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Responses to Anonymous Reviewers #1

(Note: Reviewer's comment is written in black fonts, authors responses are written in blue fonts.)

General Comment

This manuscript describes an analysis of ground-based Fourier Transform Infrared Spectrometer (EM27/SUN) observations in India from 2015-2016. The authors use the EM27/SUN data to analyze emissions of methane and carbon dioxide in Southern India. The authors show the utility of the EM27 measurements to evaluate satellite observations and then compare the observations to model results and estimate emissions. The paper is a very good analysis providing satellite data validation and emission estimates for a region of the globe that has not traditionally had extensive ground-based observations of methane and carbon dioxide.

The analysis is an important contribution in the area of using ground based and satellite observations of greenhouse gases in South Asia. They do a nice comparison of satellite data to the surface observations and then use them to help evaluate emission modeling analysis. The scientific quality is also very good, they use the well-established methods for using their surface data with the satellite observations and model results. The paper is well written, concise, and effective at communicating the key results.

My comments below are mostly minor and a few wording issues or typos. I think the conclusions could be strengthened somewhat to more clearly state the key findings.

Response:

We sincerely thank the reviewer for his/her positive and encouraging feedback, as well as for the thoughtful suggestions to improve the manuscript. We have carefully addressed all the minor comments related to wording, grammar, and typographical errors.

In response to the suggestion to strengthen the conclusions, we have revised the conclusion section to more clearly articulate the key findings of our study. The details of changes are provided in reply to specific comments below.

Specific Comments

Following is a list of comments/suggestions, which we accept as they are and have incorporated corrections/suggestions in the revised manuscript.

1. Line 35: continuously (not continuous)
2. Line 38: OCO-2's gets global coverage every 16 days (not 15)
3. Table 1, bottom row, right column: forgot word spectrometer after grating
4. Line 61: Feels like a reference would be appropriate here, maybe Laughner et al., 2024 https://doi.org/10.5194/essd-16-2197-2024
5. Line 86: Type for spelling of However
6. Line 87: delete "a few"

7. Lines 104-105: maybe the sentence beginning with "KIT has developed" could be reworded to make the point more clear, how the sun tracker works.
8. Lines 114-115: Mention the "reference IFS 125HR" is the Karlsruhe TCCON site?
9. Line 122: Type, add "The" before PROFFAST
10. Line 126: Define the acronym NCEP
11. Line 167: (NASA, USA) instead of (NASA), USA
12. Line 173: This is the first time you define the acronym OCO-2, maybe that should also be done with the first mention in the Introduction?
13. Line 188: OCO-2 uses different metrics for its bias correction. TCCON data is one of the data sets used, but models, small area analysis are also used.
14. Line 191: Typo, "contain a quality flag"
15. Line 198: Define acronym NILU
16. Line 218: Define acronym GAINS
17. Line 228: Define WetCHARTS?
18. Line 242: SWAMPS, missing S at the end
19. Table 4: Because the authors are stressing the importance of the bias and standard deviation calculations meeting the CCI requirements, you could highlight somehow (red numbers? Filled in cell?) the cases that do not match the requirements.
20. Line 364-365: Maybe instead of "increase or decrease" just use "changes" or "variations"
21. Line 369: "observed" could be removed
22. Line 376: Define acronym "JAMSTEC"
23. Line 382-383: Since the FLEXPART, maybe remove "the"

Specific Comments with detailed answers

1. Line 90-91: I was not familiar with the Pathakoti (2024) study. Do the authors feel that the larger bias seen in that analysis was due to an earlier version of the OCO-2 data? The way it is phrased is confusing. Also when the authors discuss the results in Table 4, much improved biases for OCO-2, maybe refer back to the Pathakoti study and highlight reasons for the improved comparisons?

   Reply: We thank Reviewer#1 for this question and suggestions to include more discussion on differences between our study and Pathakoti et al. (2024).

   Pathakoti et al. (2024) reported a mean bias 3.81 ppm for OCO-2 data, which is significantly larger than the bias 0.163 ppm found in our analysis for 10° x 5° lon-lat region. We believe that the higher bias reported by Pathakoti et al. is unlikely to be solely due to the use of an earlier version of the OCO-2 dataset.

Although their study does not elaborate extensively on the cause of the bias, Pathakoti et al. (2024) do acknowledge that their reported bias is higher than that typically found in the literature and they cite a lower value of 0.5 pm from Wunch et al. (2017) for reference. Therefore, we consider it unlikely that the OCO-2 data version alone explains the discrepancy.

