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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interpreting methane variations in the past two decades using measurements of CH_4 mixing ratio and isotopic composition" *by* G. Monteil et al.

Anonymous Referee #1

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This paper attempts to make use of atmospheric CH4 concentration as well as its carbon isotopic signatures to improve our understanding of past two decades of CH4 emissions. They have used a rather coarse resolution chemistry-transport model TM3 and explore 8 CH4 emission scenarios in this analysis. I find the manuscript is clearly written based on the well thought out experimental design. However, I have some recommendations as listed below, which may lead to a major revision. The paper can be accepted for publication after a revision.

Major comments:

Since this model has very coarse resolution, in vertical particularly, compared to TM5 used by M. Krol for scaling OH, I wonder whether the same scaling factor will be valid for this version of TM3. An independent check by simulating MCF would have been very useful. Since this paper is trying to validate absolute CH4 emissions, abundance of tropospheric OH is single most important parameter.

The discussion in p. 6783, 3rd paragraph (and values in Table 2) gives me an impression that the CH4 loss rate is a bit weak for 577 Tg of global total emission. It is always a bargain to increase/decrease emission or loss by OH+CH4 reaction in troposphere when the simulated CH4 growth rates are to be matched with observed values. This goes back to my previous recommendation – if possible, please check whether the scaling factor of 0.92 is appropriate for this CTM version.

In any case, the results suggest a need for reduction in emission, but how much emission reduction is right will entirely depend on the chosen OH amount. My main concern is that the results appear to be very much model dependent at the present schemes of simulation, limiting otherwise wide implication of this study.

Minor comments:

p.6772, l.10 : EDGAR 4 (all caps)

p.6773, l.12 : May be good to cite Rigby et al. (Geophys. Res. Lett., 2008) also.

p.6773, l.16-18 : Is this need for decrease in emission is for compensating for a trend in OH?

P6774, l.23 : 'joined' – any better term for this?

p.6775, l.20-21 : What about trends in transport and more importantly the temperature due to the so called climate change in inter-decadal time scales. As shown in Patra et al. (J. Meteorol. Soc. Jpn., 2009), change in temperature could affect CH4 loss by changing reaction rates because of its temperature dependence (p. 6776, line#7). Higher temperature in the year 2000s compared to the 1990s could balance some of the increase in emission in your S0 case.

p.6776, l.6 : Is this OH from a full chemistry model run of TM3? If the source of OH field were different, a more appropriate reference would be useful.

p.6776, l.22 : What is the use of 0.1x0.1 degree emission, when the CTM horizontal resolution is 10x7.5 degrees in longxlat. 1.0x1.0 would have been sufficient. In fact I am more curious about what do you meant by ‘top’ of the atmosphere (p. 6775, l.15). Does the CH4 concentration become zero at the top level due to loss by chemical reactions or it is forced to zero?

p.6777, l.3 : Indeed, you need to give a ‘source’ of these interesting statistics. Also the rice emission should have been seasonal (e.g., Fung et al., J. Geophys. Res., 1991; the GISS emission database), although seasonality in CH4 emission or concentration may not directly effect your results and conclusions (I am not sure, however).

p.6779, l.4-10 : Should this paragraph be combined with p. 6778, l.10-13. I suppose you get the 182 Tg CH4 of wetland emission after scaling the original wetland emission (whatever that is) by 1.42, and similarly for the biomass burning emission.

p. 6779, l.21-22 : This statement is in apparent contradiction with the results in Fig. 1. To my eye, both CH4 and del13C-CH4 takes about the same amount of relaxation time to reach the steady state. Either you add a statement by clarifying whether your results agrees or not with Tans (1997) in p. 6780, l.12-17 or delete this sentence.

p.6782, l18-20 : Can you really say that for the SH sites?

p. 6782, l.23 : I thought the 1995-2000 period shows good model-data agreement for the SH sites. Please check.

p. 6787, first two para-s : I am not sure a trend in OH or a slightly different scaling of OH (ref. major comments) would have been a better choice. As it appears from majority of the plots, the simulated CH4 growth rates are a bit on the higher side then observed values, say for the period of 1980-1990. Can you make another model case, say P3,

by increasing OH throughout the simulation by $\sim$1% or some % more appropriate?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 6771, 2011.