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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 #1

Received and published: 19 October 2012

The manuscript by Holmes et al. examines future OH, methane and their uncertainties using a parametric model constructed based on simulations with multiple detailed global chemistry-transport models. The work presented will be of great interest to the atmospheric chemistry and climate community, as it approaches several current topics such as the interannual variability, recent trends and future evolution of tropospheric composition, as well as the global warming potential of important radiative forcing agents. The study is also expected to have implications for more applied research in the future. The manuscript is well written and the topic is certainly suitable for Atmospheric Chemistry and Physics. Therefore, I recommend publication following some minor revisions described below.

GENERAL COMMENTS:

1) A general comment that I have has to do with the structure of the paper. It feels like sections 3.3 and 3.4, though useful, are interrupting the flow of the paper as it evolves from presenting the parametric model (Sect. 3.2) to employing it for the historical and future analysis (Sect 4, 5). I would suggest having the MCF section earlier (possibly shortly after the beginning of Sect. 3), and the GWP section later (perhaps just before the Conclusions).

2) The work presented includes a variety of approximations and assumptions, which is inevitable, as it is an ambitious effort to bring this range of models and methods together in one study. These approximations and assumptions are even more evident when looking at table footnotes and at the supplementary material in detail. However, the fact that they are mentioned at scattered parts of the text does not help the reader to have a good idea on which assumptions are the most important ones. I would recommend including a "caveats paragraph" in the Conclusions, which would help the reader understand what could be improved in future efforts building on the approaches presented here.

SPECIFIC COMMENTS:

Page 20933, Line 7: Suggested rephrasing: "...the largest atmospheric methane sink, due to anthropogenic emissions of CO, nitrogen..." to "...the largest atmospheric methane sink: anthropogenic emissions of CO, nitrogen..."

Page 20933, Lines 4-12: In the first part the authors refer to factors affecting OH (CO, NOx, VOCs, CH4 feedback), while in the second they refer to factors that affect methane. Please correct to make consistent.

Page 20933, Lines 16-20: And surface albedo.

Page 20934, Lines 6: Please change "previous approaches" to "previous parametric approaches" or "previous approaches of this kind".

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Page 20935, Lines 24-26: What about overhead ozone column and aerosols? Do they affect photolysis in the model?

Page 20936, Lines 5: Oslo CTM3 is sometimes referred as CTM3 and sometimes with its full name. It would be better to use a consistent name throughout the text.

Page 20936, Lines 4-14: Again, please briefly mention whether time-varying overhead ozone column and clouds are used in photolysis calculations.

Page 20936, Lines 13-14: References should be chronological.

Page 20938, Line 19: More up-to-date present-day values from ACCMIP: 9.7±1.5 yr (Naik et al., in prep.); 9.8±1.6 yr (Voulgarakis et al., 2012, ACPD).

Page 20938, Lines 20-25: Any ideas why GEOS-Chem shows larger variability?

Page 20939, Line 6: Is there any rationale behind the exact choice of years.

Page 20939, Line 10: Table 2: Please note next to footnote (a) that this labeling as "major cause" is based also on the actual interannual changes, not only on the sensitivities.

Also, please note that Krol and VanWeele (1997) found a 2.8% decrease in methane lifetime due to a 10% decrease in the ozone column, while Voulgarakis et al. (2009) found a 5.1% decrease in methane lifetime for a 20% decrease in the ozone column. Note though that in the former the ozone column perturbation was applied only to the extratropics, while in the latter it was applied globally.

Page 20939, Line 17-18: Why aren't the emissions variables also averaged over 40S-40N (Fig. 2)? Please explain.

Page 20945, Line 4: Why is the perturbation 5%? Is it totally random?

Page 20945, Line 20: Why does Table 5 come before Table 3 and 4?

Page 20946, Lines 5-6: Suggested rephrasing: "...tropospheric ozone RF is 30–50%

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of the direct methane RF . . .” to “. . .methane RF through its impacts on tropospheric ozone is 30-50% of the direct methane RF. . .”

Page 20947, Line 15: Please change the title of Table 3 to “Datasets used for calculation of historical. . .”

Page 20948, Lines 8-9: State more clearly why this is true (effects on UV and photolysis), as it may not be intuitive for all readers.

Page 20950, Line 2: The region over which the averaging is performed for meteorological variables (40S-40N and up to 400hPa) is indeed very relevant when it comes to methane oxidation, but arguably quantities averaged over such a region are not typically reported by GCMs as global metrics. It would be a good idea, at least in future work (but possibly also here, if it does not require a large amount of effort), to test whether the results from the parametric model using global tropospheric mean meteorological metrics (which are more commonly reported) are similar to the ones using the regional values. I would expect that the difference would not be large, at least for scenarios like RCP8.5 which feature large global temperature and humidity changes. The ozone case would be less straightforward, obviously, but still worth examining.

Another comment that I have here is that although the perturbations to temperature/humidity (Supplement, Table 1) for calculating sensitivities were global, the values used in the parametric models are regional (40S-40N). Isn't there an inconsistency introduced? Please comment.

Page 20951, Line 9: It is interesting that the parametric model and MAGICC reveal a similar evolution, even though the parametric model includes more factors. Could you comment on this?

Page 20951, Line 11: More up-to-date value from ACPD manuscript: +8.5$\pm$10.4 yr.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 20931, 2012.