Reply to comments by Jocelyn Turnbull

Review of A first year-long estimate of the Paris region fossil fuel CO_2 emissions based on atmospheric inversion (Staufer et al)

This paper uses atmospheric CO_2 observations in a Bayesian inversion to evaluate urban CO_2 emissions for Paris. It builds on earlier work by Breon et al (2015) that first developed the inversion framework, using the innovative concept of inverting for differences between two observing sites (rather than absolute CO_2 mole fractions). This work expands the dataset to a full year, which allows meaningful evaluation of how well the framework works. The inversion framework already shows some useful outcomes that can inform/improve the bottom-up inventory priors. That is, the observations imply a stronger seasonal cycle in emissions than is represented in the bottom-up data products, and this stringer seasonal cycle is also consistent with expectations. They also discuss the challenges and limitations of their inversion framework. A major challenge is that the inversion result is still strongly dependent by the prior (bottom-up) emission estimate. They show that there is still much work to be done to provide detailed information from this type of regional inversion and they provide useful suggestions as to how the inversion could be improved.

This is a very nice paper and an excellent contribution to the (still very small) urban greenhouse gas literature. It is entirely appropriate for publication in ACP with some minor revisions as noted below.

We thank Dr. J. Turnbull for this positive assessment of our study, for her comments, and for having highlighted our objective of discussing the challenges, limitations and potential improvement of the current framework as well as the first successes obtained with it. We consider that it is a critical aspect of our analysis (see answers to reviewer 2).

General comments: There is very little attention paid to the contribution of the biosphere to the urban CO_2 observations and it’s contribution to uncertainties in the inversion. The biosphere fluxes used as priors are described only very briefly, but there is no information about the quality of that prior and how much biases and uncertainties in it might contribute to biases/uncertainties in the inversion. Some discussion of this is needed in the paper.

We agree that the topic of the natural fluxes is critical for city scale inversions and we agree that we need to discuss it in this paper. However:

- Paris is a particular case in the sense that it is a very dense urban area with limited vegetation inside it.
- According to the model simulations, the computation of gradients succeeds in cancelling the impact of biogenic fluxes in the data that are analyzed (it was too rapidly mentioned in the last section of the paper).
- The weak impact of biogenic fluxes on the inversion of anthropogenic CO_2 (according to the modelling framework) has already been addressed in more detail in Bréon et al. (2015), with which we try to avoid redundancies. In addition, the specific topic of the

cancelling of the biogenic component in the simulated CO_2 gradients has also been analyzed in the Boon et al. 2015, paper.

- The new computation of the gradients proposed in this study further decreases the potential impact of the biogenic fluxes in the inversion.

Therefore, we expand the introduction by discussing the cancelling of the natural component in the CO_2 gradients according to the model. We will indicate in the result section that the amplitude of the natural component of the simulated CO_2 gradients is further decreased when narrowing the wind ranges for the gradient selection. Furthermore, we will expand the discussion on this topic based on such an analysis and carefully remind that actual gradients in the measurements may bear a larger signature of natural fluxes than the simulated gradients. See also the answer to the specific comment on C-TESSEL.

Reference:

Boon, A., Broquet, G., Clifford, D. J., Chevallier, F., Butterfield, D. M., Pison, I., Ramonet, M., Paris, J.-D., and Ciais, P.: Analysis of the potential of near-ground measurements of CO_2 and CH_4 in London, UK, for the monitoring of city-scale emissions using an atmospheric transport model, Atmos. Chem. Phys., 16, 6735-6756, doi:10.5194/acp-16-6735-2016, 2016.

Specific comments: Pg 2 line 14. “two-month” not “two-months”

It will be corrected.

Pg 2 line 26. Presumably air parcels pass over the city, rather than through it?

Yes. It will be corrected in the revised manuscript.

Pg 3. Lines 24-29. This paragraph is hard to follow. It transitions abruptly from a description of preliminary tests to describing what is in specific sections of the paper.

We will add 1 or 2 sentences explaining that this revision consists in narrowing the wind range for the gradient selection to avoid situations during which the air leaving the “upwind site” or reaching the “downwind site” does not overpass a significant part of the city and the vicinity of the other site.

Pg 3 section 2. It would be helpful if the inversion parameters were referred to by what they are, rather than by the algebra term in the equations. For the reader who is not a specialist in Bayesian inversions, it is hard to remember what y yo, x etc are referring to.

We will add the name of the variable before such labels throughout section 2 wherever it does not alter the reading of the text.

Pg 5 lines 1-4. How good is C-TESSEL for the urban area? See also my general comment above.