There are also methodological differences between our study and that of Pathakoti et al. (2024) that may contribute to the difference in results. For example, Pathakoti et al. used a 4° × 4° lon-lat box and paired satellite data with the **daily mean** ground-based observations. In contrast, our analysis used a larger spatial window of 10° × 5° and matched satellite observations with surface measurements within ±2 hours of the satellite overpass. Additionally, we applied an extra filtering step using model data to ensure that both satellite and surface measurements sampled similar air masses.

We believe the high bias found by Pathakoti might have been caused due to the daily mean values used by them instead of restricting to temporal collocation within few hours of satellite overpass.

We have included the above discussion in the revised manuscript (line number 345 to 353 in the revised manuscript).

2. Line 121: Typo, space needed before pyplot

   Reply: We thank the reviewer for this observation. However, in this case, the term "PROFFASTpylot" is not a typographical error — it refers to the name of the software package used in our analysis.

3. Line 184: Typo, misspelling artifacts

   Reply: We thank the reviewer for pointing this out. We would like to clarify that we have followed British English spelling throughout the manuscript, in which "artefacts" is the correct spelling for "artifacts." We will ensure consistency of spelling in the revised version.

4. Line 274: When discussing the time coincidence criteria, I found the wording confusing. Maybe reword the sentence to make clear that you use data within two hours of the observation, but that data can come from a three-day time period.

   Reply: We thank the reviewer for this helpful suggestion. We agree that the original wording was unclear and have revised the sentence to more clearly state that satellite data within ±2 hours of the ground-based observation are considered, and the matching is carried out over a three-day window. The revised wording in the manuscript should now make this methodology clearer to the reader.

5. Line 280: Could the authors speak to the importance of step number 3? Does it make a big difference in the bias and standard deviation results?

   Reply: We thank the reviewer for this insightful question. Step 3 in our pairing methodology is designed to filter out satellite–ground observation pairs that do not represent the same airmass, based on model-derived criteria.

To assess the impact of this step on the bias and standard deviation, we conducted a sensitivity test by repeating the analysis without applying Step 3. The results are summarized in the table below.

Satellite	Bounding Box	Species	Num of Pairs		Bias (Sat – Grd)		Scatter	
	Lon x Lat		Old	New	Old	New	Old	New
GOSAT	10x05	CH4	12	40	-0.013	-0.011	0.006	0.009
GOSAT	20x10	CH4	19	97	-0.009	-0.011	0.012	0.011
GOSAT	60x20	CH4	55	166	-0.019	-0.014	0.014	0.013
GOSAT	10x05	CO2	27	40	0.983	0.684	1.587	1.518
GOSAT	20x10	CO2	59	97	0.812	0.398	1.881	1.921
GOSAT	60x20	CO2	117	166	0.644	0.196	1.698	1.326
OCO-2	10x05	CO2	41	48	0.163	0.239	0.786	0.849
OCO-2	20x10	CO2	67	96	0.342	0.303	0.806	0.827
OCO-2	60x20	CO2	120	163	0.408	0.315	0.776	0.876
ACOS	10x05	CO2	24	37	-0.212	-0.357	1.019	1.139
ACOS	20x10	CO2	54	99	0.077	0.022	1.246	0.989
ACOS	60x20	CO2	118	177	0.163	-0.042	1.089	1.083

(Note: Old referrers to collocation with model-based criteria. New refers to collocation without model based criteria.)

As expected, omitting Step 3 increased the number of matched pairs across all satellite products, species, and spatial grid boxes. For example, in the case of GOSAT XCH_4, the number of valid data pairs within the 10° × 5° bounding box increased from 12 (with all pairing steps) to 40 when Step 3 was excluded — an increase by more than a factor of 3. Similar increases were observed in other configurations, often exceeding a factor of 2.

In terms of performance, the effect on bias was mixed. For GOSAT XCH_4, the bias marginally decreased for the 10° × 5° and 60° × 20° boxes but increased slightly for the 20° × 10° box. For XCO_2 from the GOSAT satellite, the bias decreased significantly across all box sizes. Conversely, for OCO-2 and ACOS XCO_2 data products, the bias increased for the smallest box (10° × 5°) but decreased notably for the larger boxes (20° × 10° and 60° × 20°).

