

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-950

Anonymous Referee #1

Referee comment on "How do Cl concentrations matter for simulating CH₄, δ¹³C(CH₄) and estimating CH₄ budget through atmospheric inversions?" by Joël Thanwerdas et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-950-RC1>, 2021

This paper investigates the role of pre-scribed Cl-fields on simulated CH₄ and the ¹³C(CH₄). The current inversion system does not yet include a tropospheric Cl sink, and since the group is currently extending their system to include ¹³C(CH₄) observations, this paper is a logical first step. The necessity of this paper apparently appeared when the authors performed their first inversion (presented in the Supplement). As a result, the paper is a bit unbalanced and thin in content. The title promises "atmospheric inversions", but the main paper only presents forward simulations as a series of sensitivity simulations to map out the impact of various choices for the Cl field. I therefore suggest to remove "through atmospheric inversions from the title".

For the rest, the paper is well written, and I will attach an annotated pdf with minor comments. The box model inversions are an elegant way to estimate the global impact of the Cl sink on emissions and their required signature. However, the comparison to the vertical profiles are only performed for CH₄ mixing ratios. The results indicate that the model does not perform very well, but that this likely a transport issue rather than an issue with the Cl sink. However, it remains totally unclear how well the model performs in the stratosphere concerning ¹³C(CH₄), while the action of Cl is critical here. I therefore suggest to include an analysis of the modeled ¹³C(CH₄) profiles and compare to the available observations
(<https://acp.copernicus.org/articles/11/13287/2011/acp-11-13287-2011.pdf>)

Please also note the supplement to this comment:
<https://acp.copernicus.org/preprints/acp-2021-950/acp-2021-950-RC1-supplement.pdf>