Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-530-RC2, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The BErkeley Atmospheric CO_2 Observation Network: initial evaluation" *by* Alexis A. Shusterman et al.

Anonymous Referee #2

Received and published: 2 August 2016

The manuscript describes a novel CO2 monitoring network consisting of low-cost sensors. First results from a measurement period are presented. The manuscript is well written, with mostly well-prepared figures and a clear structure. I recommend publication after the following comments have been addressed.

General comments:

1. The procedure used for bias correction is somewhat unclear the way it is described. What I understand: a CO2 background determined as the weekly minimum at a reference site is subtracted from the CO2 time series at all sites, then each timeseries is deseasonalized, and the weekly minimum of the resulting timeseries is fitted as a piecewise linear function of time to derive the time-dependent part of bias (B_temporal). After removing this time-dependent part of the bias, the mean of the weekly minima at

Printer-friendly version

Discussion paper

each site are taken as the constant bias term and subtracted from the data. Overall it looks like a high-pass filtering of the data (after de-seasonalizing), as slowly varying or constant contributions are subtracted. The question is if any contribution of constant gradients between the different stations (as expected given the differences in near-field emissions) are left after this procedure, or if the assumption really is that each site potentially "sees" background air once per week.

2. Error propagation should be included, propagating errors after pressure correction, temperature correction, water vapour correction, and bias removal (time-varying and constant). In that context it is worth mentioning that the bias error is dominant, not the precision error, when aggregating to yearly signals.

I second the referee #1 comment on the model representation error, which is really crucial as only with a transport model the observations can be quantitatively linked to the fluxes that are of interest.

Specific comments:

P1 L23: The reference "A.B. 32, 2006" should probably read "Brown et al., 2006"

P2 L24: Fig. 3 shows gradients in CO2 fluxes, not in CO2

P3 L10-25: "... sensitivity to changes <10 ppm per year are required" this is quite large compared to the 65 ppb/year. On which metric or specification is the focus (mentioned in line 24)?

P7 L15: The precision of 1-minute averages of the Picarro CRDS systems should be lower than 0.1 ppm, as for a single five second measurement is specified by the manufacturer to be better than 70 ppb (25 ppb for 5 min. averages).

P7 L17: Were the different instrument's time response taken into account in the comparison of the CarboCap and the CRDS? As the CarboCap has a diffusion driven exchange of the sample gas, the instrument response function should be quite different from the more or less instantaneous measurement characteristics of the G2301.

Taking this into account would potentially improve the precision estimate based on the comparison.

Fig. 9 should be modified, as it is impossible to discern the different time series. May be a series of time series plots with 3-5 sites per plot, all sharing the same time axis, but with different vertical (CO2) axis.

P9 L25-28: the fact that the seasonal cycles agree in summer and not in winter seems mostly related to the choice of July as a reference

Caption figure 11: is the “standard error” the error of the mean, or the standard deviation? This should be made clear.

P10 L9: what was used as lateral boundary condition for the regional WRF-STILT model? This is not specified in the Turner et al. (2016) paper focused on network design.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-530, 2016.

Printer-friendly version

Discussion paper