

Interactive comment on “Atmospheric inversion for cost effective quantification of city CO₂ emissions” by L. Wu et al.

L. Wu et al.

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Received and published: 4 March 2016

Anonymous Referee #3

COMMENT:

Overall comments. The paper is an interesting observational system simulation experiment (OSSE) exploring observational network design for estimates of urban emissions of CO₂ using atmospheric inversions.

RESPONSE:

We thank the reviewer for his/her comments that helped improving this paper significantly and for his positive assessment of our OSSEs.

Full Screen / Esc

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Discussion Paper



COMMENT:

As written, however, the paper has many serious problems. It is limited by a number of severe assumptions embedded in the inversion system, and by the lack of discussion of the vertical resolution of the atmospheric transport model.

RESPONSE:

The assumptions underlying the configuration of the transport model and of the atmospheric inversion are based on the experiments with real data by Bréon et al. 2015.

COMMENT:

The overall conclusions regarding “cheap” vs. “expensive” sensors are invalid,

RESPONSE:

As detailed below in answer to specific comments, this discussion was based on current results regarding the performances of low cost medium precision (LCMP) sensors. And this discussion will be shortened, clarified and moved to the discussion section.

COMMENT:

and the alleged ability to deploy sensors at 25m AGL is misleading.

RESPONSE:

25 magl correspond to the typical height of recent buildings in the Paris region. We have already installed measurement sites at the top of such buildings.

COMMENT:

The target uncertainties quoted are entirely dependent on the assumed prior uncertainties,

RESPONSE:

No, there were derived independently. However, we will not define uncertainty target in

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15, C12999–C13038,
2016

[Interactive
Comment](#)

[Full Screen / Esc](#)

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[Interactive Discussion](#)

[Discussion Paper](#)



the introduction anymore.

COMMENT:

and these uncertainty assumptions are often unjustified and untested.

RESPONSE:

They are consistent with those used in Bréon et al. 2015. We also conducted sensitivity experiments where observation (model + measurement) uncertainties are inflated to check the robustness of the results. The results of sensitivity tests were discussed in the supplementary material but they will now be shown in the main text.

COMMENT:

The discussion of the cost of measurement networks is unrealistic in the extreme and should be deleted.

RESPONSE:

This discussion will be improved, shortened, put into annex and summarized into a series of requirements for the deployment of 30 to 70 sensor networks in the discussion section.

COMMENT:

The manuscript contains a core of worthwhile research – an assessment of the sensitivity to a highly idealized inversion system to the network of instruments deployed - but the paper claims to be much more than this. This manuscript requires serious revision before it should be considered for publication. More detail on these points follows.

RESPONSE:

In the new manuscript, we will follow this suggestion of the reviewer to focus on the study of the OSSEs until the discussion section. Accordingly the title will be changed to “What would dense atmospheric observation networks bring to atmospheric inversion

Full Screen / Esc

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for the quantification of city CO₂ emissions?”

COMMENT:

1. The argument concerning “cheap” vs. “expensive” sensors is misleading, lacks content, and must be deleted.

RESPONSE:

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 performance for a least one year, the full study on existing LCMP 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 state that transport errors are larger than instrument errors, thus instrumental error doesn't matter, and thus “cheap” sensors are just as good as more expensive CO₂ sensors. The authors, however, present no quantitative assessment of any real “cheap” sensor or atmospheric transport errors.

RESPONSE:

The above mentioned level of error of LCMP instruments is insignificant compared to that of the atmospheric transport model diagnosed by Bréon et al. (2015) which are used and detailed in this study. We will improve the presentation of the observation (model + measurement) error configuration.

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15, C12999–C13038,
2016

[Interactive
Comment](#)

[Full Screen / Esc](#)

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[Interactive Discussion](#)

[Discussion Paper](#)



COMMENT:

The abstract makes it sound like low-cost sensors exist and have been tested and have been shown to perform well. This is wrong. Unjustified assumptions have been made so that sensor performance is irrelevant. This isn't science; it is wishful thinking. Wishful thinking should not be published in ACP.

RESPONSE:

As indicated above, we based our discussion on the cost and precision of the next generation of sensors on actual lab testing (we will clarify it in the new draft), and our assumptions on the accuracy of the model on previous studies using real data.

However, following the reviews of our manuscript, we will discuss requirements and expectations on the cost and precision of LCMP instruments in annex and briefly in the discussion section, while most of the text will focus on the OSSEs. The abstract will be revised accordingly.

COMMENT:

2. The results concerning performance of the inversion system as a function of the number of sensors is defensible within the limits of the many assumptions made by the inverse system, including the uncertainties assumed within the inversion.

RESPONSE:

Again, they have been derived from the diagnostics of Bréon et al. 2015. However, we will better highlight the assumptions underlying the OSSEs.

COMMENT:

These assumptions, however, are buried deep in the document and in sections that are often very difficult to read.

RESPONSE:

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The presentation of the inversion technique and configuration will be improved.

COMMENT:

The assumptions include prior flux errors, atmospheric transport errors, and assumed coherence in the prior flux errors. Very large coherence is assumed in the prior flux errors. The final error levels are highly dependent on these largely unjustified assumptions.

RESPONSE:

We control budgets of emissions over large areas, but this does not mean that we assume that uncertainties in the distribution of the emissions at high resolution are coherent over such areas. It means that the fine spatial scales of the uncertainty in the emissions should not much affect the type of observation that we select for assimilation (downwind-upwind gradients between distant 25magl sites for elevated wind speeds). Still, the diagnostic of observation errors by Bréon et al. 2015 accounts for the impact of uncertainties in the distribution of the emissions at high resolution (i.e. the so-called “aggregation errors”, see the answer to the detailed comment by the reviewer on this topic).

COMMENT:

As the authors state, deep in the discussion section, these results “should not be over-interpreted.” But the abstract says nothing about the numerous assumptions that limit the validity of these results, and states that 5% flux uncertainty can be achieved with 70 sensors: : with no caveats given whatsoever about the large volume of assumptions that condition this finding. It is even difficult to determine from the abstract that this study is an OSSE. 2.1) These limiting assumptions should be presented prominently and clearly in the methods section. 2.2) These important caveats about the significance of the study results should be made clear in the abstract. The abstract is very misleading and should not be published in its current form.