The scatter (standard deviation) increased slightly in most cases. There were a few instances where it decreased, although insignificantly — with the exception of ACOS XCO_2 in the 20° × 10° box, where it reduced from 1.246 to 0.989.

Overall, while Step 3 does not appear to have a uniformly large effect on the bias and scatter, we have chosen to retain it in our final analysis. This decision ensures consistency with previous studies and enhances the comparability of our results. A summary of this sensitivity analysis has been added to the revised manuscript between line numbers 295 and 298 as well as we have included the analysis without step 3 as supplementary text.

6. Lines 394-403: Maybe expand a bit more about the importance of the box sizes and the trade off between number of comparisons and improvement in bias and standard deviation. The GOSAT studies used larger boxes to improve the number of comparisons, the authors could expand on their conclusions relative to this.

Reply: We thank the reviewer for this useful suggestion. We agree that the manuscript would benefit from an expanded discussion on the role of box sizes and the trade-off between the number of comparisons and the resulting bias and standard deviation. We have revised the relevant section in the manuscript as follows:

"The biases in methane mixing ratios derived from the GOSAT satellite ranged from –9 ppb to –18.5 ppb, depending on the spatial criteria used for collocating satellite and ground-based observations. As expected, larger latitude–longitude boxes resulted in a greater number of matched data pairs; however, this did not consistently lead to reduced bias or scatter. In fact, for methane, the smallest box (10° × 5°) yielded the lowest bias and standard deviation. Notably, even when the biases differed across larger boxes, they remained within one standard deviation of the values observed for the smallest box size. These findings suggest that larger spatial boxes may not be necessary, particularly when longer time series of ground-based data are available. Similar patterns were observed in the carbon dioxide analysis.

For methane, the bias values for the smallest box size met the ESA Climate Change Initiative (CCI) requirement for systematic errors (<10 ppb). However, the biases were marginally higher for the medium box size and exceeded the threshold for the largest box (±30° longitude × ±10° latitude).

For carbon dioxide, the lowest biases were observed with the ACOS v9.2 dataset among the three evaluated. Both ACOS and OCO-2 datasets met the ESA CCI requirement for XCO_2 bias (<0.5 ppm), while GOSAT v3.05 exhibited higher biases ranging from 0.644 ppm to 0.983 ppm. All three datasets satisfied the CCI precision criterion for XCO_2 (<8 ppm), with standard deviation values ranging from 0.776 ppm to 1.88 ppm."

This revised paragraph has been incorporated into the manuscript (line number 434 to 444) to better highlight the role of box size and the observed trade-offs.

7. Lines 404-408: Similarly, the conclusions relative to the model results and comparisons to the observation could be spelled out a little more clearly. What is the key message the authors would like to get across from that part of the analysis?

   Reply: We thank the reviewer for this helpful suggestion. We agree that the conclusions regarding model results and their comparison with observations needed clearer articulation. We have revised the text in the manuscript to read:

   "We used the model to investigate seasonal changes in methane mixing ratios driven by local and regional emissions, as well as the sectoral contributions to these emissions. The model captures the broad seasonal trends in methane enhancements, with elevated values during certain months (e.g., November) and lower levels in others (e.g., June–July). On an average, agricultural emissions contribute approximately 55% to the modeled methane signal, followed by emissions from waste and wetlands.

   However, when comparing the modeled seasonal variability (ΔXCH_4, representing the contribution from emissions over the preceding 10 days) with observed seasonal changes in total column methane, the model underestimates the amplitude of variability. For instance, observed changes can reach ~100 ppb, whereas the model simulates only ~20 ppb. This indicates that a substantial portion of the seasonal signal arises from changes in *background methane levels and/or changes in photolysis rate. This differences suggests a potential limitation in using short-timescale inverse modeling approaches to accurately estimate local/regional emission fluxes.*

Overall, our study highlights the utility of satellite-based greenhouse gas observations for emission characterization over South Asia. Given the recent expansion of satellite missions by both public and private entities, there is a pressing need to develop a wider and denser network of ground-based Fourier Transform Spectrometers (FTS) in the region. Such a network would enhance the validation of satellite and model-based greenhouse gas products, leading to more robust emission assessments and improved climate modeling."

This revised text has been incorporated into the manuscript (line number 445 to 459) clarify the key messages from the model–observation comparison.

