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Comment on egusphere-2022-749

Anonymous Referee #1

Referee comment on "Theoretical assessment of the ability of the MicroCarb satellite city-scan observing mode to estimate urban CO_2 emissions" by Kai Wu et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-749-RC1, 2022

This publication investigates the potential of the MicroCarb satellite mission planned to be launched in the 2023/2024 timeframe to estimate urban CO_2 emissions. The potential is analyzed for Paris and London using synthetic model experiments for a total of 15 days distributed over three representative months of the year with varying illumination conditions, biospheric activity, and cloud cover.

The MicroCarb mission will have three different observation configurations including a "city-mode", which allows observing a target with a two times higher spatial resolution (2.25 km x 2.25) and a two or three times wider swath than in the nominal nadir mode by performing two or three pitch maneuvers when flying over the target. The study is important as it provides a scientific basis for the decision on whether a two-sweep or three-sweep mode should be implemented.

The manuscript is very well written and concise, the model simulations are state-of-the-art, and the figures are informative. I have only a few minor points and one main concern that actually not only applies to this study but to other similar studies as well. I consider the manuscript acceptable after addressing these (primarily minor) points.

Main concern:

Like other studies employing an OSSE approach with synthetically generated observations, the conclusions on the ability of the satellite to constrain (urban) emissions are quite optimistic, and I would argue too optimistic. Some of the limitations are addressed in the manuscript, e.g. in the "Conclusions and discussion" section where the results are presented as "best-case" scenario. I am worried, however, that papers like this generate unrealistic high expectations of city governments or other stakeholders, which the scientific community, once the satellites are in orbit, will not be able to meet. The study is very carefully designed and executed, but in my view the discussion on the limitations of the study induced by the different assumptions is falling short. Probably the easiest solution would be to add a paragraph or two to the "conclusions and discussion" section, which so far actually contains very little discussion.

Here is a (incomplete) list of my concerns:

- My general concern is that systematic errors in any of the components (e.g. atmospheric transport, observations, biospheric fluxes) are much more critical than random errors. The latter can always be reduced by increasing the number of observations, and since most OSSEs only consider random errors, the results tend to be overly optimistic. In the present study, the three-sweep mode simply leads to better results because of the larger number of observations, but this is not the only point that should be considered when comparing the two modes. There is only a short discussion on the critical issue of determining the background against which the urban enhancements are measured. Reliably determining the background concentration levels will likely be essential, especially in the presence of systematic observation biases, and will need a sufficient number of cloud-free pixels outside of the urban plume. This may be another important advantage of the three-sweep mode that is not addressed in this study.
- The closed-loop experiment assumes unbiased transport errors. Although this limitiation is acknowledged by the authors, there is essentially no discussion on how such errors could affect the emission estimation. Is there no information available from the meteorological community about biases and errors in their analyzed wind products? Relative errors in wind speeds are almost certainly largest under low wind conditions, but these are the conditions that produce the strongest signals in an OSSE experiment as presented here. Thus, the situations working best in this OSSE may actually be the most problematic in reality. Probably there is an optimal range between low wind speeds, where relative errors are large, and large wind speeds, where the urban enhancements are too small to be detected. Unfortunately, the present study presents no insights into this question.
- Cloud cover is also treated in a random fashion. Depending on cloud cover, a probability is estimated for a pixel to be cloud-free or not, which results in a random pattern of cloud-free observations. However, clouds tend to be organized and to obscure one part of the image more than another part. As a result, the satellite will likely be blind to emissions in some parts of the city, an effect that is not captured by a purely random pattern. Furthermore, with increasing cloudiness, also cloud-free pixels tend to show biases in XCO2 due to 3D cloud radiation effects (e.g. Massie et al., 2017; https://doi.org/10.1002/2016JD026111). As a result, the ability to constrain urban emissions will likely drop faster with increasing cloud cover than suggested by Fig. 6. Figure 6 would tell the operators of Microcarb that a 50% cloud cover would reduce the error reduction for Paris only from 13% to 10% as compared to a cloud-free case (for a 1 ppm observation uncertainty). I doubt that this will be true in reality.
- Point sources are excluded in the OSSE, but point sources are an important part of reality, not only outside but also inside cities. In many cities, emissions from waste incinerators, combined heat and power plants or industrial stacks make up a large fraction of the total CO2 emissions and the emissions are usually not constant in time. X-STILT, to my knowledge, only computes the sensitivity to emissions at the surface but not to elevated emissions. The mixture of emissions released near the surface and from stacks will pose a major challenge to quantify urban emissions.
- Microcarb as other satellites will only measure at a single time of the day and will observe a given city only very rarely in a given year (actually, it would be good to know how many samples can be obtained over a city under optimal conditions). Emissions

