Atmospheric Measurement Techniques Discussions

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Comment on amt-2022-322

Anonymous Referee #1

Referee comment on "Inferring the vertical distribution of CO and CO_2 from TCCON total column values using the TARDISS algorithm" by Harrison A. Parker et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-322-RC1, 2023

Parker et al. propose a new method to infer “limited” vertical profile information on CO2 and CO from a set of total column measurements. The idea is that vertical information can be inferred from total columns measured in direct-sun configuration by a posteriori combining retrievals from multiple spectral windows with variable absorption optical depth under variable solar zenith angles. The profile information comes from the variable height sensitivities (indicated by variable column averaging kernels) among the set of column measurements. The authors test their approach by applying it to CO2 and CO retrievals from the TCCON and comparing the results to in-situ airborne profile measurements. It appears that height information can be used to separate partial columns in the boundary layer and in the free troposphere - to some extent. I find the topic interesting and it fits the scope of AMT well, but I have major concerns:

I had major issues understanding in detail the concepts proposed by the paper. This refers in particular to how all the equations fit together and what quantities the variables represent. Without being in the inner circle of TCCON discussions, the methodology (section 2.2, equations scattered throughout the paper and appendix, equations and concepts taken from other papers, TCCON jargon) is hard to follow. I recommend that the authors make a serious effort to present the methodology in a more concise, clearer, yet complete way, that is more accessible to the general reader. Some information might be missing (formulae for calculating smoothing and noise errors). Some of the very detailed discussions in the results section could be summarized.

What is the key difference with respect to previous work by Roche et al., 2021 that makes performance better here? The two approaches are in the end quite similar, both generating vertical information by combining windows with different sensitivities. Whether the combination is realized during spectral analysis or a posteriori should not matter in principle. Is the claimed better performance here because the columns for each window are scaled individually to the same WMO standards such that line strength inconsistencies are corrected?

I do not understand equation (18) and the related discussion. Equation (12) calculates the

MAP solution. What is the least-squares solver of equation (18) used for? Does it refer to the usage of the least-squares solution as the prior for the MAP solution? If this is the case, it implies that the inversion works reasonably well in an (unconstrained) least-squares sense. Why would one then want to go through all the MAP machine (which lowers the degrees of freedom for signal)? It generally appears incompatible with the idea of MAP that a least-squares solution is taken as prior.

The validation data mostly come from airborne or tower in-situ measurements which do not cover the entire vertical column. How is the missing part of the column (either at the bottom or at the top) taken into account when comparing to the (lower or upper) partial column from the TCCON measurements? What are related uncertainties? I would think that a careful consideration is important since 1) the CO2 (maybe also CO) vertical profile is most variable in the lowest few hundred meters (i.e. extrapolating from or into the lowest few hundred meters is error prone) and 2) the targeted accuracy for CO2 partial columns is on the ppm level.

From the validation study (e.g. Fig. 4, table 4, Fig. 6, 7), I find it hard to evaluate whether the performance of the proposed algorithm is convincing or not. It would need, for reference, comparisons to the performance of the TCCON standard retrievals i.e. taking the P1 scaling factors and calculating upper and lower partial columns and comparing those to the validation dataset as well. Probably, showing performance of the priors would also be interesting.

More technical comments that jumped into the eye (but are incomplete at this stage):

L30: Is Doppler broadening really the limiting factor in the troposphere – as opposed to pressure broadening or temperature modulating the population of rotational levels?

L86f: The discussion of MOPITT appears misplaced. The vertical information for the satellite instrument MOPITT comes from combining thermal emission and absorption (Schwarzschild equation) while, for direct-sun measurements (Beer-Lambert's law) such as TCCON, vertical information comes much more indirect through line shapes and relative optical depths. If MOPITT is discussed, these conceptual differences should be highlighted. Plus, there is similar work on GOSAT and a range of other TIR satellites (IASI, AIRS, …) e.g. Kulawik et al., 2017, https://doi.org/10.5194/acp-17-5407-2017; Kuze et al., 2022, https://doi.org/10.1016/j.rse.2022.112966.

Equ. 4: x_a,i needs subscript P1, I guess?

Equ. 17: Please explain what "Avert" is and motivate the equation (what is the "star" operator?).

L308: The Xgas notation is undefined, I think.