

**Role of OH variability in the stalling of the global atmospheric CH<sub>4</sub> growth rate from 1999 to 2006 by J. McNorton et al.**

**Response to Reviewers' Comments**

We thank the reviewers for their time and constructive comments. These comments are repeated below (in normal text) followed by our responses (*in blue italics*).

**Anonymous Referee #2**

The manuscript of McNorton et al. investigates the role of OH in driving the recent evolution of methane, especially the observed decline of its growth rates in the first half of the 2000s. The conclusion is that OH may have been a key driver of this modification of the methane growth rate.

The manuscript is well written and well within the scope of ACP. Even though there have been some key studies investigating the topic of the methane growth stagnation, this is the first paper that thoroughly investigates the role of OH. This is achieved through a series of model experiments with carefully chosen set-ups. I do not have any major concerns, but there are some (mostly minor) suggestions that I list below which I believe will improve the manuscript. Following those, I expect that it will be ready for publication.

*We thank the reviewer for his/her careful reading of our manuscript and insightful comments.*

**GENERAL COMMENTS:**

I would have expected some discussion towards the end of the paper ("Discussion and Conclusions" section) on why the previous studies that investigated this stagnation in methane growth did not come up with a similar conclusion when it comes to the role of OH. This is the new bit that this paper brings, and it needs to be understood why those conclusions were not reached before. Some brief additions to the final section commenting on this aspect would be useful.

*We are not aware that other studies explicitly considered variations in OH when investigating the stagnation period. That is a motivation for this work. Lines 363-365 relate our study to a similar previous study (Fiore et al., 2006) but that study did not cover the stagnation period. We will add a few words to clarify these points.*

Since the simulations start at 1993, why would the spin-up be done for 1977 conditions? That must be creating some methane imbalance in 1993, and with methane's relatively long lifetime, this will still be there in 1997, when the period of major interest begins. I may be missing something, but even in that case, it probably means that this aspect shall be clarified better.

*The model is spun up from 1977 to get a reasonable spatial distribution by 1993, which does create a model-observation imbalance in 1993. This is corrected for by scaling the model 1993 global average CH<sub>4</sub> concentration to observed data before reinitialising the model for the 1993-2009 simulation. We will clarify this in the revised version.*

**SPECIFIC COMMENTS:**

Page 1, Lines 18-19: Sentence not very clear. How can something vary “on a timescale of many years”, within two decades?

*We agree the term “many years” could be confusing. In the revised version we will modify this to say that the variability is over multiple years, i.e. 2-5 years.*

Page 1, Line 29: Please add “of” between “and” and “atmospheric”.

*OK.*

Page 2, Line 49: 6ppb/yr: Number inconsistent with the abstract. Please correct the one that is wrong.

*OK, the number should read 4.9 ppb/yr.*

Page 2, Line 57: Please add “potential” between “second” and “explanation”.

*OK.*

Page 2, Line 60: “much more uncertainty” is unclear – please say a bit more.

*OK. This will be modified to say that the bottom-up and top-down estimates differed.*

Page 2, Line 66-67: So, the decrease in wetland emissions mentioned earlier was abandoned as a hypothesis. This paragraph needs to be connected in a clearer way with the previous one.

*We agree with the reviewer that this could be confusing. The paragraph will be changed to show consistency between paragraphs L51-61 and L62-68.*

Page 2, Line 71: Suggest adding “global mean” before “concentration”, as this symbol (“[OH]”) is used throughout the manuscript when referring to the global abundance.

*OK.*

Page 3, Lines 77-78: A recent paper by Voulgarakis et al. also included findings along these lines when it comes to the role of fires on OH variability, especially during El Nino events (see their Fig. 4c): Voulgarakis, A., M.E. Marlier, G. Faluvegi, D.T. Shindell, K. Tsigaridis, and S. Mangeon, 2015: Interannual variability of tropospheric trace gases and aerosols: The role of biomass burning emissions. *J. Geophys. Res. Atmos.*, 120, no. 14, 7157-7173, doi:10.1002/2014JD022926.

*OK, this reference will be included.*

Page 3, Lines 103-110: Need to also remind the reader of the main finding of the Montzka et al. (2011) paper, i.e. the suggested small interannual variability of OH.

*OK, this will be included.*

Page 3, Line 107: Suggest changing “this” to “that”.

OK.

Page 4, Line 117: Suggest adding “global” between “yearly” and “anomalies”.

OK.

Page 4, Line 127: Suggest changing “date” to “year”.

OK.

Page 4, Line 152: What is meant by “scaled”? Please clarify.

*More detail will be included; scaled emissions are taken from Ciais et al. (2014) top-down estimates.*

Page 4, Lines 167-168: Why were zonal means of temperature used and not 3D data? That introduces one potential extra reason for differences between the runs, i.e. not just the fact that the temperature is fixed, but also that it is not 3D-varying. What is the impact of this?

*We needed to create a time-averaged dataset (1993-2009) for the model run and so much of the 3-D variability would be averaged out anyway. We agree with the reviewer that using zonal mean fields might still influence the results; however, this difference is likely to be small (the major temperature variations, due to both height and latitude are considered). In any case we will add this note to the revised version.*

Page 4, Lines 168-169: Suggest rephrasing to “We also derive our own OH anomalies from the anomaly in the...”.

OK.

Page 5, Line 156: Need to clarify whether the specified OH field is comprised of zonal means or whether it varies with longitude. If the former, need to discuss the implications of the lack of longitudinal variations.

*The field comprises of zonal means. A sentence will be included to discuss the implications of this.*

Page 6, Line 231: It should be 0.65 rather than 0.55.

OK, corrected.

Page 7, Lines 274-275: What is meant by “multi-year” here? Suggest specifying with a parenthesis.

*This will be modified to now include (>1 year).*

Page 7, Line 276: “year-year” -> “year-to-year”.

OK.

Page 8, Line 290-291: Why are the simulations with varying winds singled-out?

*This is a miswording. The text will be changed to say that it considers all simulations not just those with varying winds.*

Page 8, Line 294: Suggest adding “and also given the lack of change in emissions” after “Therefore,”.

*OK.*

Table 3: It is not immediately clear what is meant in the parentheses next to the numbers. I suggest writing “Global mean  $\text{CH}_4$  in ppb” at the top row and “Global mean  $\text{CH}_4$  per year in ppb/yr” at the bottom row of the title of those columns.

*OK, this will be clarified (see also comment from Reviewer 1).*

Page 8, Lines 315-317: I am not sure what is meant by this sentence. May need to be expanded or reworded.

*OK, this will be reworded (see also comment from Reviewer 1).*

Page 8, Line 318: In “ $\text{CH}_4$ ” the “4” needs to be subscripted. Also, I think a “from” is missing before “1999”.

*OK.*

Page 9, Lines 346-349: This is interesting. But why could that be. An explanation, even a speculative one, would be nice. Is it perhaps due to somewhat different emissions regions for the two constituents, leading to different efficiencies of transport to regions of maximum loss?

*OK. We do not want to add unfounded speculation to the paper but we will try to expand on this result slightly in the revised version. A difference in the spatial distribution of emissions would seem to be a potentially important factor.*

Page 9, Lines 357-358: I do not see why this sentence is needed.

*OK. We wanted to make the point that ultimately it is always chemistry that removed  $\text{CH}_4$  and not transport. We will clarify this.*

Page 10, Line 369: Please add “,” before “which”.

*OK*