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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.

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We thank the reviewer for the insightful comments and suggestions on our manuscript. Below, our responses in bold text follow the reviewer's comments shown in plain text.

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.

Reply: We thank the reviewer for the positive comments.

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.

Reply: We thank the reviewer for the suggestions. We have revised the manuscript and shortened the result section.

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.

Reply: We agree with the reviewer that it is more likely a combination of different drivers instead of one single driver that lead to recent methane growth, which is also indicated by the model results discussed in the manuscript. The more important question is the relative contributions from these drivers, which, as mentioned by the reviewer, is highly uncertain. We have included additional discussions in the revised manuscript regarding the relative contributions. However, as mentioned in the manuscript, we need additional observational constraints (e.g.,

methane isotopes, ethane) to better quantify the relative contributions, which is the next step of the work. With different initial emissions, the optimized methane emissions totals would be similar but spatial distribution and seasonality could be different. We did one sensitivity test with time-varying wetland emissions (i.e., S0Origtswet) as shown in Figures S2 and S3 in the Supplement. Using time-varying wetland emissions does not alter our results much. Based on the results comparison in Figures S2 and S3, which suggest better performance by climatological wetland emissions (i.e., S0Orig), we decide to start with this wetland emission. As this manuscript is more focused on global totals, even with different initial emissions (e.g., different wetland emissions), after the emission optimization, the impacts would be very small on global averages, but could be large on regional scales. Also, the relative contributions would be different. However, the Aopt and Wopt sensitivities conducted in this work provide extreme scenarios for the contributions from anthropogenic sources and wetland sources. As mentioned earlier, additional observational constraints could be applied to refine the resulting ranges.

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

Reply: We thank the reviewer for the suggestions. We have revised the manuscript and addressed the comments point by point below.

SPECIFIC COMMENTS Section 1 – Introduction Lines 38 to 42. Removes those lines: it's stated later and better near the end of the section.

Reply: We have removed these lines in the revised manuscript.

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

Reply: We have added the reference in the revised manuscript.

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, . . .).

Reply: We have included the statement of combined changes in sources with the reference in the revised manuscript as below:

"The observed renewed growth since 2007 has been explained alternatively through increases in tropical emissions (Houweling et al., 2014; Nisbet et al., 2016) such as agricultural emissions (Schaefer et al., 2016; Patra et al., 2016) and tropical wetland emissions (Bousquet et al., 2011; Maasakkers et al., 2019), increases in fossil fuel emissions (Rice et al., 2016; Worden et al., 2017), decreases in sources compensated by decreases in sinks due to OH levels (Turner et al., 2017; Rigby et al, 2017), or a combination of changes in different sources such as increases in fossil, agriculture, and waste emissions with decreases in biomass burning emissions (Saunois et al., 2017)."

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).

Reply: We have clarified this statement in the revised manuscript as below:

"Although wetlands are in reality a major contributor to interannual variability in methane emissions (Bousquet et al., 2006; Kirschke et al., 2013), our use of climatological wetland emissions causes the interannual variability in our methane emissions to be dominated by BMB emissions."

Line 151. ". . . largely underestimate. . .". What is largely here? Please quantify.

Reply: We have evaluated the model results with initial emission inventories, which is shown in Figure S2 in the Supplement. The simulated methane DMF is

about 126 ppb lower than observed CH4 DMF from NOAA GMD surface observations (with RMSE = 120 ppb).

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.

Reply: Thank you for the suggestion. We have reordered the sections such that the section on Observations appears before 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. . .

Reply: We agree that our results depend on the individual contributions in the initial emission inventories. Using different initial emission inventories may help assess the associated uncertainties of individual sector contributions to a certain degree, but to better quantify the individual contributions and assess the uncertainties, we could apply additional observational constraints, which is our next step of the study.

Methane anthropogenic emissions from EDGARv4.3.2 are about 5-9% lower than CEDS anthropogenic emissions, mainly due to lower emissions from energy sector (Janssens-Maenhout et al., 2019). The share of ENE to total anthropogenic emissions in EDGARv4.3.2 (i.e., 33%) are also lower than those in CEDS (i.e., 38%), but both increase after 2006. We are unable to test EDGARv4.3.2 emissions in this work but will consider it for future work.

To address the reviewer's point about wetland emissions with IAV, we performed a test simulation with wetland emissions including IAV for 2001-2015 (i.e., S0Origtswet) based on an extended ensemble version of WetCharts (Bloom et al., 2017) as shown in Figures S2 and S3. The results from this simulation are very similar to those from climatological wetland emissions (i.e., S0Orig). We expect that optimization of wetland emissions will cause the original IAV in emissions to be lost.

