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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Global distribution of methane emissions, emission trends, and OH concentrations and trends inferred from an inversion of GOSAT satellite data for 2010–2015" *by* Joannes D. Maasakkers et al.

Anonymous Referee #2

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Methane's rising. Fast. We don't know why. What's happening to methane is arguably the most interesting current greenhouse problem, and there are very wide implications for efforts to mitigate climate warming. It makes the task of the Paris Agreement so much harder. Maasakkers et al discuss how best GOSAT satellite data can be used to tackle this very important problem, addressing the period 2010-2015, which includes years of very strong growth. Their approach is to use a global inverse analysis – from this not only do they obtain very interesting new estimates of methane emissions and growth trends, but also – a crucial question – they estimate the global abundance of

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hydroxyl and its trend, and in so doing propose a new proxy for OH. The paper is very carefully written and it is thorough in its discussion of the methodological approaches. It takes note of the information available, both top down (e.g. using the NOAA data) and bottom up (inventory estimates). However, the study does give very short shrift to the C-isotopic constraints – although it is likely but not wholly demonstrated that the conclusions are compatible with the C-isotopic shift observed, the paper would be strengthened if the isotopic constraints were given a bit more discussion. Overall this paper is a major contribution and should be published with only minor revisions. It will be much cited.

Detailed comments that the authors may wish to consider.

