Earth Syst. Sci. Data Discuss.,
https://doi.org/10.5194/essd-2019-128-RC2, 2019

Interactive comment on “The Global Methane Budget 2000–2017” *by* Marielle Saunois et al.

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Received and published: 6 December 2019

Rev of ESSD-2019-128

This is a very well written and thorough review of the methane budget. The abstract and introduction read easily and layout the scope of the work. The remaining 2000 lines are a bit harder to get through, but this paper is meant as a reference work and not a "beach read." For a paper on methane, it is great to see Ehhalt’s original work as one of the prime references. I have two major suggestions and then several minor/editorial comments on my read-through.

#1 As a reference work, it would be very useful to have a Table of Contents up front. The TOC should be as detailed as possible (e.g., add another level: 3.3.2.1. Lakes and Ponds) and possibly include a list of key words for each section. This would greatly help those looking for specific discussions.

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#2 The Section 3.3.5 on CH4 Lifetime contains some serious errors in understanding. I am not sure how to fix it, but the authors should think about the purpose and what they want to get across. See details below.

L217: The Prather 10% is only 68% confidence interval, perhaps if the Saunois 15% is 90% confidence, they are consistent.

L225: Probably more important here is not 'credible scenarios,' but credible mitigation strategies.

L231: do you want to mention the Paris Accord's "stocktaking"?

L243: This is a slight mis-direct. It is not the lack of some direct observations, but by my calculation, the number of OH "measurements" needed to integrate the loss of methane (1 km x 100 m parcels, every $\frac{1}{2}$ hour (clouds)) is of order 2x1014 per yr. I would put this as an impossible task without some modeling and other tricks.

L290: Here and later, you cannot reference a work in prep as a real reference. You can describe ongoing work by Staevert and colleagues that will follow up this paper and come up with a shorthand notation for this work.

L318-324: If you are being precise, please define where the edges of the 1-degree grid fall, one the 0.0, or 0.5, or? Also does what you describe mean that coastal boxes (<100% Land and <100% Ocean) have no emissions – you should definitely want to warn people as it will look funny when plotted. Also what do you do about large lakes (resolved at 1 degree)?

L383: This discussion of natural vs. anthropogenic intrigued me. We have a long history of trying to break emissions cleanly in these two camps. With natural often being ignored in the scenarios (RCP, SRES), such that in the IPCC SAR & TAR, I had to make up the natural sources to ensure a consistent budget and initial trajectory for the future scenarios. The other problem is that attribution is still not an exact science and thus declaring something like wildfires or wetland loss as "anthropogenic" is not trivial.

Let me propose that the sources be split into "pre-agricultural" and new "anthropogenic" types of sources (such as ag or industry). This allows us to work our best science on how wetland methane emissions have changed, without trying to ascribe cause. The changes in the "pre-ag" sources probably can be attributed in many cases to direct human intervention.

L529: can you make this simpler to read: "perhaps because AMAP analysed data from a wider range of inventories and projections, plus it was referenced to one year only (2005) rather than averaged over a decade, as done here."

L552: "realized" in what? the scenarios or real life?

L660ff: In terms of shale gas emissions, is the DeCarlo work on the Marcellus Shale consistent with these? Goetz, J. D., et al., Analysis of local-scale background concentrations of methane and other gas-phase species in the Marcellus Shale, Elementa Science of the Anthropocene, 1–20, doi:10.1525/journal.elementa 182, 2017.

L1026: Is this really true? If we have all positive values but some far outliers, then you would reject a positive flux because the std dev exceeded the mean?

L1174: I am unsure what the "up scaling issues" means.

L1205ff: This sentence on the Petrenko work is a bit perjorative and full of inuendo ("If it is correct", "which is questionable"). There is nothing obviously wrong with the work, unless the 14C community is worried about it, and if so state why. The sentence on L1208 about the discrepancy is fine.

L1233: I am not sure what 'termite' as a unit is?

L1500-1507: I am a little worried about this section, but have no simple fix to recommend. (1) the effect of CH4 oxidation on HOx depends on the pathway for H2CO, if it photolyzes on one path you get 2 HOx, otherwise, if OH is high, then OH-destruction it yields 0 net HOx. NOx plays a role in this, but HOx levels are also very important. (2) The short lifetime of OH should go back to Levy or Logan, not a 2004 reference.

(3) "estimate"? One can estimate OH from models without observations. (4) Many, many OH measurements are made in the free troposphere by Brune and others. I am beginning to think it best to stop at ’compounds.’ on L1501 and jump to L1507 ’Following. . .’

