GENERAL COMMENTS AND MAJOR SPECIFIC COMMENTS

This manuscript by J. McNorton et al. describes a set of chemical transport model simulations of atmospheric CH_4 during the 1990s through 2000s that use specified OH fields and year-to-year OH anomalies derived from CH_3CCl_3 measurements by previous studies and by the authors. The authors conclude that OH variations could explain a significant portion of the observed changes in CH_4 growth rate, including a drop to near zero during 1999-2006, with smaller contributions to the trends from variations in atmospheric transport and temperature.

Overall, I think this manuscript meets basic requirements for a publishable paper and has some good qualities, though it is somewhat thin on content. In its current form, it is perhaps more suited as a "letter" rather than a full-length article. Some of the work reported in the paper is mostly a confirmation that the authors can reproduce the results reported previously by others, particularly the yearly global OH anomalies derived by the authors from CH_3CCl_3 using a box model. And in my judgement, the paper makes a relatively small contribution to the body of scientific work, given that much of the work is not original or especially innovative. For example, the investigators used an OH distribution and yearly anomalies calculated by others. Also, the effects of transport and temperature on global CH_4 loss have already been studied by others (e.g. the Warwick et al. (2002) and Fiore et al. (2006) papers cited in this paper), though perhaps not for the CH_4 “stagnation period” that the current paper focusses on. Despite the shortcomings, I think the paper could become more suited for publication in *ACP* if the authors address my comments, in the process increasing the content of the paper. I do think the authors have done a good job of performing sets of CH_3CCl_3 and CH_4 simulations that test various potential influences on CH_4 trends, displaying the results thoroughly in figures and tables, and being candid about caveats and limitations of the study.

One major specific comment is that I'm not convinced that the year-to-year variations in OH can be estimated with a high level of certainty from CH_3CCl_3 measurements, given various uncertainties in the modeling, including assumed emissions (especially when emissions were still significant prior to around 2000). The authors themselves acknowledge some discrepancies between their estimated OH anomalies and those of published studies (page 6, lines 208-216). Thus, I see the findings on the contribution of OH variability to CH_4 trends as somewhat speculative. The higher correlations of the varying-OH runs with the observed CH_4 growth compared to the repeating-OH run in Fig. 5 could be a coincidence. A related comment is that the sub-periods delineated in Table 3 for trend calculations are rather short, so that the trends may not be robust. I think providing significance levels (p-values) for the trends would be helpful.

The authors make some statements in different parts of the paper that are not supported by sufficient evidence. Below, I note places where additional information or sensitivity tests could strengthen the statements.

OTHER SPECIFIC COMMENTS

This study relies entirely on the interannual OH variations inferred from CH_3CCl_3 observations and does not consider the OH variations suggested by other methods, including bottom-up, photochemical model calculations and top-down estimates using alternative halocarbons. The authors justify their use of specified OH with a comment near the beginning of Section 3.2.1 that "models with interactive tropospheric chemistry can produce a large range in absolute global mean [OH]", but they do not discuss the interannual variations in OH produced by such models. Montzka et al. [2011] show the OH variations derived from a photochemical model calculation as well as from various halocarbons including CH_3CCl_3 and note some of the differences. I think the current paper could be strengthened by considering other methods and possibly doing some sensitivity tests to assess how robust the conclusions are in the face of differing estimates of OH variations.

Section 2.1: Estimated anomalies in global OH based on CH_3CCl_3 measurements may not be accurate when applied to CH_4 given the different spatial distributions of CH_4 and CH_3CCl_3 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.

Line 172: The runs that allow temperature to vary interannually would seem to doubly apply the temperature effect, given that the OH anomalies already implicitly contain temperature variations. Could you justify this?

Lines 180-182: You could discuss to what extent could the causality actually be bidirectional, i.e. high CH_4 growth can sometimes result in low OH, so that OH isn't always the sole driver of the OH-CH_4 correlations.

Lines 368-372: I suggest making this statement more quantitative, i.e. how large are the underestimate of OH and the overestimate of CH_4 growth?

Lines 376-377: Your analysis hasn't ruled out the possibility of changes in emissions being important during the 1999-2006 time period as well. Furthermore, the picture is more complex than all CH_4 sources varying in the same direction; decreases in certain sources could compensate for increases in other sources.

Lines 389-392: Is this issue relevant to your analysis? If so, could you suggest what impact it might have on your results? And if it isn't relevant, you could omit the sentence.

Lines 392-394: Could you estimate how large of an effect this uncertainty might have on your results?

Lines 394-397: This statement is certainly true and important, although it is not new and insightful. I suggest improving the statement so that the paper ends on a stronger note.

Figure 5b-c: It's not clear to me from these plots that the runs with varying OH are in better agreement with observations than the run with repeating OH is. Perhaps you could also report the mean values of model minus observations over the different sub-periods.

MINOR COMMENTS

Lines 49-50: The post-2006 growth rate of ~6 ppb/yr cited here seems inconsistent with the 4.9 ppb/yr given in the abstract. Please reconcile.

Line 68: You should provide references for the statement that "the reasons for the renewed growth are also not fully understood."

Lines 74-75: You could include additional references such as Wang et al. (2004) and Karlsdottir and Isaksen (2000). The full references are:

Wang, J. S., J. A. Logan, M. B. McElroy, B. N. Duncan, I. A. Megretskaia, and R. M. Yantosca (2004), A 3-D model analysis of the slowdown and interannual variability in the methane growth rate from 1988 to 1997, Global Biogeochem. Cycles, 18, GB3011, doi:10.1029/2003GB002180.

Karlsdottir, S., and I. S. A. Isaksen (2000), Changing methane lifetime: Possible cause for reduced growth, Geophys. Res. Lett., 27, 93– 96.

Lines 75-77: In addition, you could explain that Wang et al. attributed the OH trend to a decrease in column O_3 amounts, and the modeled trend of Karlsdottir and Isaksen was due to changes in tropospheric pollutants.

Lines 79-81: I have not read those papers (Carn et al., 2015; Mills et al., 2015), but my understanding is that in the troposphere, volcanoes are a much less important aerosol source than human activities, and volcanic aerosols that reach the stratosphere actually promote ozone depletion and thus increased downward UV and OH. So I think the counter-intuitive conclusion of those papers needs some explanation here.

Lines 81-83: The citing of this Patra et al. paper is not really relevant to the discussion in the paragraph on OH trends, so it does not belong here.

Line 157: This sentence is somewhat confusing, as I initially thought it meant that 1977 emissions were used for the entire 15-year spin-up. I suggest that you specify that emissions from 1977 to 1992 were used for the spin-up.

Lines 315-317: I find this sentence unclear. Are you referring to the observed growth rates? Please clarify.

Line 351: You include “transport” in this sentence as playing a key role, but your results suggested a relatively minor role. Perhaps you could reword this sentence.

Table 3: I understand your usage of “/ppb” in the heading of the table but it's not clear; maybe replace "/" with "in" or ",".