Full Screen / Esc

Printer-friendly Version

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Discussion Paper



RESPONSE:

The abstract and the method section will be highly modified to better highlight our assumptions, and to be consistent with the strong reorganization of the manuscript.

COMMENT:

3. The method of relating CO₂ mixing ratio differences to fluxes is not clear, and is of critical importance to the paper. The method of choosing upwind and downwind sites is described, but how these are related to emissions of CO₂ is not described.

RESPONSE:

The presentation of the method, as well as that of the inversion configuration, will be strongly improved. The atmospheric transport model makes the link between the emissions and the selected concentration gradients as traditionally done in atmospheric inverse modeling, with the difference that instead of sampling concentrations at individual sites from the output of the transport model, here, we sample the differences of concentrations between these sites from the outputs of the model, when the wind criteria for the gradient selection are verified.

COMMENT:

4. The paper is based on a pseudo-measurement network that, as noted in the abstract, collects data at 25 m above ground. The study is based, however, on an atmospheric model that has very coarse resolution – 15 km in the horizontal

RESPONSE:

The horizontal spatial resolution of our atmospheric transport model CHIMERE for Paris metropolitan area is 2 km, with meteorological forcing by ECMWF products at a 15 km resolution.

COMMENT:

Full Screen / Esc

Printer-friendly Version

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Discussion Paper



and, as best I can determine, about 250 m in the vertical.

RESPONSE:

For efficiency, in general, meteorological, transport, or ocean models do not have regular grid on the vertical in geometric height. They have a much refined grid near the ocean/land-atmosphere boundary. In particular, here, the thickness of the first vertical levels of CHIMERE is about 25 m, and there are ~ 7 vertical levels in the PBL during the afternoon. Most of the 25 magl stations are located in the second vertical level of the model.

COMMENT:

The model has no demonstrated capacity to represent the complexity of an urban surface either in the vertical or in the horizontal.

RESPONSE:

Bréon et al. 2015 have used this CHIMERE(2km)–ECMWF(15km) model to simulate real measurements of concentrations at less than 25magl in the Paris area. Their model showed similar capability to fit the measurements than the 2km resolution meteorological and transport simulations by Lac et al 2013 who used a specific scheme to account for the urban heat island in the urban area. The CHIMERE(2km)–ECMWF(15km) model capacity to simulate the measurements is quantified by the estimates of the model errors by Bréon et al. 2015 which are used in our study (i.e. accounted for in the configuration of the observation errors in our inversion system).

COMMENT:

An OSSE is limited by the quality of the modeling system applied to the system design. The authors have no basis for claiming that their results are valid for an observational network deployed at 25 m above ground with a model that has no demonstrated capacity to resolve the details of atmospheric transport in the environment of interest. If the authors must 4.1) discuss the vertical resolution of the model; 4.2) describe how the

Full Screen / Esc

Printer-friendly Version

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Discussion Paper



model simulates the atmospheric surface layer; 4.3) explain the true limits on observational altitude in their study given 3.1 and 3.2; and 4.4) at a minimum note how the complexity of the urban surface in the horizontal, unresolved by their modeling system, could complicate the meteorology in ways that cannot be captured by their modeling system.

RESPONSE:

See the answers above. We will better discuss the difficulties related to the modeling or CO₂ transport in the Paris area, as is reflected by the high estimates of model errors for the inversion configuration. We will better emphasize that our modeling configuration is derived from that of Bréon et al. (2015) and better present it. And we will better highlight the fact that the difficulties related to the modeling of CO₂ transport over urban areas explain why we assimilate data under the condition of high wind speed during the afternoon only, when the vertical mixing is high. By this way, the complex impact of local sources and transport should be decreased.

In particular, we will better discuss our assumptions (in the method section) and corresponding requirements (in the discussion section) regarding the capability to model measurements in the core of the Paris urban area. The networks tested in our OSSEs include a significant number of sites in this core. However, Bréon et al. 2015 diagnosed very high model errors for such measurements and thus avoided assimilating them in their inversions (see a more detailed discussion on this topic in answer to a specific comment).

Finally, the sensitivity tests where the observation (model + measurement) errors are inflated will be presented in the main text rather than in the supplementary material to better discuss the weight of model errors.

COMMENT:

5. The manuscript needs further editing. It is full of detail that is hard to follow and at

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



times extraneous to the central message of the paper. The figures are out of order, and often the figure quality is marginal. The writing quality is poor and must be improved before this manuscript is suitable for publication.

RESPONSE:

We will improve the quality and concision of the text and the quality of the figures.

COMMENT:

6. The economic justification for “cheap sensors” contains a great deal of unjustified wishful thinking.

RESPONSE:

See our answers to the comment 1.

COMMENT:

For example, Appendix B states that, “The cost of calibration is estimated to be of the same order for high precision and cheap sensors. The calibration for cheap sensors can be more frequent (e.g. two days) than for high precision sensors (e.g. one week), but needs less samples of calibration gas. In addition, innovative calibration procedures for cheap sensors are possible for further reductions of the calibration cost and the temporal correlation in instrument bias. For instance, a calibration center can be set up using high precision sensors to calibrate cheap sensors. One can manage two sets of cheap sensors: one in the calibration center and the other in situ in measuring. The calibration is simply performed by replacing the measuring sensors with recently calibrated ones from the calibration center. Since this new calibration method is free of calibration gas, and since the cost of replacing sensors is very limited, one can maintain a high frequency of calibration (e.g. daily). Note that the network cost can, furthermore, be reduced when pre-existing infrastructure is available, for instance the installation could be free of cost if sharing with existing air quality monitoring platforms.” The authors are thus proposing that a 70-instrument network sprawling across a large

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



metropolitan region would have all of the instruments replaced every 1-2 days. The cost, however, is cited to be “very limited,” and Table B1 shows no added cost for personnel for replacing 70 instruments every day. I would expect that such a schedule for instrument replacement alone would take 2-3 full time personnel. Further, Table B1 assumes that 70 free platforms with suitable characteristics for monitoring greenhouse gas emissions are available! This discussion is 1) unrealistic in the extreme and 2) unsuitable for publication. This unrealistic and misleading attempt at evaluating the economics of observational systems must be deleted from this document. It has no scientific value that I can discern.

