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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.

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Anonymous Referee #1

COMMENT:

Overview: Wu et al. present a manuscript assessing the ability of different observing networks (with different costs) to quantify city CO2 emissions. The paper is appropriately placed in AMT. There is a core of work that appears sounds, and would be a useful contribution to the community.

RESPONSE:

We thank the reviewer for his/her positive assessment of our pseudo-data analysis

and for his/her suggestions that have helped improving the manuscript significantly. As detailed below, the paper now focuses on OSSEs, and accordingly the title will be changed to “What would dense atmospheric observation networks bring to atmospheric inversion for the quantification of city CO2 emissions?”

COMMENT:

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 CO2 emissions and derives answers for optimal instrument and network design with wide implications.

RESPONSE:

In the revised version, we will better separate the pseudo-data analysis on one hand and the discussions on the practical solutions for deploying dense networks on the other hand. The text will mainly focus on the former, and we will follow the reviewer’s suggestion to re-organize the latter into discussions on the performance requirements for the instruments and inverse modeling framework.

COMMENT:

This is not substantiated.

RESPONSE:

Our study compares networks with few current research-grade instruments to potential future networks with higher density. In this context, it is critical to discuss the development of low cost medium precision (LCMP) sensors (we will replace the term “cheap”). Although LCMP sensors with the precision, systematic error and cost assumed in the initial manuscript are not commercially available yet, present testing with different versions of prototypes within our laboratory (by co-authors of this paper) provide encouraging results for the repeatability and reproducibility of CO2 measurements, when external influences are properly corrected for. As the test will be continued to verify the

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performance for a least one year, the full study on the sensors shall be published in 2016. Still, we have attached a figure illustrating preliminary work for the reviewers. It indicates that LCMP sensors could yield measurements uncertainties of about 1 ppm for hourly values if regularly calibrated (every few days or weekly). This motivates us to assume that we could use LCMP sensors in the near term for atmospheric inversion.

COMMENT:

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 and useful. This should simply state performance of instrument and model that are needed/assumed

RESPONSE:

The text will be deeply reorganized in order to avoid misunderstanding about the interpretation of our results. The new text will primarily focus on the pseudo data experiments and on checking the need for dense networks if targeting precise sectorial estimates of the emissions. Based on the experiments, it will formulate requirements on the instrument performances. However, discussions on the expectations regarding the next generations of sensors and on the cost of the networks will still follow this analysis since we think that the above mentioned knowledge on current low-cost sensors justifies them.

COMMENT:

(with the model performance equally important to clearly discuss - as unknown biases within are likely the biggest challenge) –

RESPONSE:

The model performance in this study follows Bréon et al. (2015) who assimilated real measurements. Therefore, this performance should be seen as a requirement per se

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which represents the present skill of mesoscale transport models. Still, we will better acknowledge the problems raised by potential model biases in the result section.

COMMENT:

and eliminate all the discussion of cost of sensor or network and the discussions and assumptions for MRV.

RESPONSE:

Please see the answer above regarding the cost of the sensors.

The discussion on the cost of future infrastructures for large networks of LCMP sensors will be simplified and turn into a series of requirements. The discussion on the cost of the inventories will also be simplified and kept as an indicator for the cost requirements on networks and sensors.

All of this is needed to evaluate the relevance of testing 30 to 70 sensors networks in this study but will be moved in annex and briefly summarized in the discussion section of the paper.

Last, we prefer to keep a section of the introduction on MRV systems in order to provide a useful background for city scale atmospheric inversion of fossil fuel CO2 emissions, since this activity is strongly connected to such systems.

COMMENT:

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

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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.

RESPONSE:

Our reply to the general comment underlines that we will follow the reviewer's suggestion to better focus on the pseudo-data study and that we will move mentions to the cost of sensors into the appendix and in few sentences of the discussion section. The abstract, introduction and conclusions will be revised accordingly.

COMMENT:

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.

RESPONSE:

We had and will further discuss the cost of infrastructure.

COMMENT:

In reality, we don't really know what is needed or necessary for urban co2 emissions quantification.

RESPONSE:

Even though this is an emerging activity, experiments with real data such as that of Bréon et al. (2015) already give us robust insights on the requirements for urban CO2 emission quantification.

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COMMENT:

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.

RESPONSE:

Through the deployment and maintenance of the CO2 Paris network since 2010, we have acquired some solid experience about the requirements and issues for setting-up such urban networks. Also note that the co-authors of this paper gather modelers and experimentalists. The new paper will clarify our discussions on the network requirements and move them to the appendix and in few sentences of the discussion section.