Once again, we would like to thank Reviewer#1 for the time he/she dedicated to review our manuscript and to provide constructive comments which greatly contributed to its improvement.

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Responses to Anonymous Reviewers #2

(Note: Reviewer's comment is written in black fonts, authors responses are written in blue fonts.)

General Comments

The authors included 3 satellite-based datasets for XCO2 (NIES GOSAT, ACOS GOSAT, and ACOS OCO2), but only one for XCH4 (NIES XCH4). I think the XCO2 comparison is great and useful. However, almost no one that I know of uses NIES XCH4 retrievals from GOSAT for scientific studies. Almost EVERYONE uses either the University of Leicester Proxy GOSAT XCH4 product (https://essd.copernicus.org/articles/12/3383/2020/), or one of several available TROPOMI XCH4 products. This document would be MUCH more useful if it included one or both of those XCH4 products in the validation section.

Reply: We sincerely thank Reviewer #2 for drawing our attention to the University of Leicester Proxy GOSAT XCH_4 product. We acknowledge that this widely used dataset was inadvertently omitted from our original manuscript. In response to the reviewer's suggestion, we are now including a comparison using the University of Leicester Proxy GOSAT XCH_4 product in the revised manuscript. We believe this addition enhances the relevance and usefulness of our analysis for the broader community.

Regarding the use of TROPOMI XCH_4 products, we note that our ground-based observations do not overlap temporally with the operational period of TROPOMI. For this reason, TROPOMI data were not considered in our validation study.

We would also like to clarify that, while the NIES XCH_4 retrieval may not be as widely cited as some other products for methane research, it remains an actively used and valuable dataset in the scientific community. Several recent studies have utilized GOSAT XCH_4 from NIES for trend analysis, inverse modelling, and multi-satellite comparisons. For example:

Li et al. (2022) employed GOSAT XCH_4 data to produce global methane concentration maps and analyse mean annual trends.

Song et al. (2023) combined GOSAT and TROPOMI observations to generate a longer time series and study methane trends over Monsoon Asia.

Lin et al. (2024) have used both NIES and Uni. Of Leicester data to study surge in methane concentration 2020 and 2021 and found both datasets confirmed increased methane growth rate from wetlands but with different magnitude.

Given the limited number of ground-based validation studies for GOSAT XCH_4 over India, we believe that evaluating the NIES product in our analysis fills an important gap and contributes to the ongoing effort to validate satellite methane retrievals across different regions.

We have now incorporated the above discussion and the new analysis with the University of Leicester XCH_4 product in the revised manuscript.

References:

Li, L. et al. (2022). Remote Sensing, 14(3), 654. https://doi.org/10.3390/rs14030654

Lin, X. et al. (2024). Nat Commun 15, 10894 (2024). https://doi.org/10.1038/s41467-024-55266-y

Song, H. et al. (2023). Remote Sensing, 15(13), 3389. https://doi.org/10.3390/rs15133389

The modeling section of this manuscript is problematic. There is no visual correlation at all between their modeled XCH4 and the observed. Which is fine, except that when I plotted up the CAMS data at this location for the time period of interest, visually it matches reasonably well with the EM27 observations. It appears that there may be a problem with their model. Because this part of the paper is totally separate from the validation part of the paper, I suggest they remove it altogether, or fix their model so it at least partially agrees with other, more well-established models. I would like to be clear that I am not a methane modeler, so that is just my impression as a GHG data scientist.

Reply: We thank Reviewer #2 for this important observation and for taking the time to compare our results with CAMS data. We believe that there may be a misunderstanding regarding the quantity shown in our modelling results. Specifically, our model output does not represent total column methane concentrations and therefore is not directly comparable to either the EM27/SUN observations or CAMS reanalysis data.

To clarify, the modelling component of our study was designed to test the hypothesis that the observed seasonal variation in methane is significantly influenced by local/regional emissions -- such as those from rice cultivation and wetland sources. Our model is configured to simulate methane enhancements based only on emissions from the preceding 10 days, using trajectory-based transport to capture contributions from upwind regions. This time-limited approach was intentional to isolate the signal from relatively local and regional sources.

In contrast, the total column methane observed by the EM27/SUN instrument—and represented in CAMS reanalysis products—reflects the integrated influence of global background concentrations, long-range transport, atmospheric chemistry, and regional sources. Notably, CAMS data incorporate observational constraints through data assimilation, which naturally results in values that better match direct observations.

