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Atmospheric Measurement Techniques Discussions

Interactive comment on "Characterising low-cost sensors in highly portable platforms to quantify personal exposure in diverse environments" *by* Lia Chatzidiakou et al.

Anonymous Referee #3

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Comments on Chatzidiakou et al. (submitted) Characterising low-cost sensors in highly portable platforms to quantify personal exposure in diverse environments

General comments: This manuscript describes an innovative and useful development for air pollution exposure assessment using novel miniature automated gas & particle sensor systems. The manuscript represents an important addition to the literature in this field on a topic that is of considerable international public health concern. Strengths of the study include the novelty and numbers of instruments evaluated; and the diversity of field evaluation environments studied.

Improvements could be made to the manuscript to address possible issues related

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to the reproducibility of the science described. The results could also be presented in a way that clarifies the possible effects of arbitrary division of field data into test and training subsets (raised by earlier published review comments). Earlier review comments have also asked for clarification on key issue about extent of sensor drift. The manuscript would be strengthened by specifically addressing these points even if this is merely to provide adequate quantification of why arbitrary division of data, and sensor drift, are not important with the instruments evaluated in this research. I have suggested some possible ways of addressing these and other points below.

Specific comments: Abstract: 1. It would appear beneficial to include brief selected quantitative summary information on the precision, accuracy and lack of (or extent of) measurement drift over time for the instruments evaluated.

Introduction: 2. It is indicated that 'this paper further aims to create a roadmap for calibration and validation of portable monitors suitable for personal exposure quantification'. I'm not sure what you meant by this, and wondered if this aim could be stated in a clearer way?

Methods: 3. On the general scientific point about [in an ideal world] research being reproducible by other researchers to accelerate discovery (Munafò et al., 2017) it would be helpful to clarify if the monitoring systems being tested are [or will be] commercially available to wider research communities beyond consortium of authors. I appreciate that the sensors are specified, but also there may be a substantial number of person-years invested in the design and construction of the monitoring systems that it may be appropriate to elaborate on. I also appreciate it is fine to report on instruments prior to commercialisation as the ideally reproducible science specified by Munafò et al. may not always be practical in the short to medium term.

4. It would be helpful to expand on the principle of operation of the gas sensors used. The references given are useful but these tend to focus on 3-electrode systems rather than the innovative 4-electrode system that you have used. I think you could make more

of this by explaining the principle of operation of this system in a more expanded way that is accessible to a wider audience beyond specialists in the construction of electrochemical sensors e.g. a diagram similar to diagrams of 3-electrode systems in earlier references would appear to be helpful. Stetter & Li 2008 [referred to in manuscript] indicate how gas membranes, electrolytes and electrodes are key components of amperometric gas sensors. I wondered if the manuscript would be improved by providing information on the characteristics of these sensor components for the systems tested? How does gas reach gas sensors e.g. diffusion or active fan etc?

5. You refer to earlier work by Mead et al. 2013 in relation to sensor linearity and quantified limit of detection (LoD). This earlier paper seems to be focused on 3-electrode sensors. Is it ok to directly translate the finding to the newer 4-electrode sensor, or is separate quantification of the linearity and LoD of the newer sensor also required?

6. A strength of your paper is that you use a conceptually simple calibration model (Equation 1) cf. potential over-fitting of calibration data in other sensor evaluation papers. To emphasise this strength I think it would be beneficial for you to present the results of the calibration in terms of the equations fitted. If the information on these equations is too extensive they could be included in Supplementary Information.

7. It would appear useful to make your field calibration data available as a published dataset linked to your paper to allow other researchers evaluating different, but similar types of, electrochemical sensors to compare similar calibration data to your results.

8. Perhaps it would be beneficial to add a reference for Mie scattering on p5.

Results:

9. In Fig 3 why did you decide to 'draw the line' at the specific point in the time series between panel (a) and panel (b). Was this splitting of the data done in an entirely objective manner, or did you optimise the split to get 'good' calibration results e.g. maximising R2 and/or other metrics of agreement between sensor and reference data?

From what I can see your results look as if they may be fairly robust to different selection possibilities, but I think (as suggested by the earlier reviewers) it would be useful to make this clearer in the manuscript, and that you could strengthen the manuscript by quantifying the effect of any arbitrary splitting of the data against other arbitrary (or ideally - objective) data splitting decision possibilities. If you were able to develop a robust objective way of doing this it would help with reproducibility concerns outlined above.

10. The preceding point about split between training and test could similarly be clarified in more detail for data in Figs 4, 8, A2, A3, A4 and A6 (currently it is not clear what calibration data has been used to adjust the sensor outputs in these figures).

11. The quantification of sensor drift over the duration of measurements [and beyond duration of field measurements as you suggest in response to earlier reviewers] could also be beneficially included in the manuscript in the vicinity (perhaps in Figure captions) of Figures 4, 8, A2, A3, A4 and A6.

12. I recommend giving dates and the number of hours of measurements for each of the deployments in the columns in Table 3.

13. In Table 3, instead of giving RMSE as a percentage of maximum (which may [by definition] be an outlying point) would it not be better to specify the mean reference concentration and specify the RMSE as a percentage of that mean?

14. On p10 you indicate that an RMSE of less than 16% of maximum concentration is negligible. I think it may be better to express as percentage of mean (i.e. preceding point) and let your readers reach their own description of the level of agreement.

15. On page 11 second paragraph were the extreme temperatures you refer to recorded inside the monitor enclosure and within the sensors?

16. Do you know why the O3 sensor appeared to be less affected by the extreme temperatures cf. the CO, NO and NO2 sensors?

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17. Your reference to the participants on p11 could be given with further detail e.g. geographical location / time of year etc. to provide context to the comparison with the field evaluation measurements.

18. In Figure 4 caption it would be helpful to specify how far the 'PKU reference site' and the 'nearby government monitoring site' were from the indoor measurement location.

19. On p12 you assert that your results prove the suitability of the low cost sensors for quantification of indoor air pollution. It would seem appropriate to qualify the promising indication of suitability over timescales and conditions similar to the calibration period/conditions?

20. Do you have information on the response times of the sensors, and could any difference in response time between sensors for different pollutants affect the cross sensitivity calibrations determined at the static monitoring locations?

21. When the PAM was mounted on the roof of the vehicle how were the sensor inlets orientated in relation to airflow? Did the varying speed of airflow have any effect on agreement between sensors and reference instruments?

22. In Fig 6 where, and what sort of, measurements were being made between: 11.30-12.30 e.g. was vehicle static or in a quiet road with no other vehicles? Do you know why O3 measured by sensor and reference instruments diverges during this time period?

Reference: Munafò, M.R., Nosek, B.A., Bishop, D.V.M., Button, K.S., Chambers, C.D., Percie du Sert, N., Simonsohn, U., Wagenmakers, E.-J., Ware, J.J., Ioannidis, J.P.A., 2017. A manifesto for reproducible science. Nature Human Behaviour 1, 0021.

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