L1511: I think you have to recognize here that mass weighted OH concentrations are NOT a good measure of the methane loss. Since this is a methane paper, you should focus on the average methane loss rate from these models. I know this is a hobby of mine, but please look at the table in my 1990 paper (Prather & Spivakovsky, Tropospheric OH and the lifetimes of HCFCs, JGR: 95, 18723-18729, 1990), also reproduced by Mark Lawrence in 2000, and one can see that the average OH varies by 20-40% depending on how it is weighted. What is relevant here is when OH is weighted by mass and exp(-1800/T). In fact, the OH values do not tell you the methane lifetime unless you know the temperature weighting.

L1527-1560: The Holmes et al paper (2013, Future methane, hydroxyl, and their uncertainties: key climate and emission parameters for future predictions, Atmos. Chem. Phys., 13, 285–302, doi:10.5194/acp-13-285-2013) really addresses recent OH variability and should be included with this discussion. (Sorry to push my own papers again, but it is a balanced survey of OH variations from cause [all those listed] to methylchloroform-derived variability.)

L1579: It is unclear if your 60 Tg is irreversible mixing (i.e., loss in strat) or the cross tropopause flux. The amount of CH4 entering the stratosphere is 10x or more larger than this number, most of which crosses into the lower stratosphere inn the sub-tropics and is then transported into the troposphere with little chemistry. I would drop this whole sentence as it does not say much.

L1590: We have a much more accurate measure of the stratospheric loss from the Plumb & Ko relationship and the observed N2O-CH4 tracer slopes in the lower stratosphere. I do not know when this was last revisited, however.

L1654ff: This section 3.3.5 on CH4 Lifetime has some major problems. Excuse my didactic diversion here. "Lifetime" is a budgetary number since the standards and notation in the 1995/96 IPCC SAR through AR5. It is the burden divided any loss rate. Note that the total burden in the system must be used to take advantage of adding inverse lifetimes. Hence you will see the "lifetime of CH4 against trop OH loss" is the Tg (including stratosphere) divided by the Tg/yr lost to OH in the troposphere. If you use this definition carefully then inverse lifetimes can added and we can think of the lifetime due to stratospheric loss being about 160 yr as is recommended here. The OH lifetime should be noted and taken from the MIPs, it should be about 11 yr. "Perturbation time, response time, e-fold time" are used to define the decay of a perturbation about any atmospheric state (again, steady-state does not matter for these definitions). Since 1994, for CH4 we have known that this time scale is about 1.4 times the total lifetime because of chemical feedbacks whereby CH4 suppresses OH. This is well known, consistently modeled and increases all the integrated impacts by this factor. With a budget lifetime of 9.1 yr, the time scale for CH4 perturbations is about 12 yr. Any perturbation to a chemically reactive species will excite a large number of chemical modes – each with its own pattern of species and its own decay time. Hence CO is an indirect greenhouse gas because it generates a CH4 perturbation that decays with a 12-yr e-fold. "Steady state" is not required for these numbers, but the steady-state lifetime does have some magic properties. It is the effective average over all the different chemical modes (their amplitudes and time scales) excited by a perturbation.

The rest of the lifetime numbers from the recent MIPS look to be OK, but make sure that these are calculated using the full burden.

L1708: The references to Rigby 2017 and Turner 2017 really should include also the accompanying Prather and Holmes paper (Over-explaining or under-explaining methane's role in climate change, PNAS 114(21) 5324-5326, doi: 10.1073/pnas.1704884114, 2017) that points out the fundamental error in modeling CH4 perturbations for both papers (i.e., they did not include the chemical feedbacks).

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L1750. Based on notes above, I might expect the time scale here to be 12 yr rather than 9 yr, but I think it is probably close enough for this simple analysis.

L1795ff: This is a good discussion of results, but I wonder how much these inversions depend on the a priori's for lifetime, etc. I do not see how you can cover this here, but can you comment? OK, see L1988.

L1988: Yes, if everyone uses the same trop OH, then the budget total is pretty well fixed (except for T biases, and maybe ITCZ gradients).

L2154ff & L2200ff: Agreed. We continue to produce models with the same range in CH4 OH-lifetime and no means of differentiating them. I would not emphasize the vertical as our model failings include horizontal as well. We need to develop some key observational tests that reflect the reactivity of the air parcels. To start, we need to run the MIPS with some more focused diagnostics that tell us why the models CH4 lifetimes are so different. Even more interesting is that the historical trends and the scenario projections often go in different directions across the model ensemble. And we should not forget that our goal is accurately simulate CH4 loss, not just get OH correct.

L2273: This is where you might want to look at the Holmes (2013 ACP) effort to model OH variability and match it to CH3CCL3 observations and CH4 lifetimes. It is multi-model and more physically based than these inversions.

I am glad you got this paper out. It is valuable and the issues above can be easily addressed. Congrats,

Michael Prather

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