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

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First of all, my sincere apologies for the late review, there is no excuse for this.

The paper by Ostler et al deals with the impact of stratospheric CH4 on model TCCON comparisons. Given the relative importance of stratospheric methane on global flux inversions, the paper warrants publication. I find it generally well written and very suitable for ACP. I have a few comments that might help to improve the paper and make some aspects a bit more general and not too confined to TCCON comparisons only.

In general, I am not sure whether mean bias is really the best metric to use for quantifying "success", esp. as all satellite data might have a small residual bias, which can be scaled to optimize agreement (also holds for TCCON, SCIA, GOSAT and stratospheric

data). I would consider the station to station bias variability (similar to the range used by the authors) as well as the ability to capture seasonality a better metric (seasonality not quantified here). Most inversions will include a general bias correction term anyhow. Station elevation would also be an important aspect as it determines the fractional contribution of the stratosphere to XCH4 (add in Table 2, probably only important for Izana). This also changes with seasons, so a look at whether the method here improves the seasonality of models would be very worthwhile looking into. Figure 2: It would be good to also look at the latitudinal difference in a more general sense, e.g. a global average and spread of the differences as opposed to just at TCCON stations. If the global difference fields have already been computed, it would be very easy to do so but I am not sure whether this was done. This could be a valuable addition to the paper as it will increase the relevance to flux inversions. A separate DJF and JJA plot would also be good to reflect the impact on seasonality as well. Figs 3/4: While it is customary to show stratospheric variables in a log P scale, I would find a linear y-axis in pressure more useful in this case as it enable the reader to better estimate the impact on column values. Right now, the eye might often be focussed on some of the strong variations at lower p (e.g around 10hPa), which might be striking but could be irrelevant for the column integral.

One other question is how the measured fields are replaced in the models. It is stated that it can sometimes even be in the troposphere. Is it a brute force replacement (i.e. will there be discontinuities in the updated model field?). What happens if you define a transition range in p where you "smoothly" replace the model with the updated fields? Would it matter?