Reply to reviewer #2

Catherine Acquah[1], Laura Stecher[1,a], Mariano Mertens[1,2], and Patrick Jöckel[1]

[1]Deutsches Zentrum für Luft- und Raumfahrt, Institut für Physik der Atmosphäre, Oberpfaffenhofen, Germany
[2]Faculty of Aerospace Engineering, Section Operations and Environment, Delft University of Technology, 2629 HS, Delft, The Netherlands
[a]now at: Yusuf Hamied Department of Chemistry, University of Cambridge, Cambridge, CB2 1EW, United Kingdom

We thank reviewer #2 for her/his comments and the evaluation of our paper. Below, we repeat each comment (in blue) and address it (in black). Changes of the manuscript are written in italics.

The manuscript "Effects of different emission inventories on tropospheric ozone and methane lifetime" by Acquah et al. presents a modeling study of the effects of differing emissions inventories on metrics of climate and air quality interest, namely, tropospheric ozone and hydroxyl radical (OH) burdens and the resulting methane lifetime. Based on two simulations performed by the EMAC model for two different phases of the Chemistry Climate Model Initiative (CCMI), each with its own prescribed emissions inventories, the authors observed significant differences in the metrics listed above and go on to evaluate the sectors (industrial, land transport, shipping, etc.) driving those differences using a tagging approach. Tropospheric ozone is more abundant in the simulation using more up-to-date emissions and methane lifetime is shorter in both hemispheres. Some budget closure calculations are also performed to understand the sectoral contributions for methane lifetime, and burden efficiency calculations are performed to understand the influence of nitrogen oxides (NOx) emissions changes specifically on ozone burden in a given sector.

Overall I find the presented analysis to be reasonably thorough for a model-focused study, well polished, and clear in its conclusions and methods. The findings are not "ground-breaking" but are instead, in my mind, an important contribution by quantifying and systematically seeking to understand a phenomenon that modeling groups often know is happening but is not always well characterized. By describing in detail the changes in tropospheric ozone, OH, and methane lifetime that might be expected from a routine update or switch of emissions inventories, the authors are assisting the modeling community and those seeking to constrain present-day and historical values of these quantities across models and through time. The authors are clear about the limitations of the study and also do a good job of citing past work and putting into context some of the specific changes in emissions sectors and species. I offer some minor comments for clarification, readability, and correction, but otherwise regard this manuscript as a strong candidate for publication in ACP.

Thank you very much for this positive feedback!

1 Minor comments

L37: In this paragraph, sources of precursors are described but seem to me incomplete – first there's anthropogenic NOx, then natural NOx. Then anthropogenic CO only, and natural NMHC only. Why not also include natural CO (biomass burning, to the degree it is natural, and from natural methane, I presume?), and anthropogenic NMHC (industry)?

We added the following information in the revised manuscript:

line 36 (of the preprint): *Natural emissions of* CO *occur during the combustion of biomass, e.g. lightning induced fires (Zheng and Zhao, 2019), and from the oxidation of hydrocarbons, but also from plants and the oceans (Khalil and Rasmussen, 1990).*

line 37 (of the preprint): *The main anthropogenic sources of NMHC include the incomplete combustion of fossil fuels, petroleum from geological reservoirs, and the distillation and distribution of oil and gas products (Pozzer et al., 2010).*

L47: For this sentence, if the authors deem a citation helpful, Duncan et al., 2024 might be an appropriate reference, as it discusses the infeasibility of global observations of OH:

Duncan, B. N., et al.: Opinion: Beyond global means – novel space-based approaches to indirectly constrain the concentrations of and trends and variations in the tropospheric hydroxyl radical (OH), Atmos. Chem. Phys., 24, 13001–13023, https://doi.org/10.5194/acp-24-13001-2024, 2024.

Thank you, we added the citation as suggested.

L122: I'm not familiar with this phrase, "binary identical". I don't see how meteorology can be binary, but are met-dependent emissions binary, as in, either on or off? Please clarify or remove "binary".

We think that there is a misunderstanding in the understanding of "binary identical" here. What we want to express is that due to the use of the QCTM mode (see manuscript), differences between the emission inventories that lead to differences in the chemical composition do not affect the meteorology, not even numerically. Therefore, the simulated meteorological quantities are "binary identical" in all simulations. In other words there are no numerical differences between both simulations, and therefore the online calculated emissions that depend on the meteorology are exactly the same in both simulations.

Previous version in preprint:

l. 116 (of preprint): *This assures that differences of the emission inventories prescribed in the two simulations do not affect the model's simulated meteorology.*

l. 122 (of preprint): *Due to the binary identical meteorology between the simulations, these online calculated, meteorology dependent emissions are also binary identical in both simulations.*

Updated to avoid binary identical:

l. 116 (of preprint): *This assures that differences of the emission inventories prescribed in the two simulations do not affect the model's simulated meteorology, not even numerically.*

l. 122 (of preprint): *Due to the identically simulated meteorology in all simulations due to the QCTM mode, these online calcu-*

lated, meteorology dependent emissions are exactly the same in both simulations with no differences, not even numerical noise.

L150: For Figs. S6, S7, and S8, it would be helpful to include in legend that solid lines represent EMIS-01, dashed lines represent EMIS-02 instead of it being buried in one of the captions.

We have updated Figures S6, S7 and S8 in the supplement accordingly.

2 Technical corrections

- L28: Not sure that citing Seinfeld and Pandis twice in this sentence is necessary; once at the end would convey the same attribution, in my mind. Removed in the middle of the sentence.
- L60: Should this state "CCMI-2022" rather than "CCMI-2"? Corrected.
- L74: "targeted" misspelled Corrected.
- L150: In Fig. S8, "anthropogenic" is misspelled in legend. Corrected.
- L151: 1010 should be 2010 Corrected
- L261: Remove "hemisphere," redundant Corrected
- L271: Fig. S12 y-axis label has misspelling of "efficiency" Corrected.
- L286: Fig. 4 y-axis label "efficiency" misspelled Corrected.
- L334: One of these EMIS-01's should be an EMIS-02 I presume; directionality dependent on whether "CH4," as stated, or "CH4 lifetime" is the quantity being compared. We have corrected it, and we meant "CH4 lifetime".
- L413: Both instances of "from EMIS-01 to EMIS-01" in this sentence should instead state "from EMIS-01 to EMIS-02" I believe Yes, you are right. Corrected.
- L433: remove one "that" Corrected.
- L452: "available" misspelled Corrected.

References

Khalil, M. and Rasmussen, R.: The global cycle of carbon monoxide: Trends and mass balance, Chemosphere, 20, 227–242, https://doi.org/10.1016/0045-6535(90)90098-E, 1990.

Pozzer, A., Pollmann, J., Taraborrelli, D., Jöckel, P., Helmig, D., Tans, P., Hueber, J., and Lelieveld, J.: Observed and simulated global distribution and budget of atmospheric C_2-C_5 alkanes, Atmospheric Chemistry and Physics, 10, 4403–4422, https://doi.org/10.5194/acp-10-4403-2010, 2010.

Zheng, B., C. F. Y. Y. C. P. F.-C. A. D. M. N. P. R. J. W. Y. W. H. M. and Zhao, Y.: Global atmospheric carbon monoxide budget 2000– 2017 inferred from multi-species atmospheric inversions, Earth Syst. Sci. Data, 11, 1411–1436, https://doi.org/https://doi.org/10.5194/essd-11-1411-2019, 2019.