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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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The author presents an analysis of the methane global budget using an atmospheric chemistry model over almost 4 decades (1980-2017) and considering changes in both emissions and sinks. As conclusion, they provide likely scenarios of changes in sources and sinks of methane explaining the methane growth different periods of these past decades.

GENERAL COMMENT

The manuscript is well written and organize, and quite easy to follow. The methodology and different steps are well explained. Understanding the methane budget and particularly its changes is crucial to establish pathways for emission reduction. As a result, this study contributes significantly to the debate.

However part of the result section is quite lengthy and may be shortened and more straightforward– see specific comments below. The comparisons to the observation (surface and satellite) as done here, do not really help in discriminating better scenarios.

Regarding the abstract and conclusion of the paper: there might not be a single driver of the recent methane growth, but a combination of different increasing/decreasing sources/sinks (Saunois et al., 2017). Different studies (as cited) may appear in disagreement as only the major driver (one source) is highlighted (in the abstract, title or conclusion) but considering the uncertainty of the estimates, this could be a combination – with high uncertainty on the relative contributions; but this does not mean that there are no agreement between studies. One of the objective is to “investigate the possible driverS” of the methane changes. So why keeping a single source as a driver? Why not considering the combination and trying to estimate the relative contribution? Also, testing different initial emissions would further strengthen the resulting range.

Finally, I would recommend publication in ACP after major revisions addressing the general and specific comments highlighted in this review.

SPECIFIC COMMENTS

Section 1 – Introduction

Lines 38 to 42. Removes those lines: it’s stated later and better near the end of the section.

Line 43- 47. Add a general reference here (e.g. Saunois et al. (2016)).

Line 60-67. There have been other studies that tried to highlight some more likely scenarios combining changes from individual sources. E.g., Saunois et al., (2017) suggested a combination (with relative contributions) of changes from the different methane sources contributing to the renewed growth (increasing from fossil, agriculture and waste emissions, decrease of biomass burning, . . .).

Section 2 – Methodology and data

Line 125. “BMB emissions contribute to IAV”. . . because wetland emissions are considered constant in the study. Wetland emissions contribute a lot to methane emission IAV (Kirschke et al., 2013).

Line 151. “. . . largely underestimate. . .”. What is largely here? Please quantify.

Line 153. “optimization simulations”? The optimization approach is described later (from line 169), but this description should directly follow the first mentioning of “optimization approach”. Which observations are used for this “optimization”? surface marine boundary layer – described after? Please specify and put Section 2.3 Observation before Section Simulation Design.

Line 174. The relative contributions from the different sectors are also kept as in the initial emission set up, and depend on it. As a result this highly depend on the initial set of emissions. Testing other set of initial emissions would really be valuable to assess the uncertainty related to the set up and strengthen the results. EDGARv4.3.2 instead of CEDS for example. Other wetland emissions including IAV. . .

Line 180. DeltaE include IAV missing from the initial emissions. In the case of SAopt, this is attributed to anthropogenic emissions, is that realistic?

Line 184. The reader would probably want to have fast access to FigS2. . Indeed all the discussion is on emissions from SAopt and SWopt and not on the initial emissions. That would be better to combine Fig1 and Fig S2

Line 200. At which frequency are the observations considered? Frequency of the sampling of the model? Monthly?

Section 3 Results and discussions.

Line 241. Testing different wetland emission data sets – with different seasonality may help to quantify the influence of wetland emission seasonality on the observed bias.

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OH influence could be seen in simulation S2 with higher OH: is S2 performing better?

Line 244. Is it reasonable to state that SWopt perform a bit better that SAopt? So far no comparison is made between the two simulations.

Line 253: The reasons of the biases compared to HIPPO are exactly the same as those seen with surface observations. See comment for line 241.

Line 255. 2% difference with the updated GFDL-AM4.1, using optimized emissions. How much was the difference when using initial emissions? How much better is it compared to the previous version of GFDL-AM4.1?

Line 264. Isn't it by construction? As total emissions are optimized using surface marine boundary layer observations...

Line 274. Not clear about the 1-year mismatch. There is not always a 1-year delay between spikes...

Line 275-281. Are those numbers necessary in the text? What is the point? That could be summarize in a Table for the full numbers and in the text summarize to "SWopt performs better over this period while SAopt performs better over that period". The differences in growth rate are much lower than their own range of uncertainty.

