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Interactive comment on "On the role of trend and variability of hydroxyl radical (OH) in the global methane budget" *by* Yuanhong Zhao et al.

Anonymous Referee #1

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The manuscript first discusses the OH variability and trends in details in relation with precursor emissions and chemistry as used in the chemistry-climate models. Then the modelled OH fields, adjusted to the global mean OH in 2000, are used for CH4 modelling in a box model and a sophisticated 3D model. They show good agreement (?) between the two models for global total CH4 budgets. The manuscript is well written and would be alright for publication in ACP. I would like to draw attention of the authors to a few points as detailed below. Hope these are useful.

line 49: Do you need an update here? Is the present day understanding is unambiguoustoo?

line 56ff: The citations look to be very restricted in general in these two para of the introduction. May consider expansion. The TransCom-CH4 project was launched to

underdstand the sources and sinks budget in comparison with the model transport, for example.

line 72ff: I am not aware of any proven issues with weakening MCF gradient. Could you pleas expand what exactly are you talking about; meridional, zonal or vertical gradients?

line 92: are CH4 budgets from 1980 or 1986

line 114: There are 4 issues with OH from CCMI models; you seems to ignore the biases in meridional gradient in OH, and account for the other three (global totals, trends and IAV)

line 120ff: Could this also mean that the variabilities you show are not from OH concentrations but due to t-dependent loss rates & dry airmass. Is it possible to tell the readers what would you expect if you scale OH concentrations themselves not weighted by k? including showing it in a 2nd column?

line 157ff: I fail to understand why continuous inversion were not done for the period 1994-2010, using the two OH cases. This does not seem to be for reducing computing time, given that 2 years are gone for spin-up and spin-down. Please explain. Also why you need two years of spin-up/down for the box model but only 1-year for the 3D model

line 185ff: What do you mean? I see many other negative anomalies are apparently consistent among the models.

line 195ff: I agree that the CH4 growth rates are more positive during the El Nino years (discussed in the TransCom-CH4 analysis too). We have to better understand the lower growth rates in CH4 during the La Nina periods - this is quite new concept. (some people talk about Mt Pinatubo for 1993 growth rate anomaly and others do not see a negative anomaly during the La Nina)

line 200: do you need "processes" here?

line 204: Is there a reason for different unit (Tg/yr) here?

line 212: should this be "different"? Do you mean the NMVOCs are not included in some of the model or the number of species differ from model to models?

line 214: My personal choice, but I would have loved to see the actual values presented in this plot. It is fine to adjust different multi-model values to a common 1980 level.

For here and elsewhere, this is specialised journal publication, there is no need for so much space restriction; I mean this can be 1-column figure is the trends are less prominent by the increase of y-axis range. Also for the x-axis tick labels, please consider reducing number of labels or elongate the x-axis or introduce minor ticks. Presently looks a bit clumsy

line 220ff: This is a nice discussion, but I cannot assess the novelty of it given that ACCMIP and CCMI paper have discussed the OH variabilities and budgets in similar fashion, and there are papers by MPI Mainz group on the details of OH budgets. Could you please consider showing the net (P-L) OH trends in a separate panel. When you say OH loss, is the '-ve' sign in the y-tick labels appropriate and consistent with the number in the text?

line 236ff: I was probably asking to present something similar in my earlier comment for OH anomaly in Fig. 1. May be it is good to show the Net OH (production - loss) variabilities as well in a separate panel here (in % change).

line 266ff: How good are the CO emission estimations and also the satellite data? I have heard some issues with the MOPITT data retrievals. Is this model comply with surface CO observations?

line 274ff: How good are these for accounting for the effect of the meteorology. I suppose the temperature effect is taken in to account by doing the k_oh+ch4 anomaly in Fig. 1, but there are likely to have some non-linear interaction between the transport (inter-hemispheric & stratosphere-troposphere exchange) and loss by OH. This effect

may be of 2nd order but nevertheless important. Any assessment would be helpful to the readers. This is where a long-term 3-D model based inversion would have helped.

line 280ff: It is obvious from this analysis that introducing OH IAV as modelled by the CCMI models will reduce the CH_4 emission anomaly. What are the new questions/implications? 1) increase the wetland emission anomaly, 2) decrease biomass burning emission anomaly, 3) some missing process in the OH chemistry (recycling efficiency)

line 285ff: The most important question for the authors is then to convince the readers how you propose to increase CH_4 emissions by more than 25 Tg/yr in just 3 years and keep maintaining at that level for the later years.

line 301: "assess"

line 303ff: Consider adding header to each panels - again panel size could be increased for clarity. It is very hard to see the semi-hemispheric emissions in the bars. The right panel adds to the confusion how to read this plot; for me it is much easy to see what you want say from the left and middle panels.

line 366: Did Prather and Holmes estimated OH variability or trends?

line 372: I do not know true or not true? The authors, I think, understand the trends and IAV in OH simulated by the CCMs still require much testing. Firstly the global mean OH values, which you have adjusted at the very first; the amplitude and phase of the modelled IAV may not be perfect; the longer term trends are still anyone's guess (needs at least one more line of evidence); finally there is unspoken bias in meridional gradients in the modelled OH. If a true variability and trend in OH become available there would be no issues with the top-down modellers to adopt it. There are several inversions which included the OH trends and variability like you discuss here (e.g., McNorton et al., ACP, 2018)

line 382: Is there a better reference for El Nino in future climate? I am sure there are line 384: I think this is well known since the ACCMIP at the least!

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