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Reply to comments by Martin Manning

Sourish Basu et al.

Author comment on "Estimating emissions of methane consistent with atmospheric measurements of methane and $\delta^{13}\text{C}$ of methane" by Sourish Basu et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-317-AC2>, 2022

We thank Dr. Manning for taking the time to carefully read through the manuscript and the generally positive comments. The work presented here is a culmination of almost five years of effort, and it is gratifying to know that Dr. Manning considers it a step forward. We appreciate the suggestions for improvement. Please find below Dr. Manning's comments italicized, with our responses in normal font.

The relationship between this paper and Lan et al, 2021, is important and as they are in different journals it can help to repeat a bit more of what is in Lan et al here. E.g. the range of scenario options that had been considered in Lan et al before this paper adopts C_WL+ as the base for inversion analysis.

Sections 2.3, 2.4 and 2.5 contain detailed description of the setup of Lan et al (2021), which we have repeated here in order to save the reader from having to (re)read Lan et al (2021). Many of the sensitivity tests described in detail in section 2.6 are precisely scenarios that were explored in the forward runs of Lan et al (2021). If Dr. Manning could suggest additional details from Lan et al (2021) that would bear repeating here, we would be happy to add them.

In section 3, I see no mention of how stratospheric removal is treated in the TM5 model. As this leads to return of air with some isotopically enriched CH₄ back into the troposphere, and that enrichment varying with latitude, it can effect the $\delta^{13}\text{C}$ analysis.

In the submitted manuscript, lines 158 onward read "Monthly climatological CH₄ loss rates in the stratosphere due to OH, Cl and O(¹D) were constructed from a run of the ECHAM5/MESy1 chemistry transport model (Steil et al., 2003; Jöckel et al., 2006)." TM5 is a full atmosphere model, so isotopically enriched CH₄ from the stratosphere is recirculated back into the troposphere by TM5's stratosphere-troposphere exchange, which is typical among offline transport models (Krol et al., 2017).

Section 4.1 "Comparison with Thanwerdas et al 2021" seems to have been added after the rest of the paper had been written. I think it would read better if this section was merged into section 3.6 "Comparison to the GCP budget".

This is a good point, that section was indeed added at the last minute because Thanwerdas et al (2021) came out at the very tail end of our writing. We agree that all comparisons to other work and estimates should be together. We have therefore moved all such comparisons to the "Conclusions and discussion" section, which now contains

comparisons to (a) the GCP budget, (b) Zhang et al (2021) following a recommendation from another reviewer, (c) Thanwerdas et al (2021), and (d) comparisons to a few other top-down studies, as recommended by another reviewer.

The rest of section 4 starts with a summary of what has been covered in the paper and then moves on to consider options for future development. After comparing this with some other papers in ACP, I would suggest that it would be better to have section 4 just focused on how the results can be developed with future work, and to move the initial summary, currently at the beginning of section 4, to a section 5 on "Concluding remarks" that summarised some of the key points made in the paper.

This is also a good point. We have taken Dr. Manning's suggestion and reorganized the manuscript as follows. Section 3 now contains just the results from our runs. Section 4 is now called "Conclusions and discussion", which contains, in sub-sections, an enumeration of the important conclusions from our work, comparisons to other published estimates and work, improvements planned to our framework in the future, and areas that need progress to better use atmospheric $\delta^{13}\text{C}\text{H}_4$ data for disentangling the methane budget.

The Courtier et al 1998 reference given here would be better as the peer reviewed version published in Quarterly Journal of the Royal Meteorological Society, 124:1783-1807, 1998. <https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/qj.49712455002>

Good catch! We have changed the reference to Courtier et al (1998).

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