**Response to Referee #1**

We thank Referee 1 for the thoughtful and detailed comments and suggestions, which definitely helped us to improve the manuscript. We addressed all points in the revised version. Reviewer comments are in black, answers in blue. Page and line numbers from the updated manuscript are underlined. The main changes in the revised version are:

* We have clarified the role of the monsoon dynamical system in methane transport, with particular emphasis on the rapid vertical lifting by deep monsoon convection and the influence of the relative position between the AMA and organized monsoon convection during the slow upwelling from the outflow level to the UTLS region. To better illustrate this, we have expanded the relevant background discussion on rapid lifting and slow upwelling in the Introduction, thoroughly revised Section 3.2, and added panels on deep convection and lower tropospheric methane to Figure 4.
* To improve the description of the AMA mode classification, we have extensively revised Section 2.2 and added Figure 1 to more intuitively illustrate the spatial morphology of different modes and the statistical distribution of anticyclone centers.
* Several previously ambiguous terms in the manuscript, such as the definitions of “anomalies” and “enhancement,” as well as the earlier inadequate use of “transport pathway,” have been revised for greater accuracy and clarity.

The authors simulated upper tropospheric methane distributions using GEOS-Chem driven with “optimized” surface methane flux data and showed strong subseasonal variation linked to the east-west oscillation of the Asian monsoon anticyclone (AMA). What is new about this study is twofold, the use of a surface methane flux dataset optimized with observational data and the finding that upper tropospheric methane concentrations peaked in September due to rice paddy emissions from Southeast Asia. Extensive work has been conducted to examine the AMA’s role in redistributing CO, water vapor, HCN, hydrocarbons and aerosols in the upper troposphere, as summarized in the Introduction, and this paper added CH4 into that list. The study clearly shows how subseasonal variations in upper tropospheric CH4 over the Iranian Plateau to the Tibetan Plateau align with AMA variability and surface emissions. Below are my comments.

First, my technical review suggested that the authors provide a clear definition of “anomaly” at the outset, as the term requires a reference point, particularly in climatology. While this may be self-evident to the authors, an explicit definition would benefit ACP’s broader readership. However, such a definition is still missing. Moreover, their “anomaly” appears synonymous with “enhancement.” For consistency, the authors should consider using one term throughout. In cases where they quantify upper tropospheric “enhancement” over the Asian monsoon region (lines 64–65), they must clarify the reference used to determine these values, preventing ambiguity for readers.

Thank you very much for the insightful suggestion. We have added a detailed description of the definitions of “anomaly” and “enhancement” when we use this term and ensure reproducibility. For example, the “enhancement” used in P3 L74-75 is revised as “…This increase of CH4 is about 100 ppb higher than its regional annual mean volume mixing ratio (VMR) and is 3%~10% higher than the VMRs averaged over the same latitude (Tao et al., 2024; Xiong et al., 2009).” In addition, we have revised **the captions for Figure 2 and Figure 4** to include brief explanations of these terms, providing readers with clearer context when interpreting the figures.

Second, the case study seems unnecessary. The authors should move directly to the composites of the AMA modes and their corresponding methane distributions. It is also unclear whether they identified AMA modes independently and what methodology they used.

Thank you for your kind reminder. In the revised manuscript, we have added a new figure (Figure 1) along with a corresponding description in the Methods section to clarify how the AMA modes were identified and to detail the methodology employed. Additionally, we retained the case analysis—now presented as a combined figure (original Figures 1 and 2)—at the beginning of the Results section to provide readers with a clear and informative introduction to the three AMA modes, based on actual events observed during the study period. We believe this part is necessary to illustrate the covariability of the AMA and the horizonal distribution of methane in the UTLS on daily basis.

**Specific comments:**

1. Lines 116–127: When describing the probability distribution of AMA center positions and classifying AMA modes, including one or two figures would improve clarity. Additionally, the statement in lines 121–122 requires a concise summary of the different effects of AMA modes. This would not only make the paper more self-contained but also help explain the differing distributions of upper tropospheric methane concentrations associated with each AMA mode.

