

Interactive comment on “Analysis of Total Column CO₂ and CH₄ Measurements in Berlin with WRF-GHG” by Xinxu Zhao et al.

Anonymous Referee #1

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Summary

Zhao et al. present a study on total column CO₂ and CH₄ in Berlin. Their results from a high-resolution modelling framework based on WRF-GHG-GFS and VPRM-EDGAR fluxes is compared to previously published observations by Hase et al. 2014. The authors found that XCO₂ can be modelled reasonably well, while CH₄ showed a significant bias of ca. 2.7%. Using a differential column methodology the influence of variations in the boundary conditions and non-anthropogenic sources can be reduced and the model-data mismatch of the DCM-derived XCO₂ and XCH₄ was further investigated.

General comments

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Overall, the paper is well-written and the structure is straightforward. Despite only covering a few days of measurements this paper adds interesting results to a growing field of research and demonstrates the value of DCM. After addressing the general and specific comments, I would recommend this paper for publication.

The manuscript falls a bit short by not considering or discussing all potential sinks and sources of CH₄, but their assumptions are largely supported by citations and other studies. Given the high temporal and spatial resolution of the modelling framework it would have also allowed to investigate other issues in more detail, e.g. which sources in the region are dominant (are the power plants the key contributors to CO₂,ff in Berlin) or how would a change in the daily emission cycle affect the model-observation mismatch. The spatial and temporal disaggregation is mentioned as a major source of uncertainty in bottom-up inventories for cities, but this topic is not really discussed through the lens of the modelling and measurement results here.

Specific comments

Line 23: 50% seems to be an extreme example and it seems advisable to give the range and typical uncertainty of national emission inventory reports.

Line 37: Typical 'top-down' methods rely on prior information on fluxes, therefore, the assumption that they are 'independent' should be further explained.

Line 43: Please clarify what kind of 'carbon cycle processes' you are referring here.

Line 53: Please add information about the manufacturer for the EM27/SUN

Line 61: Vogel et al. 2018 also seem to be using a very similar upwind versus downwind approach.

Line 68: Previous studies have found that urban carbon fluxes are significantly higher than predicted with conventional models like VPRM (Hardiman et al. 2017; <https://doi.org/10.1016/j.scitotenv.2017.03.028>). Using VPRM is thus a limitation that should be discussed.

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Line 76: It would seem important to note that Berlin is actually a state, with more regulatory influence than other cities.

Line 93: Please clarify what is meant by 'actual meteorological conditions'

Line 115 – eq. 1: The equation for CH₄ is missing any biogenic production of CH₄. Furthermore, why is the soil sink of CH₄ considered, but not the photochemical sink? Both are responsible for the CH₄ lifetime.

Line 147: Were wind speeds in different heights also investigate? How could the Ekman spiral have affected your results?

Line 152+ : Why is only R₂ reported? It seems the root mean square difference would be an important measure of model performance here.

Line 159: ad -> and

Line 198: Please clarify why marshy woodlands cannot be a source of biogenic CH₄?

Line 210: What is meant by 'background' here? Do you consider the lowest point per day to be the background or does background refer to a theoretical X_{gas} value without any sources and sinks within Berlin (or a wider region)?

Line 213: 'owning' -> 'owing' – also 'wiggles' seems to be a fairly colloquial choice of words here.

Line 227: The soil sink for CH₄ is significantly smaller the photo-chemical sink – why was soil uptake investigated here?

Line 248: It seems the 10m wind speed is rather a 'performance metric' or 'diagnostic parameter' rather than a 'standard'

Line 251: What were wind conditions in higher levels of the PBL?

Line 268-269: Why is only R₂ given as metric of performance? Root mean squared differences could be important to investigate at all to properly judge the model-

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observation mismatch.

Line 272: Why was this specific time window chosen?

Line 295: Please clarify what is meant by 'concentration fields', I would assume the instrument measures one total column Xgas value for each time step and not a higher dimensional field.

Line 310: Couldn't this also be caused by errors in the spatio-temporal distribution of emission map or other just missing non-anthropogenic sources. Furthermore, the underlying (GFS-driven) wind data is also limited in its resolution in space and time.

Line 329: Please clarify the distinction you make between being 'pivotal' and playing a leading role.

Line 331: Please clarify 'background'. If this refers to a larger scale value without the influence of sources, then this is not surprising. You ignore photo-chemistry and assume only anthropogenic CH4 sources.

Line 345: What is a 'flux framework'?

Line 346: According to the CAMS webpage their CO2 vegetation model is C-Tessel (<https://atmosphere.copernicus.eu/global-production-system>)

Figure 3: Symbols are hard to distinguish

Figure 4: Consider changing label to 'XCO2 enhancement from...'

Figure 6: East Wind > east wind; also please consider adding the administrative boundaries of Berlin or is all of the urban area in the plot Berlin?

Figure 10: The legend is hard to read and curve for the observations are hard to see as well. Also consider adding error bars to the observed values.

Line 460: -> Why 'assessed'?

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-1116>, 2019.

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