have a diurnal cycle and change from day-to-day. However, cities would like to better know their annual mean emissions and how they evolve over the years. Because of the uncertainties in temporal variability of emissions, observations on a few days per year (if possible at all) will provide only small constraints on annual mean emissions. Some of these challenges were addressed in Broquet et al. (2018; https://doi.org/10.5194/amt-11-681-2018), which also performed an OSSE for Paris.

Minor points and corrections:

- I had to look up other documentation of MicroCarb to fully understand the city-mode. If the satellite would only perform pitch-maneuvers (as I thought after reading section 2.1), it would miss many cities or observe them only partially. However, as stated on https://microcarb.cnes.fr/en/MICROCARB/GP_mission.htm, the pointing-mirror allows also movements along the roll-axis by pointing the line of sight on either side of the satellite ground track up to ±200 km. This should be explained more clearly, because otherwise it is difficult to understand why the cities are exactly in the center of the scans.
  Furthermore, I assume that the sampling pattern shown in Figure 2 only holds when the city is exactly in nadir. How does the pattern look like when the satellite points some 100-200 km to the side (would be good to show e.g. in the Annex)? This will likely be necessary in many cases to have good coverage of a given target. Is the distortion of the pattern and the degradation of the resolution important or not? Would that affect the conclusions of the study?
- It is probably correct that urban areas are responsible for ~70% of global emissions, but at the same time it is clear that only a fraction of this is actually emitted within the city boundaries, since a large part of the power consumed by cities is generated by power plants outside. I always find this number of 70% misleading when it is presented without further explanation.
- The introduction section misses a number of references that I consider important for this paper:
  Other CO_2 satellite OSSE studies for urban areas: Broquet et al. 2018 (see above) and Kuhlmann et al. (2020; https://doi.org/10.5194/amt-13-6733-2020).
  Study on CO_2 emissions of the city of London based on aircraft observations: Pitt et al. (2019; https://doi.org/10.5194/acp-19-8931-2019).
  Study on large CO_2 emitters (including cities) as observed from OCO-2: Chevallier et al. (2022; https://doi.org/10.1029/2021GL097540).
- Page 3, Line 60: Change "pass over" to "passes over"
- P3, L68: Change "analyses" to "analysis" (singular form needed here)
- P3, L87: The ACT swath seems much closer to 30 km than to 40 km, and similarly for the 3-sweep mode seems closer to 45 km than to 60 km.
- P4, L98: Change "where g defined" to "where g is defined"
- P4, L110: I was confused by the usage of the term "scene", which apparently refers to a single observation/pixel rather than a full "scene" of a given area.
- P5, L143: The high correlation coefficient of 0.98 gives a false impression of accuracy. It only tells us that the diurnal cycle is on average well represented, which seems comparatively trivial as it follows the mean diurnal cycle of radiation. The more important message is that the fluxes are in a realistic range.
- P7, L187: Please provide more details on the "realistic noise model". Which parameters determine the noise and how was it applied in the present study? Do you explicitly consider, for example, surface reflectance, and if so, based on which data set?
- P7, L198: What is the assumption of a 25% random error for the biological fluxes based

on? I can't track this value from the material presented in the paper or in Figure A2. Is this uncertainty assumed to be purely random and spatially uncorrelated?

- P7, L200: The eigenvalue decomposition sounds interesting. Please provide more information on how it works.