

Responses to comments from Anonymous Referee #2

This paper adds to our understanding of the factors affecting methane concentration in the past. A novel modeling approach for assessing regional emission impacts on observations is provided and in many respects supports conclusions from previous studies. Unfortunately, while the introduction is concise and to the point, the main text rambles on in places and reads like a review in others. As such it can be difficult to follow and understand how the new work contributes. Specific conclusions are provided, yet they are often lost in the extensive discussion of minor points, ancillary information, and reviews of past studies that don't necessarily help the reader follow the train of thought required to draw conclusions. Section 3.5 (and 3.6) is particularly noteworthy in this regard, although the discussion of figures 12-14 also needs attention. I'm not sure what to conclude from three long paragraphs of text in section 3.5. It is only in the Summary and Conclusion section that I learn what the authors really think about constraints on OH provided by other gases (or lack of constraints). I think with some attention to tightening up the text to improve the focus on the most noteworthy issues regarding CH₄ the paper would be ready for publishing in ACP.

➤ *We thank the reviewer for comments and suggestions. We have tried to tighten up the text to provide a clearer presentation of our main conclusions (e.g., Section 3.3 and 3.5 has been revised and shortened, Figures 12-14 changed). See below for answer to detailed comments on these and other issues.*

Figures 12-14. All results are given in percentages. Please be clear what the percentages are calculated relative to. I presume it is the total emission associated with each sector and, if true, makes it very difficult to confirm the points made in the text about which sources are the dominant players in affecting growth rates during these different periods.

➤ *We agree that it can be confusing presenting these figures in relative terms. We now present absolute changes. As the reviewer states, this gives clearer information on the key sectors affecting growth rates and is also more in line with the other figures in the manuscript.*

A similar problem is encountered in Figure 8, where results from Zepplin are discussed relative to conclusions from Fisher et al (2011). Assertions by the authors that the two results are in agreement regarding the seasonal contributions shifting from wetland in summer to gas in winter isn't apparent from the figure (red line, combo of wetlands and biomass burning is always higher than the yellow line (gas)).

➤ *The referee comment is correct: This is not shown in the figure. We now state this in the manuscript. Figure 8 shows running annual means for the tracers but the first draft we*

submitted contained monthly variations. This information was removed from this figure as it otherwise got a bit overloaded with information.

In section 3.5, results from some studies are not well represented in this somewhat rambling text. Weren't the results from Manning et al. (2005) specifically relevant for OH on a semi-hemisphere scale (not global)? And the NOAA study argues for OH variability derived from CH₃CCl₃ before 1998 being artificially enhanced also because of representation issues given the sparse networks (in addition to emission uncertain- ties). It would be interesting to discuss whether the increase in OH derived from the photochemical model here is consistent or not with the CH₃CCl₃ budget (longer-term trends more than year-to-year variations), or are the uncertainties associated with deriving OH from CH₃CCl₃ too large to detect the changes are inferred here? To imagine that an analysis of the CO budget actually allows the conclusion that the OH changes are realistic seems an overstatement (p. 30920). CO is not typically used to constrain OH and OH trends because CO emissions (and their time dependence globally) are not well enough known to allow for tight constraints on OH. It is very good to point out that the CO model results (with trending OH and the given emissions) are internally consistent, but this analysis doesn't add much to the reliability in the model-derived OH trends.

➤ *We agree with the reviewer's interpretation for the two mentioned studies and adjusted the text in accordance with this. A check for consistency between the OH trends and the CH₃CCl₃ budget would be interesting. However, we decided not to do such analysis here as there are considerable uncertainties. This mainly affects interannual variations but could in turn affect the trend analysis. The uncertainties are discussed in the manuscript and we support the statement (referred to in manuscript) from Holmes et al. (2013) which concludes that better understanding of systematic differences between different CH₃CCl₃ observation networks is required before using them as constraints on inter-annual variability of CH₄ lifetime and OH. On the CO and OH trend issues we share the reviewer's views. The wording was unfortunate giving a misleading impression. We have now reformulated the text and moved this to section 3.6 (discussion of CO emissions) where we just point to internal consistency between modelled CO, OH trends and CO emissions.*

Abstract, in the last 5 lines and throughout the text where appropriate, be clear to emphasize that this is the case "in the model".

➤ *This is now done.*

Instead of using the word detach, consider as an alternative deconvolve or even identify.

➤ *Thanks. We now use "identify".*

Colors in figures are very difficult to distinguish—perhaps increase the symbol sizes or line widths. Also, in the text and caption it would help if sources were identified and described consistently throughout and, where possible, included parenthetically the color of the line referring to the source being discussed in the text.

➤ *Changes were made to figure 12-14 where absolute numbers now are shown for growth rates instead of relative. The color scaling was also changed. Section 3.3 is revised and shortened and it should now be easier to relate the text and connected figures (Fig. 5-10). To further ease interpretation, Table S1 in the Supplement lists CH₄ emission sectors and tracers used in the model simulations, and shows the legend colours in Fig. 1 and Fig. 6-10.*