Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1137-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "High-resolution inversion of OMI formaldehyde columns to quantify isoprene emission on ecosystem-relevant scales: application to the Southeast US" *by* Jennifer Kaiser et al.

Anonymous Referee #2

Received and published: 3 February 2018

Kaiser et al. use two months (August-September 2013) of formaldehyde column measurements from OMI to infer top-down isoprene emissions over the Southeast US. This region is well known for its high isoprene emissions and has been the subject of a large body of previous studies focusing on the estimation of isoprene fluxes using satellite and/or aircraft observations. Here, by using the adjoint of the GEOS-Chem CTM at high spatial resolution, the authors conclude that (i) the a priori inventory of isoprene emissions is biased high by 40% on average over the Southeast US, as was already pointed out in previous studies, (ii) the a priori inventory is likely too high by a factor of

3 in Central Texas, and (iii) the top-down isoprene estimates improve the model skill to reproduce observations of the SEAC^{4}RS concurrent campaign. The study is interesting, the manuscript is well written and clearly structured. The overall results appear reasonable and are well discussed. I therefore recommend publication to Atmospheric Chemistry and Physics after the following questions are adequately addressed.

General comments :

- There seems to be a contradiction between the GEOS-Chem simulation in the present manuscript and in Zhu et al. (2016), regarding the comparison with the SEAC^{4}RS CAMS data. In Zhu et al. (2016), GEOS-Chem HCHO columns had to be increased by 10% in order to match SEAC^{4}RS CAMS data. This might be partly explained by the 15% reduction of MEGAN isoprene fluxes in that paper. But still, it is very surprising that the GEOS-Chem model is now found to overestimate HCHO by a factor of 1.47 compared to the same CAMS data. I have serious doubts about the fact that the 24% increase in HCHO yield in the $ISOPO_2$+NO reaction can lead to an overestimate of 47%, and if so, this should be demonstrated.
- p.3, l.7 : I suggest to remove 'older chemical mechanisms and' from the sentence. 'Old' is always relative and for example the latest findings in isoprene chemistry (Bates et al. 2016, Teng et al. 2017, etc.) are not considered in the present study.
- In Section 2.2, the effect of soil moisture stress on the bottom-up isoprene fluxes is not discussed. Have you accounted for it in your simulations? The Edwards Plateau in Central Texas is often affected by drought. The strong flux decrease derived in this region might be partly explained by the neglect of soil moisture stress in MEGAN. This warrants some discussion.
- p.6, l.2 : I'm a bit confused here. In Travis (2016), not only mobile NOx emissions are reduced by 60% but all non-power plant sources (or alternatively 30% reduc-

tion of non-power plant sources and no soil NO emissions). In Chan Miller et al. (2017) a decrease of 50% in anthropogenic NOx emissions relative to NEI 2011 is applied. Please clarify what you actually did in the present work and why. Note furthermore that soil NO emissions were found to be too low over the Ozarks by Wolfe et al. (2015).

- p. 6, l. 15 : Still, it would be useful to give percentage estimate of contribution of other NMVOCs to the formaldehyde columns in Southeast US.
- p.7, l. 14 : I don't see what is the motivation of the first sensitivity inversion (with reduced errors). Please explain.
- p.10, l.4 : 50%, do you mean factor of 2 or 1.5?
- Figure 1 : The OMI SAO columns over the SEAC^{4}RS period do not look the same as in Figure 5 of Zhu et al. (2016). What is the difference? Why did you use error-weighted means? Please specify how these means are calculated (relative or absolute).
- Figure 2 : The MEGAN base emission factors shown in the upper left panel should be the same as in Zhu et al. (2016) (both use Hu et al. 2015). They are however about 10 times higher that in Figure 3 of Zhu et al. (2016). Please explain.

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1137, 2017.