General

The authors describe a regional top-down estimation of CH_4 fluxes using in-situ tower observations in East Asia. They employ the established tools CTDAS and WRF-Chem, a combination that has been used before. The manuscript presents a performance analysis of the inversion system using a simplified observation system simulation experiment (OSSE) followed by an application to real data.

The results obtained by applying the system to real data fit well in the scope of ACP. Combined with a discussion and performance analysis, these results deserve publication. However, the discussion and interpretation of the results misses some important limitations of the inversion setup.

Problems with idealized OSSE

The results for individual sectors are uncertain and likely biased. This aspect deserves more attention in the discussion of the results. Furthermore, the interpretation of the OSSE results (section 3.1) requires more caution in dealing with the highly idealized pseudo-observations. For example, the OSSE does not demonstrate the ability to distinguish sectors. The authors mention limitation on a very general level (line 850–855) but they do not address the concrete implications of the idealized test case.

I suggest to select one of the following two options to deal with this problem (see also point 13 below):

1. Put the focus on the inversion with real data. Shorten the description of the OSSE, but consider the limitation of the setup when discussing the OSSE results. In this case, I recommend that the authors critically check their statements on sector discrimination since the ability of the system to distinguish emission sectors is not demonstrated.
2. Replace or extend the OSSE by new simulations with more complex prescribed true states and analyze how the inversion system can reconstruct such true states (similar to Steiner et al., 2024). This should lead to more robust conclusions on the capabilities of the method and help with the interpretation of the results.

Writing

Overall, the writing is clear but often lengthy. I value the precise description of the model (section 2), but I found the description of the results in section 3 very lengthy. I recommend to shorten the text by avoiding redundancies and removing purely descriptive text in the discussion of figures or tables (see specific comments below).

Specific comments

1. Lines 108–110: *"Steiner et al. (2024) report regional discrepancies between inversions constrained by satellite observations and those constrained by different surface measurement networks."*
   Steiner et al. present an inversion based on surface observations and provide a qualitative comparison to other inversion results from different models and inversion systems, including satellite-based inversions. But here it seems more suitable to cite works that directly compare inversion results based on surface and satellite observations, e.g.: doi:10.5194/acp-26-10423-2026, doi:10.5194/egusphere-2026-2165 or doi:10.5194/acp-14-577-2014 (this list is incomplete).
2. Lines 121–123: The authors claim that Lagrangian approaches "provide limited constraint on larger regions such as the YRD". Considering the large variety of Lagrangian methods and the application of such methods to regional modeling, this sounds like a strong and general statement. I recommend that the authors either provide a reference that explicitly supports this statement, or they adjust this claim. In Hu et al., 2026 (references as in the paper), I did not find any support for this general statement. Aside from this, I agree with the authors that having a regional Eulerian inversion framework does provides an added value (e.g. due to the simpler extension to satellite observations).

3. Figure 1: Panels a and b are sufficiently clear, but panels c and d are only explained much later and panel d can lead to confusion.
   - Panel (c) can either be kept as part of Figure 1 or presented on its own in section 2.2.1. Keeping it in Figure 1 has the disadvantage that the reader will only learn later what is meant by a "tile".
   - Panel (d) should be presented on its own in section 2.3 since it is only relevant for the definition of one test case. If it is part of a figure that describes the overall inversion system, this can lead to confusion.
4. Line 178: When using products from the Copernicus Atmosphere Data Store (ADS), authors are asked to cite also the ADS catalogue entry. See "Citation and attribution" under https://doi.org/10.24381/ed2851d2.
5. Line 222: Here one could mention that prior fluxes for the YRD region are visualized in Figure S3. In general, I find a visualization of the prior fluxes helpful for interpreting the results. I therefore suggest to include also a figure of the prior fluxes in the whole domain in the supplement.
6. Lines 234–244: The description of the data assimilation cycle remained a bit unclear to me, but I think a more detailed explanation could be too long. I therefore suggest to explicitly refer to Peters et al. (2005) for more details on the fixed lag data assimilation cycle.
7. Lines 291–294: The authors describe a different MDM for different days of the year for station SARI. Without explanation this seems arbitrary. Why was this choice made?
8. Line 305: I do not see how the comparison of a Gaussian and an exponential localization function leads to a factor $\frac{\sqrt{\pi}}{2}$.
9. Line 318: The prescribed true state is very unlikely in the assumed prior probability density. Is there a motivation for testing such an extreme case?
10. Line 393: Is this modeled or measured wind speed?
11. Figure 4 (same for Fig. 10a): The time series show an asymmetric posterior uncertainty range. How is the uncertainty range defined here? As far as I know, the ensemble square-root filter assumes Gaussian uncertainties and should therefore provide symmetric posterior uncertainty estimates. I would usually expect to see one standard deviation (1σ) or the 95% confidence interval (2σ). To avoid ambiguities, the definition of the uncertainty range should be given in the caption.
12. Lines 457–485 and Figure 5: This paragraph on near-surface concentration fields has little relevance for the rest of the article. Since the article is already quite long, I recommend to make this an appendix or to remove this part.
13. Lines 485–507 and Figure 6: The discussion of the sector separation in the OSSE misses important explanations and conclusions. The OSSE setup is not suitable for validating that sector separation works, but it shows how sector separation fails in a special case:

