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

Anonymous Referee #1

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This manuscript presents an overview of a city-scale CO2 monitoring network, based on low-cost instruments. The manuscript is concise and well written and presents an interesting experiment. The instrumentation described appears to be well designed and mostly well tested. My primary concerns center on the setup of the network as a whole, the uncertainty quantification and the interpretation of what is or is not possible with a network of this nature.

General comments:

1. In an urban network such as this, the details of how the instruments are situated is likely to be critical. However, only a very cursory explanation is given in the text (P5, L28: 2 to 111 m above ground level…). I find it a little concerning that the instruments appear to be situated either very close to the ground (2m), or on rooftops. In urban

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Discussion paper

areas, flows will be significantly influenced by near-by obstacles within the “roughness sublayer”. It is typically assumed that this layer extends at least 2 building heights into the atmosphere (Roth et al., 2000). This is important for emissions verification, because: a) in order to calculate fluxes from concentration measurements, we need to be able to accurately simulate flows from source to receptor; b) measurements within the roughness sublayer, or worse, within the urban canopy layer, will be representative of only a very small area around them, rather than the wider region. If the BEACON instruments are all within the roughness sublayer, I suspect that the network will not be able to meet its aim of monitoring changes in city-scale fluxes, because changes will be representative of only very localized areas, and the modelling requirements of simulating complex flows around buildings, etc., will be too demanding (see comment regarding Figure 12). The authors need to provide much more detail on how they plan to deal with these issues, and whether the instruments have already been situated to account for these factors.

2. I think that the discussion of uncertainties in the instrumentation could be expanded upon further. In particular, I would like to see further characterization of instrumental drift. More details are given below.

3. A network such as this is an exciting and important development. However, I think the authors should be a little more self-critical about the potential limitations. In particular, claims such as a 2% potential accuracy on emissions estimates seem overly optimistic to me for reasons given below.

Specific comments:

P1 L30: I think it’s a bit of an exaggeration to say that national monitoring networks “give no information” on urban emissions. A network of instruments in rural areas can still see integrated signals from nearby cities.

P3 L17: With the assertion that uncertainties scale with sqrt(N), the authors are making the assumption that each sensor is an independent estimate of the city-wide concentration. Whilst I agree with the sentiment that an increased number of (well sited and modelled) instruments would lead to a decrease in uncertainty, I suspect that the uncertainty reduction on the urban scale will be nowhere near sqrt(N), which must be considered a theoretical limit. In reality, each instrument will see a footprint around it on the order of a few km, superimposed on some signal from the wider region. Even in a somewhat more "box-model"-like limit, the sqrt(N) argument would assume that all sensors "see" the entire integrated signal of the city, whereas in reality, they would only see everything upstream. Therefore, at any one time, only some subset of the network will be seeing anything close to the "whole" city.

Section 1.4 and Section 3.4: I'm not sure if the term "bias" is the most appropriate here. It appears from Rigby et al. (2008) and section 3.4 that in addition to potential offsets (which I would class as a bias), these instruments can also drift on a range of timescales. Perhaps "systematic uncertainties" would be more appropriate? Furthermore, I think that the assertion that the instruments can be considered "unbiased" if any systematic uncertainty is smaller than the precision is a little difficult. In practice, any systematic offsets could be much more critical than the repeatability. Even relatively small values could have a major impact on an inversion. I think that the paper would benefit from a more nuanced approach in which the uncertainties are more fully characterized. In particular, I think a discussion of the potential "uncorrected" instrumental drift should be shown (i.e. the authors present a method for correcting ~weekly drifts. However, to compare the data to a model, one would still need to know what the magnitude of potential sub-weekly drift is likely to be).

P5 L14: Following from the discussion above, I think that this comparison would benefit from a plot of the time-varying difference between the two instruments to assess the magnitude of the drift that one would expect in the field.

P6 L16: The running costs for a CRDS seem very high here. Furthermore, the sentence sounds like pumps, data loggers, etc are "annual" running costs, which seems erroneous to me.

P7 L4: How has the influence of wind speed and boundary layer height been isolated from other factors? Surely boundary layer height will be strongly correlated with e.g. an emissions diurnal cycle?

P7 L31: As I understand it, the correction for weekly drift makes the implicit assumption that all sites see the same minimum CO_2 concentration as the reference sites. Can the authors comment on how robust this assumption is likely to be? I'm particularly concerned that, with a vertical difference of 500masl between the sensors, this procedure could add biases into the network due to persistent vertical gradients within the network in a particular week.

P1, first paragraph: The discussion of uncertainty reduction largely focuses on another paper under review (Turner et al., 2016). However, I suspect that the estimate that the "accuracy" of Oakland emissions could be reduced to less than 2% (or even 18%) is wildly optimistic for the following reasons: a) Synthetic data studies of this nature make heavy assumptions of Gaussian PDFs and unbiased statistics; b) systematic model errors are largely ignored. In reality, my suspicion is that models will have a very tough job of accurately simulating flows at these scales. I do not agree with the assertion that "These combined error budgets are typically dominated by transport (model) error, which potentially explains why models based on BEACO2N-like networks perform comparably to or better than those based on sparser networks of higher quality sensors, for which instrument error may be reduced but accurately representing transport between observation sites is of greater importance." I suspect that, given the resolution of the flows involved, it may be even more difficult for a model to accurately simulate concentrations for dense urban monitoring network at present (such difficulties would be impossible to discern in a synthetic data experiment). Furthermore, uncertainties in inversions such as this are likely to be very non-Gaussian, and I suspect that the uncertainty budget is likely to be dominated by systematic factors in both the observations and the model.

Figure 11: This figure appears to show model simulations at three sites. However, no

details of the model are given in the text. Either the model setup should be explained, or the model runs should be removed from the figure.

Figure 12: To me, this figure of a site in a school suggests that representation issues in the current network could be severe. The authors show that concentrations were substantially lower on a day when the school was closed. The magnitude of the signal (∼50ppm) shows that this sensor must be completely dominated by the school. Therefore, can we not conclude that the sensor sees little of the wider city, and any long-term changes in concentration at this location will be indicative primarily of change in the school's emissions? Certainly, separating a city-wide 65ppb decrease from this signal (1000x smaller) would seem highly challenging.

Additional References

Roth, M. (2000) Review of atmospheric turbulence over cities, Quarterly Journal of the Royal Meteorological Society, 126, 941-990.

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