_2/\text{O}_3 - \text{FEMNO}_2$.” Whereas for evaluation purposes the FEM data were used to subtract one of the two parameters, it is reasonable to expect that a similar strategy could be employed by combining the CairClip sensor with one that reads ozone in isolation. To make this example more clear, we added clarifying text to the WSN description.

Changes to manuscript: Page 3, lines 6-9 now reads: “Selected air quality sensors included the Shinyei PM sensor, Cairclip NO₂/O₃ sensor, and the Aeroqual SM50 O₃ sensor, with the two gas sensors in conjunction envisioned to provide data supporting the separation of NO₂ and O₃ signals.”

RC: Page 11, line 7- it appears that Aeroqual sm50 units could have included this gas as well as CO. Why were these not included for the sm50-an additional base unit would have been needed, but it is a missed opportunity for a logical comparison.

AC: The Aeroqual SM50 is an ozone sensor. Please see earlier comments regarding sensor selection and that we are describing that not all sensors on the market were able to be tested at the time of the study.

RC: Page 13, line 22- “These results underscore the importance of individual sensor performance testing prior to field use, and the corresponding higher uncertainty in sensor datasets that do not incorporate field testing in their application.” While such work seems not to be the focus of this study this reviewer agrees strongly with this assessment and finds that users need to know that this paper actually finds that many of the

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sensors tested would produce quite poor data without special care and even after lab and field testing the data are at times capable of only weak agreement with what regulatory grade ambient air monitors report. I guess what I am trying to say is that it is essential for the user to understand the performance of their sensor systems and this really requires quite complicated testing. THIS STATEMENT FROM LINE 22 SHOULD BE ADDED TO THE ABSTRACT!

AC: The review comment is appreciated and we have adjusted the abstract as mentioned earlier.

[Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-131, 2016.](#)

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