Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2021-120-RC1, 2021

Comment on amt-2021-120

Anonymous Referee #1

Referee comment on "The Berkeley Environmental Air-quality and CO_2 Network: field calibrations of sensor temperature dependence and assessment of network scale CO_2 accuracy" by Erin R. Delaria et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-120-RC1, 2021

Delaria et al. evaluate the performance of the low-cost sensor network BEACO$_2$N for CO_2 measurements in the Bay Area, California. They find that, the low-cost sensors have a residual dependency on ambient (inside the sensor housing) temperature which can be corrected for by calibrating the background samples (lowest 10%-percentile) against a reference measurement (either Picarro or network median). Delaria et al. also evaluate implications for calibration of similar networks emerging at other locations around the globe. The paper is well written, the methods are robust and rigorous, and the topic is certainly of high interest to the readers of Atmospheric Measurement Techniques. I recommend publication with minor, mostly technical modifications required.

Comments/questions:

A question I have is whether the authors made any attempt to identify the root cause for the temperature dependence. From the manuscript it becomes clear that the temperature dependencies are large, vary from sensor to sensor in terms of magnitude (and sign!), and might even be intermittent/discontinuous (L171ff). While the network analysis conceived by the authors is certainly convincing in terms of delivering an a posteriori fix to the problem, it would obviously be best if the temperature dependencies were fixed on instrument side.

The other question I have is whether the authors have indications for non-linearities (i.e. ΔCO_2 depending on CO_2)? Fig. 10a shows that the median of the low-cost sensors has peaks that are substantially lower than those of the Picarro. The median might not be very indicative for this particular question. But, when deploying the low-cost sensors side-by-side with the Picarro, is there a dependency of the differences on CO_2 concentration (in particular for high concentrations)?

The manuscript is quite optimistic in claiming that the BEACO$_2$N achieved the required "1

ppm" accuracy (as requested by previous studies). First, all the quoted numbers (1.3±0.9ppm (L276), 1.6±0.4 ppm (L313)) are actually greater than 1 ppm. Second, the numbers might not be entirely representative of what the previous studies called the "mismatch error of 1 ppm at an hourly temporal resolution" (Turner et al., https://doi.org/https://doi.org/10.5194/acp-16-13465-2016, 2016). To me, it seems that for the mismatch error on hourly resolution, one would actually need to combine the network error and the instrument error (and the model error which, however, is not accessible here).

Technical comments:

L14: " , ". Too much white space.

L109, equ. 1 and 4,5,6,8: The paragraph describes the **temporal** drift of the sensors i.e. the correction (equ.1) scales with time. I got confused by the super-script "T,drift" for the CO_2 offset since I wondered whether "T" (pointing to temperature) somehow anticipates/includes what follows in section 2.3. I think it might be better to just use the super-script "drift" without a "T".

Equ.2: The equation lacks "x days".

L115: atemporal -> temporal

L119: molybedum -> molybdenum

L160: "within ±1°C of T(h) is calculated" Why would you need this additional constraint?

L169: Is the m_T time-dependent? If so (e.g. in the view of "shifts dramatically" L172), using the median over the entire period might not be a good estimator.

L171: "When it is observed either …." - Check sentence structure.

L170: Could you give a typical number for "dramatic shifts" and how often those occur?

L180: Typo “calculated”.

L193: Units missing for the two numbers at the end of the line.

L204: Sentence does not make sense, check sentence structure.

L240/241: Scale parameter? Is the scale parameter of any relevance?

L265: For the convenience of the reader, define what a semivariance / variogramm is (e.g. write down the equation for calculating gamma_nn).

L275ff: I think this is a quite optimistic interpretation of the semivariance analysis: 1) the error bar on the zero-intercept is large, 2) the found 1.3±0.9 ppm is an estimate derived from a “summer months” dataset i.e. it might underestimate the error for shorter periods. Wouldn’t the error estimate in section 4.2 need to be added? (See also main comment above.)

L293: phenomenon -> phenomena

Equ.9 and 10: I took me a while to understand the rationale here. Consider adding a bit more explanations.

L302: result is -> result in

L315: less that -> less than

Section 5: The Bay area is an area with a sea-breeze delivering pristine air on a regular basis i.e. the median 10%-percentile might work well as a background estimator. Can you say a word on whether these local conditions might be particularly favorable and whether in-land locations might have a harder time using the median background approach?

Fig.10: Isn’t it worrisome that panel b shows a clear time dependence?

Fig.11: The mean of the distributions seems negative. The numbers quoted in the panel titles are positive while only in the upper panel, the title says absolute value. Double-check whether this is all correct.

Fig.12: Why use “fraction” difference, while throughout the manuscript “fractional” difference was used?

Fig.Sx: In some places, the manuscript refers to Fig. Sx (indicating “supplementary” figure, I presume). There are no figures with the “S” label.