# Reply to reviewer #1

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We thank reviewer #1 for her/his comments and the evaluation of our paper. Below, we repeat each comment (in blue) and address it (in black). Changes of text in the manuscript are written in italics.

The manuscript "Effects of different emission inventories on tropospheric ozone and methane lifetime" investigates the influence of ozone precursor emissions on model simulations of ozone concentrations and methane lifetime. The authors applied a tagging approach to attribute differences in these variables to specific emission sectors. The manuscript is well-written, and the results provide valuable insights into inter-model differences in tropospheric ozone and OH concentrations. I recommend the manuscript for publication after addressing the following minor comments:

Thank you very much for this positive evaluation!

L120: Are there other non-methane volatile organic compounds included in the biogenic emissions besides C5H8

For the other NMHCs of biogenic origin a prescribed climatology from GEIA is used, which is the same in all simulations. We added the following information:

*In addition to the online calculated natural emissions, a climatology of biogenic emissions of NMHCs and* CO *is prescribed from the Global Emissions InitiAtive (GEIA) in all simulations.*

Line 150 and Table 3: It would be beneficial to include both absolute and relative differences in emissions between the EMIS-01 and EMIS-02 simulations. This would provide a clearer comparison of the two emission inventories.

We have included the relative differences in the Table of the revised manuscript.

Section 2.2.3 The TAGGING submodel is used to attribute O3 production and OH mixing ratios to different emission sectors. However, given the highly nonlinear chemistry of O3 and OH, a more detailed explanation of how the TAGGING method attributes O3 and OH to individual emissions sectors would enhance the reader's understanding of the results.

We are hesitating to provide a lot more details here, since they can hardly be complete, without duplicating the entire paper by Grewe et al. (2017). Nevertheless, we reformulated large parts of the section, now also explicitly stating that the non-linearity of the ozone chemistry is taken into account by the tagging method.

In equation (9), are there other ozone precursors (e.g. CO, NMVOC) also contributing to B(O3)? Are the differences in burden efficiency influenced by other ozone precursors?

Indeed, the burden efficiency is calculated only with respect to $NO_x$ emissions. Especially the biogenic sector has a lot of VOC emissions, but lower $NO_x$ emissions (due to soil-$NO_x$) emissions. The VOC emissions also contribute to ozone formation, which can lead to a larger burden efficiency with respect to $NO_x$ emissions compared to anthropogenic sectors with rather large $NO_x$, but lower VOC emissions. Since we only use this for comparing the results between the two simulations (but not for assessing the role of individual sectors), we prefer to not further detail this in the text.

The authors use two different methods to calculate changes in CH4 lifetime attributable to individual emission sources. While the two methods yield similar results for most sectors, they show divergent results for the land transportation sector (in Figure 7, method 1 indicates a large negative contribution, while method 2 shows a positive contribution). This discrepancy warrants further explanation. I recommend that the authors calculate the global tropospheric CH4 reaction-weighted OH concentrations contributed by each emission sector and simulation. This would provide a clearer understanding of how emissions influence CH4 lifetime.

We are not sure what is meant by "(in Figure 7, method 1 indicates a large negative contribution, while method 2 shows a positive contribution". Could it be that the two methods (indicated by red and blue bars) and the separation into SH and NH (shown in panel a and b) have been confused by the reviewer? Figure 7 shows that the contribution of the land traffic sector to the $CH_4$ lifetime change estimated by **both methods is negative in the NH**, whereas **both methods indicate a positive contribution in the SH**.

We followed the suggestion and added a barplot showing the tropospheric OH of individual tagging categories weighted by the reaction with $CH_4$ (following Lawrence et al. (2001)) to the revised manuscript. The plot shows the global mean OH, as well as OH in both hemispheres separately.

L354 “the CH4 reduction from EMIS-01 to EMIS-01 in the NH”. I think it should be “the CH4 lifetime reduction from EMIS-01 to EMIS-02 in the NH”.

You are right. Thank you for pointing this out. We corrected it in the revised manuscript.

## References

Grewe, V., Tsati, E., Mertens, M., Fromming, C., and Jockel, P.: Contribution of emissions to concentrations: the TAGGING 1.0 submodel based on the Modular Earth Submodel System (MESSy 2.52), Geoscientific Model Development, 10, 2615–2633, https://doi.org/10.5194/gmd-10-2615-2017, 2017.

Lawrence, M. G., Jöckel, and von Kuhlmann, R.: What does the global mean OH concentration tell us?, Atmos. Chem. Phys., 1, 37–49, https://doi.org/10.5194/acp-1-37-2001, 2001.