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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Atmospheric methane evolution the last 40 years" *by* S. B. Dalsøren et al.

Anonymous Referee #1

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Review of the paper by Dalsoren et al. Atmospheric methane evolution the last 40 years

This paper proposes an analysis of the global and regional methane cycle for the past 40 years using long-term simulations of a chemistry-transport model, forced by state-of-the-art emissions and sinks and with analysed meteorology (after 1997). It presents the impact of the different regions and processes on the atmospheric observed changes at a subset of surface atmospheric stations measuring methane and for the different important periods of the methane cycle: the pre-1990s growth, the 2000s stagnation and the post 2006 renewed growth.

The paper is an important and useful piece of information about the methane cycle for the past four decades. One original point is about the balanced analysis of both sources and sinks, whereas most studies on the global methane cycle focus on emis-

sions only. Another (related) interesting point is the analysis of the underlying processes of the OH trend found in the paper. The outline is clear and it is well-written. The figures are supporting the text. I suggest publication in ACP after accounting for the following comments and questions

General comments

1/ The rather crude extrapolation done for the emissions after 2008 limits the analysis of 2007-2012 period. My suggestion is to re-run the last period of the 40 years with less anthropogenic-source-increasing scenario the prescribed one is clearly not adapted to the observations (and now rather well documented). If too long, this solution should be replaced by more acknowledgements in the text that the conclusions about this part should not be taken with caution.

2/The paper is too long to my opinion with too many figures and no real synthesis at the end of each section (e.g. the interesting lifetime sections need synthesis and conclusions). It leads to hide and diffuse a bit too much the important results of the paper to my opinion. In particular, I suggest a substantial reduction of section 3.3. Please provide a section with more synthetiszed text and only few stations that are characteristic of the different regions, to support the conclusions of the text for the main regions. Else, the reader gest a bit lost in the large amount of local to regional results provided. Other stations can go in the supplementary with their detailed analysis. Else it is too dense

3/The "tracer" analysis is interesting but the main text should include the minimum to understand what is done, which is not the case (see specific comments)

4/In many places, the text should be more precise (see specific comments) and avoid redundancies (e.g. MCF & OH changes in several places)

Specific comments

Abstract, last sentence : "In our analysis. . .." Please provide more precise results in these relations.

P30898, l7 : It should be mentioned that Bousquet et al. provides optimized emissions against atmospheric observations. However, using only their natural+BBG do not guarantee that the atmospheric evolution will be matched as anthropogenic emissions are taken from EDGAR. This should be précised at some point.

P30898, l15 : As EDGAR is already suspected to have too large emissions and trends (e.g. Bergamsachi et al 2013), the extrapolation after 2007 is probably enhancing even more the issue. EDGAR have released their data until end 2012 now. Can you compare your extrapolation with their data and eventually acknowledge differences ? Ideally, It would be necessary to redo the end of the period with more realistic anthropogenic emissions accounting for trends more in line with IIASA ECLIPSE or EPA or at least with the latest EDGAR. I do not request it but this issue should be mentioned at this early stage of the paper and discussed later in the text.

P30899, l8 : the collapse of former USSR should be mentioned here.

P30899, l15 : why not applying BP statistics in your standard ? It seems more conservative than the simple extrapolation of EDGAR.

P30900, l25 : how do you "drive" the model ? Nudging ? which variables ? which relaxing time?

P30901, l1 : why not using ERA-I product instead of recycled meteorology ? It would have allowed to study the impact of varying meteorology on you results for the full period ?

Figure 2. Very interesting figure indeed. I ma surprised not to see more the effect of Pinatubo eruption on the loss ? Can you comment ? Also, the period after 2008 is hard to fully analyse because of the crude hypotheses on emissions changes. Again, if possible it would be good to update emissions and re run the last years to draw more robust conclusions. But I leave the option to the decision of the authors.

30903, l27: Can you give at least the relative importance of Chlorine and 01D loss in your study here ?

Figure 4 needs attention. I suggest to add a panel below the evolution of the global mixing ratio representing the atmospheric growth rate (derivative of the model and obs mixing ratios) for observations and model (as done by NOAA on its website (Dlugokencky classical double panel figure). This would reveal more clearly the model goods and weaknesses.

Supplementary, S3. It should be mentioned that " ' " refers to time fluctuations and " * " refers to longitudinal fluctuations. What is the impact of this rather technical treatment of the 18 tracers compared to simply using their relative weight as passive tracers emitted 1 month and stopped ?

