

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

## **Anonymous Referee #1**

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The present manuscript by Stecher et al. investigates the climate impacts of 2x and 5x CH<sub>4</sub> by simulations with the coupled chemistry-climate model EMAC. The study is an extension of the paper by Winterstein et al. (2019), who performed similar simulations, but with a focus on instantaneous impacts. To take into account slow climate feedbacks Stecher et al. ran the model with a mixed-layer ocean, while the previous simulations by Winterstein and colleagues used fixed sea surface temperatures and sea ice coverage as lower boundary condition, which limits the tropospheric response to increased methane concentrations. Overall, I think this is a solid model study which is of interest for a wider community. The paper is in principle well written. However, the paper refers very strongly to the previous study by Winterstein et al, and is thus extremely hard to

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follow for readers, who are not familiar with the companion paper. Given the substantial length of the present manuscript not everybody is interested in reading a second paper in parallel. Therefore, I suggest major revisions and a substantial re-structuring of the paper before publication.

I have three major concerns:

1) As mentioned above the study is strongly linked to the work by Winterstein et al. Unfortunately, both studies have been conducted with different model versions. Moreover, the reference simulation for the MLO, REF QFLX, has been performed using a third model version / set-up. I have a hard time understanding why the authors did not simply apply the same model version as in Winterstein et al.? The authors want to make us believe that the model modifications do not have a significant impact on the outcome and try to circumvent this issue by showing differences of differences, which by the way does not increase readability, but how can you be sure that the climate background state has no impact on the modelled response to 2x (5x) CH<sub>4</sub>?

2) As already mentioned above the presentation of the results is strongly linked to the paper by Winterstein et al. Without knowing that paper, I find it often very difficult to follow the argumentation. For example, the paper discusses on increase in SWV due to enhanced atmospheric methane, but Fig. 6 displays negative changes in SWV as it present the difference SWV response in the MLO and fSST runs. This way of presenting the results is not very intuitive as the reader first has to look into the supplement to find the SWV response to enhanced CH<sub>4</sub> in the MLO runs and then has to think about differences between the fSST and the MLO set-up. For the sake of readability and clarity I suggest to re-structure the paper as follows: First present the results of the MLO runs and move several of the figures provided in the supplement to the main paper, and then discuss the differences to Winterstein et al., maybe only for one case (2x or 5x), if the paper turns out to become too long.

3) The argumentation is often very qualitative, but not quantitative. A good example

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is the discussion of SWV changes and their attribution to changes in CPT and CH<sub>4</sub> oxidations. The CPT changes could be transferred into a change in H<sub>2</sub>O entry values, and from the model simulations it should be easy to calculate SWV production from CH<sub>4</sub> oxidation. With that, the importance of both effects could be quantified. This is only one example, but there are several places where some more quantification would be desirable.

Specific comments:

- The title is very general, almost the same meaning as Winterstein et al.
- L6 and introduction: It would be nice to see a short definition/description of instantaneous and slow responses / feedbacks. Maybe it would be helpful to add a schematic to the paper, which shows the considered processes and clearly separates fast and slow effects.
- L90/91: I am bit confused by the description of the applied CH<sub>4</sub> boundary condition. I thought that CH<sub>4</sub> is relaxed towards to observational data set, and that this data set is simply multiplied by 2 (5) for the sensitivity runs. Why the “equilibrium CH<sub>4</sub> fields of the respective fSST simulations”? What is the difference / advantage?
- L91 onwards: What is the advantage / difference between the relaxation approach and simply prescribing the CH<sub>4</sub> concentration at the surface? What relaxation time scale is used? With the long lifetime of CH<sub>4</sub> there should not be a large difference?
- REF QFLX: This simulation should be the same as REF fSST, shouldn't it? Does REF MLO also include the gravity wave set-up as described in Appendix B? If not, do you expect any impact?
- L127/128: What is the reason for the negative bias and observed total column CH<sub>4</sub>? This is a good example where an explanation seems to be given in Winterstein et al., but is unfortunately not summarized in the present study.
- L136/137, Fig. S1: I have also worked with the ECHAM5 MLO, and I am a bit con-

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cerned about the difference pattern shown in Fig. S1, namely the temperature difference around 60S, especially over the eastern hemisphere. In my simulation the MLO was in much better agreement with the reference SST climatology. Any thoughts about this?

- MLO: A more general question to the MLO: The MLO does not consider heat exchange with the deep ocean, but all forcing goes into the MLO. Up to which forcing strength is the usage of an MLO justified?

- L173 onwards: If Bony et al discussed a similar feature for CO<sub>2</sub>, then why not adding a short (speculative) discussion for CH<sub>4</sub>?

- L205-214: It would be nice to see some more quantification of the temperature effect on the CH<sub>4</sub> lifetime!

- L232: Why is the tropospheric CH<sub>4</sub> response marginally larger? Tropospheric is largely controlled by boundary condition? Remaining effect from CH<sub>4</sub> oxidation?

- L246 onwards: Again the argumentation in this section stays mainly qualitative (“weaker increases of OH are presumably connected...”). Although the arguments sound reasonable, it should be possible to keep track of chemical production / loss budgets in a CCM.

- L293: Which one is the limiting OH precursor ? Water vapor or ozone? I would imagine that depends on the atmospheric region.

- L297: Please add a short summary of the explanation for the O<sub>3</sub> response given in Winterstein et al.

- Fig. S9: Would be nice to see the difference in CPT for the reference simulations, fSST and MLO, as well

Technical comments:

- Eq (1): is there a bug in the listed units? E.g., units for reaction rate coefficient? [cm<sup>3</sup>

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mol<sup>-1</sup> s<sup>-1</sup>]?) Otherwise the lifetime is not in [s].

- L190: [kg], to be consistent with the other units

- L324/325: It is not necessary to additionally mention numbers listed in a table here

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