Comments on acp-2022-561

Atmospheric data support a multi-decadal shift in the global methane budget towards natural tropical emissions. Drinkwater et al.

Explaining why methane is rising so rapidly is important. We simply don't know what is going on. What's more, methane is simultaneously becoming relatively richer in 12C, reversing its centuries-long trend towards 13C. Why? – the simplest answer is that the rise is driven by new inputs of biogenic methane, though it is also possible that changes may be happening in the methane sins.

But this puzzle is not like most scientific puzzles. Figuring out the exact life cycle of a graptolite or the exact origins of an ancient volcanic ash in an ice core can be solved in a leisurely way. But understanding methane is urgent. It very directly affects the hopes for the UN Paris Agreement and the climate future of us all.

Drinkwater et al. make a very good attempt to address this great problem. Yes, some individual assumptions and parameter choices they make can be debated – that's what science is about – but the work is sound.

First I will list my own very minor requests for changes. Then, as requested third referee, I shall comment on the earlier assessments of the earlier version of this paper, and the responses of the authors.

Minor comments.

1.) Two new papers are relevant and should be considered:

Oh, Y., et al. (2022). Improved global wetland carbon isotopic signatures support post-2006 microbial methane emission increase. *Communications Earth & Environment*, *3*, 159, 1-12.

Zhang Z, et al. (2023) *Nature Climate Change*. **13**, 430–433

2.) Abstract lines 4 and 5, and in the main text conclusions - It would help general readers to have some idea of the total increase in emissions over the 17 year period – the 'acceleration' (Tg/yr/yr) is given but not the total change (i.e. how much greater emissions were in 2020 than in 2004.). That should be given in Table 1 perhaps, and mentioned in the concluding section 4. Indeed, what exactly does 'Annual Mean Emissions (This study)' convey in Table 1? Maybe that's because it's being compared with Saunois et al, but it's like saying your speed as you accelerate down a freeway is some mean between when you entered from the junction and now when you're whizzing along, foot flat on the pedal.

3.) Line 125 – maybe some discussion of Oh et al 2022 would be useful?

4.) Line 139 – Any thoughts on the OH KIE puzzle? Cantrell? Sauressig?

5.) Line 334 – Several other recent papers have also come to fairly robust conclusions that OH, while important, is not the primary driver of growth.

6.) it would help to make Table 1 more detailed, or perhaps to create an entirely new Table 2 to list all the changes in emissions and growth rates over the study years. (see comment above on the Abstract).

Comments on the authors' responses to earlier remarks

Referee 1 comments on 1. the need to assess both the robustness and weaknesses of the inversion; 2. is concerned about regional isotopic signatures; 3. is worried about the sparseness of the observational network and thus the sensitivity of the optimised fluxes to the priors; and 4. is concerned about OH.

The authors have responded with significant revisions and perhaps a softening of their conclusions, that their "results are *consistent* with result studies that have highlighted a growing role for wetland emissions".

As third referee, I agree with the good points raised by Ref 1 over the initial submission, but I also consider the authors have responded well to the comments and have made appropriate revisions. The methane problem is unconstrained – we have too few real data, whether in the measurement network or in the source signature, so we have to do the best we can. We can't put the problem off for a decade until we get more stations and better measurements.

Referee 2 also makes helpful comments.
1. Question about 2020. This year was extraordinary for methane. So it's well worth detailed attention. Note that 2021 was also extreme. Although covid obviously had impacts on air chemistry, these dramatic growth events in 2020 and 2021 were probably not primarily because of covid. Factors like the unusual triple dip La Nina and the behavior of the Indian Ocean Dipole were surely more significant. Indeed, if the growth in 2007-2018 was interesting, the changes since 2019 seem to be of a different order.
2. Table 1 – see comment 6 above on ref. 1
3. Biogenic natural vs biogenic anthropogenic. Of course rain feeds cows as well as wetlands and these two are almost indistinguishable. Note the Z. Zhang et al. (2023) revision of wetland emissions – we very badly need new real in situ observations from wetlands, especially tropical wetlands, not models.

Conclusion

This is an important paper that has been well debated in review, has responded well to helpful comments, and now deserves to be published, perhaps after some small further changes. The topic is important and urgent and the work is sound, as far as can be achieved given our lack of measurements, especially in the topics. This contribution needs to be published, to become part of the wider debate.