Our analysis finds that the amplitude of seasonal variability in the model-derived methane (driven solely by recent regional emissions) is significantly smaller than that observed in total column data. We believe this discrepancy is a key finding of our study, as it highlights the dominant role of background methane variability in shaping the observed seasonal cycle at this site. This has important implications for inverse modelling and the attribution of observed concentration changes to regional emission fluxes.

Rather than removing the modelling section, we have revised the text (line number 445 to 454) to more clearly explain the intent, configuration, and limitations of the model, and to avoid confusion with total column comparisons. We hope this clarification will address the reviewer's concerns and better communicate the motivation and value of this part of the analysis.

Finally, there are MANY English grammar (and a few spelling) errors throughout this manuscript. They are too numerous for me to list individually. Please have a native/fluent English speaker read through the document and correct these mistakes.

Reply: We thank the reviewer for pointing this out. We will take special care to correct all grammar and spelling errors in the revised manuscript. The revised version will be thoroughly reviewed by a fluent English speaker to ensure clarity and language accuracy throughout. Moreover, we believe there is mandatory and automatic language check by AMT before final publication of the work.

Primarily because of the problems with the modeling section, I cannot recommend this paper for publication at this time. If they were to remove this section and resubmit, especially if they included one or more standard XCH4 datasets (e.g. University of Leicester v9.0 XCH4 using the proxy approach, as well as a TROPOMI dataset), and fixed the numerous grammar errors, it would be suitable for publication.

Reply: We thank Reviewer #2 for the thoughtful and constructive feedback provided throughout the review. As noted earlier, we have now included a comparison with the University of Leicester Proxy XCH_4 product (v9.0) in the revised manuscript, as recommended.

Regarding the modelling section, we respectfully maintain that the results from this part of the analysis provide important scientific context. The lack of strong correlation between modelled and observed total column methane is not indicative of an error, but rather reflects the modelling setup, which was designed to isolate the contribution of regional emissions over a short time window (10 days). This result highlights the dominant role of background methane variability, and we believe it offers valuable insight into the interpretation of column measurements and their suitability for constraining regional fluxes.

We fully acknowledge the reviewer's concern and will revise the manuscript to more clearly communicate the intent and limitations of the modelling approach, and to ensure it is not misinterpreted as a conventional total column simulation.

We have also undertaken a thorough language revision of the entire manuscript to address the grammar and spelling issues raised.

We sincerely hope that the changes made in response to the reviewer's comments will lead to a more favourable reconsideration of the manuscript.

Specific Comments

1. Line 23: please add "methane" or "CH4" somewhere to make it clear which gas this sentence is referring to.
   Reply: Comment noted – we have updated our manuscript accordingly (line number 27).
2. Line 95: for OCO-2 v11.1, please reference Jacobs et al. 2024: https://doi.org/10.5194/amt-17-1375-2024.
   Reply: Thank you for this suggestion. We are including reference to Jacobs et al. (2024) in the revised manuscript (line number 99 and 191).
3. Line 161: You are referring specifically to the operational NIES full-physics retrieval. You should say this, and give a citation of the NIES retrieval. The latest citation that I know of for the NIES full-physics retrieval is Someya et al., 2023: https://amt.copernicus.org/articles/16/1477/2023/.

   Reply: Thank you for this suggestion and bringing Someya et al. (2023) to our attention – we have updated the manuscript accordingly (line number 171).

4. Line 166: "Goddard Earth Science Data Information and Services Center" has a useful acronym: GES-DISC. I suggest you use it for future references to this center in the manuscript.
   Reply: Noted – we have updated our manuscript accordingly (line number 168 and 182).

5. Section 2.2: As I said above, please add the University of Leicester Proxy XCH4 product into your evaluation. Then your document the two operational NIES products (XCH4 and XCO2), as well as each gas from a more trusted retrieval (ACOS for XCO2; University of Leicester for XCH4).

   Reply: We thank the reviewer for this valuable suggestion. As noted in our response to the general comments, we are now incorporating a comparison with the University of Leicester Proxy XCH_4 product in the revised manuscript. This addition complements the existing evaluation of the two operational NIES products (XCH_4 and XCO_2), providing a broader and more balanced assessment using widely trusted retrievals.

   The changes have been made throughout the revised manuscript from Abstract to Conclusion.