RESPONSE:

We will improve and simplify the discussions on the network cost and turn it into series of requirements. They will be put into annex and summarized in the discussion section. The aim of this cost analysis is to provide insights on whether the deployment of 30 to 70 sensor networks could be envisaged in the near future. We think that it is critical to develop this discussion after having analyzed results of OSSEs with such networks.

COMMENT:

Detailed comments.

1. Page 2, Lines 13-15. What sensors are “currently developed?” As best I can tell, no actual sensors are evaluated. This text is extremely misleading and must be modified to represent the actual content of the paper, which assumed instruments with no bias and insignificant random error exist.

RESPONSE:

We will clarify the fact that new prototypes of LCMP sensors are presently tested at LSCE and yield promising results regarding the requirements that the manuscript will derive from the OSSEs. We will strongly revise the abstract (see the answers to the main comments by the reviewer).

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

COMMENT:

2. Page 2, Lines 13-15 What defines expensive?

RESPONSE:

The last sentences of the new abstract will mention that the deployment of dense networks will make it necessary to decrease the price of the sensors whose typical cost is presently 50K euros.

COMMENT:

What defines a “megacity?” Is this different than the cities that emit 44% of global CO2 emissions?

RESPONSE:

The world “megacity” will be deleted from the abstract. The 44% direct emissions correspond to all urban areas. The megacities are usually referred to urban areas whose total population exceeds 10 million people.

COMMENT:

3. Page 2, Line 17. “25 m above ground level.” Why are the imagined sensors located at 25m above ground?

RESPONSE:

We have chosen 25 magl since this is the common height for high public or private buildings in the Paris area, and since much of them are convenient to install measurement sites on their roof. Building and managing dedicated tower sites for measuring CO2 at higher heights would be impossible (too expensive) if considering 30 to 70 site networks. We also selected this height since Bréon et al. 2015 assimilated measurements from stations at less than 25magl (the modeling skills generally increase with the height).

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



COMMENT:

Can this altitude be treated realistically in the inversion system? This is a strong recommendation that is not justified by the content of the paper. This must be carefully justified by showing that the model can simulate measurements collected at 25 m AGL or deleted.

RESPONSE:

As said above, Bréon et al. (2015) assimilated data from stations at less than 25magl. Local sources and transport may impact the concentration measurements at 25 magl in a way that is difficult to characterize using a mesoscale atmospheric transport model. This is why we select data under conditions of high wind speed for the afternoon only when the vertical mixing is high. Of note is that most of the site locations investigated in the study are outside the dense parts of the city, and they are all located without a precise definition of their specific location within the 2km x 2km grid cells of CHIMERE. This means that specific studies can be led to select locations less prone to a complex local situation (using mobile campaigns of measurements and local scale transport modeling), which has been recently done at LSCE when setting up new sites around Paris.

Still, assimilating 25magl measurement in the core of the urban area (which corresponds to a considerable number of sites in this study) is challenging and had not been attempted by Bréon et al. (2015) even though they derived typical estimates of the model error for such measurements (that have been used to set-up our inversion system). This requires being able to filter the local scale signal from the measurements at such locations (there are definitely some ideas for such filtering). These considerations will be better discussed in the manuscript.

COMMENT:

4. Page 3, lines 5-12. These statements are not justified or quantified, thus not useful.

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



1) Certainly additional measurements such as CO might improve an urban inversion, but this is not a new result. This paper adds nothing to the body of literature on this topic. Without new results, this should be deleted.

RESPONSE:

The statements about CO were deleted in the abstract.

COMMENT:

2) The statement that “cheap” sensors can improve urban emissions estimates says nothing about the quality or characteristics of the so-called “cheap” sensors. Sensor performance should be quantified, or this text should be deleted. This is wishful thinking, not a conclusion from any research performed in this manuscript.

RESPONSE:

The whole abstract has been rewritten in line with the general reorganization of the paper that is explained in answer to the previous comments of the reviewer.

COMMENT:

5. Page 3, line 26 – page 4 line 3. This is a run-on and confusing sentence.

RESPONSE:

We will rewrite this sentence.

COMMENT:

6. Page 4, line 4. What is the “city mitigation potential?”

RESPONSE:

We will clarify it.

COMMENT:

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

7. Page 4, Line 6. English needs work.

RESPONSE:

We will rewrite this paragraph to improve the language and will avoid such a use of the terminology from climate economics.

COMMENT:

8. Page 4, line 29. economics.

RESPONSE:

We will keep “climate economy” to be consistent with existing publications (please check some titles in the references of this paper).

COMMENT:

9. Page 6, line 10. “required qualities”? What are “qualities?” Please be more precise.

RESPONSE:

We will use different terms.

COMMENT:

10. Page 6, line 14. Involves.

RESPONSE:

The text will be corrected.

COMMENT:

11. Page 6, line 27. The use of continuous CO2 measurements to monitor urban emissions is far from a new idea. Please do not claim that this is a “new type of data.”

RESPONSE:

Full Screen / Esc

Printer-friendly Version

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Discussion Paper



We will remove “new type of data”.

COMMENT:

12. Page 8, lines 3-7. The authors do not employ an economic model to determine the costs of MRV vs. atmospheric inversions. They simply take the costs of these systems today, and make many unrealistic assumptions about these costs. Costs are not fixed, and today’s costs should not be used to plan tomorrow’s monitoring systems. Further evaluation of this text (Appendix B) reveals many other problems, noted above in point 6 of the overall comments.

