Atmospheric
Chemistry
and Physics

Open Access

EGU

Discussions

Interactive comment on "Analysis of Total Column CO$_2$ and CH$_4$ Measurements in Berlin with WRF-GHG" *by* Xinxu Zhao et al.

Anonymous Referee #2

This paper analyzes the potential of the WRF-GHG model to simulate the column-averaged abundances of CO2 and CH4 under urban environments. The precision of such simulations is assessed by comparing to observations carried out in Berlin using portable and low-resolution EM27 spectrometers. The combination of model and observations allow the authors to highlight the potential of using the differential column methodology (DFM) to estimate urban CO2 and CH4 emissions and identify the main driver processes responsible for such emissions. The paper is well structured, but extremely concise and the results are not properly discussed in depth. I suggest this paper might be suitable for publication after general and specific comments listed below are addressed.

General comments:

1) The useful and potential of the differential column methodology (DFM) is investigated to evaluate the urban emissions. It is especially interesting the author's suggestion to eliminate the wind influence on this methodology. But, in general, this promising database (simulations and observations) is poorly exploited by the authors. The discussion and attribution of GHGs sources is very simple, not being supported by robust statistics methods (eg, multivariate analysis,...) and, in some parts of paper, only using one day of observations. Also, I would recommend to the authors to go in depth in this work by providing flux emission estimations from this database, similarly for example to Viatte et al. (2017). Specially, from the observation point of view, it is crucial to analyze the potential of DFM technique to provide actual GHGs flux emissions. Other interesting point would be to assess the improvements of WRF-GHG simulations if the atmospheric GHGs observations from EM27 instruments were assimilated.

2) The precision of the WRF-GHG simulations is assessed by comparing to the EM27 column observations carried out between 23rd June and 11st July 2014 in Berlin. But, the authors use different days depending on what they want to discuss without a precise explanation. For example, the wind comparison is only performed for the period from 1st July to 5th July, the comparison of column-averaged concentrations is carried out for the 3rd and 4th July. But, for the DCM analysis the authors include the 1st and 2nd July, but they rule out the 4th July for section 4.1. However, for section 4.2 the authors use 3rd, 4th, 5th, and 6th July. This limitation on dates also affects the robust of the found conclusions, since the size of the compared database is very limited (for example, the section 4.2 is almost supported by the comparison of one day, analyzing correlations with only ten points). This is very confusing. Why do not the authors use the whole field campaign observations for the wind and concentration comparison? As authors pointed out, to check the DCM approach stable conditions are needed, but these conditions must be justified and supported by the results from this work itself. Therefore, I would recommend to authors to include the whole period of the field campaign for the comparison of the wind and column-averaged concentration fields (sections 2 and 3), which will also support the identification of GHGs sources and

sinks presented in the paper. From these comparison results, the authors could clearly identify the optimal conditions to analyze the DCM approach.

Specific comments

Line 74: The novelties and obtained improvements with regards to the work of Pillai et al. (2016) should be discussed in more detail in the paper.

Line 125: One of the novelties of this work is to provide GHGs high-resolution simulations (1kmx1km). In this sense, it would be nice to discuss the impact of using, for example the EDGAR V4.1 inventory as anthropogenic fluxes at a spatial resolution of 0.1° (about 10 kmx10 km), on the simulated concentrations fields, or how the spatial resolution is treated in the VPRM model.

Line 129: The authors mention that the time factors for the assimilated anthropogenic fluxes could introduce considerable uncertainties. Please, explain more in detail this issue, eg, the value of these uncertainties or how these could affect the GHGs concentration simulations.

Line 152. The field campaign is carried out between 23rd June and 11st July 2014. Why is the period from 1st July to 5th July only used for the wind comparison? Since EM27 observations are performed during day-time, it would be interesting to include the performance analysis for wind comparison distinguishing between day and night-time.

Line 159. Explain possible reasons for the discrepancies between the model and observed wind fields. Maybe the model is not able to capture very fast changes of air masses, which will strongly affect the GHG comparisons.

Line 166. The influence of the EM27 vertical sensitivity is neglected in the model-observation comparison. But, the EM27 averaging kernels show a dependence on the solar geometry, which is considered in similar studies such as Vogel et al. (2018). Please justify more in detail why not to smooth the GHGs simulations (using the EM27

averaging kernels and a priori profile).

Line 176. For the comparison between simulated and observed column-averaged concentrations, the authors limit the analysis to the 3rd and 4th July. But, according to the wind comparison, the 3rd July shows the highest discrepancies between model and simulations. Therefore, observations and simulations could account for different air masses with different GHGs concentrations and influenced by different sources, which should be considered in the discussion. To account for this, the authors could plot the simulated-observed differences as a function of the wind differences. Although the EM27 stations do not coincide with the meteorological stations, figure 2 shows that the study area is very homogenous with regards to wind fields. Thereby, these differences could be a proxy of the model inconsistences.

Adjust better the scales of figure 3 to clearer see the dispersion and distribution of the data, especially for CH4. Now, it is hard to identify what database shows more variability, which could be interesting to know if the simulations are underestimating or overestimating the real CH4 variability.

Line 187-194. Although applying the offset seems to improve the comparison between observations and simulations, the model is not capturing well the observed CH4 variability (R2 is too small), thereby model and observations are not reflecting the same air masses, sources (industries or natural processes). The authors superficially mention the possible influence of the tropopause height in the simulations, but without quantifying this impact. Have the authors considered the possible influence of the PBL? Or the shape of the constant a priori profile used for EM27 retrievals? Please include a more detailed discussion of the possible reasons for these discrepancies.

Line 195-235: As mentioned in the general comments, the discussion and attribution of GHGs sources is very simple. Please consider to include a robust statistics analysis to support the main conclusions of this section.

Line 248. Have the authors analyzed the vertical distribution of the winds within PBL

for the comparisons?

Line 260-289. A plot showing the CH_4 and CO_2 enhancement observed and simulated as a function of the wind directions or differences between simulated and observed wind directions could help to explain better the results of the section 4. Regarding to Figure 7 and 8, why are not the wind fields considered in Figure 7 similarly to Figure 8? Why does not Figure 8 include the 1st July? Why are not the performance values for CO_2 included in the text?

C5