Line 283. Does it imply that the results (following results on the emissions changes) are less robust for this region? This is unfortunate as most of the methane emissions occur in the NH.

Line 286. Indeed. Related to wetland emissions? Is it related to the missing IAV in wetland initial emissions?

Line 301. Could the bias model sampling be overcome/reduced? Does the bias change with the grid box choice for coastal sites?

Line 306-319. The model observation comparison using GOSAT and SCHIAMACHY is not really helpful. As the model is optimized against surface observations, it's saying that surface observation and satellite data see similar trends (but are offset by latitudinal biases), that biases exist in the satellite data (latitudinal biases), and that uncertainties in the transport model (especially in the stratosphere but not only) can explain the difference between model and satellite columns. Nothing new for an atmospheric model assessment, and this comparison does not discriminate one simulation from the other. This part may be removed and put in the supplementary material.

Line 325. By construction?

Line 332-333. This sentence should be removed and put in Section2.

Line335_336. This sentence could be removed with causes of interannual variability put in sentence of line 329-330. Also 1991-1992 high IAV is also related to Mt Pinatubo eruption (decrease in CH_4) (ref. e.g., Banda et al., 2016, https://www.atmos-chem-phys.net/16/195/2016/; Dlugokencky, E. J. et al. Changes in CH_4 and CO growth rates after the eruption of Mt Pinatubo and their link with changes in tropical tropospheric UV flux. Geophys. Res. Lett. 23, 2761–2764 (1996); 21. Bousquet, P. et al. Contribution of anthropogenic and natural sources to atmospheric methane variability. Nature 443, 439–443 (2006)).

Line 343. Is it possible to overplot Naus et al. OH level on Figure 8?

Table 3. Single values are provided for the emission estimates while several simulations were performed. The authors compare their initial emission to the literature – which could have been done in Section2. However they stated that there initial emission were underestimate and then all the paper is on the optimized emissions. So Table 3 should compare their optimized emissions with the literature!

Line 351-353. Total natural emissions from bottom-up estimates are much lower than top-down because there are not constrained and just an “addition” from independent individual source estimates, knowing the large uncertainty of each natural source... The initial emissions should be much comparable to top-down estimates, as the large

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source from freshwater is not included in the initial emissions (about 100Tg in the bottom up methane budget).

Line 353-354. Not really, see comment above. The difference between the initial emission of this study and the bottom-up estimate from Kirschke et al and Saunois et al., is mainly driven by source not included in the initial emission set up (freshwater), and probably double counted in the bottom-up budget. Estimates for other natural sources from Kirschke et al., 2013 and Saunois et al., 2016 should be added. This is also due to the use of a climatological value for wetland emissions from the 2000s applied to the whole period. As IAV and trends are missing in the initial emissions, some signal is lost compared to estimates reported by Kirschke et al. and Saunois et al.

Line 356. Remove Saunois et al., 2016 as the study starts in 2000.

Line 358 and following. Well, that would be better to show the range of total anthropogenic and wetland optimized emissions and to compare these ones with the literature.

Table 3. It also presents values for the more recent period. These values may be compared with the updated global methane budget recently released in ESSDD Saunois et al., 2019 (https://www.earth-syst-sci-data-discuss.net/essd-2019-128/)

Section 3.3.1. These results are quite expected knowing the distribution of anthropogenic emissions and wetland emissions. There are interesting but could be more when compared against observations. If the surface methane DMF and growth rate observed values at each sites are over plotted on these spatial distributions, does it help in discriminating which optimized emissions fit the best?

Line 400-401. By construction?

Line 406. .. are KEPT constant. . .

Line 432 and line 437. How can we explain higher sink?

Line 453. Indeed…Is testing a different initial inventory an option? It would help, confirming or infirming some results…

Line 454. Other sectors (than wetland?)

Section3.3.2: could this section be shortened? It's quite hard to follow… Instead of describing each simulation in detail (with numbers etc..), would it be shorter to conclude for each period, which sector(s) drive the changes (increase…) and if the two simulations agree or not? And keep the details and numbers for the supplementary….

Line 457 and following. The two sensitivity tests may be presented in Section 2 as well. And included in the above discussion..

Section 3.4 Sensitivity to OH. How does the model compare with the other CCMI models? (see Zhao et al., 2019 Fig 4 and 7. https://www.atmos-chem-phys-discuss.net/acp-2019-281/acp-2019-281.pdf)

TECHNICAL COMMENTS

Line 87. 1980-2017 instead of 1980-2014

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