RESPONSE:

The present costs of inventories are still indicative about the typical order of magnitude of the costs in the near term. However we will improve and shorten this analysis to provide very simple notions regarding the typical requirements on the cost of the sensors and networks in appendix, and, briefly, in the discussion section.

Please see also our responses above to main comment 6.

COMMENT:

13. Page 8, line 10. “are currently developed.” If they are currently developed, please provide some citations that describe the performance characteristics of these sensors. Some imagined sensor characteristics are described in Appendix A, but without any evidence of the realism of these claims.

RESPONSE:

As detailed in our responses to the main point 1, currently prototypes for such LCMP sensors are tested at LSCE and we illustrate first results that are promising regarding the potential for having instruments with such specifications in the near term. This work has been funded by the climate KIC innovation projects, such as MIRI-ADE and SMEVOUCHER (<http://www.climate-kic.org/projects/miriade/>). Unfortunately,

Full Screen / Esc

Printer-friendly Version

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Discussion Paper



these studies have not been published yet due to non-disclosure agreements, but publications are planned for 2016.

COMMENT:

14. The introduction has a long discussion of greenhouse gas emissions targets and issues, but presents little insight into the performance of existing urban inversions.

RESPONSE:

The discussion on the uncertainty targets will be removed (as discussed above). We feel that the discussions on MRV systems provide useful insights on the context for the monitoring of city CO₂ emissions. Therefore we would like to keep it and we will try to strengthen and clarify it.

COMMENT:

15. Page 9, lines 17-24. This text needs considerable editing. It is very difficult to understand.

RESPONSE:

We will improve this paragraph.

COMMENT:

16. Page 9, lines 23-24. What are “city inventories that would not have access to the same level of information as national inventories.”?

RESPONSE:

This will be removed due to the reorganization of the manuscript.

It was connected to the part of the introduction stating that “Admittedly, inventories of city emissions are known to suffer from incomplete and uncertain data (see Appendix A for a brief review of city inventories). For instance, there is usually a lack of precise statistics regarding the total amount of fossil fuel that has been consumed within the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



cities.”

COMMENT:

17. This entire paragraph on “notional targets” should be simplified and clarified.

RESPONSE:

We will not define such notional targets a priori anymore. Instead, we will just give highlights on the levels of posterior uncertainties from the different OSSEs in a specific 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.

COMMENT:

18. Page 10, line 25 – page 11, line 2. I believe these are hypotheses, not statements of fact. Please clarify. If they are facts, please include appropriate citations.

RESPONSE:

These are not statements of facts, neither hypothesis. They are deduction based on physical basis (sectoral emissions are driven by different dominant factors) and mathematical reasoning (split of budgets for different sectors implies negative correlations). We will modify the text accordingly and we will better discuss the assumptions underlying this derivation.

COMMENT:

19. Page 11, lines 2-3. I don’t understand this sentence.

RESPONSE:

We will rewrite this sentence.

COMMENT:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



20. Page 11, line 8. reducing the reduction?

RESPONSE:

We will correct the text. We wanted to write “reducing the emission”.

COMMENT:

21. Page11, line 4. What is the purpose of this paragraph?

RESPONSE:

We wanted to connect the uncertainties in the monthly to annual budgets of the emissions to that in the trend monitoring, since trends are key indicators for climate plans. This analysis will be moved into the discussion section, expanded and improved.

COMMENT:

22. Page 11, line 14. This paragraph is very difficult to follow and requires significant editing. Please explain the methods and assumptions clearly.

RESPONSE:

We will rewrite this paragraph to better explain the methods and assumptions.

COMMENT:

23. Page 12, line 11. I don't understand the purpose or content of this paragraph.

RESPONSE:

The aim of this paragraph was to provide a way of extrapolating (with a wide range of uncertainty) results of uncertainties at the monthly scale to the annual scale since the inversion are applied to a 1-month period only while the annual scale is more relevant politically and correspond to that of most of the inventories.

COMMENT:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



24. Section 2.2. Notional costs. This section of the paper has serious problems. There is little information that serves as the basis for the cost of conducting an urban emissions inventory of a given accuracy. There are questionable assumptions about the cost of an atmospheric inversion (e.g. cost of sensors is the primary cost). The assumption about “cheap” sensor accuracy and precision makes the distinction among sensors meaningless, but there is no actual evaluation of any sensors. There are no assessments of actual transport errors. Assumptions about costs made in Appendix B appear to be extremely unrealistic. The claim that this study examines the benefits of low cost, poor performance vs. high cost, high performance sensors is false and should be eliminated from the paper. The assumptions about the costs of inventory vs. inversion are also highly questionable and should also be deleted. RESPONSE:

As already mentioned in answer to the major comments 1 and 2 by the reviewer, we now focus the paper on OSSEs. These discussions on the costs are greatly shortened, improved, moved in appendix with clarified assumptions, and briefly summarized in the discussion section. See our response to main point 6 for the aim of this cost analysis.

COMMENT:

25. Section 3.1. This introduction to the mathematics needs to utilize terminology that is specific to an urban atmospheric inversion. A “background” estimate of what, for example? Observations of what? The theory is not new. The application must be clear.

RESPONSE:

The atmospheric inverse modeling community has hardly managed to use common mathematical terminology and we cannot say that, today, urban atmospheric inversion has already developed into a widespread activity involving a large community. However, we will try to clarify this section and modify our terminology.

COMMENT:

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

26. Page 15, line 8. “control a vector x?” What does that mean?

RESPONSE:

The control vector is the set of variables that are controlled by the inversion, and we will try to improve its presentation. There is no misuse of the term control here. The atmospheric inverse modeling community generally erroneously call it the state vector even though it is not the state vector of a dynamical system (because of adopting the vocabulary of meteorological data assimilation for which the control vector is the state vector of the meteorological dynamical system, while, in atmospheric inversion, this is the input vector of the considered dynamical system i.e. the transport model).

