This study presents a multi-year top-down assessment of methane (CH4) emissions over Europe using in-situ and satellite data. While there are several studies looking on European CH4 emissions this study combines in-situ and satellite data and extends the studying period to 2023. The manuscript is well organised and overall scientific sound. However there are several key concerns that need to be revised/clarified. The manuscript lacks discussion in several statements the authors make. I first include some general comments, followed by comments about the parts of the manuscript that require the most attention/revision and later on specific lines, for minor adjustments/corrections/suggestions.

Some general comments:
Please use "Tg y^{-1}" as in your abstract and not "Tg a^{-1}".
Also for the units in your map-figures use mg m^{-2} d^{-1} or mg m^{-2} h^{-1}. That way its easier to compare your results with other studies.
Use the term "flux"/"fluxes" when you describe/show results with units "Mg Km^{-2} a^{-1}".

Major comments

Data session

Section 2.1:
Could you elaborate more on which TROPOMI product you used (SRON?, BREMEN?) and how are you dealing with the over-passing orbits

Section 2.2
The fact that you compute daily daytime averages using data between 10 and 16 is technically correct. However this is valid only for the surface stations (see for example in Steiner et al. 2024, Ioannidis et al. 2026).
Figure 2 shows stations that are not surface stations, e.g. PUY, OXK. These are mountainous stations which mean only nighttime values should be assimilated because during night these stations are more representative of free-tropospheric conditions and are not influenced by pollution from daytime upslope winds which usually are not well represented in coarse resolution modelling.
If you want to assimilate daytime observations from mountainous sites then you need to make sure you are using the correct sampling height (e.g. in the middle between the model topography and the real altitude). If you did that already please mention it in the paper.
Otherwise, is there a scientific, other, reason you decided to keep daytime data from mountainous stations? If not, I recommend to either remove them completely from your assimilation or use nighttime observations from these sities.

Section 2.1 and Section 2.2
You don't mention the model-data mismatch you used for the in-situ and tropomi data.

Section 2.3
Are you using CH4 freshwater a-priori emissions? Emissions from rivers/lakes but also coastal wetland are important. Are they included somewhere as an input? Discuss in the manuscript.

Table S1: There is no way readers would go through your excel table and check the numbers. Add a nice barplot in the supplementary material showing prior/posterior emissions per year and per country in your domain.

Section 5

Please rephrase the title. ‘Correction’ is misleading. This term indicates that the a priori emissions you used are wrong, while both bottom-up and top-down emissions have uncertainties.

This session also lucks a bit of discussion about figure 8. Discuss about the spatial distribution, patterns in the a-priori, the a-posteriori. It is also important to know whether posterior emissions show similar spatial patterns as the prior. Papers usually also show a map of the a-priori emissions.

I also miss some discussion on geological and wetlands ch4 emissions since there are important over Europe and their prior (e.g. for geological) have large uncertainties.

Section 5.1
You talk about agriculture and waste emissions and then in line 265 you say something about urban emissions which first does not fit with the sentence before and also contradict what you say later on about urban areas in lines 279 to 284 (specifically the lines 282 to 284). This part confused me a lot; the way to wrote it and what your take away message want to be. Please revise carefully the part about urban emissions.

I miss a discussion and plots on the seasonal cycle of CH4 emissions over Europe (as a whole or for different European countries or for specific sub-regions etc). Previous papers, like Bergamaschi et al. 2018 (https://doi.org/10.5194/acp-18-901-2018), East et al. 2024 (https://doi.org/10.1029/2024GL108494), Ioannidis et al. 2026, discuss the seasonal cycle of CH4 emissions. It would be really interesting to compare the seasonal cycle between the a-priori and the top-down emissions from the different inversion simulations, e.g. how do in-situ only or satellite only assimilation drive the seasonal cycle compared to in-situ + satellite assimilation. Your paper would be more complete if you add a session on seasonal cycle and discuss your results also with respect to previous studies.

Minor/More specific comments:

Line 65: add “and whole Europe “ after “...in the Benelux countries” and also Ioannidis et al. 2026 in the references there after Steiner et al. 2024

Line 70: say which tool (transport model, inversion technique) you are using for the inversion analysis

Lines 86-90: It would be useful to know the number of observations across the study domain for all years, not only 2019. You can easily add for example a barplot in the supplement material showing the number of observations per month and per year.

Line 97: delete flagged as – its written twice

Session 2.2: It would be useful to add a table in the supplement material showing if the station is a mountain site, the elevation, inlet height etc similarly to Steiner et al 2024 or Ioannidis et al. 2026.