COMMENT:

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

The new manuscript will revert the logic flow for the analysis of the different scores of uncertainties in the inverted emissions. While the analysis of the result will ignore any uncertainty target, the discussion section will evaluate them (not putting too much weight on such an evaluation) by using different assumptions on the impact of monthly mean uncertainties for the derivation of annual budgets or trends. By such, the new manuscript will avoid giving the impression of building its analysis on strong assump-

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tions. COMMENT:

As constructed, the author's gives strong weight to total annual CO2 fluxes. These are perhaps not the most useful value from a city network, nor the most robust result from inversions. RESPONSE: This paper follows the current emphasis of bottom-up inventories and MRV systems on annual estimates (as will be better discussed in the introduction). Annual estimates provide both the baselines and the trends, which are both required for the analysis of the impact of climate plans. Note that such an application has been the main justification for deploying CO2 urban networks. COMMENT:

There is relatively little discussion of the bias error problem in inversions in trying to get accurate net annual fluxes. RESPONSE: Remember that our inversion configuration follows Bréon et al. 2015 who did not detect major model biases when analyzing misfits between simulated afternoon CO2 gradients and real afternoon CO2 gradients measurements. In particular, data are assimilated in the afternoon only to avoid well known model biases on the vertical transport during nighttime. This supports our assumption that the model is not strongly biased. Regarding potential biases in the measurements: the paper will better explains how calibration strategies should allow preventing large biases in the measurements for periods longer than several days to a week. As a consequence, the measurement "biases" are rather seen as error correlations with a timescale of about several days to a week. We showed sensitivity tests with higher observation (i.e. model + measurement) errors to account for long temporal scales of correlations (which is a better characterization of what could be considered as "model or measurement biases"). These tests will be analyzed and discussed in the main text. COMMENT:

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). RESPONSE: We already addressed the matter of detecting trends in section 2.1 of the first version of the manuscript, and this it will be expanded. Further, model errors are barely "biased" in a mathematical sense. They have tendencies to under-estimate or over-estimate

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some processes strongly influenced by the large to local scale meteorological conditions and which thus hardly summarize into absolute or relative errors that could be similar for month to month or from year to year or even constant over short time scales. Therefore, it seems difficult to assume that trends could be far more easily monitored than the annual emissions.

COMMENT:

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.

RESPONSE:

We will improve the presentation of the gradient inversion method.

COMMENT:

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

RESPONSE:

The abstract will be strongly revised in line with the general corrections to the manuscript discussed above.

COMMENT:

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

RESPONSE:

The connection between typical uncertainties at the monthly scale and at the annual scale using a very wide range of assumptions on the month to month correlations

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will be better discussed and used for a simple evaluation of the results in a specific paragraph of the discussion section. Our assumption that the scores of uncertainty in winter can be extrapolated to summer month is based on a sensitivity test where the uncertainty in the biogenic fluxes was artificially increased to typical levels for summer months. So we think that this assumption is quite robust. This is merely due to using the gradient approach, which levels down the sensitivity of the inversion to the natural fluxes. This will be better discussed.

COMMENT:

Line 26: Based on the level of assumptions made, would seem unfair to asset the system can meet the requirement on bias errors are essentially unaddressed. Final sentence of Abstract: This is an assertion that is unsubstantiated in this manuscript.

RESPONSE:

Again, the abstract will be strongly revised in line with the general corrections to the manuscript discussed above. Regarding the biases, see our answers to the general comments above.

COMMENT:

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.

RESPONSE:

The uncertainty target aimed at helping to analyze the results. It will not be defined a priori anymore, and it will be ignored in the result section. As mentioned above, we will just give highlights on the levels of posterior uncertainties in a paragraph of the discussion section based on the extrapolation of the uncertainties at the monthly scale into a wide range of typical uncertainties at the annual scale. The assumptions underlying this extrapolation will be better presented.

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COMMENT:

p. 30698 line 27-28. This is not a new type of data. There is long literature cited in this paper that is using this data.

RESPONSE:

We meant that the city scale inversion is a recent activity and a source of new types of data for the community of the anthropogenic emissions inventories. Still, we will remove “new type of data” in the new manuscript for clarity.

COMMENT:

p. 30699 line 15,15: Should also probably mention at least Salt Lake City which has the longest running urban CO2 network.

RESPONSE:

We agree with the reviewer. We will add information on the Salt Lake city network.

COMMENT:

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

RESPONSE:

See our general answer to the major comments of the reviewer. The large modifications of the text to better focus on the pseudo data experiments will remove this type of sentence.

COMMENT:

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.

RESPONSE:

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Thank you for your suggestions. We will check the redundancy and will improve the succinctness and quality of the text.

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