COMMENT:

27. Section 3.2 The terminology in the section “control variables” should be replaced with physically meaningful terms.

RESPONSE:

The inversion controls scaling factors to be applied to emission budgets over various time periods, areas and set of sectors of activity. We will abusively replace control variables by emissions wherever the text does not need to be rigorous (which we have done) but some parts of the text will have to stick to the mathematical reality.

COMMENT:

28. Page 16, line 14-15. I do not believe that computational constraints are a primary limit on the resolution of the inversion. Either modify this discussion or provide a citation that demonstrates this claim.

RESPONSE:

The inversion relies on the full computation of the matrix corresponding to the linear observation operator. This full computation requires in principle as many transport simulation over 1 month as the number of control variables. The inversion also requires the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



inversions of matrices whose size is the number of control variables, which is another source of computing limitations. This will be better explained in the new manuscript.

COMMENT:

29. Page 16, line 27. “rest” is an unfortunate choice, since it has another meaning. “Remainder” would be better.

RESPONSE:

We will follow your suggestion and change “rest” to “remainder”.

COMMENT:

30. Page 17, line 1. Again, why are computational constraints invoked? What computational constraints? It is entirely possible to resolve an urban region at high resolution given current computing resources. This is not a real limit on urban inversion systems. The true reasoning for this coarse spatial resolution should be explained.

RESPONSE:

Without spending too much time on discussing each type of inversion systems, we just remind that some of them are easily limited, due to computational constraints, in term of the size of the observation vector (e.g. analytical systems which base the computation of the matrix corresponding to the observation operator on computations of the sensitivity of each assimilated data to the fluxes), others in term of the size of the control vector (e.g. analytical systems which base the computation of the matrix corresponding to the observation operator on computations of the impact of each controlled flux into the atmospheric concentration). At first glance, variational system could appear not to be limited for the size of the control vector nor for that of the observation vector, but actually, it is limited in terms of number of minimization iteration, and the larger the control space is, the more iterations should be needed to converge. And computing uncertainty covariance matrices from variational systems requires a large number of OSSEs which is extremely difficult due to computational limitations. Finally,

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Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



all types of system need to address the inversion of covariance matrices whose size is a function of the number of control variables. Addressing the computation of HTR-1H can be another source of limitation regarding the size of the problem.

Here, we use the type of inversion that is the most adapted to the test of a very large number of different observation networks with a large number of station locations. See above for the description of its computational limitations.

COMMENT:

The atmospheric transport resolution applied for this study is exceptionally coarse.

RESPONSE:

Again the horizontal resolution of CHIMERE simulations for the Paris area is 2 km. There are 19 vertical layers up to 500 hPa with a 25m resolution close to the surface. The original ECMWF 15 km meteorological data were interpolated to the CHIMERE grid. See our answer to main comment 4 on this topic.

COMMENT:

31. Figure 2. The regional colors and regional boundaries are not clear. There are lines on the map that do not correspond to the colors. What are the regional boundaries?

RESPONSE:

The colors marked out five zones of (2 km x 2 km) grid cells actually used for the definition of the control vector in the inversions. These five zones are meant to evenly split the Ile-de-France region, and do not necessarily follow the boundaries of the different administrative borders in this region. We will improve this figure.

COMMENT:

32. Page 17, line 18. Nordbo et al (2012) reported no flux tower measurements that were carbon neutral. Every observational data point in their paper reported a net an-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

nual carbon source to the atmosphere. The paper cannot be used to justify that urban areas are carbon neutral. (Nordbo et al (2012) also referred to Minnesota as a city, and used 500 m resolution data to derive urban fraction for flux tower sites.)

RESPONSE:

Nordbo et al (2012) performed regressions between green-area fraction and urban emissions and estimated that a city with 80% green-area fraction would be carbon-neutral. We will rewrite this sentence for clarity.

COMMENT:

33. Page 18, lines 4-5. Please define afternoon and high wind speeds. The details are important.

RESPONSE:

They will be detailed in Sect. 2.4.3 in the revision. We will add a pointer (“see Sect. 2.4.3 for details”) to the text.

COMMENT:

34. Page 18, line 3. How are upwind and downwind sites defined?

RESPONSE:

This will also be detailed in Sect. 2.4.3 in the revision. The section 2 will be improved to better connect the rest of the text.

COMMENT:

35. Figure 3 caption. “uniform” not unifrom.

RESPONSE:

It will be corrected.

COMMENT:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



36. Page 19, line 3. What is the purpose of random selections of networks? It isn't likely that networks will be determined via a random process.

RESPONSE:

Indeed, the idea of comparing random samples is to investigate whether one can improve the results by conducting network design studies with OSSEs. Presently, the design of the network is more generally driven by practical issues regarding the infrastructure (agreements with potential hosts of the site, ability to fix inlets at height, and the rest of the infrastructure at a given site). Both considerations should be balanced equally in the design of the network. Here, the samples are driven by some practical consideration so they are not totally random. Still, in some cases we see significant sensitivity to the sampling and thus the asset of conducting network design studies. We now better discuss this in the text.

COMMENT:

37. Page 19, line 6. Figure 5 is referenced before Figure 4.

RESPONSE:

We rearranged the order of figures.

COMMENT:

38. Page 19, lines 13-15. How does sampling at 25m above ground, “avoid dominant influence of local emissions on concentration observations?” I don't know of any published work that shows that 25m is high enough above the surface to avoid being dominated by local emissions. I don't know how “avoiding dominant influence” or “local emissions” are defined. See the 4th main comment above. This is an unjustified and highly misleading claim that should not be published.

RESPONSE:

See our answers to the general comment 4 and minor comment 3 on the same topic.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Again, Bréon et al. successfully assimilated real measurements from peri-urban sites at less than 25magl by selecting data under specific wind conditions and during the afternoon only, and we derive our estimate of the model transport errors at such sites from their diagnostics. We rely on assumptions regarding the ability to exploit 25magl measurements in the core of the city (by filtering the local scale signal from urban measurements), which will be termed as requirements in the new version of the manuscript.

Furthermore, the 25 magl height corresponds to the installation of the sensors on pre-existing infrastructures such as the roof of public/private buildings.

We will improve the text to better discuss these points.

