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.

RC1: 'Comment on egusphere-2026-4883', Anonymous Referee #1, 04 Sep 2026

In Gaubert et al., the authors use multiple simulations of the CESM model, with different meteorology and emissions, to understand relationships between OH and the methane growth rate, as well as the drivers of interannual variability in OH. The paper is suitable for publication in ACP once the below comments are addressed:

General Comments:

The paper could be strengthened greatly by re-writing for clarity. The enumerated main conclusions in the final section are important and definitely supported by the work, but I'm not sure I would have realized those were the major points you were trying to emphasize. I would consider reducing some unnecessary text (there are many paragraphs that are simply repeating numbers in tables and figures) and reorganizing some paragraphs (e.g., the paragraph starting in line 418 is about 1.5 pages in length) to try to improve clarity. There are also, not infrequently, grammatical errors that need to be corrected.

We thank the reviewer for this constructive feedback and actionable suggestions to improve the readability of our manuscript. We have extensively revised the manuscript for clarity and conciseness. The manuscript has undergone a thorough review and editing process to correct grammatical errors, awkward phrasing, and typos throughout. Regarding this comment and the other comments below, please have a look at the new manuscript and the tracked change version as significant changes were made.

Your focus is primarily on globally averaged statistics, which for CH4 is appropriate, but for CO, which is much shorter lived, and the OH budget, which can vary substantially in the horizontal and vertical, some discussion of their spatial heterogeneity is warranted. How does the relative importance of the different OH budget terms vary spatially? Does looking at your results from a more regional perspective change the results in Figures 10 & 8 significantly?

We added a figure of the spatial distribution of the CO budget terms (new Fig. 8) and another figure that includes an evaluation of the spatial distribution of the CO bias. We

added a comparison with the scaled Spivakoski OH to evaluate the spatial distribution of the OH bias (new Fig. 11). Finally we added maps detailing the tropospheric OH sources and sinks totals, which in turn are broken down by channels. These results highlight the role of fire CO emissions in the CESM-CH4 simulations. The fires in this simulation are from FINN2.5, which estimates higher emissions, especially in the tropics, and there are strong negative trends (globally). Beyond the well documented overestimation in the tropics, it is important to note that the tropical fires play an important role in regulating the OH budget and therefore the methane loss.

The manuscript has been extensively revised to include discussions in several sections, including the conclusions.

Specific Comments:

Figure 1 caption: I assume the text at the end starting with “At the bottom right,…” is from a previous version of the figure. If not, your meaning here is unclear and the text should be changed.

Thank you. We updated the final sentence as follows to better describe the schematic (figure page 5):

“*On the right, CH_4 emissions show the source of atmospheric methane, while oxidation by OH provides its dominant atmospheric loss. The bidirectional coupling between CH_4 and OH indicates that changes in OH affect CH_4 abundance, while changes in CH_4 modify the OH sink. These interactions, together with short-lived halogen (SLH) chemistry and the broader reactive chemical system, regulate atmospheric oxidative capacity and CH_4 abundance.*”

Line 163: should be “emissions from vegetation are estimated…” not “is estimated”.

Corrected.

Figure 2, panel g: Some discussion about the cause of the dramatic difference in lightning NOx between the ERA and MERRA2 datasets starting in 2020 is warranted, particularly given results you discuss in Section 6. Do you know what caused the sudden drop in the ERA emissions? Do you have any indication which is likely to be correct?

We have not identified the exact reasons for the lightning NOx discrepancies, although the formulation implemented in CESM2 depends on cloud-top height, which changes between meteorological datasets. We believe understanding the exact reasons are beyond the scope of this study. We have stated this as future work in the conclusions.

We added around line 520:

"*The nudging to ERA-5 leads to lower lightning in East Africa and across South America (except for the Amazon basin) and across the southern hemisphere while ERA-5 leads to higher lightning in India, Indonesia and parts of West Africa and the Amazon basin (Fig. S3).*"

The 2022 East African drought was analyzed by Funk et al., (2023). They found no significant differences between ERA-5 and MERRA-2 during the 2022 East African drought.

