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

Anonymous Referee #2

Received and published: 27 May 2016

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
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
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.