COMMENT:

39. Page 19, line 18. I cannot understand what the authors are trying to say about H1. Please rewrite in clear language.

RESPONSE:

We will clarify this section.

COMMENT:

40. Page 20, line 7. Is a 15km resolution ecosystem flux model appropriate for an urban scale study? This seems exceptionally coarse.

RESPONSE:

Paris is an intensive urbanized area. As the computation and gradient assimilation technique focus on the urban area where ecosystem plays small role, the coarse resolution of the vegetation flux simulation should not have significant impact on our inversion results. Indeed, we use the same vegetation flux simulation as in Bréon et al. 2015 where real data are assimilated. Furthermore, the spatial resolution of the NEE should not have a high impact on the assessment from OSSEs. The role of having high resolution product is to increase the fit with the “actual” fluxes but the gain from

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

this improvement for inversion would be small, as can be seen by our sensitivity studies about NEE.

COMMENT:

41. Page 20, line 28. Figure 4? The figures are out of order.

RESPONSE:

We will pay attention to the order of figures.

COMMENT:

42. Page 21, line 16-18. What is the area covered at 2km x 2km resolution? There are 2km x 10km grid boxes? Why?

RESPONSE:

As we used the same modeling framework as Breon et al. (2015), we referred to it for details about the CHIMERE model grid (see Fig. 1 in Breon et al. (2015)). The 2 km x 2 km area covers Ile-de-France, and the 2 km x 10 km grid boxes are defined for areas outside of Ile-de-France. We used coarser resolution for these outside areas (our objective is Paris urban emissions) to save computational time. A figure showing the model grid will now be provided in supplementary material.

COMMENT:

43. Page 21, H2: The transport model is only a 15km resolution model with 19 levels up to 500 hPa? This is very coarse resolution. Each vertical level, if evenly spaced, is approximately 250m. It is not unusual for coarse resolution models to have very unrealistic surface layer behavior when they are applied to CO₂ simulations, and a 15 km horizontal resolution model cannot take into account realistic structures in the urban surface energy balance and changes in urban roughness. How can this model be used to evaluate the suitability of measurements 25 m AGL over the highly complex urban surface? What is the profile of CO₂ close to the surface? The lack of description

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



of the fidelity of this model for this task is a major weakness of the document. The OSSE is only as good as the model used for the OSSE. No relevant model evaluation is presented in this document.

RESPONSE:

Again, we use the same modeling framework as Breon et al. (2015) where model evaluation is presented. Please see our responses to specific comments 3, 30 and 38 and to general comment 4 for details on model resolutions and on our assumptions regarding the ability to fit with 25 magl measurements. We will better emphasize that the configuration of our inversion system is strongly connected to that of Bréon et al. (2015).

COMMENT:

44. Page 21, lines 25-27. I don't understand "depending on the simulation: : :)" Sometimes you have initial and boundary conditions, and sometimes you don't? Please clarify. How are boundary and initial conditions optional? What determines whether or not you include CO2 boundary and initial conditions?

RESPONSE:

When willing to compare the model to the data, we need to account for boundary and initial conditions. However, for OSSEs, we do not need to account for CO2 boundary initial and conditions and the transport model (i.e. they are not included in the control vector) if assuming that uncertainties in such conditions can be accounted for in the observation error R (as done here). The mathematical framework of the inversion explains it. We will try to make it clearer through improving the explanation of the mathematical framework and by improving the part of the text pointed out by the reviewer.

COMMENT:

45. Page 23, line 1. What does "read from the ECMWF meteorological product" mean?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



RESPONSE:

We will rephrase it.

COMMENT:

46. Page 23, line 11. Why 22.5 degrees? Is there any justification for this value? Plume dispersion widths will vary with wind speed, wind shear and turbulent intensity. What is the origin of this fixed value? What limitations does this fixed value place on the results of this study?

RESPONSE:

22.5° is clearly resulting from a trade-off between the need to select enough data so that the observational constraint is strong and not too much hampered by model and measurement errors, and the need for ensuring that we do not depart too much from the objective of assimilating “downwind-upwind” gradients. This value has definitely some arbitrary nature. But we will publish soon results from a 1-year inversion experiment using real data demonstrating that this yield very good results unlike wider wind ranges for the selection of gradients.

Having an even more complex strategy defining the wind range as a function of other meteorological conditions could be more adapted, but this is a first step in this direction and for the OSSEs here, it would not have been relevant.

It is now better discussed.

COMMENT:

47. Page 23, line 13. Is that 7-16% of observations once the afternoon hours have been selected? Or is that 7-16% of the total number of possible observations?

RESPONSE:

It is for the total number of possible observations. We will clarify it.

ACPD

15, C12999–C13038,
2016

[Interactive
Comment](#)

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



COMMENT:

48. Figure 6. The wind rose graphics are too small to read. RESPONSE:

We will improve Figure 6.

COMMENT:

49. Page 23, line 21. This paragraph is incomprehensible.

RESPONSE:

We will rewrite the paragraph to clearly present how we assess the contribution of individual flux components to CO₂ concentrations.

COMMENT:

50. Figure 7. Why are any differences that are not in the afternoon hours displayed? They are irrelevant to this OSSE.

RESPONSE:

We will remove plots of data that are not in the afternoon hours.

COMMENT:

51. Page 24, lines 5-9. A modeling system with 250m vertical resolution will have difficulty representing mole fraction differences at 25m above ground at any time of day. Figure 7 displays time series and differences that might be seriously influenced by the ability or inability of this modeling system to represent vertical mixing very close to strong sources and sinks at the earth's surface. Evaluation of the near-surface vertical profiles created in the model is essential to ensure that these results are not simply artifacts of unrealistic surface layer mixing. The quantitative horizontal gradients (the focus of the following paragraph) are very dependent on this vertical mixing.

RESPONSE:

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Please see our response to specific comments 3, 30 and 38 and general comment 4 on the vertical resolution of the model. We generally have something like 7 model vertical levels within the PBL during the afternoon.

