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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Accelerating methane growth rate from 2010 to 2017: leading contributions from the tropics and East Asia" *by* Yi Yin et al.

Anonymous Referee #2

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This is a very interesting study about a recent increase in the global growth rate of methane and the use of inverse modelling to disentangle the underlying causes. 6 different inversion set ups are used that lead to very consistent results, which is encouraging. The setup of those inversions addresses uncertainties in the treatment of OH, although the results seems to show very little sensitivity to it. Because of this, the added value of CO and CH2O measurements that are used remains unclear. Besides the treatment of OH, some other factors require further attention as will be explained below.

GENERAL COMMENTS

I was surprised to see that the posterior scaling factors for OH remain so close to 1. It is mentioned that low variability is in line with some earlier studies. However, what I am more surprised about is that offsets aren't larger, given the much larger uncertainty in global OH. Looking at figure S6, I see quite a substantial difference in the prior simulation using the two OH fields. It suggests a sizeable difference in the methane lifetime between TRANSCOM and INCA-OH. Surprisingly, this difference does not lead to an OH correction in the inversion, for one are both fields. This suggests, that the updated emissions account for the difference. However, looking at Figure 2, I don't really see a systematic difference between the emissions using the two OH fields either. This must be explained.

The validation presented in the supplement concentrates on CH4, which is fine. However, I was surprised not to see anything about CH2O and CO, and how well inversions 1 and 3 fit those data. This makes it very difficult to judge the performance of these inversion components, and how much we can expect them to influence the estimates for CH4.

It is concluded that the largest contribution to the growth rate increase comes from East Asia and the Tropics. I wonder whether this conclusion may be influenced by the fact that these are also very large fluxes, with large uncertainties. Therefore, you expect the largest adjustments to those fluxes. Suppose the inversion wouldn't know where to put an emission correction. Then the cheapest solution is to distribute it evenly across the globe in terms of fractional deviation from the a prior uncertainty. If that were the case, I suspect that East Asia and the Tropics would stand out also. If East Asia and the Tropics are singled out as main causes explaining the increase, shouldn't that be measured in comparison to this "none-informative" reference rather than absolute emission deviations from the prior?

The supplement provides some evaluation of the inversions against surface and total column data. However, I am missing statistical information on the fits, necessary to judge if the a priori and observational uncertainties are chosen in a realistic and statistically consistent manner. This information (e.g. chi2) should be provided.

SPECIFIC COMMENTS

Page 1, line 14: I do not think that the '[xx]' notation is correction for representing mixing ratios. In chemistry, the notation is used for concentrations, which is obviously something very different. In my opinion, there should be no confusion between concentration and mixing ratio.

Page 6, line 123: which "meteorological reanalysis"?

Page 6, line 133: Although I understand the rational for using a climatological prior, I nevertheless think it is a problem when investigating the magnitude of trends. Depending on the weight of the prior, the solution will underestimate the trend. As figure S11 confirms, the trend in the climatological prior is biased. Looking at Figure 2b, I get the impression that the trend in the observations is indeed underestimated by the inversion optimized fit. Surprisingly enough the climatological a prior does not affect the a posteriori estimated trend in OH, which I had expected would have accounted for at least part of the missing trend in the posterior solution.

Page 6, line 139: Which information supports the 20% uncertainty in weekly OH per latitude band? I wonder what happens if you integrated the a priori uncertainty in OH globally and per year. The number would probably become very small. Maybe that explains why global mean OH is almost not adjusted in the inversion?

Page 8, line 202: By 'loss rate' you mean 'sink' or 'life time'? I guess 'sink' although 'loss rate' suggest rather 'life time'.

Page 9, line 206: One way to judge how well the inversion is capable to independently estimating the sources and sinks of methane is to look at the posterior correlation between global OH and the global emission. To be able to judge this, it is necessary to provide information on that correlation.

Page 12, line 245: It would be good to refer to Monteil at al (2013), who were the first

to report the difficult to jointly fit surface measurements and GOSAT column retrievals.

Page 13, line 274: Looking at figure S9, I find it hard to be convinced by the argument raised here. For China, the p-value is quite high – so the significance of the positive trend is only low. For the Amazon it looks better. However, I still doubt that it is a good idea to only take the seasonal maximum. It makes the analysis sensitive to extreme events and outliers. Looking at the seasonal coverage a longer common period of data coverage could have been defined. At least some other points should be tried to confirm the robustness of these trends.

Page 13, line 264: The description of regional emission changes is rather silent about the USA. Numerous papers have discussion the increase in fossil fuel related emissions in the past years, potentially explaining a large fraction of the observed global increase in methane. However, I do not see that back in figure 7, which would be worth mentioning.

Page 14, line 275: The difference between OH and emissions that is mentioned here happens by design, since OH is only allowed to be changed in a zonally uniform manner. There is no reason fundamental reason why the sink couldn't change in similar patterns as the source.

Page 17, line 342: Here a connection is made between d13C measurements, and a model analysis that does not account for d13C. Then, how do you know that your results are consistent with d13C? I wonder about the validity of the qualitative arguing in this paragraph. Looking at figure S12, the lags between emission anomalies and d13C responses as well as their amplitudes are difficult to connect between Figures a and b. In reality it is even much more complex due to atmospheric transport variations. Therefore, the way of arguing that it fits together is too easy in my opinion.

Figure 5: What are the small plusses in this figure?

Figure 6: I'm assuming that this figure shows the diagonal of the averaging kernel.

Please mention this somewhere explicitly.

Figure S11: This figure only shows inventory estimated trends. I was surprised not to see the inversion results in the same figure. Since the inventory trends were not used in the a priori, it would be a great way to independently assess the consistency of the inventories and atmospheric data. The fact, that it the posterior fluxes are not included suggests that the comparing might look very good. In either case, some discussion of it is needed. TECHNICAL CORRECTIONS

Page 2, line 21: ‘O(1D)’ io ‘O(‘D)’

Caption of fig 2: "Deseasoanlized"

Page 15, line 290: “anthropgenic”

Figure 6, caption: ’are shown’ io ’is shown’

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