

Interactive comment on “Model – TCCON comparisons of column-averaged methane with a focus on the stratosphere” by Andreas Ostler et al.

Anonymous Referee #2

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The manuscript by Ostler et al. examines the how errors in stratospheric CH₄ distributions affect XCH₄ (the column-average mole fraction of CH₄). The motivation is that inversion analyses often adjust surface emissions to match observed XCH₄, but those emission estimates would be wrong if the model's XCH₄ error originates in the stratosphere. Ostler et al. find that 3 current models do indeed have sufficiently large errors in stratospheric CH₄ that XCH₄ is altered by 5-40 ppb, with a systematic latitudinal structure which is large enough to impact emission estimates at a meaningful level. Differences among current stratospheric CH₄ observations from satellites imply about 5-10 ppb uncertainty in XCH₄, which will likely require more in situ stratospheric measurements to reduce further.

The methods are sound; the figures and analysis are good; and the paper is generally well written. I have a significant criticism of the analysis behind Fig 7, but this is a

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secondary issue that does not affect the main analysis. I think this paper deserves publication after addressing the issues below.

Figure 7 attempts to derive stratospheric mixing rates (between tropics and mid latitudes) from the CH₄ vertical profiles in the tropics. A similar method has been established by Strahan et al (2011), whom the authors cite, with N₂O profiles instead of CH₄. N₂O has no loss in the lower stratosphere, so the vertical gradients of N₂O in the lower tropical stratosphere is due mainly to mixing with low-N₂O air in the higher latitudes. Ostler et al. attempt the same technique with CH₄, but CH₄ does have a significant chemical sink in the lower stratosphere, so the assumption underpinning the technique is violated. I suspect that is why the mixing rates suggested in Fig 7 are at odds with the mean age and ascent rates as described further below. Because the analysis is flawed, I believe Fig 7 needs to be cut. If the authors have N₂O simulations and observations, they could use those as a better diagnostic of mixing rates.

Figs 6 and 7 are not entirely consistent with Fig 5. Fig 5 shows that all 3 models have very similar vertical profiles of mean age in the tropics. Fig 6 shows that TM5 has faster vertical ascent in the tropics than the other models, so it should also have greater horizontal mixing between the tropics and mid-latitudes in order to achieve the same mean age as the other models. However, Fig 7 suggests that horizontal mixing in TM5 is not any faster than the other models. I suspect that the use of CH₄ instead of N₂O as a mixing diagnostic may contribute to this inconsistency.

The MIPAS measurements are averaged for each month, then used as “truth” to replace the model stratosphere fields for comparison to TCCON on individual days. During a month, the tropopause will move up and down in altitude, especially near mid-latitude and subtropical jet streams, which drives a significant change in XCH₄ since CH₄ mole fractions are generally higher in the troposphere than in the stratosphere. As a result, the stratospheric partial column of CH₄ observed by MIPAS will not be correct for the particular days on which TCCON observations are available. The authors mention this issue very briefly but make no attempt to quantify it. I believe it deserves

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greater scrutiny, or better explanation of why it is minor compared with other issues.

Clarity issues:

Title: The hyphen in the title can be misinterpreted as meaning that everything after it is clarifying “Model”. To avoid any ambiguity I suggest something unambiguous, such as, “Evaluation of column-averaged methane in models and TCCON with a focus on the stratosphere”.

On Page 1 Line 30 (P1L30), it is not clear that the model-TCCON agreement is improved by *substituting* the MIPAS-based stratospheric CH₄ observations *in place of* the model’s stratospheric CH₄ simulation. Similarly on line 34, it’s not clear that the simulated stratospheric CH₄ is again replaced with a different satellite CH₄ product.

P1L33: “respectively” is not needed.

P1L35: “These findings imply. . .” sentence is not clear to me. I think it contains two claims: “These findings imply that model errors in simulating stratospheric CH₄ contribute to model biases” and “Current satellite instruments cannot definitively measure stratospheric CH₄ to sufficient accuracy to eliminate these biases.”

P2L5: The stratospheric chemistry community has devoted a lot of time, research, and papers to understanding these issues. Some of those papers are cited in this work, but a great many are not. Perhaps the specific models used in this work have not been part of those studies, but it seems over broad to say that the these issues haven’t been studied adequately.

P2L33: What is a “residual bias”? Residual after doing what and compared to what?

P3L1: I believe there are too many negatives (cannot, without, unambiguous), e.g. “without” should be “with”.

P3L2: What is a “bias function”?

P10L4: Check sentence grammar.

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