COMMENT:

52. Page 25, line 16, delete “can”. “Even though a few cities: : :” Why is this relevant?

RESPONSE:

We deleted “can”. We just wanted to indicate that the setup of the prior uncertainties may have to be higher for other cities for which the quality of the prior knowledge (of the available bottom up inventories) is not as good. Our posterior uncertainties in the inverted emissions could thus be viewed as being optimistic for the “average city case”. This is now rewritten.

COMMENT:

53. Page 26, line 2. I do not see how Figure 9a illustrates the point being made in the text. This needs significant work. What happened to figure 8? I see that Figure 9 is a correlation matrix, but no dimensions are described. As presented, this is nearly incomprehensible. It is very good that the authors are trying to explain these critical assumptions, but the presentation is not sufficient to understand the assumptions.

RESPONSE:

We will improve the text related to Figure 9. The dimension in Fig. 9 is 834, same as the dimension of the control vector. The corresponding scaling factors are grouped by sectors. Prior sectoral estimate errors were assumed to be independent, as can be read in Fig. 9a as zero correlations.

COMMENT:

54. Section 3.5.2 states that the R matrix is assumed to be diagonal, but then notes that the errors are reduced for intersite differences because of the large coherence

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

in space in errors between stations. This is inconsistent. What is the impact of this inconsistency on the validity of the results?

RESPONSE:

The R matrix applies to “downwind-upwind” gradients. Assuming that R is diagonal means that there is no temporal autocorrelation for such gradients between two sites, or that there is correlation of the errors in the direction orthogonal to the wind. When we claimed that there is a large coherence in space of the errors, we meant that this error was highly correlated when following an air parcel, i.e. along the wind direction. We still acknowledge that some source of model error could be correlated between different gradients corresponding to close locations. However, formulating the spatial correlation in model error is a very difficult issue. Its detailed investigation would be beyond the scope of this paper. We followed Breon et al. (2015) to set a diagonal R. We will clarify and better discuss this.

COMMENT:

55. Page 27, lines 8-10. The assumed transport errors are huge, and are a critical set of assumptions in your study.

RESPONSE:

This comment that the assumed transport errors are huge contradicts the previous concerns raised by earlier comments 30 and 38, the corresponding general comment regarding the ability of the model to fit the observation. Those numbers arise from diagnostics by Bréon et al (2015). They account for the difficulty to model the measurements in the Paris area with our inverse modeling configuration.

COMMENT:

The lack of spatial and temporal correlation is also a significant assumption.

RESPONSE:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Bréon et al. 2015 did not account for such spatial and temporal correlations. See also our answer to the previous comment on the spatial correlations of the observation errors.

However, the sensitivity tests that we presented in the supplementary material and that will be presented in the main text check the impact of inflating R , which can be viewed as a test of sensitivity of temporal correlations in R . Indeed increasing the standard deviation of the observation errors instead of modeling their autocorrelations is a common technique in atmospheric inversion, e.g. see Chevallier, GRL, Impact of correlated observation errors on inverted CO₂ surface fluxes from OCO measurements, 2007. Indeed, when analyzing the fluxes at the monthly scale, it is critical to know what is the resulting observation error for data averaged at the weekly to daily scale. Whether a given uncertainty on these averages arises from a high STD of the observation error at the hourly scale but low temporal correlations or a lower STD but significant temporal correlations should not play a critical role for monthly mean results. This will be discussed in the section dedicated to the sensitivity tests.

COMMENT:

I find it very surprising that the sensitivity of your results to these assumptions is insignificant (line 10). I do not have supplementary figure S1. The results of the paper should depend heavily on these assumed errors. A statement that says the dependence is insignificant with no results presented to justify this statement is not defensible.

RESPONSE:

If assuming a 0D inversion problem (with one control variable and one observation) where the observation operator is an identity matrix, the posterior variance will be written $BR/(B+R)$. Depending on the relative weight between B and R , we can see that the result can be weakly impacted by large changes to R (if the weight of B is far larger). The problem is even more complex when accounting for the transport. It is

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

not surprising to find such a low sensitivity to R at the monthly scale, which is a scale at which the projection of the uncertainty in the prior emission into the concentration space is very high. This will be discussed in the section dedicated to the sensitivity tests.

COMMENT:

Results. 56. How is the mixing ratio difference between two sites attributed to a flux correction? This is not clear. This is fundamental and must be defined.

RESPONSE:

The link between flux and gradients is based on the sampling of the gradients from the output of the atmospheric transport model forced by the fluxes, and recovering flux from measured gradients relies on the inversion approach. In this study, we focus on the diagnostic of the uncertainty reduction enabled by the inversion (since the inversion is a statistical approach), and for this we do not need to derive, in practice, flux corrections. This is now better explained in the text.

COMMENT:

57. Page 27, lines 19-22. Airlines, powerplants and nighttime emissions from all sectors will have essentially no observational constraints from the methods proposed, save for extreme assumptions about the coherence of the errors. While there is some reason to believe that corrections to daytime emissions from roads or buildings might have some coherence with nocturnal emissions from roads and buildings, there is no reason to believe that airport emissions will be detected by two sites that are located in a region that contains an airport, but which do not encompass the emissions from the airport. It is fundamentally incorrect to say that the proposed network would reduce uncertainty in total emissions.

RESPONSE:

We have defined specific control variables for the airports and the power plants. There-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

fore, if the system diagnoses uncertainty reduction for these sources, it is because their signature is detectable in the set of observation that is assimilated. It does not rely on extreme assumptions regarding the correlations between uncertainties in airport or power plant emissions and in other sectors since we ignore such correlations in our inverse modeling set up.

Regarding daytime and nighttime emissions, we control them separately. As mentioned by the reviewer, there is some reason to believe that corrections to daytime emissions from roads or buildings might have some coherence with nocturnal emissions from roads and buildings.

Finally, if we reduce strongly the emissions for some specific sectors, mathematically, we reduce uncertainties in the total emissions. Reducing the uncertainty in total emissions does not require reducing uncertainties for all sectors of emissions.

