We thank the reviewers for their time, valuable comments, and constructive feedback. Incorporating these suggestions has led to significant revisions in the manuscript, including the addition of 4 new figures. For clarity, our point-by-point responses are highlighted in blue text throughout this document.

RC2: 'Comment on egusphere-2026-4883', Anonymous Referee #2, 09 Sep 2026

This is an interesting, well written study examining the extent to which OH variability influences methane growth rate over 2003-2024 and the underlying drivers of the OH variability. Gaubert et al. run 8 different model simulations, with different setups for methane (prescribed vs. interactive), emissions of other reactive gases and aerosols, scaling's of CO emissions, and meteorological nudging datasets. They examine how the year-on-year change in OH relates to the year-on-year methane change and the drivers of this OH change.

Changing the emission inventories (10%) and changing the meteorological nudging datasets (8%) had the largest impacts on OH.

Overall, this paper presents a useful addition to the field with a rigorous evaluation against observations and well explained decomposition of the drivers, and I would encourage its acceptance once the authors have responded to my comments below.

Major comments

The meteorology plays an important role, via its impact on BVOC emissions and lightning NO_x. Can the authors give any indication as to which re-analysis dataset is considered more accurate, particularly in relation to the variables in the locations which affect the aforementioned processes?

Unfortunately, we cannot provide a conclusive answer at this point, as there are too many parameters and variables to verify to make this evaluation short enough to fit in this study. The model includes many parameterizations that are themselves uncertain and respond interactively to nudging, for instance, the advection schemes, the physics (clouds, convection), as well as the BVOC and lightning parameterizations. Moreover, the reanalyses have errors that vary in space and time. We can add that the nudging itself is uncertain, with parameters that are often arbitrarily defined.

All of these factors make the attribution of errors to a single process or a given reanalysis dataset to be challenging. In particular, we believe that the nudging can help us understand errors about our Community Atmosphere Model rather than the reanalysis themselves. In future work, we are planning to focus on certain parameterization (convection, BVOC), and to evaluate them with an independent and appropriate set of observations.

We added to the conclusions (around line 662):

"*To better constrain which reanalysis dataset represents these processes more realistically, future work will focus on evaluating how key parameterizations (e.g., convection, biogenic emissions) respond to nudging. We will evaluate these simulations against independent observational datasets such as aircraft field campaigns and satellite observations.*"

I also note that the emissions of isoprene quoted are on the high end of current estimates (e.g. 389 TgC/yr from CAMS-GLOB-BIO v3.1 Sindelarova et al., 2022) and this should be noted.

This is already noted in the manuscript in section 2.1, around line 170:

"*Our isoprene emission estimates are higher than recent TROPOMI inversion (Sfendla et al., 2026) and of the CAMS-GLOB-BIOv3.1 dataset, who estimated isoprene emissions to be 389 Tg C yr−1 for 2000-2019 (Sindelarova et al., 2022). However, it is well within the range of the most recent studies that derived isoprene emissions using Cross-track infrared sounder (CrIS) isoprene retrievals, with 456 ± 238 TgC yr−1 over 2013–2020 period (Li et al., 2025) and 628–711 Tg C yr−1 for 2019-2020 (Yoon et al., 2025).*"

We added to the conclusions (line 651):

"*Satellite observations of isoprene (Wells et al., 2020) have the potential to constrain the magnitude as well as the temporal and spatial distribution of the isoprene emissions (Choi et al., 2025; Li et al., 2025; Yoon et al., 2025, 2026). However, these top-down estimates currently diverge, even for the global and annual total fluxes, and some, including ours, tend to be higher than a bottom-up dataset (Sindelarova et al., 2022).*"

My understanding is that Mozart T1 schemes already includes quite comprehensive isoprene chemistry, as well as the newly added SLH mentioned by the authors. Could the authors comment further on how the T1 scheme differs from the MOM scheme in the context of OH recycling?

The MOZART T1-SLH mechanism is a heavily lumped representation of isoprene chemistry and is only attempting to represent schemes like MOM with a few species and reactions.

