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# ***Interactive comment on “Derivation of tropospheric methane from TCCON CH<sub>4</sub> and HF total column observations” by K. M. Saad et al.***

**Anonymous Referee #2**

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The authors present a method aiming at removing the stratospheric component of ground-based TCCON (Total Carbon Column Observing Network) methane measurements at various sites, such that the remaining variability for CH<sub>4</sub> can be attributed to sources, sinks and transport affecting methane in the troposphere. The method employs coincident HF measurements, a reservoir species strongly correlated with CH<sub>4</sub> in the stratosphere. This relationship is characterized using ACE-FTS simultaneous observations of HF and CH<sub>4</sub>, in six latitudinal regions of the stratosphere. Results are compared with those derived following Washenfelder et al. (2003), and with in situ aircraft measurements.

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This paper is well written and clear, and the proposed method should allow producing additional valuable observations of methane in the troposphere. However, my concern is that most comparisons remain essentially qualitative, making difficult any current or future objective evaluation of the performance of the Saad et al method. I would recommend publication of this manuscript in AMT, after careful definition, evaluation and addition of objective metrics or indicators.

More specifically, my comments, suggestions and recommendations are as follows:

Pg 3473, Ln 11 (and Pg 3475, Ln 15): why this restriction to a subset of TCCON sites, because of observing limitations, available manpower? Please specify.

Pg 3475, Ln 8: I would write: “these retrievals are \*also\* sensitive to error in the instrument...”

Pg 3478, Ln 7: is this threshold very strict? In other words, do you lose many measurements when applying it?

Pg 3478, Ln 15: how are the ACE-FTS errors evaluated? We would like to know more about the quality, reliability, error estimations and filtering of the ACE products. How do they compare to HALOE data, used previously? A brief description and/or appropriate references are needed (also true for H<sub>2</sub>O, your sentence Pg 3480, Ln 1-2).

Pg 3478, Ln 21: why is Fig. 2 introduced before Fig. 1? Or do you mean Fig. 2 of Washenfelder et al. (2003)?

Pg 3478, Ln 26: I am wondering if the sparser data from 0-30S and 0-30N could not have been merged, to get more meaningful or robust statistics for the tropical region.

Pg 3479, Ln 2: “NH slopes are more steep than their zonal counterparts”: is this still true when accounting for the statistical uncertainties affecting the various slopes (e.g. at 2-sigma)?

Pg 3479, Ln 8: “For 2013”: but you indicated before (pg 3478) that the ACE-FTS

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dataset was limited to 02/2004-12/2012?

Section 2.2, Pg 3479: I have several concerns here: if I am not wrong, the ACE-FTS occultation measurements go at best down to 6 km altitude. How did you select the ACE-FTS measurements, was the lowest available tangent altitude a criterion for selection? You mention that when ACE information was missing, you used the TCCON priors. Is this the best approach? What about an extrapolation down to the surface level using a mean value from the available ACE-FTS tropospheric profile? A sensitivity study and its brief description would be helpful.

Pg 3480, Ln 25: it is unfortunate that results from the very high-latitude site of Ny Alesund are not included. What is the reason for this?

Pg 3480, discussion of Figure 3: could you characterize and provide a measure of the quality of the intercomparisons, i.e. slopes of the linear fittings, R factors...?

Section 3.1, Pg 3481: does your correction generally results in lower intra-day variability for tropospheric methane, when compared to Washenfelder (see error bars on Fig. 8)? If yes, can you quote this improvement, e.g. by providing typical relative standard deviations for both methods? And would this be verified at other TCCON sites?

Pg 3498, Figure 8 caption: I would identify the blue dots in the caption (e.g. “Daily mean median total (blue) and tropospheric...”

Pg 3501, Figure 11: do you need to make a distinction between off and on shore winds data? This is not discussed in the text. If yes, I would use symbols allowing for a better identification of the two subsets (i.e. two different colors).

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