Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1110-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "2010–2015 methane trends over Canada, the United States, and Mexico observed by the GOSAT satellite: contributions from different source sectors" *by* Jian-Xiong Sheng et al.

Anonymous Referee #2

Received and published: 20 March 2018

General comments ——————

This paper is mostly an update of the Turner et al. paper of 2016 aiming at estimating trends in methane emissions over North America as inferred from inversion of GOSAT satellite atmospheric weighted columns. Basically, two more years of data are assimilated and the method to estimate the background is revised. The methodology used here has been criticized in details in Bruhwiler et al (JGR, 2017), main arguments being a too short time window for data assimilation making the GOSAT trends sensitive for instance to changes in atmospheric transport, seasonal biases in GOSAT data towards summer months (less clouds = more data), and influence of the choice of the background. In this paper, the authors address only partly these criticisms and add an original sectorial analysis of the inferred trend.

My main concern on this paper is that it does not fully address the extensive criticisms made in Bruhwiler et al. A window of 6 years is still very short to make a robust trend analysis for a species like methane with a 9-year lifetime and I am not sure that adding 23 months compared to Turner is enough. The inferred trend is very noisy (0.2 ± 0.7 ppb.a-1) and moving to percentages is a bit misleading considering the very low value inferred especially when considering the remaining bias of GOSAT data of 4-6 ppb (PVIR4 report from Buchwitz et al., 2016). Nothing seems to be done for the seasonal bias and only the question of backgrounds is addressed in detail. The authors may consider looking at the Cressot et al paper (ACP 2016) on the detectability of emissions at regional scale to figure that trends are very hard to detect with the not-so-dense and biased GOSAT data. The text also lack precision in many places (see specific comments).

Some part of the work in interesting such as the methodology for the sectorial analysis but I think that more time is needed to extent the timeseries and be able to use this approach more safely and provide a reliable update of the Tuner et al. paper addressing all the issues raised since they published it.

Specific comments ——————-

P2 - L10: you may also mention decreasing BBG and quote Worden et al (2018) paper in Nature Comm.

P2 – l14: please add that, contrary to surface networks, the GOSAT data have residual biases of 4-6 ppb as stated in the PVIR reports (Buchwitz et al). Also, the spatial coverage is enhanced by GOSAT but the number of clear-sky scenes is to so huge, and temporal coverage is probably smaller than continuous surface in-situ measurements

P2 – l16-17: there are other reason in Bruhwiler's paper to be added here: impact variations of atmospheric transport linked to short-term window of assimilated data (6-7 years is still short to me), seasonal bias of GOSAT data. You cannot only pickup what arrange you and have to address all limitations raised by previous work.

P2 – l19 : This is not precise enough. short-term trend may depend on local to regional conditions but longer trend is a global signal and one station is enough to get it.

P2 – l20: lack of precision. which version of EDGAR ? 4.2 has too large emission and trend especially in Asia. EDGAR4.3.2 partly corrects this issue. Please be more precise. Also, the dependency to prior assumption may be loose or tight depending on the associated error structure.

P2 - l22-23 : Adding 2 years compared to Turner et al., 2016 does not convince me that the time period will be long enough to overcome the issues raised in Bruhwiler et al (2017). 10 years ($\sim$ methane lifetime) would be a minimum to start extracting reliable information on methane trends to my opinion.

P3 – l6 : 0.7% is 12 ppb. Are you talking of random error or systematic errors ? please be more precise as systematic errors (estimated at 4-6 ppb from PVIR report of Buchwitz et al) ultimately limit the use of GOSAT to estimate emission trends of a few ppb/yr or less.

P3 – l9-10 : the opposite is clearly shown in Bruhwiler's paper whith surface emission changes appear only weakly sensitive to surface emissions. Please rephrase.

P3 – l16 : " the low (10th -25th) percentiles of the deseasonalized GOSAT methane Observations Âż : unclear to me. Which observations ? on which area ? how is it specific to the 0.5x0.5 location. Please rephrase to be more clear and explain what you do exactly.

P3 – l20 : how did you choose these upper bopund 25th percentile ? did you try other range and how sensitive is this choice on your results ?

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Discussion paper

P4 – l3-4 : the trend on enhancements does not seem to be significant considering the error bars. Please provide more quantitative results on this.

P4-l8: Is EDGAR 4.3.2 very different than 4.2 over North and central America ?

P4 – l19-20: did you try not doing so as it reduces largely the number of wetland-dominated pixels.

P4 – l24-25 : what about atmospheric transport ? summing only columns above the high emitting pixels does not account for transport and the potential plume sampling by other GOSAT data. It would be worth mentioning this to clarify what is it you do here.

P5 – l15 : are they all supposed independant ? How robust is this significance ? Although tighter than in Tiuner et al., the PDF is still broad with a sigma of 0.66

P5 – l16-17 : 10.8 ppb enhancement might be due to other causes as stated in Bruhwiler et al. Please mention that this is a maximum and which of the causes raised in Bruhwiler's paper may still apply here. I strongly recommend to add in the following that inferred numbers are maximum number, potentially smaller because of limitations raised in Bruhwiler's paper.

Figure 3 : just ot be sure : the grey bars for wetwhimp and Bloom do reflect the totals for the common pixels ? if not please correct.

P5 – l29 : what about pixerls emitting a lot but with a balanced share of emissions (ivestock & oil&gas) ? Yopur method should discard them. How does it influence your results ?

P6 – l1 : replace ambiguous "interannual" by "year-to-year" or equivalent

P6 – l14-15 : US oil&gas activity (figure 5) show stalled variations in 2014-15 whereas your analysis find a fast increase from 10 to 20% (figure 4). Isn't that contradictory ? Please comment in the main text.

P6 – l20-22 : how do emission factors for swine and cattle compare ? it would be worth to add the cattle number in comparison with the swine emission factor range given. Is this increase really significant for methane emissions (uncertain range of small emissions of 0.01-0.2 Tg/yr)?

P6 – l28 : interannual → year-to-year or equivalent

P6 – l30 : “ wetland areal extent“ : this is very controversial ads there is no consensus of wetland extent and their evolution (see Poulter et al., 2017 also). Please mention this controversy here.

P6 – l33 : please note in the text that the “trend” you infer for CONUS is mostly after 2012 (“total” line on figure 4). The inversions reported in Bruhwiler 2017 stop in 2012. Please mention these two elements in the main text. Again, waiting more time to get longer time series would avoid limitations in trend analysis. . .

P7 – l1 : Are the stations shown on figure 7 used in the CT inversion ? please precise. Do some other surface stations not shown here show some trend ? If not please mention it at it reinforce your point.

P7 – l8-9 : But this does not discard the possibility that the trend found in your paper is not due to emissions but to other factors as stated in bruhwiler’s paper. Please mention this here as well.

P7-l12 : I recommend to change “ significant increase in US methane emissions“ into “significant increase in total US methane emissions after 2012”

Conclusion : please develop more the main limitations of your study either at the end of result section or in the conclusion.

What about OH changes in your method ? you do not mention your assumptions on OH. Please specify them somewhere in the text.

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