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Interactive comment on "A first year-long estimate of the Paris region fossil fuel CO_2 emissions based on atmospheric inversion" *by* Johannes Staufer et al.

Anonymous Referee #2

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1 Overview:

Review of "*A first year-long estimate of the Paris region fossil fuel CO_2 emissions based on atmospheric inversion*" by Staufer et al.

Staufer et al. present a year-long estimate of CO_2 emissions in Paris using the "Gradient Method" developed by Breon et al., ACP (2015). This manuscript is largely just an extension of the Breon et al. (2015) paper. They now use a longer record of observations but they include fewer sites and use more stringent data selection. The manuscript is fairly well written. However, I have some serious concerns with the manuscript. In particular, the authors need to justify some of the crucial assumptions

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in the "Gradient Method".

2 Major comments:

I have some major concerns with assumptions made in the "Gradient Method".

2.1 Spatio-temporal offset between upwind and downwind measurements

The authors assume, as in Breon et al. (2015), that the difference between measurements made at two of their surface sites are directly comparable and that the difference between them can be related to the city-scale emissions. However, it's not clear to me that this is a valid assumption. My issues with this assumption are: (1) a temporal lag between the measurements, (2) a spatial offset between the measurements, and (3) a poor choice of model to evaluate this. The critical question is: "Did the downwind measurement actually originate near the upwind site?"

Temporal Lag: The authors touch on this issue in the final paragraph of Section 3.2. The upwind and downwind sites are quite far apart in space and it will take a few hours for the airmass to travel from the upwind site to the downwind site. I did a quick back of the envelope calculation for the transport time using distances from Breon et al. (2015; Table 1). GIF is 23 km from the Paris centre, GON is 16 km from the Paris centre and the EIF site is directly between them. This would suggest these sites are about 40 km apart (Google Earth puts the distance at about 39km). A 3 m s^{-1} windspeed (the minimum windspeed criteria) would give a transit time of about 4 hours, quite a bit longer than the temporal lag of 2 hours reported in Staufer et al. (although it's not clear how they estimate their temporal lag). This means that the upwind measurement should be compared with an observation that is at least 2-4 hours later in order to relate it to the city-scale emissions (assuming there were no changes in wind direction over a 2-4 hour period).

However, a 4 hour difference in the observed airmass will make it difficult, if not impossible (without a model), to relate the two measurements because CO_2 has a strong diurnal cycle (see, for example, Fig. 1 from McKain et al., 2012 for the Salt Lake City diurnal cycle). It would be incorrect to compare measurements made at different hours because they are influence by external processes (like boundary layer growth).

The authors do touch on this issue in the last paragraph of Section 3.2 and show that it is a problem (inversion is not consistent with their other inversions) but disregard it anyway.

Spatial Offset (vertical mixing): It is unlikely that the airmass measured by the downwind instrument was actually in the direct vicinity of the upwind instrument, there is almost certainly some spatial offset. The author's data selection criteria are designed to minimize the horizontal offset between the downwind airmass and the upwind instrument but there is also probably a vertical offset. As mentioned above, the downwind airmass will take a few hours to travel from the upwind site to the downwind site and there was almost certainly some vertical motion during this period. It's possible that the downwind airmass was not even in the boundary layer when it passes the upwind instrument.

This leads into my next point.

Poor choice of model to evaluate this: The authors are using an Eulerian model, CHIMERE, for their atmospheric transport. However, a Lagrangian Particle Dispersion Model (LPDM), such as FLEXPART or WRF-STILT, would be more appropriate for this work and would allow them to answer the critical question from above ("Did the downwind measurement actually originate near the upwind site?"). An LPDM will

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simulate trajectories for individual measurements which would allow the authors to easily determine if the downwind measurement actually originated near the upwind measurement. This would greatly simplify the “data selection criteria” because the authors would just need to find a minimum distance between the downwind trajectory and the upwind measurement site. It would also provide an appropriate time-lag for each measurement.

Instead, the authors have chosen to develop a set of “data selection criteria” that are extremely difficult to evaluate because they do not actually know the trajectories of their measurements (e.g., they spend a lot of the text arguing that the Breon et al. criteria are not stringent enough). It also means that they end up throwing out most (92%) of their data. . .

2.2 Aggregation Error and Design of the Control Vector

The authors use a very crude control vector. It is a single scaling factor for the entire Paris region (with some temporal resolution). This means that they assume the gradient between their two sites is representative of the ENTIRE Paris region and that the entire Paris region should be scaled up or down. As the authors mention (Page 6, Line 25), this will induce large aggregation errors because different parts of the city can no longer vary independently.

The authors acknowledge that this is a problem in Section 4.2 when they say: “The inversion results, however, are significantly affected by changes in the emission distribution. It reveals the need to rely on robust, high resolution emission maps such as those produced by local agencies like AIRPARIF.” It seems more likely that this issue is due to aggregation error. For example, the authors could deduce a large underestimate in emissions if the downwind airmass happened to pass over some point source that is missing in the inventory. Because the authors only have a single scaling factor for the entire Paris region, the whole city would be scaled up and the emissions would now be overestimated. So the question is, “Are these gradients actually representative of the ENTIRE Paris region?”. This is why groups typically solve for fluxes at high-resolution.