COMMENT:

58. Page 28, line 4. Define “gain.” Or do you mean uncertainty reduction?

RESPONSE:

Yes, it is uncertainty reduction in percentage compared to prior uncertainty. We changed “gain” to “gain in uncertainty reduction”.

COMMENT:

59. Figure 8. The total uncertainty in your inversion approaches an asymptote as the total number of sites increases. Why? What is the limiting factor in your inversion system?

RESPONSE:

We examined in details this saturation effect in the fourth paragraph of Sect. 3 in the revised draft using DFS (see that paragraph for more details on DFS) which quantifies the efficiency of the observations assimilated. As the number of sites increases, it was

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

shown that the information from observations becomes more redundant, in a manner specific to the network type.

This is a combination of lack of sensitivity to nighttime fluxes, to emissions when the wind conditions do not correspond to the gradient selection criteria, and to sectors dominated by local sources for which assimilating gradients between sites that are distant by less than 5km would be required.

We will add a discussion on this in the text.

COMMENT:

60. Page 29, line 8. $DFS/d < 10\%$? When you divide DFS by d, you get a number less than 10%? Please clarify. Do you mean DFS gained per measurement pair added is less than 10%?

RESPONSE:

We mean the DFS gained per measurement (concentration gradient) is less than 10%. Each measurement has a maximal DFS of 1, and a minimal value of 0. The maximal DFS value for the inversion system equals to the number of measurements (d), hence DFS/d accounts for the percentage of observations (related to the signal but not the noise) that are effectively assimilated by inversions.

We will clarify it in the text.

COMMENT:

61. Page 29, line 10. English, “the Iowa state of USA?”

RESPONSE:

We will change to “Iowa State of USA”.

COMMENT:

62. Page 29, lines 11-12. The authors state, “Such small amounts result from the
C13034

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

diffuse nature of atmospheric transport and from the uncertainty in atmospheric modeling.” The authors, however, have utilized only crude assumptions about atmospheric transport modeling. Their assumptions are not the truth about atmospheric transport errors. This statement appears to be unjustified.

RESPONSE:

Again, see our previous answers to your comments atmospheric transport errors. This specific sentence will be improved.

COMMENT:

63. Page 29, line 13. “the rate of effectively assimilated gradients decreases.” What does this mean? This does not make sense.

RESPONSE:

It is related to the saturation effect discussed in answer to comments 59 and 60. We will improve this sentence for clarity.

COMMENT:

64. Page 29. Lines 12-17. I cannot understand this sentence. Is this a comparison to the network studied in the Wu et al (2011) paper?

RESPONSE:

These lines explain how the redundancy (quantified by DFS) of the information from observations for inversions evolves with denser networks. This sentence will be improved for clarity.

COMMENT:

65. Page 29, line 28. “this corresponds to a level 1 quality.” What does this mean? What is a “level 1 quality?”

RESPONSE:

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

It used to be explained in section 2.1 and it is not recalled in the first paragraph of Sect. 3. Now we do not put too much weight on such uncertainty targets as in the previous version of the manuscript.

COMMENT:

66. Page 30, line 3. The entire discussion of network design vs. uncertainty reduction is entirely dependent upon the assumed nature of coherence in the flux errors. Huge spatial coherence is assumed (entire regions have a single correction factor for a single sector of emissions). This assumption is severe and is likely to dominate any results regarding optimal spatial network design. The results, however, do not note any dependence on the assumed uncertainties in the prior flux errors. The results also reportedly show insignificant dependence on very large assumed atmospheric transport errors, but this lack of sensitivity is not shown. Page 32, line 24, admits these limitations, but this is buried into the recesses of the paper. It is dishonest not to present these limitations prominently in the abstract. As noted by the authors, “The results obtained in this study should not be over-interpreted.” That sentence belongs in the abstract, and it needs to be explained in the abstract.

RESPONSE:

Regarding the coherence of the error, controlling large regions with a single correction factor does not mean assuming that errors at higher resolution are entirely correlated. This means that uncertainties in the emissions at higher resolution must be accounted for in the computation of the model errors (we call it the aggregation errors, see Kaminiski et al. (2001)). The diagnostics of model error by Bréon et al. (2015) include a diagnostic of the aggregation errors since in their framework they apply emission correction factors for the full Ile-de-France region. Here, the control resolution is higher and smaller aggregation errors could be expected.

The list of networks to be tested and the control areas have been built consistently to avoid artefacts from the aggregation. With our configuration, even though a single

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



correction factor for a large area is used, having as much sites as possible around the most prominent sources of the area will give a better control on the average budget. And, indeed, in the end, as would have been expected with a high resolution inverse modeling system, the “best” networks definitely correspond to that who provide a strong constraint on most the largest sources within the areas. So the results do not seem to be biased by the coarse scale of the control vector.

The total prior uncertainty is fixed to 20% in this study and follows the setup of Bréon et al. (2015). This number is based on the expert judgement regarding uncertainties in regional inventories such as that of AIRPARIF for the Ile de France area as discussed in Appendix A.

Still, some approximations in our setup of the sectorial uncertainties can impact the results of the computation of uncertainty reduction. It could raise concerns regarding the analysis of the absolute values of uncertainty reduction for a given network. However, the comparative analysis of the uncertainty reductions when using different networks but the same inversion setup (i.e. the network design analysis) should bring more robust conclusions.

Regarding the lack of sensitivity to the observation errors, see our previous answers to comments on atmospheric transport error.

We will better discuss all these points and better focus on the network design (OSSE) component of the study (and the abstract will be rewritten).

COMMENT:

67. Page 33, line 3. From this point on, the text has no specific connection to the results of this study. This text is extraneous and should be deleted.

RESPONSE:

We agree that these texts are not directly related to the results. These are about the perspectives on how to further improve inversion performances for practical atmo-

spheric inversion of citywide CO₂ emissions. They are considerably shortened in the revision.

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

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15, C12999–C13038,
2016

Interactive
Comment

Full Screen / Esc

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Discussion Paper

C13038

