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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric inversion for cost effective quantification of city CO_2 emissions" *by* L. Wu et al.

Anonymous Referee #4

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The paper by Wu et al., entitled "Atmospheric inversion for most effective quantification of city CO2 emissions" seeks to answer the question: how much uncertainty reduction in carbon emissions from cities can urban networks observing atmospheric CO2 yield? This is a very timely topic, as the COP21 meeting is underway in Paris as I write this review. The paper is written well, and the inversion methodology is sound. However, I have one major concern regarding the assumptions underlying cheaper sensors that may render results from the "cheap" network overly optimistic. I would like the authors to address this concern before the paper is published in its final form.

MAJOR COMMENT: The authors appear to be making a lot of assumptions regarding "cheap sensors" that are not substantiated by evidence. In short, I am not aware of cheap sensors that can perform as well as the authors assumed. Can the authors cite

specific peer-reviewed references that illustrate the ability for these sensors to perform as well as assumed?

If there are systematic errors in the cheap sensors there could very well be erroneous emissions that would be solved for by the inversion system. For instance, if the cheap sensors measure systematically higher CO2 mixing ratios over several hours, the inversion would retrieve higher emissions, naturally. This could require such sensors to be calibrated at significantly higher frequency (e.g., hourly), rather than the multi-day frequency assumed by the authors. Would this be feasible?

Note that the comment about "systematic errors should not have long autocorrelation timescales" on Page. 30706 Lines 9∼10 is erroneous. By definition, systematic errors have a non-negligible autocorrelation timescale!

A type of measurement network that the authors have yet to explore is the combination of deploying both high-precision and cheap sensors in the field, which may be a likely way forward in the near-term, while the cheaper sensors are still undergoing improvement. The high precision sensor(s) would help detect gross errors in the cheap sensors, helping to prevent systematic errors in the retrieved fluxes, as mentioned in the aforementioned scenario.

MINOR COMMENTS: 1) Page 30696, Line 12: The "Glaeser and Kahn 2010" reference appears to be missing

2) Page 30701, first paragraph: I found this paragraph difficult to follow, and it took several readings for me to rasp the main ideas. Reword?

3) Sect. 3.4.3 H3: It would help the reader to explain here the scientific reason for why the CO2 gradients are considered. I realize that the reason can be found in the Breon et al. (2015) paper, but it helps the reader with a sentence like what is mentioned later: ""...large spatial coherence of the errors from the model boundary conditions and from the estimate of the fluxes outside the IDF area, whose cancelling is the main aim of the

gradient computation." I suggest this point to be mentioned earlier, in Sect. 3.4.3

5) Page 30718, Line 21: "not correlated in time neither in space" => "not correlated in time or in space"

6) Page 20725, Line 20: a missing key reference on the use of stable carbon isotope measurements to partition anthropogenic vs biogenic sources is Pataki et al. [2003]: Pataki, D. E., D. R. Bowling, and J. R. Ehleringer (2003), Seasonal cycle of carbon dioxide and its isotopic composition in an urban atmosphere: Anthropogenic and biogenic effects, Journal of Geophysical Research, 108(D23), 4735, doi:4710.1029/2003JD003865-004735, doi:003810.001029/002003JD003865.

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