Reference: Funk, C., Fink, A. H., Harrison, L., Segele, Z., Endris, H. S., Galu, G., et al. (2023). Frequent but predictable droughts in East Africa driven by a Walker circulation intensification. Earth's Future, 11, e2022EF003454. https://doi.org/10.1029/2022EF003454

Section 2.2: It would also be worthwhile to try and evaluate the accuracy of your modeled OH, since you talk about the OH budget later on and any extrapolations about the impacts of OH on CO and CH4 in the actual atmosphere depend on your modeled OH accuracy. While obviously a more difficult comparison given the diurnal variability in OH, at the very least, showing that your modeled spatial and temporal variability agree with in situ observations (e.g. from ATom or KORUS-AQ) or inversion derived trends within reason would further strengthen your argument.

Given the short lifetime of OH and the lack of observational constraints, it is quite difficult to benchmark modelled OH performance. However, we agree our goal is to better represent OH at multiple scales across the Earth System.

Our modelled OH is evaluated using the methane lifetime as a metric of global bias with respect to methyl chloroform inversion.

In order to further evaluate the OH distribution, we added a comparison with ATom OH in the supplement. We also added a comparison with the scaled Spivakoski OH (new Fig. 11). In short, the simulated OH shows good performance for the best two simulations (as already indicated by the methane lifetime estimates).

Figure 4: I'm not sure the coloring of your boxes is particularly helpful. I found it somewhat confusing. Maybe you could just consider, putting in bold, values above/below a certain threshold?

Thank you. We removed the box colors and now highlight key values in bold.

2018-05 – 2022-12

(a) TROPOMI XCH_4

				NDACC FTIR		
	Bias (%)	uRMSE (%)	r	Bias (%)	uRMSE (%)	r
CESM	+1.9	1.17	0.93	+1.8	**2.83**	**0.99**
CESM-CH4	**+0.4**	**1.01**	**0.95**	**+0.2**	**2.68**	**0.99**
CESM-CH4-CMIP7	-6.0	**0.98**	**0.95**	-6.1	**2.78**	**0.99**
CESM-CH4-CMIP7-CO	-4.6	**0.98**	**0.95**	-4.7	**2.72**	**0.99**
CESM-CH4-CMIP7-ERA5	**-0.5**	1.17	0.93	**-1.0**	**2.73**	**0.99**
CESM-CH4-CMIP7-CO-ERA5	**+0.9**	1.17	0.93	**+0.5**	**2.75**	**0.99**

2010-01 – 2021-12

(b) GOSAT XCH_4

				NDACC FTIR		
	Bias (%)	uRMSE (%)	r	Bias (%)	uRMSE (%)	r
CESM	**-0.3**	1.26	0.89	**+0.2**	2.78	**0.99**
CESM-CH4	**-0.7**	**0.74**	**0.95**	**-0.2**	**2.52**	**0.99**
CESM-CH4-CMIP7	-6.2	0.96	**0.94**	-5.6	**2.76**	**0.99**
CESM-CH4-CMIP7-CO	-4.9	0.87	**0.95**	-4.3	**2.67**	**0.99**
CESM-CH4-CMIP7-ERA5	-1.9	0.82	**0.94**	-1.3	**2.56**	**0.99**
CESM-CH4-CMIP7-CO-ERA5	**-0.6**	0.84	**0.93**	**+0.0**	**2.54**	**0.99**

2018-05 – 2022-12

(c) TROPOMI XCO

				NDACC FTIR		
	Bias (%)	uRMSE (%)	r	Bias (%)	uRMSE (%)	r
CESM	**+0.4**	**16.26**	0.86	**-0.7**	**14.62**	**0.88**
CESM-CH4	**-0.2**	**16.28**	0.85	-1.2	**14.64**	**0.88**
CESM-CH4-CMIP7	-24.6	17.26	0.86	-26.1	15.60	0.86
CESM-CH4-CMIP7-CO	-19.5	**14.97**	**0.90**	-20.1	**13.58**	**0.89**
CESM-CH4-CMIP7-ERA5	-12.0	18.32	0.81	-15.5	16.97	0.82
CESM-CI14-CMIP7-CO-ERA5	-6.3	**16.04**	0.86	-9.0	15.04	0.86

2003-01 – 2022-12

(d) MOPITT XCO

				NDACC FTIR		
	Bias (%)	uRMSE (%)	r	Bias (%)	uRMSE (%)	r
CESM	+3.0	**15.65**	**0.88**	**-0.6**	15.35	0.89
CESM-CH4	+2.9	**15.68**	**0.88**	**-0.8**	15.37	0.89
CESM-CH4-CMIP7	-19.8	20.18	0.79	-25.2	16.42	0.89
CESM-CH4-CMIP7-CO	-13.3	**16.57**	**0.86**	-18.3	**13.84**	**0.91**
CESM-CH4-CMIP7-ERA5	-10.2	18.81	0.82	-16.5	17.10	0.86
CESM-CH4-CMIP7-CO-ERA5	-3.9	**15.38**	**0.88**	-8.8	**14.39**	**0.90**

Section 3.2: Is there any spatial or temporal variability in these statistics? Numbers averaged globally can hide all sorts of issues, particularly with something like bias where you can have areas of large negative and positive bias that cancel each other out. Adding a map or two would be helpful (even as a supplementary figure).