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

Reply: The IAV of methane emissions are mainly dominated by that from wetland and biomass burning. However, IAV could also exist in anthropogenic emissions due to the dependence of microbial methane sources, such as rice paddies, on soil temperature and precipitation (e.g., Knox et al., 2016). The optimization to match observations resulted in higher IAV in total emissions than in the initial emissions. Because the purpose of S0Aopt is to investigate the role of changes in total anthropogenic emissions (anthro plus BB) rather than individual sectors, we applied this IAV to all sectors which we acknowledge introduces some unrealistic IAV in the anthropogenic emissions. We chose this experimental construct to limit the number of sensitivity simulations.

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

Reply: We have combined these figures as new Fig 1 in the revised manuscript.

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

Reply: Since the frequency of the model output is monthly, the observations are

also monthly-based.

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

Reply: We agree that different wetland emission datasets can help assess the impacts of wetland emission seasonality on the observed bias. We did a sensitive test with time-varying wetland emissions (i.e., S0Origtswet) for 2001-2015 as shown in Figures S2 and S3 in the Supplement. In terms of seasonality, we did not find much difference between S0Orig and S0Origtswet. In a future study, we plan to simulate wetland emissions in the land model (LM4.1) coupled to AM4.1 to better capture the spatial and temporal variability of wetland emissions than prescribed emissions.

The S2 performance is similar to S0 as we re-optimized methane emissions based on higher OH case. Higher OH case does not change the spatial and temporal variability of OH but only the magnitude of OH levels.

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.

Reply: As we optimize the global total emissions instead of spatial distribution, globally, the performance of Wopt and Aopt is very similar to each other, however there are regional differences because of the differences in the spatial distribution of emissions. For example, in the Southern Hemisphere, Wopt performance is very similar to Aopt. In the Northern Hemisphere, Wopt performs better than Aopt at KUM, POCN20, ASK, and TAP sites, while it performs worse at KEY, WIS, and UTA. It is really site-specific. We have included site-specific comparisons between S0Wopt and S0Aopt in the revised manuscript as below:

"S0Aopt and S0Wopt simulate very similar surface methane DMF and their comparison with NOAA-GMD observations at individual sites show both simulations to be biased low over Southern Hemisphere sites, but the low bias decreases northward (Figure S5 in the Supplement). The simulations are biased moderately high (with RMSEs up to 40 ppb) over tropical regions (e.g., POCS15, POCS10, SMO, POCS05, POCN00, CHR, and POCN05). These sites are mainly remote sites and surface methane DMF represents background methane levels. The overestimates are likely due to overestimation of emissions over Southeast Asia (e.g., Saeki and Patra, 2017, Patra et al., 2016, and Thompson et al., 2015), which could affect these remote sites through transport. However, the model predicts surface methane DMF relatively well at Ascension Island (ASC, 8oS, 14.4oW, 85 m), which is also a remote site without impacts from East Asia. The high biases over the tropics suggest a need to improve regional emissions (e.g., Southeast Asia). Moderate overestimates also occur at Mahe Island (SEY, 4.7oS, 55.5oE), a location that could be affected by air masses from polluted areas over the tropics and Northern Hemisphere. Over middle and high latitudes of the Northern Hemisphere, both S0Aopt and S0Wopt simulate surface methane DMF relatively well at most sites, except at Key Biscayne (KEY, 25.7oN, 80.2oW), Tae-ahn Peninsula (TAP, 36.7oN, 126.1oW), Park Falls (LEF, 45.9oN, 113.7oW), and Mace Head (MHD, 53.3oN, 9.9oW). KEY, MHD, and TAP are sampled under onshore winds, whereas LEF are affected by local sources and model transport. The high biases at these sites could be due in part to model sampling bias (e.g., model grid box overlapping land while samples are collected at coast with onshore winds) and uncertainties in local emissions (e.g., possible overestimation in the emissions over East Asia). On the other hand, both S0Wopt and S0Aopt are able to capture monthly variations in methane at most of the sites except at LEF, where R = 0.4 for S0Wopt and 0.5 for S0Aopt, respectively. In general, both S0Wopt and S0Aopt are able to reproduce the surface methane DMF and capture the trend at most sites (e.g., with R greater than 0.5 at 98% of total sites and with RMSE less than

30 ppb at 74% of total sites). As shown in Figure S5, S0Aopt in general better estimates methane trends and growth over low latitudes of the Southern Hemisphere (e.g., SMO) and middle/high latitudes of the Northern Hemisphere (e.g., ASK, KEY, WIS, UTA, NWR, UUA, LEF, CBA, STM, and ALT) than S0Wopt. Based on the site-level comparisons between S0Wopt and S0Aopt, anthropogenic emissions over Southeast Asia are likely overestimated in both S0Aopt and S0Wopt, while they could be underestimated at WLG and NWR in S0Wopt but be reasonably well represented in S0Aopt."