6. Line 176: Also please reference Jacobs et al. (2024) for v11.1 updates.
   Reply: Noted – we have updated our manuscript accordingly (line number 191).

7. Line 216: Please add "anthropogenic" in front of "emissions inventory", to make it clear what kind of emissions the ECLIPSE dataset quantifies.
   Reply: Noted – we have updated our manuscript accordingly (line number 230).

8. Section 3: Please explain why a global model such as CAMS (assimilating in-situ data) is not suitable for your evaluation. It's not clear why you had to build your own methane model over India, when many already exist.
   Reply: We thank the reviewer for this important question. As noted in our response to the general comments, our motivation for including the modelling component was to evaluate the relative contribution of recent regional emissions -- specifically, those occurring within the past 10 days and likely to have influenced the observation site -- toward the observed methane concentrations.

   We would like to clarify that we did not build a new model for this purpose. Instead, we used the well-established FLEXPART model, an open-source Lagrangian dispersion model widely used for source–receptor analysis of trace gases that are relatively stable in the atmosphere. FLEXPART is particularly suitable for isolating the influence of regional sources over short time periods, which cannot be achieved using global reanalysis products like CAMS.

   CAMS data are extremely valuable for global-scale assessments, especially since they assimilate observational data. However, their framework does not allow for the kind of backward-in-time, source attribution or trajectory-based emission sensitivity analysis that our study aimed to perform.

   We have revised the model-related text from line number 445 to 454 in the revised manuscript to explain more clearly our approach and model output.

9. Section 4.1, regarding Colocation Criteria. First, Buchwitz et al. 2017 using +- 4 deg latitude +- 4 deg longitude, which is easily checked by reading the paper. +- 10 latitude and +- 30 deg longitude is ridiculous. It is huge, and isn't even over India anymore! Second, please be clear if you are using only GOSAT observations over land, or if you include GOSAT observations over ocean. There would be many more of the latter than the former. Finally, have you tried to loosen your CAMS constraint? That seems perhaps overly tight.

   Reply: We thank the reviewer for this detailed comment.

Regarding the reference to Buchwitz et al. (2017), it appears the reviewer may be referring to a different publication than the one we cited. The article cited in our manuscript (Buchwitz et al., 2017, ESA GHG-CCI Final Report) describes the following colocation approach on page 19, second-last paragraph and copied verbatim:

"*To match satellite with TCCON data we used the following procedure. First, we select all satellite data within 30° longitude and 10° latitude of a TCCON site. Then, for each satellite point, we select all TCCON measurements which meet the following conditions: the measurements needed to be taken within 3 days of the satellite measurement and be performed during the same time of day (within 2 hours). Furthermore, the difference between the total column mole fractions, derived from the CAMS model output, interpolated to the satellite's coordinates and those interpolated to the TCCON's coordinates cannot exceed 0.25 ppm (for XCO_2) and 5 ppb (for XCH_4).*"

This colocation criterion guided our inclusion of a larger bounding box (±10° latitude and ±30° longitude), as one of the three tested configurations.

We fully acknowledge the reviewer's concern that this box size may extend beyond the geographic domain of India. To address this, we deliberately explored three different spatial box sizes in our analysis (10° × 5°, 20° × 10°, and 60° × 20°). This range was chosen to illustrate the trade-off between increasing the number of matched data pairs and maintaining spatial representativeness. We believe this approach offers transparency and allows readers to assess how bias and scatter vary with colocation criteria.

Regarding the reviewer's second question, we confirm that no distinction was made between land and ocean for GOSAT observations, as long as the data fell within the specified bounding box.

In response to the third point, we conducted a sensitivity analysis by removing Step 3, which involves filtering based on CAMS model values to ensure air mass similarity. Omitting this step significantly increased the number of matched pairs. For instance, in the case of GOSAT XCH_4, the number of valid pairs in the 10° × 5° box increased from 12 to 40. Similar increases were observed for all satellite products, species, and box sizes—often more than doubling the number of matches.

In terms of impact on bias and scatter:

For GOSAT XCH_4, bias slightly decreased in the 10° × 5° and 60° × 20° boxes but increased marginally in the 20° × 10° case.

For GOSAT XCO_2, the bias decreased across all box sizes.

For OCO-2 and ACOS XCO_2, bias increased for the smallest box but decreased notably for the larger ones.

