

Interactive comment on “Thermal infrared laser heterodyne spectro-radiometry for solar occultation atmospheric CO₂ measurements” by Alex Hoffmann et al.

Anonymous Referee #3

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The paper by Hoffman et al. presents a laser heterodyne spectrometer that enables measurements of the column-average mole fraction of carbon dioxide (XCO₂) and, potentially, its vertical profile. The technique is carefully evaluated through retrieval simulations and through spectrometer testing in the lab. An appealing advantage of the proposed technique could be the vertical profiling aspect. The weak point of the study is the imbalance between lengthy discussion of theoretical and in-lab performance compared to a rather short section on the atmospheric deployment.

In fact, while more than a year of atmospheric data seem to be available, only one day of XCO₂ is discussed. Why is that? Is there unforeseen real-world problems? I would assume that the real-world vertical profiling capability, for example, suffers from

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real-world spectroscopic line parameter and line-shape uncertainties. I would urge the authors to discuss such real-world issues in more depths.

Nevertheless, the paper certainly deserves publication in AMT, since a relatively new atmospheric measurement technique is presented, its technical aspects are well documented and, the paper does include a first attempt on atmospheric deployment. I recommend taking into account the points below:

P8,L30: Make clear that 8 DFS refers to the entire state vector, not the CO₂ profile part. Mention the number of DFS for the CO₂ vertical profile. It would also be essential to describe where the height information comes from i.e. pressure/temperature dependence of the absorption lines. It might be worthwhile mentioning that spectroscopic parameter or lineshape errors would be highly detrimental.

Table 1, Figure 1, P8 first paragraph: The reason that the H₂O retrievals are off the truth is that the a priori state vector is not equal to the truth and that the averaging kernel is not the identity matrix, right? So, actually, this is just a spurious smoothing effect driven by the (accidental) choice of prior and true H₂O profile. I would think that this is of minor relevance for performance evaluation of a new instrument concept and it might distract the reader from the relevant parts.

Section 3.2: I am not convinced that a passive solar tracker is the preferred system for an application that uses an extremely narrow field of view (1/8 of the sun diameter) thus heavily relying on exact tracking of the solar disc center. The upper limits discussed would make a large contribution to or even exceed the tolerable error budget for XCO₂. If cloud occurrence and subsequent loss of the solar tracking was the only concern, one could think about an active system that goes into passive mode once the intensity on the detector decreases.

P10, L30: How would the proposed system enable a significantly higher number of observations? Integration times of 90 s are not particularly fast. The FTS (Bruker HR125, EM27/SUN) typically used for ground-based XCO₂ measurements can be at

least as fast (and probably still provide better SNR than the proposed LHR).

Section 5.2: If I understand section 5.2 correctly, only the XCO₂ error bars as estimated by the retrieval are discussed. Given that no real validation against independent data is possible (1 GOSAT overpass is essentially insignificant), the authors could discuss how the estimated precision (1.9 ppm) compares to the observed data scatter e.g. estimated through the standard deviation of all soundings with respect to the moving average. One might assume with some justification that, on the timescale of 15 min, XCO₂ is constant.

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