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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Investigation of the global methane budget over 1980–2017 using GFDL-AM4.1" *by* Jian He et al.

Anonymous Referee #1

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The work describes CH4 budget using an atmospheric chemistry-transport model that is developed at the GFDL. The authors have taken in to account all possible causes of variabilities in CH4 budget, such as the emissions and loss due to tropospheric hydroxyl (OH). As shown in the manuscript, OH variability is of as much importance as the emissions in explaining the CH4 growth rate variabilties in different decades in the period of 1980s to 2010s. The manuscript is generally well written. However, I felt toward the end of the manuscript is a bit of stretch and could be reduced (I have made some suggestions in my specific comments). The manuscript can be accepted after a major revision.

Specific comments: Line 49 & 62ff: can the growth rate discussions in the introduction be made concise and put together at one place.

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Line 80ff: I think there are other prominent inverse modelling results trying to explain the recent regrowth of CH4 concentrations.

Line 135-137: This is a quite strange statement. After reading the whole manuscript I do not believe you have tried to address these couple of issues to a great extent. May be remove?

Line 156: Not from wetchart? I mean does wetchart not have IAV?

Line 206: Not clear if this is after LNOx scaling? please make this statement precise (e.g., Control).

Line 249ff: "The meridional curve" needed some clarifications here, e.g., selected sites within a latitude band to get the mean CH4 at 5 different latitude bands or something like that.

Line 296: Sometimes the sites like Key Biscayne are sampled by moving the model grids to the ocean side. You might check that out.

Line 315ff: The tropical bias in all HIPPO is a bit strange! Not OH but transport (or emissions)? I am suspecting this because the bias due to OH would appear at all altitudes (timescale $\sim$1yr), because the bias is in the lower troposphere, if the vertical transport is slow, you would find more CH4 is accumulated in the lower troposphere (timescale$\sim$week)

Line 326: do you run CH3CCl3 & SF6?, say within the CCMI framework?

Line 346ff: suggesting too much emissions in the NH, where most of Anthro emissions are...May be you can test this better by site-level comparisons.

Line 353ff: I cannot find this 1 year mismatch (please be clear), instead I find a persistent offset during 1984-1991 (how the major and minor ticks marked in Fig. 5; the labeled ticks only should be major?

Line 369: How can you say that? I thought your optimization was not good for this

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period, because the number of observation sites may not have coverred the global reasonably well. I mean biased high toward the NH. Could you check how many SH sites you have data before 1988.

Line 374: Most likely due to an overestimation of China emissions (e.g., Saeki and Patra, GOSL, 2017, and references therein) (regional inversion is needed for adjusting such regional emission biases)

Line 378: "...which is also a remote site" and remote from China emissions

Lines 394ff: I am not very sure if the comparison with GOSAT/SCIA are adding any values to this work. Better be kept aside for a full paper, unless the reasons for the mean offsets are figured out and discussed. For instance you could compare your results with the ACE-FTS data to find out if there is any bias in the stratospheric CH_4 as there is no significant offsets in the tropospheric CH_4 is seen in comparison with surface data and HIPPO.

Line 426ff: The emission increase in the 1990s is apparently linked to OH increase in AM; which sector can provide this extra emissions. I think this result is very different from what I have seen in the literature, and thus needing some explanation. Surprisingly, the emission increase rate in the 1990s is greater than the recent regrowth period.

Lines 484ff: The discussions using Fig 9-11 aren't that interesting as presented. I would recommend the authors to move these plots to the supplement or show 1-2 panels in the main text; for example all the 4 panels in Fig 9 & 10 are essentially showing very similar distributions. The S0Aopt and S0Wopt are also showing similar behaviour. This is mainly because the emission (E)-a priori emissions are the same in both the simulations, and the correction emissions Del-E following Anthropogenic or Wetland emission patterns only play minor role.

I am actually curious if you could use some of the continental sites, e.g., NWR, LEF, SGP or TAP, and use the model-measurement comparisons to say whether the S0Aopt or S0Wopt are more realistic.

Line 519: Such high correlations are a bit surprising, if I see the lines in Fig. 12. For example AGR show -ve trend, yet show positive correlation. How is that possible?

Line 633: This is similar to the essential conclusion in some other publications as well, where ENE and Animals were made responsible for the post-2006 CH_4 growth rate. I guess it is extremely difficult to separate emissions from Animals and Wetlands by 13C signature in CH_4.

Lines 638ff: I am curious if inconsistency between the tropospheric OH and CH_4-loss by OH are arising from the spin-up. Did you spun-up the simulations using different OH from the 1970s?

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