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Discussions

Interactive comment on "Constraining fossil fuel CO_2 emissions from urban area using OCO-2 observations of total column CO_2" *by* Xinxin Ye et al.

Anonymous Referee #2

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The manuscript "constraining fossil fuel CO2 emissions from urban area using OCO-2 observations of total column CO2" by Ye et al. studies the inversions of urban CO2 emissions based on CO2 data from the OCO-2 mission. It analyzes both Observing System Simulation Experiments and experiments with the real CO2 data from the OCO-2 mission to assess the level of precision and accuracy that could be expected from such inversions.

This is a critical topic in a context of the preparation of various missions for the monitoring of CO2 anthropogenic emissions from space while there is still a lack of studies addressing the feasibility of this monitoring in details. This study gathers tools and ma-

terial that can feed the understanding and discussions in this research field. I think that the authors can produce an important paper with such a basis.

However, I also think that the text needs a strong improvement and that critical issues need to be addressed:

1) Most of the text needs a careful rewriting. At first sight, it looks concise. But when reading it into details, especially when trying to understand how some of the curious results (see below) have been obtained, it appears to be confusing, hasty, approximative, full of gaps, and unstructured. A major issue is the lack of logical flow and of robust focuses. We often do not know where the authors go, and why they make their choices. Large parts of the manuscript look like piles of independent pieces of explanations, analysis and discussions that have not been linked or synthesized. Some of the comments below illustrate this. The second half of the abstract, sections 1 (the introduction), 4 and 5 give an illustration of the lack of logical structure. The second half of the abstract, sections 2.3 and 3.2 (among others) provide a good illustration of the lack of clarity, especially when dealing with the experimental protocol.

2) I do not understand central results and figures of this manuscript. It may be because I do not really understand the details of the computations.

- section 3.2.a and Figure 8: I do not understand what is done when several tracks from different days are used together. Do the authors gather the corresponding 100 perturbed wind conditions together, forming a much bigger ensemble of perturbed wind conditions, and then look at the distribution of S arising from this larger ensemble ? In that case, the convergence they show in Figure 8 would only be an illustration of the decrease of the sampling errors for the distribution of the transport error when increasing the sampling size, i.e., nothing about the asset of using different OCO2 tracks. Or do the authors do things in such a way that N tracks bring N independent statistical transport errors together, which would decrease the overall weight of the transport error ? Then, in the absence of prior and measurement error, the curve on

Figure 8 should converge towards 0. My lack of understanding is a real concern since this is one of the main results that are cited in the discussion, conclusion and abstract.

- section 3.2.b: an OSSE is conducted with synthetic perturbations of the transport. I cannot understand how the bias in the results can be related to a bias of WRF in the real world. My very simple understanding is that this bias is just a consequence of the way the perturbations are chosen and applied in the synthetic world, combined with the non linearity of WRF. Again, this corresponds to one of the main results in the abstract, discussion and conclusion.

- section 3.4 and Figure 11: the text at lines 11-12 page 11 says something really strange for me: "bimodal distributions for three tracks due to negative perturbed wind speeds caused by large errors added on small absolute wind speed". What is a negative perturbed wind speed ? What do the authors do with such a wind speed ? Furthermore, they are a lot of negative S in Figure 11 (the text does not speak about it). How can it be possible ? Would that mean that the authors diagnose negative enhancements of XCO2 along the simulated or observed OCO-2 tracks ? I would not understand it but I do not really know how the authors diagnose the enhancements since section 2.3.1 and the corresponding equation on page 6 lack of rigor. Is there a link between these negative scaling factors (turning cities into sinks of CO2) and the "negative perturbed wind speed" ?

3) The study of the uncertainty in the ecosystem fluxes in the PRD area does not fit with the rest of the paper and the authors did not make much effort to connect the results and discussions corresponding to this study to the other ones. I think that this should be removed, which would help improve the structure and logical flow of the paper. We get a strong feeling that the authors wanted to value an experiment they had done in the PRD region by inserting it artificially into an independent paper. It is all the more problematic that the dissymmetry between the experiments in the "plume cities" and LA is already a bit difficult to admit (with these two cases only, we can already get the feeling of following two independent studies in parallel). There is no justification for not

applying the range of tests on the transport errors to the PRD area. And there is no reason for not applying the tests applied in the PRD area on the biogenic fluxes to, at least, LA, and, maybe, Cairo. Using VPRM in LA when assimilating real data vs. using an ensemble of larger scale products for the OSSEs in the PRD area (i.e., different types of tools) emphasize the feeling that the PRD sections are disconnected from the rest. At last, I think that the analysis in 3.3 are unclear and debatable so it would have to be extended and improved. On a similar topic: the oscillation of the text between distinguishing or mixing the background and biogenic components is problematic, especially since the background is defined several times in different ways in this paper. This, in addition to the problem stated above, leads to strange sentences at the end of the paper ("biogenic fluxes are critical for cities located in well vegetated areas ... Background mole fractions of XCO2 for urban areas require more consideration ... More sophisticated biospheric modeling can help to develop a better determination of the background XCO2").