In T1, Isoprene oxidation can ultimately form C5-hydroperoxyaldehydes (HPALDs), and the HPALD photolysis is included in the model. There are other recycling mechanisms such as the ozonolysis channel, two ISOPOOH channels (photolysis and reaction with OH) as well as other lumped species XO2/XOOH that can recycle OH.

We found that species were missing in the VOC contribution to the OH source, and our total is now around 6 %, which is still small compared to the value estimated in Lelieveld et al., 2016.

We added to the section on the OH budget (line 426):

"*This is likely driven by the more explicit representation of OH recycling pathways in MOM, whereas MOZART-T1 represents the products of the OH + isoprene reaction with only a few lumped species.*"

And in the conclusions (line 656):

"*Regarding CESM, future work should focus on the integration of an improved organic chemistry mechanism (TS2) for isoprene and monoterpenes (Schwantes et al., 2020, Mirrezaei et al., 2026) with the SLH updates used here.*"

The CT-CH4-2025 emissions are themselves derived assuming a prescribed OH field, as the authors note. How does this affects the claim that the methane emissions are not a major driver of OH variability?

We have an experiment where the CH4 emissions are constant (CESM-CH4-2003), we just repeat the emissions from 2003 as derived from CT-CH4-2025 for twenty years (2003 to 2022), the CH_4 burden is reduced by 1.5 % (compare to CESM-CH4) and changes the mean OH by 0.4 % (Table 2). This is despite an important change in CH4 emission flux strength.

Since this simulation does not have an interannual variability in CH4 emissions, it shows that the lack of interannual variability in CT-CH4-2025 due to a prescribed OH field is likely to have only a small impact on OH itself. Instead, interannual variation of CH4 emissions impact the CH4 growth rate significantly. Therefore, a better representation of OH spatial and temporal variability in methane flux inversions will have a strong impact on the derived CH4 emissions. Note that there is no need to use a complicated model to assess the first order impact of CH4 on OH. A growth rate of say 5 ppb still remains small when the background is 1800 ppb and when this background only contributes to 13 % of the OH loss.

We further found that CH_4 emissions contributions were negligible in our loss-weighted regression.

We want to emphasize that this is because of the slow methane chemistry and the relatively slow changes in methane burden between years. When a longer time period is considered,

the methane impact becomes important, and it is likely that even with high recycling probability, the decadal trends in methane had an important effect on OH.

I appreciate single model studies allow the researchers to examine the response in more detail compared to multi-model studies where there are fewer common variables between models. Nevertheless, it would be good for the reviewers to highlight that the results presented here are from a single model with a single chemical scheme and that comparable experiments in other models would be useful in assessing the robustness of the findings.

We had a section in the conclusion (line 695):

"*This study highlights that further work is needed to reduce OH uncertainties, which will be critical to improve estimates of the CH4 budget, but also for the Earth's tropospheric chemistry system as a whole.*"

We added at the end (line 702):

"*Finally, a model intercomparison project for methane emission-driven simulations with interactive atmospheric chemistry is essential to quantify the spread in tropospheric oxidant feedbacks, improved OH and short-lived halogen chemistry representations, and methane lifetime estimates across Earth system models."*

Minor points

In figure 3, as panels (a)-(f) relate to a range of gases, please change legend's black line to read "observations" not "CO (ppb)".

We updated the figure.

In figures 10 and 11, why in places is the net line clear from the individual contributions (e.g., 2006 in panel (d)). I would have thought that, at most, I could sit on the maximum of the stacked contributions.

We are thankful for the catch. In figure 10, We updated the figure, the plotted channels sum to the net line.

But for Figure 11 and 12, the separation is expected and physical. The black line is the simulated OH anomaly and the dashed line is the regression reconstruction, that is the sum of the colored bands.

What are the C1 and C2 VOCs in Figure 10?

They are OH loss channels grouped by carbon number, following the same family definition of Lelieveld et al. (2016) that we use for the OH budget of Fig. 8 (Sect. 4). CH_4 and CO are kept as separate channels. C1 VOC is the single-carbon organics other than methane and CO and C2+ VOC is all the volatile organic compounds with two or more carbon atoms.