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Interactive comment on "Network design for quantifying urban CO_2 emissions: Assessing trade-offs between precision and network density" *by* Alexander J. Turner et al.

Anonymous Referee #2

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The focus of this manuscript is to assess the efficacy of a cost-effective measurement strategies in retrieving urban emissions over the San Francisco Bay Area. This is done by characterizing the tradeoff between the measurement network density and the model-data mismatch error that includes uncertainties in the model and measurements. The analysis is based on pseudo observations and inverse technique using a coupled model, WRF-STILT. It is certainly a novel piece of work and is beneficial to other urban measurement network designs and associated studies. However, some parts of the manuscript need improvements or additional details to better understand the results and their interpretations. Also some clarifications are necessary to improve the manuscript (see the comments below). Hence I would recommend this manuscript

for publication after addressing my concerns and comments listed below.

My major concern is about the footprint calculations presented in the manuscript. As far as I understand, what it is shown in Fig. 2 is the averaged footprints for all sites in the network in which the footprints are calculated separately for each sites. In that case, I am surprised with such a low value for the averaged footprints on the western side of the model domain even if there are many sites (especially the line source/high-way is on that side, Fig. 1). Although a part of this can be explained with the prevailed wind direction, I don't find enough reasons to justify the shown structure. i.e., it is difficult to believe that those sites don't give much information on surface fluxes for this period. Please clarify and also give additional details (e.g. set up of STILT receptor locations, how strong is the advection, details of vertical mixing etc.).

Another criticism is that the inverse framework, although it is a critical component of this study, is not well explained (Sect.4). For example, it is not very clear to me how the state vector is defined for this experiment. What is the spatial and temporal resolutions of the posterior fluxes? This is important to follow the inversion results. This section needs major improvement w.r.t giving additional details.

Before getting into the result section, I have been surprised with the last sentence of the abstract. The reported error estimate of the posterior fluxes (5%) is for the best case OSSE and the inversion experiment (rather I would say that it is for "the most idealized case") in which the total model-data mismatch error is assumed to be 0.005 ppm. Since this mismatch error is totally unrealistic in the current scenario, it is not fair to include this "best case" result in the abstract unless the model-data mismatch error (+ other assumptions) is explicitly specified here. Since it is misleading, I would recommend authors to either remove this sentence or provide an error estimate for more reasonable scenario.

Fig. 3 and associated statements: I can't see a remarkable performance of inversion in retrieving posterior fluxes as one would expect here, given that the inversion uses

a loose prior (100 % uncertainty), used all 34 sites, and “unrealistically” low mismatch error (=0.005 ppm which includes model error, representation error, and instrument error). The spatial structure in the CO2 fluxes is captured only for a few parts of the domain. Unfortunately, this says to me that the most of other sites are not much useful in this case, which is hard to believe. This again points back to my concern regarding the footprint calculation. Need to clarify.

Other comments:

L24: Radiative forcing is variable over the years. Please give the value w.r.t year. 1.82 W m-2 looks more like the 2011 year values.

L60: The issue is not only with the spatial resolution, but also with the large uncertainty ranges (reported or expected). This issue needs to be addressed clearly in the manuscript to draw the importance of the high resolution inversion modeling, which is to reduce the uncertainty of the emission fluxes. Also mention about the temporal resolution. This is also important especially when cities have peak traffic, industrial, or commercial hours. Need to be mentioned/addressed in the manuscript.

L92-95: From Fig.1 (bottom panel), I see that the natural sources accounts for about 17% (peak to peak, according to CT2013B) of the total fluxes and are varying as expected. This is considerable in comparison with the Bay area traffic sources which accounts for $\sim$50% of the total fluxes. Hence I would expect that using the natural fluxes at coarse resolution (1âŰę $\times$ 1âŰę) can generate additional uncertainty and may not be appropriate in this high resolution modeling scenario. Please comment on this.

Fig.1: What is “other Anthro” (red line) based on?

Section 4: This section needs further improvements to better explain the inversion technique used in this study. Please modify. Also indicate the dimension of "m" and "n".

Mathematical formulas (e.g. Sect. 4): Please use standard formatting as followed by the most of the authors/textbooks. For e.g. prior fluxes, xb in which “b” is subscript.

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