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# *Interactive comment on* "Effects of Strongly Enhanced Atmospheric Methane Concentrations in a Fully Coupled Chemistry-Climate Model" *by* Laura Stecher et al.

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Stecher et al. use a mixed-layer ocean chemistry-climate model to study the climate and atmospheric composition (ozone, water vapour) response to large idealised 2x and 5x methane forcings. The rapid adjustment response using fixed sea surface temperature (SST) simulations was reported elsewhere (Winterstein et al. 2019). The study here therefore focuses on the characteristics of the slow - surface warming mediated - climate feedback response. In particular, the authors discuss the factors driving changes in tropospheric methane lifetime, the non-linearity in the response with increasing methane forcing, the factors influencing stratospheric water vapour, as well



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as the radiative impacts due to the associated changes in atmospheric composition. Finally, they also contrast radiatively and dynamically driven temperature changes.

This is a nice modelling study presenting several interesting and novel results. I agree with the second reviewer in the sense that sometimes references to the Winterstein paper could be reduced, or at least supported by additional figures and explanations. However, this should be possible to achieve with fairly straightforward adaptations. Below I list several suggestions for minor revisions subject to which I recommend rapid publication.

**Two thoughts on the wider context:**

- This work only considers the effects of increased methane in isolation, which is useful to separate its effect from those of other climate forcing agents. However, given the dependency of methane on, e.g., OH, I would expect that simultaneous CO2 forcing found in the real world could strongly interact with this picture, possibly even in a non-linear fashion. I assume that the reduction in OH driven by methane increases, for example, would be largely offset by increases in tropospheric OH under additional CO2 forcing? I am not asking that the study is revised in this sense, but the potential of such interactions should be mentioned somewhere, unless the authors can make strong arguments against this idea. A simple way to achieve this would be to add another clarifying sentence to the paragraph l. 204-214, where you discuss the importance of water vapour and ozone changes, which will also be driven by CO2 forcing and the associated tropospheric warming, thus impacting OH.

- Did the authors look at changes in the tropospheric circulation at all (cf. Chiodo Polvani 2016, Nowack et al. 2017)? I don't think any study has explored the specifics of the response to methane forcing, with its coupled effects on ozone and stratospheric water vapour before. I am NOT referring to the difference between the fixed SSTs and MLO runs here (Figure 2), as this might indeed be beyond the scope of this work. If the model set-up allows (fairly short simulations and constrained ocean response), a short section on some central aspects of the tropospheric circulation response could further increase the impact of this paper. Otherwise, maybe suggest this point for future work with fully coupled ocean models. I could also imagine that the (lack of) tropospheric circulation changes might affect the stratospheric circulation response, e.g. through wave forcing and propagation, which might be worth commenting on.

- Chiodo & Polvani. Reduced Southern Hemispheric circulation response to quadrupled CO2, Geophysical Research Letters (2016).
- Nowack et al. On the role of ozone feedback in the ENSO amplitude response under global warming, Geophysical Research Letters (2017).

**Minor comments:**

- l. 6-8: it might be the passive use of verbs that makes this paragraph slightly hard to read, or also the reference to the Winterstein study. After all, all you seem to say is that: “Strong increases in CH4 reduce hydroxyl radical concentrations in the troposphere, thereby extending CH4 lifetime. We find that slow climate feedbacks counteract/dampen this effect (through increases in tropospheric water vapour and ozone(?); maybe mention the mechanism).
- l. 11-13: Maybe more explicitly say as well that the middle-upper stratospheric changes cannot be explained by changes in cold point temperature.
- l. 25: would rephrase “influenced”. After all water vapour concentrations are also influenced anthropogenically, only is the effect indirect.
- l. 58-60: I am fairly sure that some of the NASA-GISS simulations by Drew Shindell might have had similar model set-ups but probably looked at other research questions?

- l. 85: Why not attempt a sensitivity analysis of the entire transient data following Gregory et al. GRL (2004) as well? Is the signal too small for the slope to be derived robustly? Gregory et al. A new method for diagnosing radiative forcing and climate sensitivity, Geophysical Research Letters (2004).
- l. 99: I suppose methane is not an emission flux then? Would be good to clarify to avoid misunderstandings.
- l. 139: One way of quantifying the importance of the climatological surface temperature differences would be to compare the global mean surface temperatures. I assume those differences should be smaller but possibly more relevant. Given that the MLO simulations are also free-running, could those effects also just represent some form of internal variability, which, if I understand correctly could still affect the sea ice distribution through atmospheric variability and its effect on SSTs? Higher latitudes can show similarly large variability for fully coupled ocean models. Similar arguments could apply to the NH (cf. l. 143). Looking at Fig. S1, I would think that the overall difference is positive, but the visual effect overemphasizes those changes in SH high latitudes which make up quite a small area. For climate sensitivity aspects, I would actually be more interested in the differences in tropical low-cloud regions which appear to stand out?
- l. 174/175: "would be beyond the scope"?
- l.180: It would indeed be useful to see the overall response, the rapid adjustment response and the difference due to slow feedbacks as subplots next to each other.
- l. 193: the tropopause is defined how?
- l. 228: revise sentence
- l. 258: you mean ‘stratospheric abundance’



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- Figure 6: another case where it would be useful to see the overall response as well instead of just the difference to the rapid adjustment response. Same for Figure 7. 2x2 panels?
- l. 334: the efficacy of ERF methane of close to 1 appears surprising to me – see e.g. the 145Hansen et al. Efficacy of climate forcings, Journal of Geophysical Research (2005).
- l. 368: how is this calculation of the effect on stratospheric temperatures done precisely? Could you provide more detail about the calculations? Are they expected to be robust in different regimes of the atmosphere, e.g. in the lowermost stratosphere vs the tropical upper stratosphere? What is "addst" in equation (2)?

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