We added maps of the spatial distribution of the bias for CH4 (GOSAT and TROPOMI) and CO (TROPOMI and MOPITT). It is helping to assess the zonal distribution of the CO budget and the important role of anthropogenic underestimations (northern hemisphere) and fire overestimations across the tropics.

Line 330: “Despite different OH…”. It would be useful to have maps showing the climatological mean OH for your different simulations so readers can understand the spatial variability in OH in the different models. The spatial variability, while potentially not very important for CH4 given its long lifetime, would be important for CO, which has a much shorter lifetime.

Thank you for the suggestion. We have added climatological mean OH maps and their comparison with Spivakoski et al., (2000). We also added a breakdown of the OH budget for the two best simulations.

Line 418: Sentence starting with “Tropospheric OH generally follows…” needs a citation.

The whole section has been rewritten (line 485 of the new manuscript), but this sentence specifically is now:

"*The interannual variability in tropospheric OH exhibits a negative correlation with the ENSO (e.g., Turner et al., 2018; Patra et al., 2021; Anderson et al., 2021), with enhanced OH during La Niña (Murray et al., 2013) and reduced OH during El Niño (Prinn et al., 2001, 2005; Duncan et al., 2003).*"

Line 427: “…, confirming that interannual variability is controlled almost entirely by OH chemistry”. Do you mean IAV in the loss of CH4 or in the CH4 growth rate?

We mean IAV in the loss of CH4, we added "sink": "*confirming that interannual variability of the CH_4 sink is controlled almost entirely by OH chemistry.*"

Line 469: Does this methodology take into account cross-correlations between the different terms, for example surface NOx and anthropogenic CO emissions? If not, how will that affect your conclusions, as I don’t think it would be appropriate to add the various contributions since they aren’t independent.

The method accounts for cross-correlations by fitting all predictors of each basis jointly within each latitude band. so their cross-correlations enter the least-squares solution, and each coefficient is conditional on the other predictors. The fitted terms are then combined using regional CH_4 by loss weights (Text S1, Fig. S9). The bands in (new) Figs. 14–15 therefore sum to the reconstructed OH anomaly without requiring independence. Their variance shares are calculated from covariance with OH, rather than by adding individual variances (Text S2)..

Our purpose in Sect. 6 is to interpret the OH anomalies alongside the production and loss pathways from the model chemical budgets. Related studies also retain correlated predictors (Strode et al., 2026) or combine chemical drivers through a steady-state relationship (Murray et al., 2021).

We have added diagnostics to examine this distinction. We assessed collinearity within each regional regression using Variance Inflation Factors (VIF),

$VIF_j = 1 / (1 - R_j^2)$

where (R_j) is obtained by regressing driver (j) on all the remaining drivers, with an intercept and using the same monthly anomalies. VIF quantifies how correlations with the other predictors inflate the variance of the estimated coefficient. For the globally aggregated forcing predictors, VIFs reach approximately 3.8, and surface NOx and anthropogenic CO correlations are −0.07 and 0.56 in the two configurations (Fig. R1; Fig. S13 in the revised Supplement). The regional correlations are sometimes much higher: this pair reaches 0.84 and 0.97 in the northern extratropics (Fig. R2; Fig. S14).

The regional weighting helps explain the implications for our conclusions. The tropical bands carry approximately 74% of the CH4 loss with OH weight (Fig. S9). Most of the isoprene and lightning-NO covariance contributions are from the tropical bands (Fig. R4; Fig. S16). These predictors have tropical VIFs of about 1.3–2.0 (Fig. R3; Fig. S15). In the ERA5 configuration, about 69% of the surface-NOx covariance contribution comes from the northern tropics (Fig. R4; Fig. S16), where its VIF is 1.8 (Fig. R3; Fig. S15) and its correlation with anthropogenic CO is 0.44 (Fig. R2; Fig. S14). These leading contributions therefore exist mainly where the corresponding predictors have relatively little collinearity. The weighting does not remove collinearity; it shows that much of these associations originate where the relevant predictors are less collinear.

