Comments on "Using high frequency observations of δ13C-CH4 and δ2H-CH4 and uncertain regional isotopic signatures to estimate sources of UK methane emissions"

This study presents an inverse modelling framework that incorporates high-frequency observations of δ13C-CH4 and δ2H-CH4, together with uncertain regional isotopic signatures, to estimate fossil fuel (FF) and non-fossil fuel (non-FF) methane emissions over the UK. The authors evaluate the method using synthetic experiments and apply it to real observations, exploring the role of isotopic information in constraining sectoral emissions.

The topic is timely and relevant, particularly given the increasing interest in improving source attribution in methane emission inversions. The integration of dual-isotope observations into a Bayesian inversion framework is potentially valuable. However, the current manuscript lacks sufficient clarity in its motivation, methodological justification, and interpretation of results. In particular, the added value of isotope information is not convincingly demonstrated, and several methodological assumptions and limitations are not adequately discussed.

Therefore, the paper deserves further work before publication. Major revisions are required to make it a solid and valuable contribution to the literature. I request that the authors consider the following points as they revise this manuscript.

Major comments:

1. The Introduction (Section 1) is difficult to follow and does not clearly establish why isotope observations are needed in methane inversion. The current structure mixes background information, previous studies, and methodological descriptions without a clear logical progression.

   The manuscript does not clearly answer: What specific limitation in conventional (concentration-based) inversions is being addressed by isotope data?

   The discussion of previous isotope-based studies lacks synthesis and does not clearly position this work relative to existing literature.

   The use of Section 1.1 is unnecessary given that no further subsections exist; this affects readability and structure.

2. A key conclusion of the manuscript is that incorporating isotope observations leads to only limited changes in emission magnitude and uncertainty compared to methane-only inversions. However, this important result is not critically discussed.

   Does this imply that isotope information provides limited constraint under current observational conditions? Is the limited impact due to: insufficient observational coverage (e.g., single site), large uncertainties in isotopic signatures, or model structural limitations?

At present, the manuscript reports this result but does not provide a clear interpretation of its implications

Specific comments:

1. Line 5: The term “uncertain isotope signature” is unclear. Please clarify whether this refers to isotopic signatures with associated uncertainties, or a specific methodological treatment. A clear definition is needed.
2. Line 10: The statement “limited impact on emissions uncertainty or magnitude” is ambiguous. Does this imply that isotope observations provide little additional constraint compared to methane-only inversions? Please clarify.
3. Line 20: For clarity, move (of approximately 10 years) immediately after “short tropospheric lifetime”.
4. Line 30: Please provide references to support the statement regarding current methane trends not aligning with low-emission SSP scenarios.
5. Line 35: The argument that accurate quantification is difficult due to multiple sources is not well developed. The authors should explicitly discuss: source heterogeneity, data limitations, and uncertainties in inventories and observations.
6. Lines 70–75: Additional references are required to support the statements made in this section.
7. Line 75: The causal relationship is unclear. The availability of low-frequency isotope observations does not directly explain their use in global-scale inversions. Please clarify the logic.
8. Line 85: The term “new-resolution” is vague. Please specify whether this refers to temporal or spatial resolution, and quantify it.
9. Line 90: Please clarify whether isotope-based inversions systematically produce higher emissions than inventories, or if this refers to specific studies.
10. Lines 95–110: The logical flow is unclear. The discussion shifts between source attribution and isotope applications without clearly linking them. Please reorganize for clarity.
11. Line 125: Please specify the atmospheric transport model used.
12. Line 235: The filtering strategy needs further justification. In particular, why are data points with strong local emission influence removed? These may contain useful source information.
13. Line 245: The reported isotope uncertainties (e.g., 0.25‰ and 1.82‰) appear small. Please clarify whether these are realistic and how they are derived.
14. Line 250: Please define “pollution event” explicitly.
15. Tables: Table captions should be placed above the tables. Please revise accordingly.

16. Figure 7: The colorbar should be differentiated between absolute emissions and emission differences for clarity.
17. Line 585: The term “novel inverse modeling” is not sufficiently justified. Please clearly specify what is novel compared to existing inversion frameworks.