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

Response to Reviewer's Comments

We thank the reviewer for his/her further time and comments. These comments are repeated below (in normal text) followed by our responses *(in blue italics)*.

Reviewer 1

Overall, I think the authors have addressed most of the comments in my first review. However, I have a few additional comments that I think are important to address:

In my comment about the robustness of the subperiod trends, I should have suggested calculating and reporting the standard errors (accounting for autocorrelation) rather than the statistical significance, or p-values, of the trends. The former indicates how well we know the trend, while the latter indicates whether a calculated trend is significantly different from zero, which is not exactly what we are interested in here. So apologies for the mistake, and please make the change.

We agree the inclusion of standard errors is important to show the robustness of the calculated trends. We have updated Table 3 to include the standard errors for each of the subperiods and added text to the figure title. We used unsmoothed data. We tested the unsmoothed data for autocorrelation with a lag of multiple months and found no noticeable correlation and so no autocorrelation correction was applied to the final standard error calculation. We found that the size of the errors does not noticeably influence the conclusions made.

I don't think you adequately addressed the comment from both myself and the other reviewer about anomalies in global OH based on CH3CCl3 measurements possibly not being accurate when applied to CH4. I see that you added a sentence at the end of Section 3, but it barely adds anything to the discussion.

The original comments were:

Reviewer 1: Section 2.1: Estimated anomalies in global OH based on CH3CCl3 measurements may not be accurate when applied to CH4 given the different spatial distributions of CH4 and CH3CCl3 and, to a lesser extent, different temperature dependences of their reaction with OH. The authors state at the end of Section 3 (lines 348-349) that this needs to be considered, but they do not actually consider it in their analysis. They should at the least emphasize this caveat more in the paper and discuss its implications for their findings.

Reviewer 2: 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?

We have expanded the text at the end of Section 3 and also added two figures and text into the Supplementary Material S2. In particular, by now showing the spatial gradients in CH_4 and CH_3CCl_3 (Figure S2) we illustrate how transport variability could have a larger effect on CH_4 than CH_3CCl_3. Moreover, this shows that it is better to derive OH from a 'well-mixed' species like CH_3CCl_3 in its period of decay than from CH_4 which has a lot of spatial variability. In that sense applying the OH variability derived from CH_3CCl_3 decay to CH_4 is better than the opposite. We think that this new information has *clarified the point we made in the original submission which led to the comments in the first review. Please see the new text and Supplement S2 for more information.*

Be sure to proofread your latest additions to the manuscript. For example, your statement in Section 3.1 on "a bidirectional effect" seems incomplete to me, since you discuss the potential impact of [CH4] on [OH] but never mention explicitly in that paragraph your assumption that OH is the primary driver of the correlation. Also, note that concentrations of CO and VOCs sometimes co-vary with [CH4], such as during years with high biomass burning, so the driving of the correlation in the direction of [CH4] to [OH] may be stronger than suggested by your rough estimate.

OK, thank you. We have rewritten this part of the paper ('We assume that this correlation… study') to mention that we are assuming that OH drives CH_4, and that concentrations of OH and VOCs may co-vary with CH_4.

We also discovered that a preference was set inside our Word file to switch off the spell checker. We have now switched it on and discovered around 5 simple spelling errors.