Thanks a lot for your suggestions. We have added a figure illustrating the horizontal distribution of wind and geopotential height for the three AMA modes, along with a detailed explanation in the revised Method*s* section.

1. Study period: The study period should be stated upfront rather than buried near the end of Section 2.

Thanks for your recommendation. We’ve emphasized the study period in the first sentence of Section 2 (P3 L87) and mentioned this period again at P5 L129.

1. Lines 132: Figure 1 – Explain what ΔCH4 in Figure 1b represents.

Thank you for the kind reminder. We have added explanation of ΔCH4 in revised text (P7 L171-172) and revised caption of new Figure 2b (P8 L186-193).

“Hovemöller diagrams of anomalies in (a) geopotential height and (b) CH4 concentrations at 150hPa for JAS 2020. Anomalies are calculated with respect to the daily mean values averaged over the main ASM region (15°N–40°N, 15°E–135°E).”

1. Lines 133-134: The statement about thermal heating in the Tibetan Plateau (TP) and subsequent westward migration due to instability needs supporting references or evidence.

We rewrote this paragraph and remove this information about “thermal heating” because it is not relevant to our main storyline “the covariability of the large-scale circulation of ASM and the horizontal distribution of CH4 at 150 hPa on daily basis”. Please check the first paragraph in section 3.1 (P7 L169-185).

1. Line 134: “the AMA center over predominantly hovered east of 75°E” – It doesn’t read right.

We have revised this sentence with “the AMA center remained predominantly east of 75°E.” (P7 L179-180).

1. Line 135: Provide a brief explanation of how the AMA position influences the ΔCH₄ distribution.

We revised this sentence to: “This subseasonal variability of AMA significantly modulated the CH4 variations in the middle to upper troposphere (see Figure 1b), similar to the behavior of tracers such as CO (Pan et al., 2016), primarily due to anticyclonic confinement (Ploeger et al., 2015).”

1. Lines 146-148: Clarify what is meant by “stirring interaction.” Additionally, explain the mechanism by which boundary layer air is lifted, as this is key to transporting surface methane emissions into the upper troposphere. The authors should support these statements with references or their own evidence.

We agree that this point was not supported sufficiently. We added a subfigure in Fig. S1 to show the CH4 VMR near surface with OLR pattern representing the deep convection. Here we revised this sentence to: “For example, at IP mode on Aug. 12st, the high methane center locates southern edge of AMA. This pattern results from the “stirring” interaction between convection-uplifted boundary layer air from the Indian subcontinent (as shown by the overlap between the main monsoon convection source and high methane regions near the lower boundary in bottom panel of Fig. S1) and the surrounding air, which is similar like “stirring” interaction proposed by Pan et al. (2016).” (P9 L205-208).

1. Line 176: The term “pathway” should be reconsidered unless streamlines or variables indicating a dynamic process are presented.

We recognize that our use of “transport pathway” was somewhat misleading. In response, we have revised the text to: “the subseasonal oscillations of the AMA significantly influence the methane distribution and its transport efficiency from the lower boundary to the UTLS.” (P20 L198). Additionally, we have replaced the inappropriate use of “transport pathway” throughout the manuscript.

1. Line 198: The phrase “(unit: %, referring to zonal mean)” is unclear. The authors should explicitly define how anomalies were calculated (per the first comment) and clarify what is being presented. It appears they are referring to zonal means of anomalies within their domain.

The definition of “anomalies” is unclear. Here the relative anomalies refer to global zonal mean at each latitude within each pressure layer for corresponding mode composite. To be clearer, we revised the figure caption (P15 L280-287). In addition, we added one figure with unit in absolute values of VMR (Fig. S3) as a supplement.

1. The anomalies are more precisely relative anomalies referring to the zonal mean methane VMR within the domain (…). Add Figure in ppbv as a supplement.

Thanks for your suggestion. We added a figure in ppbv has been added in the revised supplementary file as Figure S3.