We added to the text (around lines 540-550 of the new manuscript) :

"*These regressions are used to explore the drivers of OH anomalies alongside the production and loss pathways diagnosed from the model budget. All predictors within each basis are fitted jointly in each region, so adding their fitted terms to reconstruct the OH anomaly does not require independence. However, correlated predictors cannot be fully separated and their terms are conditional associations rather than independent contributions (Figs.S13 and S14). We quantify this with the variance inflation factor,* $VIF_j = 1/(1 - R_j^2)$, *where* R_j^2 *is obtained by regressing predictor* j *on the other predictors of the same band (VIF* $= 1$ *if uncorrelated; Figs.S13 and S15). The leading contributions (lightning NO, isoprene and, in CESM-CH4-CMIP7-CO-ERA5, surface NOx) come mainly from the tropical bands (Fig.~S16). Where predictors share anthropogenic or fire sources, such as surface NOx and CO, their strong correlation in some bands increases the uncertainty of their individual contributions, which are therefore better interpreted as a combined emission contribution (Figs.S14 and S15).*"

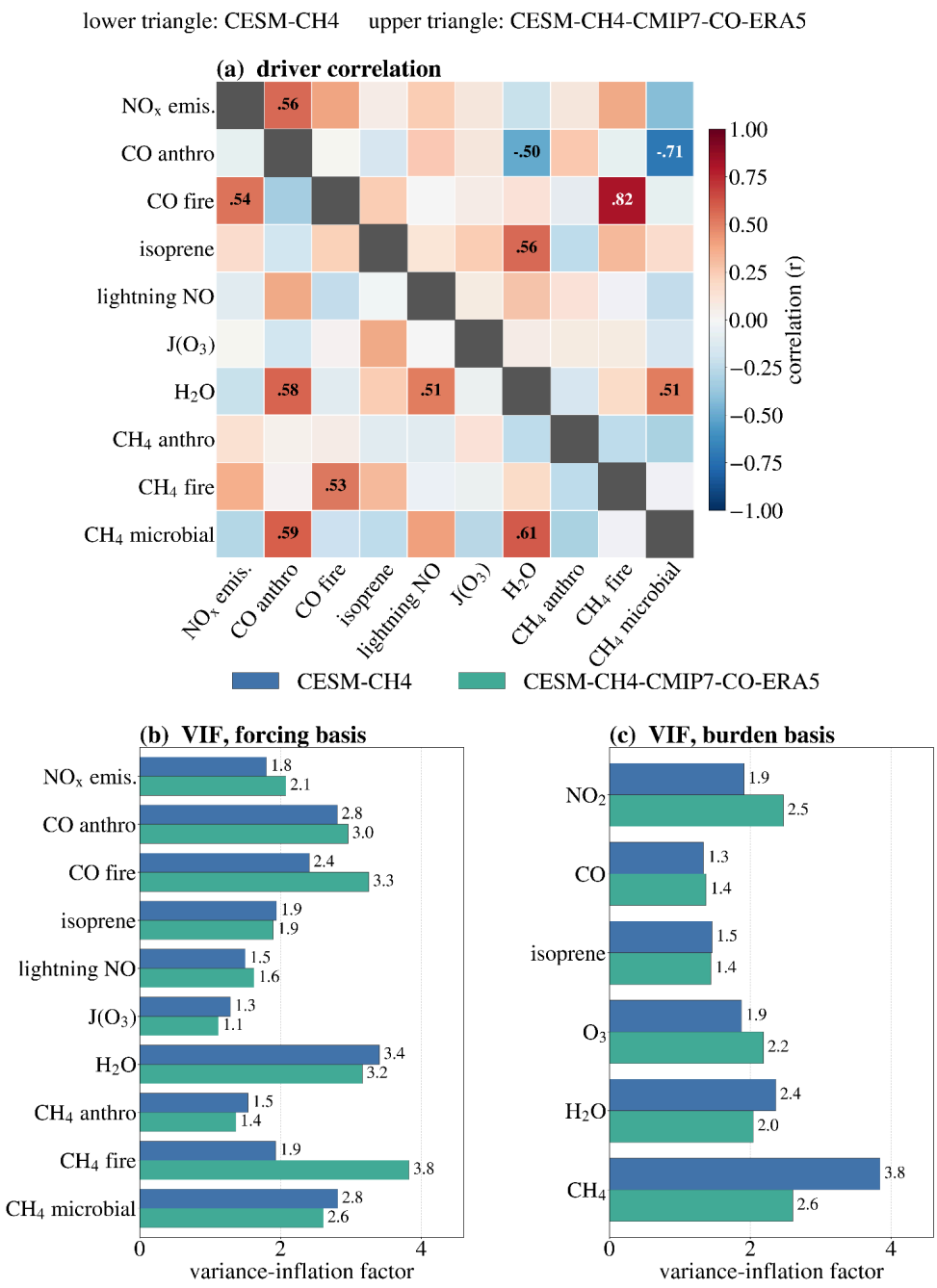


Fig. R1: Collinearity of the loss-weighted global predictors, 2003–2022. (a) Correlation between predictor anomalies; lower triangle CESM-CH4, upper triangle CESM-CH4-CMIP7-CO-ERA5, values shown where |r| > 0.5. (b, c) Variance-inflation factor, VIF of each predictor in the forcing (b) and burden (c) bases.

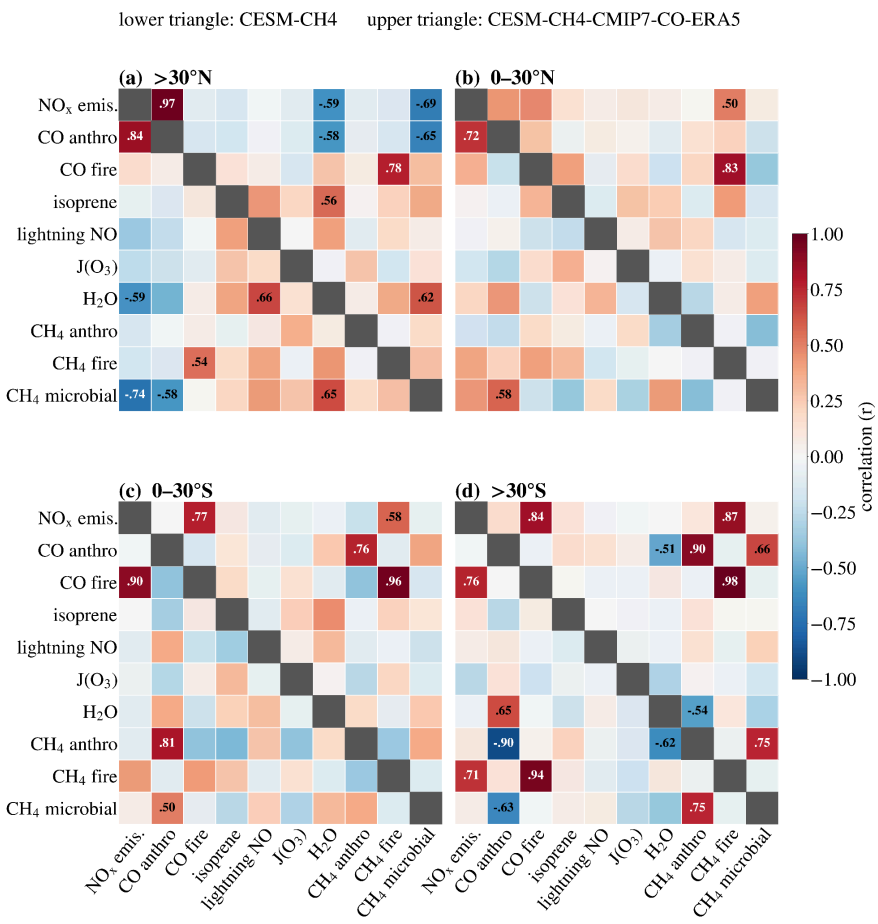


Figure R2: Correlation between forcing-basis predictor anomalies within each regression band, 2003–2022: (a) >30°N , (b) 0–30°N, (c) 0–30°S, (d) >30°S. Lower triangle CESM-CH4, upper triangle CESM-CH4-CMIP7-CO-ERA5