The scatter (standard deviation) increased slightly in most cases, except in a few, such as for ACOS XCO_2 (20° × 10°), where it decreased from 1.246 to 0.989 ppm.

Although the CAMS constraint (Step 3) does not dramatically affect the results, we have chosen to retain it for consistency with previous studies. A summary of this sensitivity analysis has now been included in the revised manuscript.

A table of values comparing with and without model-based criteria is now included as supplementary text and a brief summary of impact of step 3 is included in the revised manuscript from line 295 to 298.

10. In section 4.1, you should point out that you do not make an averaging kernel (AK) correction, which could cause slight discrepancies. It would be better if you did this of course, but it seems like you do not.
    Reply: We thank the reviewer for this observation. We confirm that averaging kernel (AK) corrections were not applied in our analysis, and we agree that this may contribute to minor discrepancies in the satellite–ground comparisons. We have now noted this explicitly in Section 4.1 of the revised manuscript and briefly discussed its implications.
    To provide additional context, we refer to the study by Sha et al. (2021; https://doi.rog/10.5194/amt-14-6249-201), which evaluated the impact of smoothing and a priori profile corrections on validation results for XCH_4 and XCO from the Sentinel-5 Precursor mission using data from TCCON and NDACC-IRWG stations. Their analysis showed that smoothing effects are most significant in regions with strong dynamical variability, such as high-latitude stations influenced by the polar vortex. However, for low latitude stations, the impact was small. Specifically, they found that the maximum difference in XCH_4 bias with and without smoothing was less than -0.25% and the average difference was -0.14%. Our stations being a low latitude station (13.5° N), we expect negligible impact of not applying a priori alignment and smoothing correction. Nonetheless, we appreciate the reviewer's suggestion and have addressed this point explicitly in the revised manuscript (line number 299 to 304).

11. Line 319, at the end of the sentence add something like "over the 9 month period of comparison".
    Reply: Noted – we have revised the entire sentence to address next comment (comment#12) in the revised manuscript (line number 355 to 358)

12. Near Line 320. It is worth pointing out that GOSAT v305 XCO2 performs significantly worse in the fall-winter months (Nov, Dec, Jan, Feb) than the spring months (Mar Jul).
    Reply: Thank you for the suggestion. We have updated our manuscript accordingly (line number 355 to 358).

13. Section 4.2 – add "of Methane" or "of CH4" to the title of this section, since CO2 is not studied here.
    Reply: Noted – we have updated our manuscript accordingly (line number 361).

14. While I am not a methane modeler, I loaded up and plotted the CAMS XCH4 dataset (version v20r1) from their top-down inversion that assimilates in-situ data. It looks nothing like the FLEXPART model result provided in this manuscript, but does look surprisingly similar to the EM27 observations. I can only conclude that something is wrong with their model results, given that there is no visual correlation at all between their model results and the EM27 observations or CAMS. I really don't see the value in section 4.2, given the apparent problem with their model results.
    Reply: We thank the reviewer for this follow-up comment. As noted in our earlier responses, the seasonal variability simulated by our model is not expected to match the observed total column methane concentrations unless that variability is primarily driven by local or regional emissions. Our results indicate that the observed seasonal cycle over South India is largely governed by background methane variability, rather than by short-term changes in nearby emission sources. This interpretation is supported by similar findings reported in Chandra et al. (2017; https://doi.org/10.5194/acp-17-12633-2017).

It is important to clarify that the CAMS XCH_4 product is a reanalysis dataset that assimilates satellite observations. Since our ground-based observations already show good agreement with the satellite data, it is reasonable that CAMS would also correlate well with the EM27/SUN observations. However, this agreement reflects the assimilated nature of the CAMS product and does not provide insight into the specific contribution of local emissions.

We emphasize that the lack of correlation between our FLEXPART-based model results and the observed or CAMS values is not indicative of an error in the modelling setup. Rather, it provides valuable insight into the limited role of regional emissions in driving seasonal changes in total methane concentrations at this location. We believe this is an important conclusion with implications for the interpretation of column observations and the design of inverse modelling strategies.

Therefore, we plan to retain Section 4.2 in the revised manuscript. We have revised the text in conclusion section (line number 445 to 454) to clarify the modelling intent and the value of the findings.

Once again, we would like to thank Reviewer#2 for the time he/she dedicated to review our manuscript and to provide constructive comments which greatly contributed to its improvement.