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.

Reply: Please see reply to 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?

Reply: With initial emissions, the simulated methane profiles are about 12% lower than HIPPO measurements. For the standard version of GFDL-AM4.1, which uses prescribed methane concentrations, the simulated methane profiles are about 5% higher than HIPPO measurements in the Southern Hemisphere and 4% lower in the Northern Hemisphere.

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

Reply: The match of global means is certainly by design, but not that for latitude bands. As shown in Figure 4 in the main text, the model is still able to capture methane trends and variability at different latitude bands. We have revised the sentences as below:

"S0Wopt and S0Aopt are also able to reproduce global annual mean surface

methane DMF (with root-mean-square-error (RMSE) = 10.4 ppb in S0Wopt and 11.6 ppb in S0Aopt) over 1983-2017, which is expected from emission optimization. Meanwhile, both simulations are able to reproduce the methane timeseries very well (with R = 1.0 in both S0Wopt and S0Aopt) over different latitude bands as shown in Figure 4."

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

Reply: We corrected a bug in the scripts for model evaluation and updated all the relevant plots. We then updated results and discussions in the revised manuscript as below:

"Table 3 summarizes methane growth rates during 1984-1991, 1992-1998, 1999-2006, and 2007-2017. S0Aopt and S0Wopt simulate very similar methane growth rates as their emission totals are the same. During 1984-1991, both S0Aopt and S0Wopt slightly overestimate methane growth rates by 2 ppb yr-1, possibly due to fewer available observations used for emission optimization during this time period than afterwards. After 1991, the simulated methane growth rates are in general comparable to the observations (with annual mean difference within $\pm$1 ppb yr-1). The major discrepancies in the simulated methane growth rates and observations occur over the tropics and high northern latitudes as shown in Figure 4. Over the tropics, both S0Aopt and S0Wopt overestimate methane growth rates (by about 5-10 ppb yr-1) during 1984-1990 when there were limited observations available, but are able to reproduce methane growth rates relatively well afterwards. Agreement of the methane growth rate is worse in the Northern Hemisphere than in the Southern Hemisphere, especially at high northern latitudes, mainly due to the large bias during 1984-1988 and a slight shift in peak growth (or peak decrease) during 1997-2005. The number of observational MBL sites does not provide adequate coverage of the globe, especially in the 1980s, which could have different impacts on the Northern and Southern Hemisphere

when optimizing global total emissions. In general, S0Aopt estimates slightly better methane growth rates than S0Wopt, especially over 30-90o N. The biases in methane growth rates also suggest a need to refine regional emissions."

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.

Reply: As suggested by the reviewer, we have summarized the growth rates in the table and revised these sentences in the revised manuscript (see reply to comment line 274).

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.

Reply: We corrected a bug in the scripts for model evaluation and updated all the relevant plots. After the correction, both Aopt and Wopt are able to reproduce methane growth rates despite there is a slight mismatch (1-2 years) during 1998-2008. But compared to other regions, the correlation is slightly worse over 30-90o N. This suggests a need to improve/optimize regional emissions.

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

Reply: Many reasons could lead to such biases over 30-90N. Uncertainties in the IAV of both wetland and anthropogenic emissions could lead to such bias. The biases also suggest a need to improve/optimize regional emissions.

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

Reply: Yes. If we move the grid box to the ocean side, the bias is reduced sig-

nificantly. For example, at KEY and MHD sites, the RMSEs are reduced from 90 ppb to 33 ppb and from 50 ppb to 20 ppb, respectively.

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.

Reply: As suggested by the reviewer, we have moved this part to the Supplement.

Line 325. By construction?

Reply: Yes. Both simulations reproduce the global growth rates by design. But the model with optimized emissions is still able to reproduce the growth rates at different latitude bands.

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

Reply: As suggested by the reviewer, we have moved this sentence to Section2.

Line335-336. This sentence could be removed with causes of interannual variability put in sentence of line 329-330.

Reply: As suggested by the reviewer, we have combined these sentences as below:

"As shown in Figure 5, the optimized emissions in general increase during 1980-2017, with an annual mean of 580±34 Tg yr-1 (mean±standard deviation) and show much larger interannual variability during 1991-1993 and 1997-2000, which

is likely due to the strong El Niño events during 1991-1992 and 1997-1998 as well as the Mt Pinatubo eruption in 1991 (Dlugokencky et al., 1996; Bousquet et al., 2006; Bândă et al., 2016)."