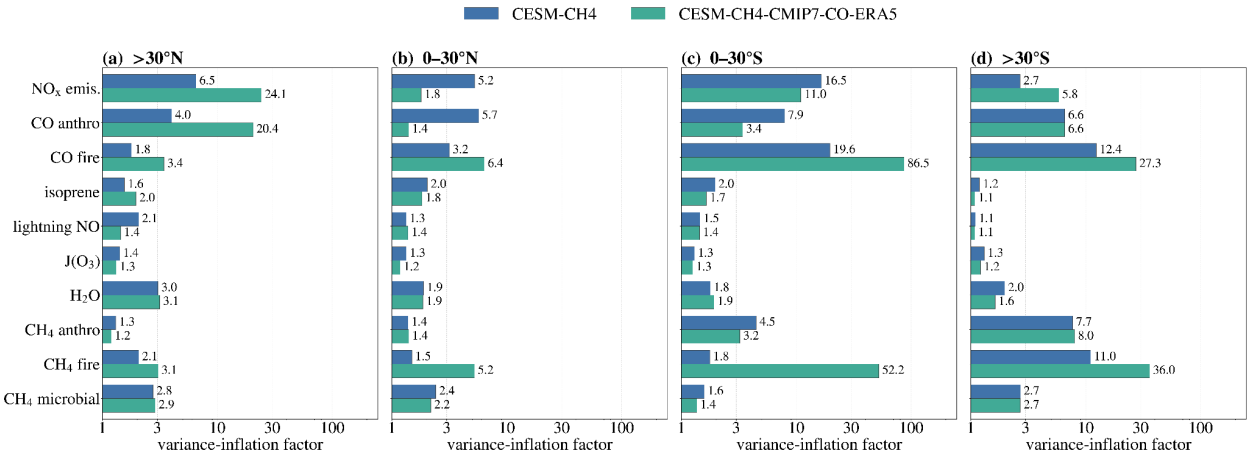


Figure R3: Variance-inflation factor, VIF, of each forcing-basis predictor within each regression band for CESM-CH4 (blue) and CESM-CH4-CMIP7-CO-ERA5 (green): (a) >30°N, (b) 0–30°N, (c) 0–30°S, (d) >30°S.

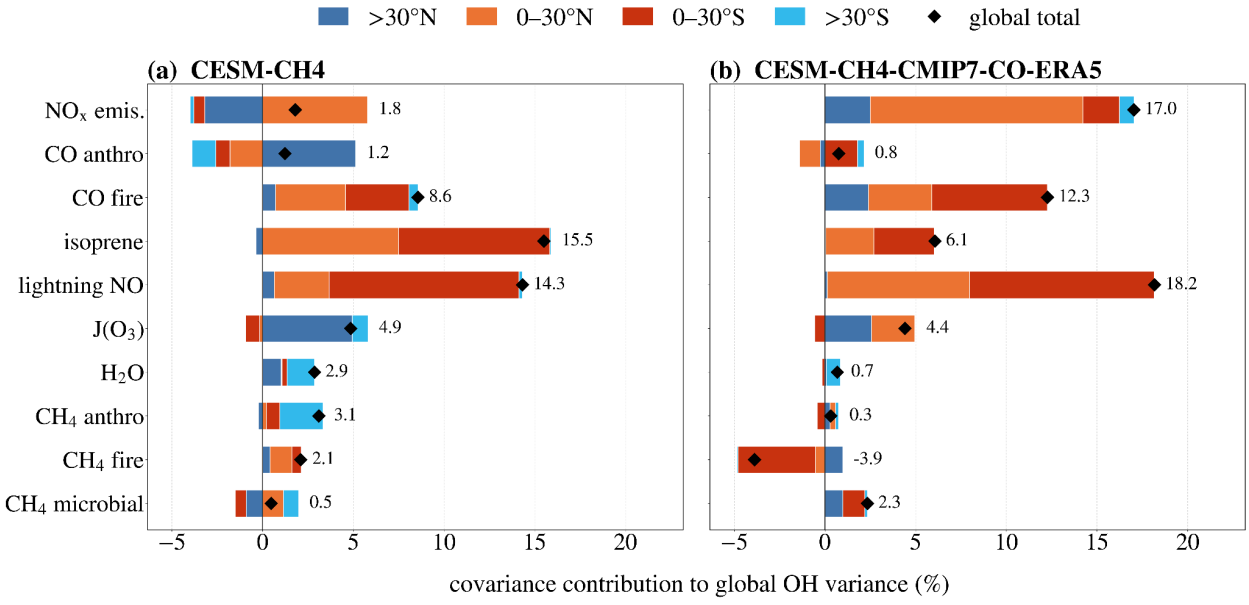


Figure R4: Covariance contribution of each forcing-basis predictor to the variance of the global loss-weighted OH anomaly, split by regression band, for (a) CESM-CH4 and (b) CESM-CH4-CMIP7-CO-ERA5.

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.