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Atmospheric Measurement Techniques Discussions

Interactive comment on "Characterization of lower-cost medium precision atmospheric CO_2 monitoring systems for urban areas using commercial NDIR sensors" *by* Emmanuel Arzoumanian et al.

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Reply: We would like to thank the reviewer for the comments and suggestions, which have guided our revisions of the manuscript

This paper reports a series of experiments evaluating one particular low cost CO2 sensor. The paper is not especially well organized. It reads as a long list of experiments. However, analysis that synthesizes the observations and context from other related work is lacking. There is an extensive knowledge base of performance for such NDIR

instruments, including manufacturer literature (e.g. LiCor, Vaisala), and field evaluation in other contexts.

Key ideas in the description of NDIR sensors that this paper could be better organized around include:

Reply: We agree that the previous version of the manuscript was indeed not organized clearly enough and we have significantly streamlined it. We have furthermore moved multiple figures into the supplemental materials.

1) that they measure absorption which is proportional to number density but the atmospheric quantity of interest is dry air mixing ratio. The measured quantity must be converted using the ideal gas law and subtracting water number density to give the dry air mixing ratio. Many of the figures are some form of confusing intermediate product along the way to a dry air mixing ratio.

Reply: This study does indeed give the different steps between the raw output of the instrument, which is supposed to be mole fractions, towards calibrated dry air mole fractions eventually. The description of the fundamental spectroscopy can be found in Hummelgard et al. 2016. Our step by step approach was deliberately chosen to highlight, which cross-sensitivities and instrument characteristics cause the deviations of this "raw" mole fraction data and calibrated data that reflects dry air mole fractions.

2) that a second order correction is associated with pressure broadening of the CO2 absorption lines.

Reply: This secondary effect was not accounted for as the first order corrections allowed to achieve our repeatability target, furthermore this effect did not seem to influence the linearity of the instrument in the tested range (330-1000ppm). The new manuscript version now mentions this additional source of uncertainty.

3) that knowledge of zero is as challenging as knowledge of response to CO2. The paper neglects to acknowledge or build on related work by Shusterman et al. Atmos.

Chem. Phys., 16, 13449-13463, 2016 and Zimmerman et al. Atmos. Meas. Tech., 11, 291-313 2018 and likely others.

Reply: The reason for introducing the linear drift term in the multi-variable calibration was indeed due to the issue of a non-stable zero of the instrument. We have clarified this point in the manuscript. We have also significantly extended the discussion on other work on lower-cost CO2 sensors, although this paper is intended as a technical description of one specific instrument (better reflected with the new title).

Overall, I recommend a substantial revision to improve the clarity. Cutting the number of figures in half and targeting them to identified issues with performance would be welcome.

Reply: Thank you for this suggestion. The manuscript was substantially restructured to address this issue.

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