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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An attempt at estimating Paris area CO_2 emissions from atmospheric concentration measurements" *by* F. M. Bréon et al.

Anonymous Referee #1

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General Comments:

Using five atmospheric CO2 monitoring sites, this paper presents top-down (inversion) based CO2 flux estimations for a region around Paris. In this study, Chimere transport model in combination with different flux datasets (both anthropogenic and biospheric) is used to simulate atmospheric mole fractions of CO2. The inversion is based on Bayesian approach and is performed independently in two different set-ups: one set-up uses the mole fraction measurements and the other uses concentration gradients as measurement vector (y). The study further investigates on the usability of Eiffel tower measurements in these inversion set-ups to generate meaningful flux estimations. Overall, this paper holds valuable information on this area of research, although some analysis needs further clarification. The major issues are mentioned below.

Major Comments:

My major concern is that the paper is often too far away from being succinct and clear, which distracts readers a lot. The problem is not only with the inclusion of irrelevant information (for e.g. figure legend explanation inside the manuscript text that could be done (or already done) in respective figure caption), but also with the inappropriate formulation of the sentences (sometimes it is clumsy and repeatedly said). The authors need to address this and reduce the unnecessary details significantly in order to maximize the scientific impact of this study. Additionally, the paper should be re-organized a bit for sharing effectively its flow of thoughts with the readers (for e.g. (1) it is far better if authors describes the components of the model (sect. 2.2 + 2.3) after introducing the model (sect. 2.4), (2) I don't follow the reason why authors provide a part of "Discussion" (sect. 2.6) here, before the result sections – Please reorganize it: one option is to shift sect(s). 2.5+ 2.6 to the result sections).

The second major issue is with the Eiffel tower simulations (EIF) which do not capture the observed variability and gives a model-measurement bias of -13 to -30 ppm that is too large for this kind of applications done in the study. Since 300 m tower measurements is capable of giving valuable information about larger area and is easier to represent in the model in terms of (local) near surface process, it is important to check further why there is such a big bias in the model consistently(or where and why the model fails to reproduce the observations). Knowing that improper vertical mixing produces considerable model-measurement mismatches in mesoscale models, more careful analysis is required in this aspect to establish robustness of the current inversion method (relevant for other sites as well), before utilizing it in the inversion or applying "quick fix". It is not clear to me from the text or figure whether the largest mismatch occurs in the diurnal cycle. I "assume" that it is mostly during night where the model might already "see" free-troposphere in that height. This could be examined by a comparison of simulated mixing heights with observations (ceilometers- Jussieu?). I do not see such a remarkable improvement in results after applying "quick fix" (re-

moving EIF observations from the inversion system). This is something to be noticed seriously. Without this kind of analysis and quantification or qualification, I would think that these results are somewhat preliminary to be published.

My last concern is about the concentration gradient method used in the study (sect(s). 2.6+ 5). The discussion on the contribution from the background is somewhat unclear and hard to follow. My first question is how valid these kind of assumptions (on setting measurement sites as background reference stations and relay on simulated wind speed) inside Paris area. Many are different here: orography effect, measurement level etc. Second question is how it is made sure that the background "reference site" concentrations had already "fully" reached in respective "measurement" sites and no parts are missing due to couple of other reasons, which depends on wind speed (advection) in addition to wind direction. In my understanding, this gradient method performs some filtering of the data which is difficult to model (in terms of both transport and flux); however there is high probability that these "difficult to model" data contains valuable information about fluxes, than just "background noise" as assumed here. As already shown, the negative values in Fig. 9 clearly indicate the failure of these assumptions. One must be more careful in performing these kinds of "filtering". More valid analysis is required in terms of expected variability of the background, before applying this method.

I recommend authors to address these issues (+ specific comments below) before publishing it in ACP.

Specific Comments:

Fig. 3 + 4: Indicate spatially averaged values?

Fig. 5: a bit disappointed to see that y-axis is not properly set to include the "peaks". Please redo the figure with proper y-axis setting.

Fig. 6: "Note the weekly cycle with lower values during Saturdays and Sundays" It is

not obvious in the figure. Please mark these days in the time series.

p. 9653: AirParif inventory: Did this study use hourly emissions? Fig.3 shows only week days, Saturdays and Sundays. Please make it clear.

p. 9654: "Figure 2 shows an example. . ." October or November? text and Figure details differ in terms of period. Please check the whole manuscript.

p. 9655: "The direct simulations and the atmospheric inversion thus account explicitly for the biogenic fluxes" Not clear. Please rephrase.

p.9666: Sect. 3.6: Not clear how you determined the matrix H.

p.9667: "One may then blame . . ." "blame" -> Use better word

p.9668: "One notes that the posterior estimate of the afternoon NEE . . ." Do you have an explanation why it became slightly positive?

p.9670: "The second line shows. . ." "line" -> panel

p.9673: "we have set a lower uncertainty for the gradient" "lower uncertainty" -> how much (%)?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9647, 2014.