    The true state of the OSSE is far beyond the prior uncertainty range. The inversion system will try to find an emission configuration that fits to the observations while remaining sufficiently close to the prior uncertainty range. It will therefore prefer to adjust sectors with a large absolute prior uncertainty. This explains the strong adjustment in natural fluxes (for which a larger relative uncertainty was assumed). Sectors with small absolute prior uncertainty (livestock, other) will show only small adjustments. The resulting biases affect the sector attribution unless the results are purely data-driven, i.e., unless the observations are sufficient to fully constrain the sector emissions. See Appendix C of doi:10.5194/acp-25-17187-2025 for a more formal explanation.

    Thus, the results presented in Figure 6 are to be expected. But the interpretation of the results must take into account that the results are strongly driven by the prior. The results do not show that

the system can estimate the natural fluxes. The OSSE only shows that the setup attributes changes in total fluxes to the sector of natural fluxes. To estimate whether the system can distinguish sectors, one should analyze the averaging kernels and conduct further OSSE experiments with different true scaling factors for different sectors. (See the general comments at the beginning of this document.)

In conclusion, the OSSE shows a sector attribution that can be explained by possible biases. It does not demonstrate the correct attribution of sector emissions in a non-trivial setup. Since the mentioned biases remain relevant in the application to real observations, one should be careful when interpreting the posterior sector emissions.

14. Lines 492–507: This paragraph only repeats what is shown in the figure. Such descriptions should be much shorter (1–2 sentences) and focus only on the most important conclusions that can be drawn from the figure.
15. Lines 525–565: This lengthy description of Table 4 can be shortened by focusing on the most relevant results. The interpretation could take into account that some results are strongly influenced by the simplified OSSE setup and provide little insights into the performance for real data: A higher prior uncertainty (Case 4) should improve the results because the true state is far from the prior. The region-based aggregation (Case 6) and fewer flux categories (Case 7) implicitly change the prior error structure by changing the state space.
16. Figure 8: I recommend to add uncertainties (error bars) to all bars (prior and posterior).
17. Lines 683–690: See explanation above on biases in the sector attribution.
18. Lines 691–698: The authors describe the posterior error correlation of the sector emissions. This is one indicator that shows the sectoral trade-offs. This paragraph misses a clear conclusion, i.e., that the sectoral trade-offs are relevant and lead to higher uncertainties in emission estimates for individual sectors. For a more detailed analysis of the sector separation, one could analyze the averaging kernels (see, e.g., doi:10.1142/3171 or doi:10.5194/egusphere-2026-2165).
19. Figure 11: For the column observations in SH and HF, the model-observation mismatch shows a bias. Is this bias a physical signal of the emissions? Or is it an artifact of missing averaging kernels or bad model representation of the column? These questions may be difficult to answer, but it they are relevant for the interpretation of the data. I therefore recommend to briefly discuss this aspect.
20. Section 3.3.2: This section contains some repetitive statements that can be removed to shorten the text. To make clear what I mean, the following comments are deliberately more detailed than usual:
    - Lines 753–754: *"In the discussion below, we use the ensemble mean value."* This statement seems obvious to me and can be removed.
    - Lines 760–761: *"All estimates are reported in* $\mathrm{Tg\ yr^{-1}}$*."* The units are (and should be) provided together with the numbers, thus this sentence can be removed.
    - Lines 762–782: These paragraphs only repeat what is shown in Figure 13. I suggest to reduce this to 2–3 sentences that highlight the most important conclusions from Figure 13.
21. Figure 13: An error bar is provided for the posterior emissions, but not for the prior. I recommend to provide also the prior uncertainty in Figure 13 (a) and (b).

Technical and minor issues

22. In the HTML abstract, a ± sign is missing.
23. line 52: "**an** in situ station"
24. line 96 and 97: "pixel size" is needed in the first parentheses: "pixel size ~5.5×7 km"
25. Figure 1: The resolution of Figure 1(c) should be improved.

26. Table 3: In this table it should be mentioned that SH and HF are total column observation sites. For example, this could be indicated in the "Site type" column.
27. Table 3: In the column "Notes" I assume that "2022" means that the full year was considered? Then why not call the column "Time span" and write "full year" instead of "2022"? Moreover, the line break in "7 July" is confusing.
28. Line 432, grammar: "Because …" should not start a new sentence here but is a continuation of the previous sentence.
29. Line 445: *"Northern Japan shows the best agreement"*: This is redundant, shorten the text here.
30. Lines 572: *"The real-data inversion shows that existing bottom-up inventories are generally biased high […]"*: It is a matter of personal style how scientists present their results. But in general I would suggest to use weaker formulations for results that are subject to substantial uncertainties, e.g. write "indicate" instead of "show". The same applies to lines 589–598.