Also 1991-1992 high IAV is also related to Mt Pinatubo eruption (decrease in CH4) (ref. e.g., Banda et al., 2016, https://www.atmos-chemphys.net/16/195/2016/; Dlugokencky, E. J. et al. Changes in CH4 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)).

Reply: We thank the reviewer for providing the references. We have included these references into the revised manuscript.

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

Reply: We have included OH anomaly from Naus et al. (2019) in the Figure 6 in the revised manuscript.

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!

Reply: Table 3 includes numbers for optimized emissions as reflected in rows of and sum of sources. Also, for and sum of sources, we include estimated ranges based on different OH levels as well. To address the reviewer's comment, we have included estimated ranges for individual sectors based on the Aopt and Wopt under different OH levels, which is now shown in Table 4 in the revised manuscript.

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

Reply: We thank the reviewer for the explanation. We have included this discussion in the revised manuscript as below:

"Since there is no observational constraint on bottom-up estimates, total natural emissions are simply summed over independent individual sources, which could be overestimated in the bottom-up approach considering the relatively large uncertainties in each individual source. In addition, in the bottom-up estimate from Kirschke et al. (2013) and Saunois et al. (2016), some other natural sources, such as freshwater, are not included in the initial emission inventories in this work; however, they are likely double counted in the bottom-up estimates (e.g., high-latitude inland waters are likely also considered as wetland areas) as pointed out in Saunois et al. (2019)."

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.

Reply: We thank the reviewer for the explanation. We have included this discussion in the revised manuscript (see reply above) .

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

Reply: Removed.

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/)

Reply: We have updated Table 3 (now Table 4) with values from Saunois et al. (2019).

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?

Reply: The overlay plots have been generated as shown in Figure S7 and Figure S9 in the Supplement. But S0Aopt and S0Wopt gives very similar methane DMF and growth rates. We included site-level comparisons in Section 3.1. As also suggested by the other reviewer, we have removed this part into the Supplement.

Line 400-401. By construction?

Reply: For global trends, the match of model simulations with observations are by design.

Line 406. .. are KEPT constant. . .

Reply: We have corrected this in the revised manuscript as below:

"Since wetland emissions and other natural emissions are kept constant every year in S0Aopt, with increases in OH levels during 1983-1998, all tagged natural tracers show a weak decreasing trend."

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

Reply: Although concentrations of tracer CH4WET decrease, with the increases in OH levels during 1999-2006, CH4WET sinks also increase, which could be higher than wetland emissions. Also, during this time period, CH4WET decreasing trend is very week (e.g., -0.6 ppb/yr). During 2007-2017, wetland emissions are lower compared to 1999-2006 (see Figure 1) while OH levels are higher. Therefore, the decreasing trend (e.g., -4.6 ppb/yr) is much larger compared to 1999-2006.

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

Reply: See reply to General comments above.

Line 454. Other sectors (than wetland?)

Reply: Yes. For S0Wopt, except wetland, which is optimized, all other sources are based on the initial emission inventories.

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. . ..

Reply: We thank the reviewer for the suggestion. We have shortened and revised this section as Section 3.3 in the revised manuscript.

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

Reply: As suggested by the reviewer, we have included two sensitivity tests in Section 2 as well as in Section3.3.

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-physdiscuss.net/acp-2019-281/acp-2019-281.pdf)

Reply: In general, our OH trend is within the range of OH trends in Zhao et al. (2019). From 1980 to 2000, OH in AM4.1 increases by 4.7%, comparable to 4.6$\pm$2.4% in Zhao et al. (2019). During 2000-2010, OH anomaly varies from -0.29 mole cm-3 to 0.34 molec cm-3.

TECHNICAL COMMENTS Line 87. 1980-2017 instead of 1980-2014

Reply: Thanks, corrected.

References: Bloom, A. A., Bowman, K. W., Lee, M., Turner, A. J., Schroeder, R., Worden, J. R., Weidner, R., McDonald, K. C., and Jacob, D. J.: A global wetland methane emissions and uncertainty dataset for atmospheric chemical transport models (WetCHARTs version 1.0), Geosci. Model Dev., 10, 2141-2156, https://doi.org/10.5194/gmd-10-2141-2017, 2017.

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Please also note the supplement to this comment:

https://www.atmos-chem-phys-discuss.net/acp-2019-529/acp-2019-529-AC2-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-529, 2019.