

## ***Interactive comment on “Atmospheric inversion for cost effective quantification of city CO<sub>2</sub> emissions” by L. Wu et al.***

### **Anonymous Referee #1**

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Overview: Wu et al. present a manuscript assessing the ability of different observing networks (with different costs) to quantify city CO<sub>2</sub> emissions. The paper is appropriately placed in AMT. There is a core of work that appears sound, and would be a useful contribution to the community. However, as currently presented the manuscript is misleading and improperly substantiated. Most importantly, the paper is written as though it assesses different cost systems for quantifying urban CO<sub>2</sub> emissions and derives answers for optimal instrument and network design with wide implications. This is not substantiated. The authors have done a nice pseudo-data experiment which could be the basis of a re-written publication. I therefore would only recommend publication after major revisions. Writing a manuscript focused on a pseudo-data study evaluating different network performances in a specific inversion framework would be interesting

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and useful—this should simply state performance of instrument and model that are needed/assumed (with the model performance equally important to clearly discuss- as unknown biases within are likely the biggest challenge)- and eliminate all the discussion of cost of sensor or network and the discussions and assumptions for MRV.

Major Issues: My major concerns are centered largely on the context and conclusions drawn in the paper. This manifests most notably for a couple topics:

Cheap vs. expensive sensors (much of discussion on page 30706, though found throughout): The authors act as though they are rigorously assessing the use of different cost sensor. However, they simply assert a performance capability of cheap sensors that has yet to be demonstrated or tested. This is the extent of the real comparison—the authors basically assert that cheap sensors can work as well as expensive ones in the context of inversions, and then do inversions where the cheap sensors ‘win’ simply because they are cheaper. This is not any type of real analysis or test. I suggest the authors remove all mentions of cost of sensors and assessment of that. They should instead focus on the pseudo-data study that relies on observations of a certain, assumed quality.

Assessment of cost of inventories and networks (mostly on page 30705): This is a very simplistic and naïve assessment. It really seems to be focused on concluding cheap sensors are better, in particular by asserting that original purchase cost dominates total cost. In reality, we don’t really know what is needed or necessary for urban co2 emissions quantification—this is why a pure pseudo-data study would be useful (and is what I recommend this manuscript be turned into)! Making simple assumptions about cost that ignore practical experiences about measurement location cost and access and calibration/maintenance needs let alone ignoring possible operational personnel costs for mainlining networks and inversion systems renders really makes this analysis portion not relevant and useful.

Assumptions/assertions of what inversion error is useful: This is again a simplistic

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analysis that is not robust or really helpful. I would prefer if the authors focus on the capability of different inversion systems as determined by the pseudo-data study with clearly defined assumptions about error and performance of the modeling system. As constructed, the author's gives strong weight to total annual CO<sub>2</sub> fluxes. These are perhaps not the most useful value from a city network, nor the most robust result from inversions. There is relatively little discussion of the bias error problem in inversions in trying to get accurate net annual fluxes. Actually trends have been though to be easier to detect and help with bias errors- and this is not addressed in here (McKain et al., PNAS).

How exactly fluxes are derived, and the details of the 'gradient' method are not clear. This would be much more valuable and useful to spend time discussing in the revision than all the time on cost and MRV.

Detailed Issues: Abstract, line 8: This is not really what the authors are doing in this paper Line 14: This is an unsubstantiated claim about cheap sensors Line 19: Performing the analysis only in January (when biosphere is weakest), and extrapolating to the whole year is an iffy proposition that relies on large assumptions Line 26: Based on the level of assumptions made, would seem unfair to asset the system can meet the requirementâ€”bias errors are essentially unaddressed. Final sentence of Abstract: This is an assertion that is unsubstantiated in this manuscriptâ€”the authors have really conducted a pseudo-data experiment and not determined that networks of cheap sensors could actually inverted emissions to within 5% uncertainty.

p. 309698 line 27-28. This is not a new type of dataâ€”there is long literature cited in this paper that is using this data.

p. 30699 line 15,15: Should also probably mention at least Salt Lake City which has the longest running urban CO<sub>2</sub> network.

P. 30700 Line 10-11: This really undermines any assessment of cheap versus expensive sensors.

The writing is quite labored and redundant at times, and could really use revision to improve clarity and succinctness. Example of redundancy page 30701, Lines 1-5.

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 30693, 2015.

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