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Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "Quantitative evaluation of the uncertainty sources for the modeling of atmospheric CO_2 concentration within and in the vicinity of Paris city" *by* Jinghui Lian et al.

Anonymous Referee #1

Received and published: 31 July 2020

General Comments

The study investigates potential sources of error for estimating urban CO2 emissions using atmospheric observations. Understanding and mitigating theses errors is necessary to produce more accurate emission estimates, and the authors suggest several criteria to select data to avoid the impact of these error sources. While the study focuses on Paris, the methods and results presented are more widely applicable and of interest to other urban emission estimation schemes using atmospheric data and transport models.

The methods of the paper focus on examining a set of clearly described WRF-

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Discussion paper

Chem forward model runs, with the CO2 emissions, boundary conditions and physics schemes varied. These comprise a logical set of factors to explore, with the authors acknowledging this is a subset of all possible error sources but is still shown to be important. The detailed analysis of these results links well with the corresponding conclusions drawn (suggestions ii and iii of the abstract). I believe this paper is a useful contribution to the field, providing quantifications of significant sources of uncertainty in current models and providing a framework for further urban systems to examine their own uncertainties. However, I do have a few concerns with other aspects of the paper, as detailed below. Therefore, I recommend this paper for publication in ACP once the issues outlined below have been addressed.

Specific Comments

Introduction - The study could be seen as an extension to previous works in looking at sources of uncertainty (Martin et al 2019 https://doi.org/10.1016/j.atmosenv.2018.11.013) and are complementary to other recent studies on uncertainties in estimating urban emissions (such as Balashov et al 2020 https://doi.org/10.5194/acp-20-4545-2020). The context set out in the paper could be improved by including comparisons to such other studies.

Page 1 line 28 – Value quoted is for scope 2 emissions, but inversions only estimate scope 1 emissions. Either this should be made clear or the authors should use scope 1 emissions value.

Page 6 line 6 – The use of the KNN outlier removal needs greater justification and is my greatest concern with this paper. The authors claim that this algorithm removes observations of sources too local to be resolved or meteorological conditions that model is less skilled with (which are valid reasons for removing data points) but provides no evidence that this is the case. As it is, the algorithm may just be arbitrarily throwing away data that highlights systematic over or underestimates in the emissions field that is needed for an inversion. Either it needs to be demonstrated that the algorithm only removes points that are linked to these conditions, or a different method, preferably based on physical reasoning, should be used.

Page 7 line 2 – The authors say that individual measurement errors are negligible compared to model-data differences. However, model-data comparisons are made on hourly time scales and there will be (potentially large) variation within the hour. How is this sub-hour variation accounted for in model-data comparison?

Page 8 line 17 – The authors suggest upwind-downwind gradient can be used even in growing seasons as natural biogenic fluxes do not completely offset anthropogenic fluxes – but this still requires good knowledge of biogenic fluxes as they will make a major contribution to the observed mole fraction difference. The authors note several lines later that the biogenic fluxes show systematic errors, does this contradict the first statement?

Page 10 lines 1-21 (and Figure 11) – This section should be reworked for clarity. My understanding is that the authors have averaged the difference of tracer concentrations between runs across time (by month) and horizontal space (by land type) to calculate the individual values shows in figure 11 – but this should be made clearer in the writing. For many of the values, the standard deviation is large w.r.t. the mean difference. A different type of plot that shows the distribution, such as a violin plot, may be more appropriate.

Page 11 line 13 – A strong conclusion for the use of KNN outlier removal that is not justified, see above.

Figure 12 – Why use a cumulative distribution and not a histogram – what are the authors trying to show with this choice?

Technical Comments

Page 8 line 18 – suggest "(the SAC station had unfortunately measurement gaps)" -> "(unfortunately the SAC station had measurement gaps)" and dates of gap added

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General note on figures – Rainbow colour schemes should be replaced with perceptually uniform colour schemes, and red-green colour schemes should be avoided due to Colour vision deficiency (colour blindness)

Figure 4 – Both the dark blue filled contour and red contour are called the 'threshold' but the two are not in agreement (red contour seems to be the correct one)

Figure 6 – A note to remind the reader that this figure is for January only would be helpful

Figure 7 – This figure is dense, which hinders clarity. I suggest the wind direction and mf timeseries to moved to new windows with a shared x axis. The boundary layer height should also have a larger contrast to make it more visible

Figure 10 – Showing the line of transect for the south-north slice on the lat-lon plots would make interpretation of the figure clearer

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