This model is definitely not perfect for modeling ecosystems within and in the vicinity of urban areas. It has a relatively low resolution compared to that of the transport model. This should, in principle, increase the challenge of dealing with the impact of uncertainties in the natural fluxes since significant NEE is simulated on grid cells with high emissions at the edges of the urban areas. However, the fact that the signature on the simulated gradients from such fluxes is low demonstrates that these drawbacks from the C-TESSEL product do not have critical impacts on the inversion. In the revised manuscript, this will be discussed based on references to Bréon et al. 2015 and Boon et al. 2015 when we present the C-TESSEL product.

Pg 5 lines 15-18. What is the measurement quality for the 1 hour means that are used in the inversion?

Based on the regular analysis of target gases, and intercomparison of side by side measurements (at Gif sur Yvette) the accuracy of the hourly averages at the three sites is better then 0.4 ppm which makes the measurement errors for the hourly averages negligible compared to the signals and other sources of errors discussed in this study.

Pg 6 lines 3-4. There are other studies that show that ATMs do poorly at night. It would be better to reference some work other from outside your own research group.

We will reference to Geels et al., ACP, 2007 in the revised manuscript.

References:

Geels, C., Gloor, M., Ciais, P., Bousquet, P., Peylin, P., Vermeulen, A. T., Dargaville, R., Aalto, T., Brandt, J., Christensen, J. H., Frohn, L. M., Haszpra, L., Karstens, U., Rödenbeck, C., Ramonet, M., Carboni, G., and Santaguida, R.: Comparing atmospheric transport models for future regional inversions over Europe – Part 1: mapping the atmospheric CO2 signals, Atmos. Chem. Phys., 7, 3461-3479, doi:10.5194/acp-7-3461-2007, 2007.

Pg 6 lines 30-32. The justification for discarding the GON-MON gradients needs to be given. Is it that the sites are too close together so that emissions are not large enough to create consistent enhancements? Or is there a major flaw in the methodology?

The explanation was given at Pg 6 lines 11-12: we definitely consider that the sites are too close from each other so that in the GON-MON gradients, we face uncertainties in the high resolution emission mapping rather than uncertainties in the city-cale budget of the emissions. Furthermore, the Roissy Charles de Gaulle airport and its very specific type of emissions (with very specific problems for simulating their injection heights) is located between the two sites. The text at this place will be slightly expanded in the revised manuscript.

Pg 6 line 26. “BR2015” should be “Breon et al 2015” I think.

Yes, it will be corrected.

Pg 7 plines 10-14. One of the gradients had far more impact than the rest, so was excluded? This seems important - what is the justification for excluding it? How different was the inversion when this gradient was included?

We provide a detailed answer to this technical topic below. However, as explained at the end of this answer, it has a negligible impact on the study.

We based our judgment on an objective computation of the “observation impact” which consists in indicating how much a given data, or more precisely a model-data misfit, drives the increment applied to the fluxes given a set of data assimilated (it is given by the product between the gain matrix K and y0_i - H_i x_b where y_i is the corresponding observation). Through the least square minimization of the misfits to the data, it can happen that one data point out of tens of data points is the dominating driver of the inversion results. It happens, for instance, if the corresponding model-data misfit is far larger than the other ones, or if at the corresponding time, the atmospheric transport is such that the sensitivity of this data point to the fluxes is far larger than that of the other data points (typically if the PBL is very shallow at the corresponding time). The large model-data misfit can reveal a very high observation error and giving too much weight to a single data is dangerous (we prefer to work with situations where there is a more balanced fit to most of the data). For similar reasons, inverse modelers generally filter out data corresponding to model data misfits that are more than several times the standard deviation of the whole set of model-data misfits they use (e.g., Chevallier et al 2010). Looking at extreme observation impacts can be viewed as an alternative way to detect situations where we could give an excessive weight to large observation errors.

We made such an analysis when running a preliminary *ini* inversion. We actually removed two data points in November (instead of 1 data point in November and 1 data point in December as erroneously said in the manuscript). The data points removed had both an impact of nearly -0.3MtCO2 on the emission budget for November (i.e. ~ -0.6MtCO2 in total). The impact of other data during this month were not more negative than -0.1MtCO2 (such a situation was extreme as demonstrated by the fact that we removed 2 data points only). In both cases, this was driven by high prior model-data misfits in combination with relatively low vertical mixing conditions in November.

Opposed to what the manuscript could have let think, these data correspond to wind directions for which the gradients are not selected in the reference configurations of the observation vector. Therefore, the impact of this removal applies to the *ini* experiment only, and it is quite negligible for our study.

We will expand a bit the text to discuss this but given the weak impact it has for the results, we will keep it short.

