

Geosci. Model Dev. Discuss., referee comment RC3  
<https://doi.org/10.5194/gmd-2021-173-RC3>, 2021  
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## Comment on gmd-2021-173

Anonymous Referee #3

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Referee comment on "An improved carbon greenhouse gas simulation in GEOS-Chem version 12.1.1" by Beata Bukosa et al., Geosci. Model Dev. Discuss.,  
<https://doi.org/10.5194/gmd-2021-173-RC3>, 2021

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**Review of the manuscript: "An improved carbon greenhouse gas simulation in GEOS-Chem version 12.1.1" by B. Bukosa et al.**

### General Comments

The authors present coupled greenhouse gas simulations with GEOS-Chem focusing on CO<sub>2</sub>/CH<sub>4</sub>/CO and its global chemical interactions. Such simulations will be definitively needed towards a consistent description of long-term atmospheric chemistry and for realistic assessment of climatic change by earth system models. I fully acknowledge the work done here and I am convinced that the new developments implemented in GEOS-Chem are a big step towards these goals. When reading the abstract, I got the impression that the paper follows a clear outline by first comparing coupled and uncoupled simulations and then evaluate the new model version with observational data. The authors present a sound and informative introduction to the scientific and computational problem of consistent chemistry simulations, which comprise processes representing a broad range of timescales as well as trends and interannual variability. I also liked the detailed budget term quantification as presented in Table 2. Unfortunately, I got lost after different versions of the OH input fields were introduced. For me it remained unclear why the authors are not able to stick to a single OH field which then can be used for both, the uncoupled and the coupled simulations. In the discussion part of the manuscript, the introduction of a third simulation (coupled-origOH) lead to unnecessary confusion and an overload with information details which made it hard for me to extract the major conclusions. Overall, I would not recommend to publish the paper in its current form but I encourage the authors to submit a revised manuscript based on more consistent coupled and uncoupled simulations.

## Specific Comments

Line 49: What do you mean by "outside source regions"?

Lines 49-56: For CO budget terms you can also refer to Stein et al. (2014).

Line 51: Publication year is missing.

Lines 52-54: It would be interesting to see also the numbers for the chemical production by NMVOCs (used as input for your simulations).

Lines 60-63: Can you give a reference here?

Lines 82-84: Is the spin-up time sufficient? You doubt this later on (Lines 506-508).

Lines 96-97: I would expect that a single reference full chemistry simulation is used for all simulations presented here.

Line 134: How does GEOS-Chem handle Biomass burning emissions? It is known that such emissions need to be emitted throughout the troposphere following a vertical profile.

Line 295: Exchange "tropospheric column" by "mid troposphere".

Lines 348-349: I would expect to have the same P(NMVOC) for all model runs. Give numbers!

Table 1: Publication year is missing.

Figure 1: I like this figure. It could even improve if you orient your coupled and uncoupled flows from left to right.

Figure 4: From your description I would expect that  $P(\text{CO})_{\text{CH}_4}$  (for all years) and  $P(\text{CO}_2)$  (for 2010-2017) remains exactly constant for the uncoupled runs (except for leap years).

Figure 7(b): This reads like "Surface Loss" as parameter.

### **References:**

Stein, O., Schultz, M. G., Bouarar, I., Clark, H., Huijnen, V., Gaudel, A., George, M., and Clerbaux, C.: On the wintertime low bias of Northern Hemisphere carbon monoxide found in global model simulations, *Atmos. Chem. Phys.*, 14, 9295-9316, doi:10.5194/acp-14-9295-2014, 2014.