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Interactive comment on "Tracking city CO_2 emissions from space using a high resolution inverse modeling approach: A case study for Berlin, Germany" *by* D. Pillai et al.

Anonymous Referee #1

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Review of "Tracking city emissions from space using a high resolution inverse modeling approach: A case study for Berlin, Germany" submitter for possible publication to ACP by D. Pillai et al.

This paper analyzes the potential of the ill-fated CarbonSat spaceborne radiometer to estimate CO2 emissions from a medium-sized city such as Berlin. "Ill-fated" is used above as the FLEX mission (vegetation fluorescence) was selected by ESA rather than CarbonSat for the forthcoming Earth-Explorer-8. Nevertheless, an instrument similar to CarbonSat may be proposed in the near future and the present study is therefore of interest to quantify the information content of such space observatory. The analysis is based on a classical Bayesian inversion that provides an estimate of the posterior

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uncertainty. It makes use of an atmospheric transport model at 10 km resolution and a detailed evaluation of the random and systematic error of the observations. The impact of these errors on the emission estimates is quantified. In addition, the impact of other sources of error, such as the contribution from biospheric fluxes to the measured CO2 and the imperfect knowledge of the spatial distribution of the emission are also evaluated. It is shown that the Berlin city emission can be evaluated with a relative uncertainty on the order of 20% and up to 30% when all sources of errors are combined. Note that the impact of atmospheric modeling error is not evaluated (and this is clearly stated in the paper). The paper presentation is very clear. It provides new results that can be of interest to a wide community. There is no doubt that this paper should eventually be published. I nevertheless would like to recommend a few changes (rather easy) that would, I think, improve the paper.

One criticism is that the accuracy needs in terms of city emissions are not discussed. In the introduction, it is explained that an independent evaluation of the emissions is needed to verify the impact of mitigation efforts. If this is the primary objective of the mission, I would thing that an accuracy of a few percent is needed. The introduction also mentions the objective of inventory verification. However, the accuracy that is required for such objective is not mentioned. Finally, the introduction mentions the potential of independent measurements of CO2 for emission trading. However, I believe that emission trading is at the scale of industries, and not at the scale of the cities. I thus recommend a better description of the needs with a quantification of the accuracy requirement. Clearly, this question has a strong impact on the results. Indeed, the abstract conclude by saying that “CarbonSat is well suited to obtain city scale CO2 emissions”. Since there was no requirement set, one cannot draw such conclusion.

I have been confused with the size of the state vector λ. On the one hand, I understood (p7, line 20 and below) that only the scaling to prior emissions of the Berlin region was retrieved (the spatial and temporal variations of the emissions are assumed) so that there is a single element in the state vector. On the other hand, on page 8 (around line

5), it is stated that the prior uncertainty is a matrix with no correlation, which clearly indicates that there are several elements in the state vector. Please clarify.

Page 9 around line 30. I could not understand why a 500 km swath instrument leas to 25 valid observations during the year while the same with a swath width of 240 km leads to much more than half of this number. I would have expected that, as the swath is reduced by a factor of slightly more than 2, the number of valid observation be reduced by a factor of significantly more than 2. Please discuss. Is this number typical of what can expect for cities with similar cloud cover as Berlin, or is the CarbonSat orbit centered over Berlin which makes it a favourable case ?

P10 l-34. “A quite high scaling factor” (between CO2 and aerosol optical depth). Please explain the reasoning to state that it is a high scaling factor. Indeed, eventualy a CarbonSat-like mission may be used to monitor cities that are not as “clean” as Berlin. For such cities, the scaling factor chosen may be an underestimate.

I was surprised by the discussion of the “clean pixel method”. In the present state, it is very hard to understand and it comes at od with the rest of the paper. I strongly suggest to remove this section.

P13 l 6. 14C is mentioned. I do not think that anyone believe that 14C can be measured from space. The present paper is about spaceborne observation and I think it is misleading to mention 14C here. As for the other tracers (CO, NOx), the authors do know that, when adding this source of information, one also adds an unknown variable (relative fraction of emissions). Thus, I think it is misleading to suggest that the concomitant measurement of these gases would allow a distinction of the biogenic and anthropogenic contributions.

The “conclusion section” is more a summary than a conclusion

P15 l 23. I could not understand the argument in the sentence “By showing that the systemic error of the retrieved fluxes. . .”. Please rephrase

Finally, I have a recommendation for discussion : The simulations are made at 10 km resolution and the authors do not mention a significant loss of information from the original 2 km of the CarbonSat instrument. One should then wonder what is the added value of the high spatial resolution of CarbonSat. It seems that 10 km resolution is good enough to observe the plume from the Berlin city

Figure 2 is mentioned but not discussed. It does not bring anything to the paper and I thus strongly suggest to remove it.

In Figure 3, it seems that the original data (10 km resolution) went through spatial smoothing. I would like to see the pixels. Also, a zoom over the Berlin region would be appropriate

Figure 4 : Please use Y axis that start at zero. The current presentation is somewhat misleading. The difference between IER and EDGAR are surprisingly large. I wonder whether there are arguments to favor one versus the other.

Figure 6: There are features I do not understand: Around day 75, two successive prior emission values show differences by a factor of two. Based on Figure 4, I cannot understand how the weekly, seasonal or daily cycles can explain a difference by a factor of two (assuming the observation is around 11 when there are little hourly variations). Please investigate

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