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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climate versus emission drivers of methane lifetime from 1860–2100" *by* J. G. John et al.

Anonymous Referee #2

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This study explores the variation in methane lifetime and the factors affecting it over the past century and possible realizations of the coming century. In particular, it focuses on the role of climate changes and changes in emissions of short-lived species in driving these changes. As the climate impacts of methane are effectively dependent on its lifetime as well as its abundance (a fact which could be brought out more strongly in the opening paragraph of the introduction), this is an important topic worthy of detailed study. This is the first study to tackle this issue in a consistent way, and is therefore very useful. Overall the paper is well written and clearly presented, and is appropriate for publication with only minor revisions.

General comments

The introduction states that the paper examines "the role of changes in emissions

versus climate on atmospheric methane lifetime" (p.18069,l.9) While this abbreviated statement is suitable for the title and abstract, a clearer, more precise statement is required in the text. Does "emissions" refer to methane, short-lived species, or both? Does "climate" refer to influences on emissions, atmospheric photochemistry, or both? This only becomes apparent after careful reading of section 2. Given the chemical feedback of methane on its own lifetime and the climate feedback on natural emissions, it would help the reader to be clear at the outset about which factors are being compared.

The analysis in section 4 is very interesting, but is somewhat speculative in places, particularly in sect 4.3. The attribution aspects here need to be more firmly grounded. Given the sensitivity studies performed, it should be possible to provide a more quantitative attribution of the effects of the different emission and climate changes over this period. I appreciate that many of the relationships are not linear, and therefore that this would only be approximate, but an important goal for the paper should be to resolve some of the uncertainty in the trends and attribution that are apparent from previous studies so neatly summarized in Table 2.

There needs to be a discussion in the conclusions (or perhaps the end of Section 4) on the influence of the factors neglected in this study. What are the expected effects of interactive vegetation, soil and fire emissions, or aerosol interactions with photolysis? While detailed analysis may not be possible here, any evidence on the likely magnitude of these effects and their influence on the results would be useful.

Specific comments

p.18069,l.21: Add uncertainty terms on these emission estimates to quantify "fairly well known".

p.18070,l.9: Better estimates of methane lifetime are now available from Prather et al., GRL 2012.

p.18070,l.18: Note here that methane integrates the OH abundance over a wide range of different environments due to its long lifetime. Localized OH changes may differ greatly in magnitude and sign in different places.

p.18072,l.14: Some comment is required on the impacts of the "restoration" (relaxation?) process applied to CH4. The CH4 emissions aren't modeled, and this should be explicitly stated; the effective emissions are therefore driven by the model vertical transport processes as well as just the applied boundary conditions. I assume that the boundary conditions are taken from the earlier CMIP5 runs (or observations?) but it would be good to refer to figure 5 here.

p.18072,l.19: "likely alter the findings". It would be useful to know by how much.

p.18073,l.9: The ensemble naming is nearly self-explanatory, but not quite; while the full specifications are provided in Table 3, it takes some effort to decipher the differences between ensembles. Please state in the text (briefly) what the individual simulations are designed to target.

p.18075,l.1: Please state how this dynamical tracer tropopause definition is applied; are the calculations done online, or is this based on monthly or annual output? Given that the thermal tropopause is likely to change under climate change simulations and that the tracer tropopause is dependent on tropospheric chemistry, how sensitive are the calculated lifetime changes to this tropopause definition?

p.18075,l.4: How is the standard deviation here derived? Is this over the annual lifetimes along the full length of each ensemble member? In the historical and future simulations where there are underlying trends, have these been removed first? How should the reader interpret this variability?

p.18076,l.14: The word "solely" is inappropriate here given that there are a range of competing factors influencing the lifetime.

p.18077,l.25: The change in the reaction rate constant increases by a factor of 6, not

the rate constant itself!

p.18079,l.19: Several of the models shown in Table 2 show this same decrease without any change in meteorology, and therefore greater justification is needed for the attribution of the effect here to changing water vapor. In fact, earlier studies have already noted decreased methane lifetime over the past decade contrasting with the increasing lifetime over centennial timescales (e.g., Gupta et al., GRL 1998; Wild and Palmer, GRL 2008). These studies did not account for changes in climate or aerosol, and recent lifetime decreases could therefore be attributed to the geographical redistribution of emissions of short-lived species from mid-latitude regions towards the tropics. Can the contribution from this effect be estimated from the current simulations? The focus here on global-scale correlations is likely to miss the influence of this type of localization.

p.18080,l.17: Can you provide any more specific insight into how this discrepancy might arise, given that all the model studies in Table 2 (despite different formulations) agree on a positive trend?

p.18082,l.18: increasing temperature, and also humidity.

p.18082,l.27: Previous studies have noted that the runaway effect seen in a box model does not occur in 3-D models due to the interactions of chemistry with transport processes. What is interesting here is that the runaway effect is even less likely to be seen in a GCM due to the negative feedback of methane on its lifetime through climate.

p.18083,l.12: "due to anthropogenic emission increases in CH4", is this really what is meant here? The 5% increase is due to increased CH4 emissions offsetting increased OH from increased NOx emissions and climate changes.

p.18084,l.11: This decline is not discussed in the text of section 5. Does this decline reflect greater acceleration in warming than in CH4?

Table 2 provides a valuable summary of previous studies, but it would be more useful

if the Method column indicated which of the model studies used interannually varying meteorology and which didn't. Those which didn't (e.g., Karlsdottir et al., Dalsoren and Isaksen) may be expected to show different trends and/or attribute them to different causes.

Figs 3 and 4: Separation of these figures seems artificial, as they show related variables from the same runs. I recommend that they are combined so that key relationships (such as correlations between temperature, humidity, and lightning) can be seen more easily.

Consider moving Fig 5 earlier, as it provides a useful summary of the emission and concentration pathways and therefore helps the reader interpret the ensemble scenarios described in Section 2.

Minor comments

p.18071,l.6: "increase in OH" or "positive OH trend"?

p.18073,l.2: personal communication citation not required here, as L. Horowitz is a co-author. (also p.18077,l.19)

p.18084, l.19: The full ACCMIP project name is repeated here unnecessarily.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 18067, 2012.