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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Future methane, hydroxyl, and their uncertainties: key climate and emission parameters for future predictions" *by* C. D. Holmes et al.

Anonymous Referee #2

Received and published: 21 October 2012

The manuscript is a timely, important contribution. It presents a new parametric approach to projecting changes in atmospheric methane lifetime including several sources of uncertainty, such as changes in anthropogenic emissions, meteorology, and climate-chemistry feedbacks. This approach allows for projections to be made and most importantly an uncertainty estimate, without requiring large ensembles with full climate-chemistry models. Important findings include the identification of the most important variables leading to inter-annual variability in hydroxyl radical and thus methane lifetime.

GENERAL COMMENTS: The text needs clarification in a few places but is generally

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Interactive Discussion

Discussion Paper

well-written with clear figures and tables. The general comments from referee #1 will improve the flow of the paper. I recommend publication once those and the points below are addressed.

It is not clear the extent to which the authors tested the role of feedbacks from natural sources, notably isoprene and wetlands. Section 3.2 states that the simulations neglect biogenic VOC variability but Section 5 asserts that they were found to have a minor impact on present-day inter-annual variability. This should be clarified in Section 2. How would inclusion of dust or wetland methane emissions change the conclusions?

This distinction between inter-annual variability versus long-term trends is made several times in the paper but could be more clearly framed early on. The underlying assumption is that the variables responsible for driving inter-annual variability in these models over the past few decades will remain important over the next century, as well as long-term trends in anthropogenic emissions. It seems possible that other drivers, for example, aerosols, clouds, convection, natural emissions, could contribute to trends over the next century. Is the uncertainty estimated here sufficiently large as to cover those possibilities?

The conclusions, and possibly the abstract, should place the GWP in the context of previous estimates since this is a significant change. It should be noted that this new GWP estimate primarily reflects a new estimate for the stratospheric ozone response to methane based upon results from one model. This contrasts with the other findings that are based on three models and in several cases estimates from the literature. How well does the Oslo CTM3 model represent stratospheric ozone chemistry?

SPECIFIC COMMENTS:

Section 2. Consider a Table to compare the CTMs more easily.

Section 2.4 The assertion that biomass burning is a major cause of OH variability should be supported by reference(s) or clarified that this is a finding from this work. Do

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the GEOS-Chem lightning scale factors vary each year?

Section 3.1. P 20939 Line 24-25 Labrador et al. (2005) http://www.atmos-chem-phys.net/5/1815/2005/acp-5-1815-2005.html could be included in the references listed.

P20940 Line 19-20 is awkward since the models seem to agree fairly well in the inter-annual variability of the sensitivity.

P20940 Line 24-25 suggests future climate will be more El-Nino like but this is not a robust finding, see Collins et al. Nature Geoscience 3, 391 - 397 (2010).

P20941 f=1.34 but 1.33 on P20946. Do the adopted values include the new results plus previous findings or changes with the parameter?

Section 3.2 p 20942 needs to define the sensitivity parameter. Lines 15-20 are confusing.

Section 3.3. This section is very important as it is the only evaluation in the paper and some important complications in interpreting the measurements are found which seem worthy of inclusion in the abstract and/or conclusions, though this needs to be properly placed in context of the strengths and value of the measurements as pointed out in M. Krol's comment. The correlation between the two networks does not look very strong and thus it could be of interest to show the other collocated sites in the supplemental information.

Section 3.4 Was this simulation with 5% more methane used to determine the sensitivity to CH4 abundance in Table 2? Somewhere it should be explained how that parameter was determined.

Section 4. The choice not to include uncertainties to CO and VOC emissions seems arbitrary since the other reference listed includes uncertainties. Some justification is needed.

Section 5. Does the 35% decrease in biomass burning apply specifically to NOx, or

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is it uniformly occurring for all fires such that CO/NO remains constant? Are the 3D water vapor and temperature fields not available from CMIP5? Some typos in referring to Figures 6 and 7. It seems important that the uncertainty range for the projections here do not overlap zero, a robust change of sign, whereas the range from ACCMIP does. Should we place more confidence in the results of this study than the multi-model ensemble? Another new study in ACPD with only one climate-chemistry model (John et al. : http://www.atmos-chem-phys-discuss.net/12/18067/2012/acpd-12-18067-2012.html) reports changes in methane lifetimes under the RCP8.5 scenario and is consistent with the findings here.

Throughout the text, and in Table 2, it should be clarified as to whether the temperature and water vapor are only averaged below 400 hPa and 40N-40S. Are the stratospheric columns only 40N-40S in Table 2?

In Figure 1, the parametric model developed from the Oslo CTM3, which represents more climate-chemistry interactions than the other CTMs, captures the least amount of variance. Does this imply that these interactions, not included in the other models, may be important in driving lifetime variability?

Figure 6. Are these single-year differences? If so, probably better to average at least 5-year periods at the beginning and end.

Table 2. The sensitivities for convective mass flux and water clouds are similar to biomass burning. Why are they deemed less important? While the variables are not exactly comparable, some of these sensitivities could be compared to those reported in Spivakovsky et al., 2000, their Table 6.

Table 4. Why the difference between the CH4 abundance change reported in Table 4 and Figure 7 in 2100?

Figure S6 Consider using the same y-axis scale to aid the reader in determining which variables are most important.

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