2.3 Boundary Layer Heights

Accurate simulation of the boundary layer heights is crucial for modelling urban CO_2. It controls the size of the box over which the emissions are mixed (e.g., CO_2 concentrations are lower during the daytime even though emissions are peaking). However, there is no discussion of the boundary layer heights in the model. Are they reasonable?

2.4 Resolution and representativeness of the measurements, prior, and meteorology

The authors use a fairly coarse resolution model (CHIMERE at 2km with meteorology at 15km resolution) even though previous work (e.g., McDonald et al., 2014) has shown that 1km is necessary to resolve highways. From the abstract of McDonald et al. (2014): “High CO_2 emission fluxes over highways become apparent at grid resolutions of 1 km and finer.” Why is 2km sufficient here?

Along a similar vein, the authors use surface observations from 4 to 7 meters above ground level. Are these measurements actually representative of a 2km $\times$ 2km region (which is assumed since their grid is 2km $\times$ 2km)?

2.5 Separating fossil fuel and biosphere fluxes

You don’t have measurements that would distinguish between the two. It seems that most of this separation is from assumptions you’ve made, not data. Further, it seems that there could be compensating errors because that separation is unconstrained. As such, it seems like the title might be overstepping. "Fossil fuel" should probably be

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removed from the title.

Also, what do the biosphere fluxes look like in the different inversion cases? Are they unchanged or could they be compensating for some of the changes you're seeing?

3 Minor comments:

3.1 Dimensions of your matrices?

What are the dimensions of the different matrices (e.g., are $\mathbf{H}^{\mathsf{map}}$, $\mathbf{H}^{\mathsf{trans}}$, and $\mathbf{H}^{\mathsf{samp}}$ all the same dimension? How are they multiplied together?) Even just reporting them in terms of something like: "$\mathbf{H}$ is an $n_{\mathbf{y}} \times n_{\mathbf{x}}$ matrix" (can use other notation) would be helpful.

3.2 Clarify how you are constructing the $\mathbf{H}$ matrix

From Section 2.3.1, it seems that you're doing a brute-force construction of the $\mathbf{H}$ matrix (e.g., perturbing each element in your control vector in a separate simulation), is that correct?

4 Specific comments:

Page 1, Line 4: Can you reliably estimate 6-h emissions with just 4 hours of obs?

Page 4, Line 5: Are these errors unbiased if the PBL height were wrong? Wouldn't that induce a systematic bias? Was that tested for?

Page 4, Eq. 1: This form assumes that $n_{\mathbf{y}} > n_{\mathbf{x}}$, is that true? Given the small number of observations, I would have guessed the other form would be more computationally efficient.

Page 4, Lines 26-27: However, the diurnal cycle is probably largely unchanged because you're only using afternoon observations. I doubt there's much change to any other time periods.

Page 5, Lines 28-31: What about vertical gradients? These sites are all in the boundary layer so the airmasses might be fundamentally different. . .

Page 6, Lines 17-26: Why not solve at a finer spatial scale? This would greatly reduce the aggregation error.

Page 7, Lines 27-28: How do you estimate this? I get 3-4 hours using the cutoff windspeed of 3 m s^{-1} (see Major Comment 2.1).

Page 7, Lines 30-31: Why not just do a more traditional inversion with finer spatial resolution? You could jointly solve for the background concentration (see, for example, Henne et al., 2016).

Page 9, Lines 25-31: Confusing. A plot of your covariance would be useful. Even just plotting a single row (would just be a simply x-y line plot) would be really helpful here.

Page 13, Lines 34-35: Misleading. It sounds like you're using a Lagrangian Particle

Dispersion Model, but I'm pretty sure you aren't since there was no mention of one.

Pages 14, Lines 1-4: This is what you should be doing though!

Pages 19, Lines 30-35: Again, I think this is the correct way to use the gradient method. I don't think a lack of data is a good justification for not using it. I think that you should either: (a) use the gradient method and the lag approach, (b) provide a better justification for why you don't need to consider temporal lags, or (c) use a traditional inversion with finer spatial resolution (with time-dependent footprints).

Pages 20, Lines 11-12: This is what most groups already do... A more traditional Bayesian inversion with an LPDM would allow you to solve at high spatial resolution without inducing large aggregation errors.

Figure 3: (b) and (c) labels should be flipped because they don't agree with the caption.

Measurement Sites: I don't think the paper lists the height of the measurement sites. I had to go back to Breon et al. (2015) to find it.

5 References:

Henne *et al.*: Validation of the Swiss methane emission inventory by atmospheric observations and inverse modelling, *Atmos. Chem. Phys.*, doi:10.5194/acp-16-3683-2016, 2016.

McKain *et al.*: Assessment of ground-based atmospheric observations for verification of greenhouse gas emissions from an urban region, *Proc. Nat. Acad. Sci.*, doi:10.1073/pnas.1116645109, 2012.

McDonald *et al.*: High- resolution mapping of motor vehicle carbon dioxide emissions, *J. Geophys. Res. Atmos.*, doi:10.1002/2013JD021219, 2014.

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