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Comment on acp-2022-561

Anonymous Referee #2

Referee comment on "Atmospheric data support a multi-decadal shift in the global methane budget towards natural tropical emissions" by Alice Drinkwater et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-561-RC2>, 2022

The paper by Drinkwater et al. studied changes in regional methane emissions and $d^{13}C$ source signatures over the period 2004-2020, using two inversion frameworks that assimilated in-situ and GOSAT observations respectively. They found a progressively emission increase from tropical regions accompanied by lighter $d^{13}C$ signature, and concluded a multi-decadal shift in global methane budget towards tropical natural emissions (wetland emissions notably). The subject of the paper fits in the long-term research interests in the community regarding the decadal changes in methane budgets and underlying drivers. In general, I find the paper interesting to read, and relevant to the scope of ACP. However, there are a few major concerns that may weaken the robustness of the main conclusions, and hopefully can be addressed in revisions.

One of the biggest issues is that the inversion results presented in this study lack independent evaluation. While the two inversions based on in-situ and GOSAT observations respectively do show some consistency in the overall emission trends at the global scale and large latitudinal bands (Fig. 4 & Fig. A2), there are clear discrepancies between the two inversions for big regions regarding emission increments after inversion (e.g., Boreal North America & China in Fig. 2), magnitudes of posterior emissions and emission trends (e.g., Temperate North America & Tropical Asia in Fig. A1). The good model-data agreement at some selected sites as shown in Fig. A4 is expected, as these stations were assimilated in inversions and most of them are marine boundary layer stations, where observations are normally reproduced by models. In fact, I would expect poor model performance at some difficult sites such as KRS and BKT even though they were assimilated. I suggest the authors examine model performance at all sites assimilated, and if possible, include non-assimilated sites or observations from other platforms like aircraft campaigns or TCCON sites, so as to evaluate the robustness of the inversion results for big regions.

In particular, I notice that the emission trends for China since 2012 are somehow higher than the estimates from several recent papers (Lines 207–210, 0.72 Tg/yr and 1.34 Tg/yr inferred from in-situ and GOSAT data versus 0.36 Tg/yr from Sheng et al. 2021, also check out the papers by Liu et al. 2021 and Zhang et al. 2022 and references therein). The emission trend inferred from GOSAT data (1.34 Tg/yr) seems beyond the upper limit

of previous estimates for the similar period, and contradicts with the recent slowdown of emission increase in China (Liu et al. 2021). Do you have any explanation for that?

For the optimization of $d^{13}C$ signature, I don't quite understand the methodology. It's not clear whether regional methane fluxes and $d^{13}C$ signatures were solved simultaneously or sequentially? According to the description of methodology in Lines 154–163, it seems that the solution of regional $d^{13}C$ signatures relies on the solution of regional emissions. I wonder how much errors in estimates of regional emissions would impact the solution of $d^{13}C$ signatures. Can we trust the results presented in Fig. 3 if the emission trends detected for certain regions are not robust?

The lighter $d^{13}C$ signature in tropical regions doesn't necessarily imply an increase in natural emissions (wetland emissions in particular). The tropical regions are known for their agricultural practices and related methane emissions, and recent studies suggested emission increase from agricultural sectors in tropical countries (Stavert et al. 2022; Zhang et al. 2022b), which could also lead to lighter $d^{13}C$ signature according to Table 1. Is it possible that agricultural sectors also had substantial contribution to the recent trends in tropical emissions and $d^{13}C$ signature? How much was it compared to the contribution from wetland emissions? In the abstract the authors claimed that "the satellite remote sensing data provide evidence of higher spatially resolved hotspots of methane that are consistent with the location and seasonal timing of wetland emissions" (see Lines 318–320 as well), which is not clearly shown in this paper. The authors also cited a few papers that reported large CH_4 anomalies or trends in Eastern Africa or Amazon, which seems to confirm their conclusions. But it's not clear how much wetland emissions from tropical regions contributed to the signals detected in this paper.

The use of climatological OH fields for the reference runs is fine, given the large uncertainty in the long-term OH trends and variabilities. The authors should be aware of the range of uncertainties among recent studies (see e.g., Turner et al. 2017; Naus et al. 2021; Patra et al. 2021; Zhao et al. 2020 etc. and references therein), and discuss how this could impact methane budgets and variabilities. The paper by Lan et al. 2021 cited in the introduction (Lines 39–41) seems to deny the hypothesis proposed by Turner et al. 2017. Why did you choose decreasing OH by 0.5%/yr for the sensitivity test that followed this hypothesis? The choice of 5% uniform drop in OH for 2020 is also problematic, given the large spatial and temporal variability in OH changes in response to reduction in NO_x emissions due to COVID lockdown.

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