

[Interactive
Comment](#)

Interactive comment on “Constraining regional greenhouse gas emissions using geostationary concentration measurements: a theoretical study” by P. J. Rayner et al.

Anonymous Referee #2

Received and published: 28 March 2014

1 General

This paper studies a new concept of CO₂ and CH₄ emission measurement from space. An application is shown for an idealised city. The paper is well written and informative.

However, the city toy model is very simple and does not really support the high-profile conclusions. It is actually not clear what one should conclude. More work may be needed to make the model capture some of the main features of the real world, i.e. not those from the clear-cut and emission-homogeneous city hosting a single power plant and set in a background without CO emissions (i.e. without any road) represented

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



here.

For instance, CO measurements are said to “play a vital role in constraining combustion source” (p. 1380), but this could just be a consequence of the artificially uniform CO:CO₂ emission ratio. Indeed CO:CO₂ from traffic varies with fleet composition and speed, hence with the road type: it is not homogeneous within a true city. From the results presented, one may conclude the opposite of what has been written: the concept fails for CO₂ because it requires a uniform CO:CO₂ emission ratio.

The toy model is even simpler for CH₄, and the corresponding text is reduced to 12 lines only, results included. Urban CH₄ emissions may come from leaks at unknown locations to a large extent, a specificity which is not accounted for in this simple set-up.

Last, throughout the results, the atmospheric flow is supposed to be perfectly known, which dramatically helps the inversion.

None of these assumptions are justified or tested in the paper, which is hard to understand in a pure simulation context (measurement free) where the chosen set-up fully drives the results.

2 Detailed comments

- p. 1369, third paragraph: the evocation of Carbonsat is not completely fair because it misses its imaging capability (in clear sky) that damps the issues raised.
- Section 2.1.2: the authors should also report typical pixel resolution at mid and high latitudes, and the specific pixel resolution for Shanghai, a city used here for inspiration (Section 4.1).
- p. 1377, l. 19: the authors should explain how the steady-state response yields the discontinuous pattern of Fig. 1.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



- Section 4.1, first paragraph: The unit of the uncertainty is surprisingly given for a full year, while the simulation includes six days only. Depending on temporal correlations, the given numbers may correspond to many weekly values. Assumptions about spatial and temporal correlations should be explicitly written and justified.
- p. 1379, l.9-18: the authors should indicate the sampling density that they get, given the thresholds on aerosols and clouds chosen in Section 2.3.1.
- Fig. 3: the authors should write the temporal reference of the fluxes (I guess six days).
- p. 1380, l.7: how do the authors extrapolate their weekly result to a full year? Again, assumptions about spatial and temporal correlations (and corresponding justifications) are missing.
- Section 5: the uncertainty description is even rougher than for CO₂.
- p. 1382, l.21-22: does the offset also need to be homogeneous in time within the presented framework? Anyway, I do not see any reason why the aerosol field would be constant.
- p. 1383, l.15: This “ameliorating factor” may actually have little power because the plume spreads over time and merges with other plumes.
- p. 1384, l.1: the assumption used here is strong, not weak.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 1367, 2014.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)