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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of temporal and spatial variability of atmospheric CO_2 concentration within Paris from the GreenLITE^{TM} laser imaging experiment" *by* Jinghui Lian et al.

Anonymous Referee #3

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Lian et al., present long open-path spectroscopy measurements for the City of Paris from December 2015 to November 2016 in conjunction with in-situ observations from towers in and around Paris as well as WRF-Chem simulated observations from two different urban canopy schemes. It is assumed that the authors are using the GreenLITE measurements along with the in-situ tower observations to discern which WRF-Chem urban canopy scheme can best represent vertical mixing and transport in urban areas.

My main concern with this article is that the specific objective/conclusions of the paper are unclear. The authors have conducted a lot of work analyzing data from many components, but it is uncertain as to whether they have drawn any solid conclusions.

The objective, which I assume is using GreenLITE and the in-situ observations to evaluate WRF-Chem urban canopy configurations, should be more clearly stated in the Introduction and the title of the paper should be changed.

Without a clear narrative, the paper mainly comes across as a presentation of data which is difficult to evaluate as a reader. I do not recommend that this work be accepted for publication in ACP without substantial revisions to clarify scientific objectives/conclusions.

I have the following suggestions regarding the technical components of the analysis.

Main Comments:

(1) The observational network: can you provide some indication as to areas in which the observations are most sensitive especially CDS, JUS, and any others that are situated in or adjacent to major sources/sinks? I understand that the authors use WRF-CHEM and not a Lagrangian approach so that footprints cannot be generated but having some understanding would help the comparison of GreenLITE and in-situ observations presented later in the paper since these two different types of measurements represent different spatial extents.

(2) GreenLITE campaign – can you provide more description than citing the Zaccheo paper as to how the GreenLITE observations are calibrated?

(3) WRF-Chem – Is this paper an analysis of WRF-Chem urban canopy models for cities like Paris (evaluated using GreenLITE and in-situ observations)? If so, please substantiate/provide reference for the claim that "previous sensitivity tests indicate that different physical schemes in the WRF-Chem model lead to mean differences of 2-3ppm on the simulated CO2 concentrations over Paris, whereas the various urban canopy schemes lead to much larger differences." This seems like the motivation for much of the work presented within the paper, but I am not sure that this claim, if substantiated, holds true across most urban areas.

a. If this is not the focus, and the purpose is to use the meteorology to understand the variability of the measurements, then I believe the authors should pick a model and use it throughout the rest of the analysis. It seems (from Figure 5) that the BEP model is largely better. The rest of the analysis using UCM could be put in the supplementary information. As an aside, I do think that the authors could use the ensembles in a way that would help them draw some robust conclusions. Their ensembles provide some measure of the atmospheric transport and dispersion uncertainty which can be used to contextualize their comparison between GreenLITE and the in-situ observations (S1 and S4).

(4) Anthropogenic Fluxes – The use of the anthropogenic fluxes in the analysis should be reconsidered or better explained. For example, does IER have any temporal variability? If so, please explain. If not, the authors could consider scaling using published methods. The authors could use other emission products that have temporal variability if needed. The loss of spatial scale (e.g. going from 5-10km) seems less important than preserving some temporal structure in emissions. Other products are also also more recent and thus more represented of ex-urban fluxes which constitute a large portion of CO2 inflow to Paris. Could the authors also further explain "we interpolate the emissions to the WRF-Chem grids following the principle of mass conservation?" This is unclear in both its meaning and why it is important. As with the WRF-Chem comments, the authors could use an ensemble of anthropogenic emission products (those outside of Paris) to help contextualize the GreenLITE and in-situ observations in terms of emission uncertainty (refer to Martin et al., 2018).

(5) Biogenic Fluxes – The use of VPRM to represent the urban biosphere is an active area of research and there are lots of questions as to how well a biospheric model captures the urban biogenic emissions. When VPRM was optimized using flux data, were urban towers used to help parameterize the "urban" areas of Paris? The paper mentions that the western portion of Paris has much green space and thus biogenic sources might be important in this area of the city and impact the analysis. How was

Paris-VPRM (or VPRM) validated, e.g. comparison to in-situ data from towers outside of the city that are surrounded by vegetation (maybe OVS)? Has it been used in other studies? How does it vary as a function of time in comparison to the anthropogenic fluxes like what is shown in Figure 3?

(6) Results – (4.1) There are a lot of moving pieces in this analysis and it is hard to ascertain the main conclusions from the statistical analysis. Do you think that the uncertainties associated with the other components (e.g. anthropogenic emissions and vprm sources and sinks) would have changed some of these results especially during the growing seasons or per your analysis of the seasonality of the sectors? From the Table, it is unclear that BEP outperforms UCM for much of the year. (4.2.1) Why did you use the wind per ECMWF versus wind measurements at the upwind tower(s)? I am sure, on average, the ECMWF winds are similar to what is measured at the towers but since you are comparing hourly measurements, this may make a difference. Also, how much time does it take to traverse some of the towers that are farther apart (e.g. COU and SAC)? Did you compare observations from similar times or did you account for a lag in the measurements via travel time? As with 4.1, I am not sure what the main takeaway is from this analysis.

Minor Comments:

Be specific as to what model you are using. I think in most cases you are referring to WRF-Chem models but there are others too such as VPRM, etc.

Grammar should be checked in many places throughout the article to improve clarity. Examples include lines 34 through 36 (page 2), $\sim$10 (page 8).

Figures should be modified to improve clarity:

For example, Figure 1 should include a depiction of adjacent urban areas to show how remote AND, COU, OVS, and SAC are from ex-urban sources. This will help the reader know whether or not they sample "clean" air.

Figure 2 should include roads and other infrastructure in the second panel especially since the authors have made spent time discussing sectorial emissions. Also note (a) and (b) on Figure 2.

For Figure 4, zoom into similar area as in Figure 2 to show if VPRM is capturing urban biospheric flux which can significantly impact the urban fluxes especially their variability.

The authors should consider moving Figure 5 to the supplemental information. It doesn't provide much information, especially given the timeframe that makes it hard to see, expect to show that the UCM transport model yields extreme outliers in the winter.

For Table 3, explain by what criteria did you color code the Tables. It seems like the better models for the correlation coefficients have "red" shading where int the RMSE and MBE the colors are switched (aka blue is better while red is worse). I would remove "all hours" to make the table clearer - all hours not really needed.

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