P30905, l9. It is unclear and not straightforward how equation 1 comes from the text the supplementary (S3). This paragraph should be clarified for the reader to have enough information in the main text. I suggest to phrase in simple words what equation 1 represents. You want to represent the contribution of all the different tracers at different stations after removing seasonal cycle (<>) and north/south differences ([]). It would help the reader to have things written with words at this stage. - B is not clearly defined. - "if some prerequisites discussed in thesupplementary are met. Âż : please be more precise here, unclear.

P30905, l18 : "recent regional-local emission or transport changes Âż : as you remove the longitudinal mean, would not it be only (or mostly) East/west changes that you can analyse ? Please be more precise here.

P30905, l20 : I agree with the argument of time/space coverage, but the R2 argument is a bit weak. For stations with poor model performances, it is critical to study them and analyse why the model fails. The different tracers can bring information on this. I strongly suggest to add an analysis for such stations (if existing) with some text & hypotheses for the causes of low performances. Else it gives the impression that the authors have (a bit) chosen the stations at “their convenience”. (p30906, l 15-16 is too short on this aspects)

P30905, l20 and P30906, l14-15 : What do you exactly correlate (deseasonlized totals, full signals, ..)? This is a bit confusing. It should be precised in the text.

Fig 6-10 : Using the marine boundary layer latitudinal synthesis from NOAA to get [<observation>], you could probably compute <observation> - [<observation>] as well and compare to the same model term. Did you try this ? It would worth trying.

P30907, l21 : “This indicates that the contribution to CH_4 from regional emissions are small and that long-range transport from other latitudes is decisive Âż. I do not fully agree as Cape Grim is one of the only site where, the B(tracer – mean(tracer)) term explains the growth after 2000. Please provide explanations in the text.

P30908, l19 : Keybiscane analysis. This requires attention. Is the coal increase from EDGAR reliable ? Can you cross this increase with EPA inventory and see whether this is consistent or not ?

P30909, l3 : “i.e. other locations at the same latitudes have a larger trend in CH_4. Âż Pleas be more precise here. As Europe also shows reductions the blame is probably on Asia as shown by following figures.

P30912, l1 : for Minamitorishima, I do not understand why B(tracer – mean(tracer)) term is constantly decreasing. With the pattern of individual emission change (mostly increase). Please provide explanations in the text.

P30913, l28. I think there is now a majority (if not a consensus yet) to agree that OH variations inferred for the 80s/90s from MCF are too large (e.g. Montzka et al 2011). I would be more clearly state this point that wetland variations are most probably overestimated in Bousquet et al., 2011 for this period.

P30914, Pinatubo analysis. OH changes are not mentioned in this analysis whereas it probably explained a lot of the changes. Why so ? Ir it because “changes in meteo-

rology (temperature, water vapor, etc.) and volcanic SO_2 and sulphate aerosols in the stratosphere Âż are not accounted for ? You should at least specified their expected impact on methane through OH changes (reduction).

Figure 12 : Why coal and gas are largely positive in the southern hemisphere for this period ? Please comment on that in the text.

P30918, l7 “Much of this is due to intensification of oil and shale gas extraction in the US and coal exploitation in China Âż. Are gas emissions from gas extraction in the US increase in EDGAR4.2 ? I am not sure this inventory accounts for the shale gas for instance. Please precise.

P30919, l7-9 : “who attributes much of the recent increase in total emissions to wetlands Âż I suggest to add Âń for the period 2007-2009 Âż as Bousquet et al study does not cover the most recent years (since 2010).

P30921-22 : please provide a conclusion to the literature analysis performed about OH changes. There might not be a consensus but it is worth summarizing where we are at the end of the part.

P30922 : “An increase in NO_x emissions increases global OH as long as it takes place outside highly polluted regions Âż : what happens in Asia so ? It is important to estimate the impact of such highly polluted regions on you conclusions about OH impacts in this paper. Please provide at least hypotheses.

P30924 : Are these two equations to represent methane lifetime very dependent of your model ? It would be important to assess somehow the genericity of these equations as it may be useful for other scientists in the community.

P30925, l19 : Âń that our applied emission inventories are reasonable Âż i suggest to rephrase : that our applied emission inventories and computed transport and chemistry are reasonable.

P30925, l27 Âń The model overestimates the growth in all regions, in particular in Asia

Âż . . . after 2006

P30926, l28 : “. . . model results after 2009 due to lack of comprehensive emission inventories Âż. Edgar4.2, although not perfect as noticed in the paper has released data until 2012. There is also IIASAS and EPA having projections for the next years. I would rephrase suggestions that inventory should improve and account for consistent suggestions that Asian emissions are overestimated in EDGAR.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 30895, 2015.