Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-547-RC2, 2019

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 #2

Received and published: 6 August 2019

This paper describes urban measurements of CO2 by in-situ and by the novel open-path laser system "GreenLITE" with multiple reflectors and transceivers deployed in the Paris area. Observations are compared to high-resolution WRF-Chem simulations with a representation of CO2 fluxes from anthropogenic emissions and biosphere-atmosphere exchange. The paper is well written, and I recommend publishing after the following minor comments are addressed.

General comments:

For the WRF-Chem modeling CO2 emissions at annual and national scale for scaling the high spatial resolution emissions to the year of interest haven been taken from the

Global Carbon Atlas (GCA), however it is unclear what these data are based on (e.g. UNFCCC reporting, BP statistical reports, or other sources). The Global Carbon Atlas has some missing links in the "Data contributors" section making traceability of the emissions impossible. This needs to be clarified.

It is somewhat unclear how the statistics shown in Table 3 and Fig. S4 have been calculated for the GreenLITE vs. WRF-Chem measurements in section 4.1. Have the data from all chords related to e.g. T1 been combined and then the statistics is derived, or has each chord been treated independent and the resulting statistics shown in Table 3 and Fig. S4 reflect the average across all chords?

The discussion of the results in section 4.2.2 regarding the spatial gradients between different chords of the GreenLITE observations and the simulated counterparts, as well as the corresponding discrepancy between observations and model results should at least mention the potential impact of turbulent eddies and thermals. Those are likely to form in a convectively unstable atmosphere, i.e. during summer, and are unlikely to be represented properly in the MYJ PBL scheme (a local closure model) deployed in the WRF-Chem simulations (c.f. Xiao-Ming et al., 2010). Ref.: Hu, Xiao-Ming, John W Nielsen-Gammon, and Fuqing Zhang. 2010. "Evaluation of Three Planetary Boundary Layer Schemes in the WRF Model." Journal of Applied Meteorology and Climatology 49 (9): 1831–44. doi:10.1175/2010JAMC2432.1.

Specific comments:

P1 L36: I suggest replacing "have been used" with e.g. "have been or will be used" as you are refering also to future satellites.

P7 L21: please rephrase "low atmosphere", e.g. "lower part of the atmosphere"

P7 L38: "in some respect superior" this should be formulated clearer. What I see from Table 3 is that RMSE with the BEP model is always better for T1 than for JUS, and better for T1 than for CDS with one exception.

P8 L21, P8 L37, and P10 L19: please rephrase “The std values”, e.g. “Standard deviations”

P9 L38: What is the difference between the first two of the three hypotheses? Is H2 meant to refer to only transport model deficiencies, excluding inaccuracies in emissions? This should be made clearer. Also it should be made clear at the end of section 4.2.2 which hypothesis remains the most probable one.

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