Reference:

Chevallier, F., P. Ciais, T. J. Conway, T. Aalto, B. E. Anderson, P. Bousquet, E. G. Brunke, L. Ciattaglia, Y. Esaki, M. Fröhlich, A.J. Gomez, A.J. Gomez-Pelaez, L. Haszpra, P. Krummel, R. Langenfelds, M. Leuenberger, T. Machida, F. Maignan, H. Matsueda, J. A. Morguí, H. Mukai, T. Nakazawa, P. Peylin, M. Ramonet, L. Rivier, Y. Sawa, M. Schmidt, P. Steele, S. A. Vay, A. T. Vermeulen, S. Wofsy, D. Worthy, 2010: CO2 surface fluxes at grid point scale estimated from a global 21-year reanalysis of atmospheric measurements. J. Geophys. Res., 115, D21307,-doi:10.1029/2010JD01388

Pg7 lines 20-31. There are a number of minor typos in this section.

We will correct them**.**

Pg 7 llines 20-31. Ylag experiment. How does the ylag experiment account for the evolving boundary layer during the day?

The boundary layer (BL) does not evolve only in time, but also in space. The concern about a varying BL for the gradients was raised in a more general way during the review of the study of Bréon et al., 2015 (see the discussion with reviewer 1 http://www.atmos-chem-phys.net/15/1707/2015/acp-15-1707-2015-discussion.html). Because of the variability of the boundary layer, the gradients by themselves cannot be a perfect representation of the enrichment of CO_2 in the air when crossing the city, even though we expect it to be a good proxy for it. But, in any case, the assimilation of gradients better characterizes it than the assimilation of individual CO_2 measurements for any wind direction, especially since the signature of remote fluxes on the atmospheric CO_2 concentrations is expected to be well mixed and does not to evolve much with BL variations over short durations and distances.

Above all, our atmospheric transport model simulates this variability of the boundary layer and therefore the inversion accounts for it when assimilating the gradients. The uncertainty in the modelling of the boundary layer is part of the model errors. Such a model error is a traditional source of error in inverse modelling systems, which we do not overcome by assimilating gradients. The role of setting-up the R matrix in the inverse modelling system is to account for such errors.

This topic will now be discussed earlier when we recall the concept of gradients in the introduction but we will mainly refer to the similar discussions in Bréon et al. 2015.

Pg 8 lines 16-21. This paragraph should come before rather than after the preceding one.

We will reverse the order of the paragraphs.

Pg 8 lines 29-31. Are these the emissions that are in the model domain but outside Ile-de-France? Not clear.

It is a combination of the influence of these emissions, and that of the model boundary and initial conditions (any component of the model that is not controlled by the inversion). The beginning of the sentence at line 31 and the phrase “emissions outside Île de France” instead of “emissions outside Île-de-France but within the modeling domain” are indeed misleading.

We will clarify these sentences in the revised manuscript.

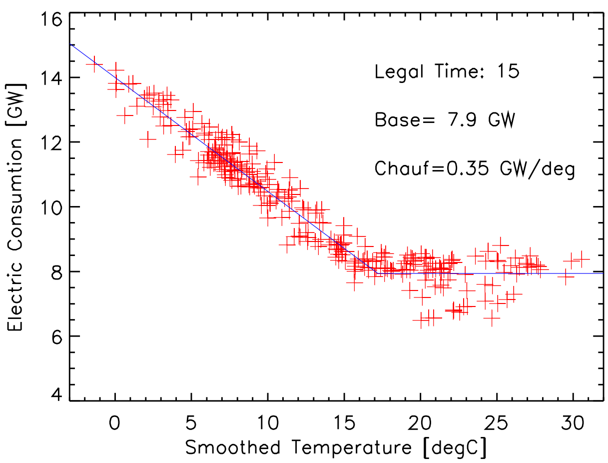
Pg 10 line 1. (first sentence). Is the conclusion you state from your work or elsewhere? If the latter, please reference.

It comes from our own work. We will clarify it in the revised manuscript.

Pg 11 lines 17-19. Please reference the independent analysis that shows the temperature dependence.

We will clarify the fact that this analysis has been conducted by one of the co-authors of this paper (François-Marie Bréon) so that we cannot cite it as a personal communication.

To complement our answer, here are some details about the analysis he produced and which has not been published. The figure below shows the variations of the electric consumption in the Île-de-France region as a function of the temperature in 2013 at 15:00. Similar figures for the other hours of the day demonstrate a similar threshold of the temperature at ~19° below which the consumption is highly (negatively) correlated to the temperature (with similar slopes for each hour). The electricity consumed in Île-de-France is mainly generated by nuclear power plants. Given their clear dependence on atmospheric temperature, these variations, however, very likely reveal variations in heating behavior. And, a large part of heating in Île-de-France is based on gas consumption, which is responsible for a large part of the CO_2 emissions in the Paris area. Assuming that users of gas and electric heaters display similar heating behavior, this result supports the assumption of the temperature dependence described in the manuscript.



