

Interactive comment on “Investigating the yield of H₂O and H₂ from methane oxidation in the stratosphere” by Franziska Frank et al.

Anonymous Referee #3

Received and published: 28 March 2018

This paper purports to test the assumption that the yield of water vapor from methane oxidation is equal to 2. I found the paper to be confusingly written and likely to mislead readers who do not know the field well.

The most important thing: reading this paper might lead the reader to conclude that the assumption that $dCH_4/dH_2O = 2$ is not a good one. In fact, we have many observations (they are referenced in this paper) that show it is an excellent assumption throughout most of the stratosphere. I agree that the assumption breaks down at high altitudes.

The reason the assumption is good in the lower stratosphere, even though the calculated yield there is less than 2, is that the lifetime of CH₄ there is very long (100 years). Almost all of the oxidation of methane in the stratosphere is occurring in the mid-stratosphere, where the yield is 2. This air is transported down into the lower

C1

stratosphere, so the yield in the lower stratosphere just reflects mid-stratosphere photochemistry.

This needs to be clearly laid out in the paper. Otherwise, readers will be misinformed.

Assessing the quality of the assumption that $dH_2O/dCH_4 = 2$ would require a different analysis. All one would have to do is show regressions of H₂O versus CH₄ in various regions of the stratosphere (from either observations or models with full stratospheric chemistry). This comparison would show you if that assumption is good.

In fact, the paper is really about H photochemistry, not the assumption that $dH_2O/dCH_4 = 2$. There's a lot of discussion in the paper that revolves around the details of stratospheric photochemistry. So one possible suggestion that I think would improve the paper would be to remove the present motivation of the paper (testing if $dH_2O/dCH_4 = 2$) and replace it with a more accurate characterization of the work described (investigating H photochemistry and sensitivities).

A few smaller comments: 1) I would eliminate Fig. 1 below 100 hPa. This region is not relevant to the paper.

2) Why do the authors spend so much time looking at OH sensitivity? That section should be motivated better.

3) I don't understand why the direct and effective yields of water vapor in the lower stratosphere are equal. The direct yield is the water vapor produced directly from methane oxidation. However, there's also a contribution from oxidation of H₂ (lifetimes of CH₄ and H₂ are similar in the lower strat.). That would be included in the effective yield. Thus, the effective yield should be larger than the direct yield, right? I'm confused.

4) This emphasizes that I don't particularly understand the way the authors have defined effective and direct yield. It seems to me that direct yield should be production of water directly from methane oxidation and effective yield should be the direct pro-

C2

duction plus the yield of water vapor from H₂ oxidation and minus the loss of H₂O from photochemistry. Is this how they view their definitions? If so, they should perhaps re-phrase that part of the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-170>, 2018.