Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-1056-RC2, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulating CH_4 and CO_2 over South and East Asia using the zoomed chemistry transport model LMDzINCA" *by* Xin Lin et al.

Anonymous Referee #2

Received and published: 28 April 2017

This study presents a detailed comparison between CO2 and CH4 simulations from de LMDzINCA model and the available measurements over South East Asia. It is meant as a first step in preparation for flux inversions, to identify observed signals pointing to short-comings in the a priori fluxes or transport model uncertainty, in support of the inversion set-up. To this end a comparison is made between different model versions, and the added value of increased model resolution is assessed. The manuscript is well written, and the difference between model and measurements is carefully assessed. However, there should be a more efficient way to arrive it the main conclusions, e.g. by summarizing the performance in only a few key figures. This would also help to make the final conclusions more quantitative. In its current form, the scientific message is

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Discussion paper

not so clear. In my opinion, publication in ACP would require more than just model performance documentation. Therefore, additional effort is needed to strengthen the scientific significance of this work.

GENERAL COMMENTS

The conclusions describe the performance of the two model versions in qualitative, and sometimes rather vague, terms such as 'generally capable', 'moderately improves', 'fairly well', etc. Some key numbers are needed quantifying the performance, and the significance of performance differences. For example, one would expect improved resolution to pay out more on performance metrics addressing short-term variability, or at sides that are more influenced by small scale variability in the sources and sinks. Different temporal scales are addressed separately, but, to improve the scientific significance, the relation between them could be addressed in further detail.

The idea to compare CO2 and CH4 is interesting, however, it is difficult to compare the model performance between the two. It is like comparing apples and oranges, since the spatio-temporal scales that are influenced by the emissions of these tracers in relation to variations due to transport are so different. It is suggested that the emission uncertainty is more important for CO2 than for CH4, but because of the correlation between flux and transport uncertainties (e.g. the rectifier in case of CO2) it is not possible to really separate these influences. If without this problem, the question remains what it means for the potential of the inversion to contribute to our understanding of the fluxes. The results suggest that this potential is better for CO2, whereas I don't think this can really be objectively quantified just from forward simulations.

I see the value of assessing the benefits of improving model resolution. The trouble here, however, is the limiting vertical resolution. The conclusion that this resolution needs to be improved seems quite obvious to me, to the extent that I even wonder why this was not done from the start. It seems a necessary prerequisite for assessing the benefits of improved model resolution.

Why has the vertical profile comparison to CONTRAIL been limited to CO2. It is true that CH4 was measured only on a small subset of samples, but to include this could nevertheless be important to separate the influence of diurnal variations in emissions and PBL mixing. To me it seems that there is also some unexplored potential comparing diurnal cycle mismatches between CH4 (PBL mixing controlled) and CO2 (PBL mixing and flux variation controlled).

Since the aim was to prepare for inversions, what are the implications of this study for the inversion setup? I mean, the implications that are mention don't seem to have any practical consequences (except for the need for improved vertical resolution).

SPECIFIC COMMENTS

line 163: How is the OH scaling done? A single scaling factor?

line 194: Given the inter-annual variability of biomass burning, wouldn't it be better to use a climatological mean emission distribution for the extrapolated years?

section 2.2: Differences between calibration scales are mentioned but except for AMY CH4 it is not clear how these differences have been accounted for.

line 364: Is this after subtracting longer term components?

line 408: Given the short regional transport times it is unclear how errors in OH could play a role.

line 627: Would the improvements in PBL dynamics that are mentioned work in the right direction?

Table 1: How about the seasonal variation in anthropogenic CH4 emissions? (why are they taken into account for CO2 but not for CH4?). How about the temporal variability of biomass burning. It seems relevant to make use of available information regarding its sub-monthly variability, in particular when assessing the impact of improved resolution is an important goal.

TECHNICAL CORRECTIONS

Line 132: ‘representthe’

Line 612: ‘Here’, where?

S4: Why does the legend show blue colors? It would be better to leave this part out given that positive hotspots are in blue also.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1056, 2017.