Pg 11 lines 27-32. This paragraph seems spurious.

We will shorten it. We will remind how the uncertainties shown in the figures are derived and will not warn about the fact the uncertainties will not be analyzed in detail (this will be explained in the discussion only). A sentence will be added on the uncertainties arising from the inversions at the end of section 3.2.

Pg 12 lines 22-24. Can a problem with a bias in one observation site during the month of December be ruled out as a cause of this December anomaly?

We do not think so since both types of gradients GIF-MON and GIF-GON drive the inversion in the same way (see the next sentence at l.25). We thus cannot connect it to a bias at MON or GON. And a local bias at a given site would impact SW gradients too and would probably impact the site during other months.

Pg 13 “r2” not “R2” in several places.

We will apply this correction.

Pg 16 line 22. You have not demonstrated that GON-MON gradients are “not evidently related to the whole city emissions”. See also my earlier comment.

We think the small distance between these two sites and the fact that emissions between two sites are significantly impacted by the Roissy Charles-de-Gaulles airport is sufficient to assume that the gradients between these two sites cannot be representative of the city-scale emissions.

We will remind in this sentence that the improvement is assessed through the analysis of the inverted emissions.

Pg 16 lines 24-31. nd pg 17 lines 18-20. Have you tried inverting with the AIRPARIF2010 prior? Does it pull the posterior values down even further? Or not? If every inversion pulls the values lower, does this imply a fundamental flaw in the inversion?

We did not have access to the spatialized inventory corresponding to the AIRPARIF (2013) report on the emissions for 2010 (in which we extracted the so-called AIRPARIF2010 annual data) for this study. Still, we have conducted tests with prior values lower than that of AIRPARIF2008 in the FLAT_3.0 experiments and it is one of the topic of the paragraph on p17 line 18 to p18 line 2.

In the case of FLAT_3.0H, the inversion increases the emissions, so that our system does not systematically pulls the values lower. Our argument regarding the decrease of the annual emissions in the FLAT_3.0M experiment is that this decrease, in practice, does not arise from a systematic decrease of the monthly emissions, and that it is quite small (so that we have a strong convergence from the ensemble of prior values to the ensemble of posterior values obtained in the set of experiments).

In the paragraph p17 line18 to p18 line2 we will more clearly separate the discussions on a.) the potential impact of using relative prior uncertainties in the convergence of the results, and b.) the decrease of the annual emissions when using the day-to-day variations from AIRPARIF2008.

Pg 17 lines 1-2. “a large fraction of the Paris emissions are due to domestic and commercial heating”. Add “believed to be” or something similar.

We will change the parenthesis (43%) into (43% according to the AIRPARIF2008 inventory) in this sentence.

Pg 17 lines 29-31. This is a very awkward sentence.

We will reformulate it.

Pg 18 lines 15-18. Be clear here that your inversion solves only for mid-pm observations, and your analysis does not exclude that the poor representation of the diurnal cycle could have a strong impact on nighttime emission estimates.

Actually, we must acknowledge an error when running the FLAT_mD experiments. The actual results from FLAT_mD are far closer to those from FLAT_mM than to those from FLAT_mH, opposed to what was indicated in the manuscript. This is far more logical given that the inversion can control 6-hour mean fluxes and thus, to some extent, the day to day variations. As highlighted by the reviewer, it, however, cannot control nighttime emissions except through the indirect extrapolation of the information driven by the correlations in the prior error covariance. Still, of note is that the hourly mean emissions for the 11h-16h window is only ~30% above the hourly mean emissions for the whole day in the AIRPARIF2008 inventory.

We will correct the results of experiment FLAT_mD and make a clear conclusion regarding the impact of the description of diurnal cycle in the prior estimate of the 6-hour mean budgets.

Pg 20 lines 13-15. Presumably actual nighttime measurements would be useful to constrain nighttime emissions as well!

Initially, the sentence assumed that, in principle, it should be easier to model parts of the PBL's transitional phases in late morning and late afternoon, during which we have some level of mixing, than to model the PBL at nighttime. But, such a discussion would be too loose at this point and we agree with this comment by the reviewer. The sentence will be rewritten to encompass nighttime data.

Pg 20 line 18. The mention of Recife seems irrelevant.

We will remove it.

Pg 20 line 25. What is GB?

This refers to Gregoire Broquet.

Figure 3. Caption is inconsistent with labelling on graph (a, b, etc). Also, on the right hand panels, the numbers at the top are hard to follow (they are fine on the left panel).

Thanks for pointing to the inconsistent figure labels. It will be corrected.