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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric methane evolution the last 40 years" *by* S. B. Dalsøren et al.

Anonymous Referee #2

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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

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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.

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.

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)).

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 uncertainties). 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.

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

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

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 30895, 2015.