4) The assumption that a change in the wind speed can be simulated by stretching the XCO2 images and rescaling the concentrations needs to be discussed and justified. I think that this assumption is not obvious and that is does not perfectly fit with many model formulations, such as a large range of Gaussian ones.

5) The paper focuses on transport, biogenic and background errors. This justifies the lack of discussion on the measurement errors in the sections dedicated to the OSSEs. However, these measurement errors are nearly ignored when analyzing the results with real data, in the discussion, in the conclusion and even in the introduction (e.g. on p3), while other studies indicate that this is a critical component of the problem. As a consequence, the presentation of the conclusions from section 3 often seems biased and misleading. E.g., lines 11-18 p14 (but also lines 18-19 p1 in the abstract) do not really say that the 15%/5% numbers relate to transport errors only and that the potential of the OCO2 data themselves (i.e. not that of the modeling framework) is not fully investigated. This could strongly favors over-optimistic reading and citations

of this paper (such as when this paper itself summarizes in a very optimistic way the conclusions from Hakkarainen et al. 2016 on p3 line 3). In such a context, I also think that the title of the manuscript itself is misleading.

6) Section 4 does not deliver the type of discussions that are expected when reaching the end of section 3. Section 3 does not go really deep into the result analysis, in particular when looking at the tests with real data (while these tests will certainly be brought forward when citing this paper). Therefore, section 4 should provide more insights on the results. However, the first paragraph in section 4 is merely (if we except 2,3 lines) a summary of what has already been said in section 3, and the rest of section 4 is the piling of small and independent discussions on the perspectives.

7) I could list a lot of minor issues. However, nearly all of them relate to comment 1 and I will thus wait for a later step before undertaking a detailed list. That said, I would like to mention:

- that the references could be complemented by Schwander et al. 2017 (Schwandner, F. M. et al., Spaceborne detection of localized carbon dioxide sources, Science, 358 eaarn5782, 2017) which could potentially impact lines 4-5 p3. Nassar et al. 2017 (Nassar, R., et al., Quantifying CO2 Emissions From Individual Power Plants From Space, Geophysical Research Letters, 44, 10045-10053, 10.1002/2017GL074702, 2017) is also relevant for the discussion on page 3 since power plants and cities are sometimes considered as similar targets and since the quantification of their emissions using OCO2 data raise similar challenges.

- section 2.2.1 and table 1: the information about the spatial resolution of the outer domains is not really interesting if there is no information about their spatial extent.

- regarding the integration of the data along the OCO2 tracks: as said before, the section 2.3.1 and its equation are unclear; I did not understand what is the spatial and/or temporal representativity of the dots in figure 5; and I am not sure about the meaning of the spatial location of the OCO2 dots in figure 6.

- The paper often considers the temporal variability of the emissions as a side problem while one of the central tests studies the asset of using several satellite tracks. The day to day variability of the emissions is merely ignored. The seasonal variations are mentioned but they are not confronted to the statements regarding the number of OCO2 tracks required to balance the transport errors, nor to the actual number of available tracks per year.

- p7 : lines 29-31: i) due to the variations of the wind as a function of the vertical (including wind curl in the PBL), modifying the PBL should have an impact on the XCO2 fields ii) if the PBLH and vertical mixing near the surface do not have much impact, why having perturbed the PBL schemes in these experiments ?

- p11 l 32 If I am not wrong, such a scaling was not applied for Cairo and Riyadh: why ? how to rely in this factor 1.288 while the inventory are assumed to be uncertain ? why not just acknowledging that the estimations correspond to the satellite track times and discussing the extrapolation into daily budgets later ?

- section 3: I feel that the results when using real data (especially in LA) raise severe doubts regarding the potential of the inversion strategy or of the current OCO-2 data, and that this is not highlighted in the conclusion and abstract of the paper. At least, this deserved more discussions in section 4.

- there are a lot of awkward sentences throughout the text such as "These cycles could be compensated by optimal sampling strategies but only active sensors will be able to sample across clouds and at night. For future missions, the sampling bias might be compensated by more frequent tracks or targeted view modes." at the end of section 4

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