Line 101: You say in the text that Fig.2 shows the average CH4 concentrations for 2023. The caption in Fig.2 says 2019-2023 average. Which one is the truth? In any case show 2019 similarly to TROPOMI figure.

Line 106: How can you really make that statement when you plot different years?

Line 115: Provide a table that summarises all the input a-priori emissions, their original spatial resolution and their temporal resolution.

Line 115: How do the geological emissions from Maasakkers et al. 2019 compare to Etiope et al. 2019 especially over Italy?

Line 131: Interesting that GEOS-chem includes stratospheric loss fields. Are there tools provided by the GEOS-chem community or output parameters from your model that you could use to discuss the importance (or not) of those loss fields on CH4 emissions in Europe? Especially for OH?

Line 159: "with a 50% relative uncertainty assigned to each emission flux". Each input flux? Or are your grouping the input fluxes into categories (e.g. anthropogenic, natural) and you optimise (assign uncertainties) to each category? Are your anthropogenic emissions only EDGAR emissions or there are the sum with biomass burning? Your text is vague; clarify and please explain how and which input emission fluxes you are optimising and their uncertainty. Also move here from the supplement material the text about the spatial and temporal correlation scale. Also it's missing information on the window length you are using.

Line 197: What about TCCON data? There are observations in a few sites in your domain for your simulation years. Is there a reason you did not include them in your validation?

Lines 214 to 218: Why do satellite data offer little information? Limited coverage? Someone would expect that satellite data would better constrain the emissions than 31 points in a big domain. Elaborate a bit more in your text.

Line 225: For Europe here you mean your whole domain as shown in your figures?

Line 248: replace " the inversion's corrections relative to prior emissions" with "the increments, calculated as the difference between the posterior and the prior fluxes".
Line 248-249: you say top 10 methane emitting countries in Europe. Based on what? Elaborate or add a reference.

Line 259: replace corrections with adjustments.

Line 261: ".. in these countries .." which countries? As shown in Fig. 9? Be more specific.

Line 269: "… this suggests that systematic biases may persist in national level livestock emission…" well no because your sentence before is about urban emissions measurements. Also the Netherlands is a really specific example regarding the livestock emissions. Add a more general discussion for the whole Europe and refer to previous studies.

Lines 271, 279, 361: replace corrections with adjustments

Line 272: interesting results for UK because of the long range flow influence in the UK and especially when using in-situ observations (which are only 2 in the UK as shown in your map). Do you get these results over this part of the domain because of the correction you applied in your nested domain? Can you add a bit more discussion about your findings and not just describe them?

Line 311: Do you mean wetland emissions by the term natural?

Line 354: Add a reference at the end of this sentence "… and a recent surface-data inversion".

Line 370: Even when using only satellite data?

Lines 371 to 376: give some numbers for the trends

Comments for the figures:

Figure 1: I do not understand what you mean when you say
"Observational data constraints for the inversion are shown below the black dashed line"
I could not link the above sentence with anything on your main text; please elaborate.

Figure 2: Correct caption or be consistent with what is written in the main text. See comment earlier.

Figure 3. OK but move it to the supplement material. Also what is OtherAnth? First time I see this term in this plot, not defined in the text.

Figure 5: caption – this is the average of the 5 years right? You say annual inversion in the caption.

Figure 7: Your caption does not agree with the tile of Fig. 7a. Revise.

Figure 8: Title Fig.8a also TROPOMI + ICOS? Add to be consistent with Fig.8b
For Fig.8b: Can you change the colour of the colourbar and use the gradients of one colour for the negative values (usually blue) and the gradients of another colour for the positive values (usually red), with 0 being white? Otherwise its difficult to see the differences in your map.

Figure 9: Are the green diamonds and purple circles data re-gridded to your domain? Because these studies are in a different resolution than yours so their country-totals would be different than yours.

Figure 9 and 10: Are you showing the sum or the average of CH4 emissions for 2019 to 2023? Clarify in the caption.

Figure S5: Why don't you compare your results against TCCON observations?

Figure S6: When you say independent ICOS observations you mean the 31 stations you used in the ICOS-only and ICOS-TROPOMI inversion right? Remove independent its confusing.

Figure S8: Both TROPOMI-only and ICOS-only inversion agree on the upward adjustments over Italy, while they disagree on the sign over the Netherlands. Can you elaborate on the text why? This is an interesting finding. Also both agree on the downward adjustment over Moscow where Figure 5a shows values above 0.6 so there is quite good constrain. However it's still puzzling to me how the constrain is so high over Russia when using only ICOS data while there are not in-situ data over Russia. Elaborate on the text.