Page 1 Line 14 Maybe give ± 2 error on 546Tg. (e.g from table 2) P2 L4 "must"? – maybe "is likely to be". Also 'sources' – maybe better to say 'activities'. In general a 'source' is a thing like a cow, an emission has a flux, and an activity includes all manner of sins. P2 L5 No mention of the enormous number of tropical human-lit fires, from cow dung in India to grass and dead leaf fires in the Sahel?? P2 L8 Being picky – "concentration" is in a bucket of water. Mole fraction? Mixing ratio? Or here maybe the 'methane burden'. P2 L9 Being picky again - Citing the growth change in % is referring to a moving target. It would be much better to cite growth in parts per billion – i.e. not a % of a changing total. And does the text mean 1% of the whole burden or the anthropogenic increment? By saying 1% on Line 9, the text implies growth rates were around 16 to 17 ppb/yr in the 1980s – well, they were very high, but not that high, except perhaps in 1991. Also, 2014 growth was notably high: nearly 13 ppb in about 1820 – that's more than 0.4%. P2 L25 – GOSAT – maybe should mention Miller et al. Nature Communications 10, Article number: 303 (2019), either here or in the next paragraph. P4 L9 – 'aseasonal' – I'm not sure this is valid. Gas use in Eurasia at least is very winter-dependent, and gas pumping scheduled accordingly. Biomass burning in both the boreal realm (summer) and tropical (dry season) is very seasonal, and in the tropics is almost entirely anthropogenic – there are few lightning bolts in the dry winters. P4 L13 – note that Petrenko et al.s work suggests the geological emissions are much smaller than previously suggested. Petrenko, V.V. et al. (2017) Minimal geological methane emissions during the Younger Dryas–Preboreal abrupt warming event. Nature doi:10.1038/nature23316 P4 L30 – 2.4% - quantify that in Tg and then the increase in ppb. A % is a moving thing. Also on P10 L33 P5 L30 – 'concentrations' again. . ..and also on P5 L1 P6 L8 to 10 – Mention Naus, et al. (2019) Constraints and biases in a tropospheric two-box model of OH. Atmospheric Chemistry and Physics 19, 407-424. and also perhaps Lelieveld, J., et al. (2016), Global tropospheric hydroxyl distribution, budget and reactivity, Atmos Chem Physics, 16, 12477-12493. P6 L 13 and P5 Table 1 – Cl sink of 9 Tg/yr – should mention Hossaini, R., et al. (2016) A global model of tropospheric chlorine chemistry: Organic versus inorganic sources and impact on methane oxidation. Journal of Geophysical Research: Atmospheres 121.23 (2016). P6 L27-30 – the seasonal bias and correction – this is a weakness of the inputs and although it is comforting to know it doesn't affect the results significantly it should be target for future improvement. Seasonal fitting is a tool in identifying specific sources of emissions – wetlands emit less in drought; biomass burning is limited in the wettest periods. So it's important to get seasonality right. P7 L6 – s.d. 13 ppb – i.e about the same as growth in a strong-growth year. P7 L30 – 'correlation in the observational error' maybe discuss this a little more – what is the 'regularization factor' allowing for? P8 L9 – ratio of elements 1009/7 – is this equalisation of OH and emissions correct : how about the prescribed non-OH sinks. P8 L15 – impact of methane change affecting OH – maybe it can be neglected globally but is that true at all latitudes (and longitudes)? P9 L4 and P 11 L16 – Soil uptake is probably quite strong in many moist well aerated wet tropical savanna woodlands, and also Cl uptake in the boundary layer may be strong in some locations. Thus negative emissions are likely in some areas. P9 L19 – 'perviously' P9 L26 – significant sources in high northern latitudes and strongest OH in tropical troposphere. P10 L22 – should probably discuss the Naus et al paper somewhere – maybe here? (see earlier comment on P6L8) P11 L2. Very interesting, especially overestimate in China. Maybe compare with Figure 1 in the Miller et al GOSAT paper – they see growth in China, India and tropical Africa. P11 L30 - 32 – in the East Asian context maybe mention Thompson et al, 2015 (Methane emissions in East Asia for 2000-2011 estimated using an atmospheric Bayesian inversion. Journal of Geophysical Research, 120)? Or 2018 (Variability in Atmospheric Methane From Fossil Fuel and Microbial Sources Over the Last Three Decades. Geophysical Research Letters, 45). P12 L17 – this finding on lower Chinese emissions is well substantiated and is a major conclusion that should be discussed in a bit more detail. P12 L18 – no mention of tropical wetland increases? Why are they excluded from likely sources of growth? Or tropical fires if there is a concurrent shift in another emission source that's masking the isotopic impact?? P12 L29 – absence of information north of 60N is a problem as many of Russia's largest gasfields are north of this line, and even in Canada and Norway there are gasfields are nearly at 60N. P12 L35 – Uzbek leaks – makes intuitive sense. P13 L 3 Likewise, intuitively Venezuela's industry is probably leaky and UNFCCC far from actuality. Note wetlands fringing Lake Maracaibo also likely a major source of quasi-natural emissions. P13 L18 – compare with Miller et al (2019) map? – China, India. P13 L20 – inside the error bounds of Ganesan et al, but sense is a bit different as this analysis suggests India is really quite strongly increasing. Should mention the surge in Indian coal production and general air pollution (India's GHG emisisons will soon surpass the European Union's). P13 L27 – source types. Here the paper should say more about the isotopic constraints and the various measurement-based papers by Schaefer et al, Nisbet et al and Schwietzke et al. There is a throw-way sentence at the end of the paragraph, but all the hypotheses about trends need to be consistent with the well-evidenced isotopic shift to lighter values. So far in the paper the discussion has been without isotopic constraint and that is OK, but only for the isotopes then to be used as an independent check on the inferences. For example the statement 'no source type shows a global decrease' – e.g. if biomass burning hasn't decreased a bit, it is then feasible but fairly constraining to make that statement tally with the hypothesis that a declining proportion of methane sourced from biomass burning is masking the isotopic impact of a fossil fuel increase?

P14 L16 – independence of CH4 and OH abundance constraints – this is very interesting and valuable if correct. P14 L24 – note that soil and Cl sinks are prescribed. P15 L3 and also P 16 L22– OH trend - rather different from favoured hypothesis in Turner et al (2017). P15 L 10 – any comment on the extraordinary 13 ppb growth in 2014? Table 2 State emission trend in ppb or in Tg/yr per year. $0.84\pm0.04\%$ seems rather high compared to the NOAA record? P16 L33 – give error on 546 Tg/yr. P17 L17 – high lat biases in the stratosphere – interesting and may have isotopic impact. Fig 2 – would be good to have this large when typeset. Likewise Fig 3 top and Fig. 4. Fig 5 – anthropogenic biomass burning presumably fits in "other" – but this seems very small. African fires in particular are very large indeed and globally for example Saunois et al have the biomass burn & biofuel total more like 30-35Tg/yr. Fig 6 – Norway has a large gas industry. Australia has a large coal seam gas and also offshore gas industry. Turkmenia, UAE and Indonesia are all pretty big. Shouldn't they be on the chart? Much bigger producers than India for example. Fig. 10 – add some comment on the very high and global growth in 2014?

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