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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Attribution of the accelerating increase in atmospheric methane during 2010–2018 by inverse analysis of GOSAT observations" *by* Yuzhong Zhang et al.

Anonymous Referee #2

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This article analyses the role of wetland and livestock on the global CH4 growth rates in the period 2010-2018. The authors have also carefully assesed the role of OH on the CH4 growth rate. The article is generally well written, and has built upon their earlier analysis. I have doubt on the simplicity of the emission optimisation for only two major sectors, while CH4 has so many other natural and anthropogenic emission sectors those vary very differently at interannual and longer timescales. Detailed comments below. The manuscript may be considered for publication after addressing some of these comments.

Specific comments: lines 38-39 : need references?

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lines 76ff: I find later that the CTM configuration and other details are discussed later. May be remove this paragraph !!

lines 90ff: I think this is not an ideal choice because the values of CH4 depends on assumed CO2 concentrations. We know that our understanding of CO2 fluxes over many data void regions are poor. How do you estimate regional bias, say over South Asia, Southeast Asia, Amazon/Brazil, Africa etc.

line: that's the lower bound, what about the oceans?

line 137: Did you apply any scaling to termite emissions?

lines 170ff: 1-year spin-up is too short by any standards for long-lived species. What about the stabilisation of vertical gradients? You cannot get that right by the unbiasing method you describe. For future studies please make a spin-up for 10 years or so. Its worth the computing cost, given the large amount of follow work and analysis is done for any publication.

Figure 2 and associated text: Does these correction factors extend till the poles? The problem is that the stratosphere is dynamic as well. Are such seasonal bias correction factor good for getting the profiles right.

My worry here is also that if the stratosphere is not spun-up well these bias correction factors will not be time invariant. So the trends in anthropogenic or natural emissions you derive later may not be free of these correction factors.

Figure 5 and associated text: Very tough to accept the results for the a posteriori patterns, e.g., there is a strip of increased emissions along the Himalayan region! Is this arising from not properly accounting for the orography in the coarse resolution model? Or this may be an artifact of the proxy retrievals by miscalulating CO2 over northern part of the Indian subcontinent.

Figure 6 and associated text: I like this analysis but it is not clear at all, if the results are independent of the priors! For example, if you if you started with the priors from the

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UNFCCC will you get the same a posteriori emissions?

The question is also same for the trends in derived emissions. Some sensitivity tests are need for clarifying robustness of the a posteriori emissions.

See for example Patra et al., JMSJ, 2016. That paper is also relevant for other discussion in the paper where you discuss trends of emissions over China, and from Animals etc.

lines 340-344: How confidently we can talk about the oil and gas emission trends - given that the cited references are so small scale compared to the gridcell you optimize here. For example, can you gather the cited references by model gridcells and check the validity of the inversion and vice versa?

lines 366-377: I am a bit confused why/how all the anthropogenic emissions are linked with the livestock population? Is this because your a priori emissions only accounts for the livestocks? For example how is the trends in waste management in these developing nations?

Page 18-20: I do not know but I have a feeling that the inversion is set in such a way that all the regions are having somewhat similar increase in emissions, either from the animals or from wetlands. The authors are in the best position to judge and find the reasons behind such outcomes from their inversion system.

If you can explain the global CH4 growth rates using emissions from only two emission sectors then we have an oversimplifications of CH4 sources. I cannot prove anything in favour or against the proposed mechanisms here but many of the hypothesis are based on apparently single line of evidence, i.e., the GOSAT proxy XCH4 retrievals. Aren't Maasakkers et al., Lunt et al., Pandey et al., Parker et al. using the same XCH4 data sources?

In addition to the trends analysis, it would have been useful to discuss how and why are the large interannual variability in some regions as shown in Fig. 10. Are there irregular

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data gaps or a particular climate anomaly affecting CH4 emissions regionally?

line 470: The NH/SH OH ratio of 1.02 is an interesting result from this exercise. Could you please comment here how are the NH/SH ratio of CH4 emission changed from a priori to a posteriori cases? I suspect the derived NH/SH OH ratio depends on how well freely the inversion system is allowed to adjust emissions vs that for OH.

page 23: How can we get assured that the OH and CH4 emissions can be optimised in one inversion system. Is there a dipole effect? For example, if you do more or less number of iterations, will the global total emission and global mean OH will be different? I see that this issue is addressed later using Fig. 14, but still not convinced that the inversion system is separating the wetland vs livestock emissions well.

Figure 13: You attributed emissions from livestock increase to the animal population, but what appears here is that the increase in emissions from tropical and extratropical wetland are the greatest. Could you propose a mechanistic viewpoint ?

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