

Interactive comment on “Role of OH variability in the stalling of the global atmospheric CH₄ growth rate from 1999 to 2006” by J. McNorton et al.

Anonymous Referee #2

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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.

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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.

- 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.

SPECIFIC COMMENTS:

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

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

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

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

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

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.

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.

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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.

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.

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

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

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

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

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?

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

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.

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

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

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parenthesis.

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

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

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

Table 3: It is not immediately clear what is meant in the parentheses next to the numbers. I suggest writing “Global mean ΔCH_4 in ppb” at the top row and “Global mean ΔCH_4 per year in ppb/yr” at the bottom row of the title of those columns.

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

Page 8, Line 318: In “CH₄” the “4” needs to be subscripted. Also, I think a “from” is missing before “1999”.

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?

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

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

Interactive comment on *Atmos. Chem. Phys. Discuss.*, doi:10.5194/acp-2015-1029, 2016.

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