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Interactive comment on "Thermal infrared laser heterodyne spectro-radiometry for solar occultation atmospheric CO_2 measurements" *by* Alex Hoffmann et al.

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In supplementation of the points raised by D. Feist in his previous comment, I would like to add the following remarks: (1) The comment wrt to airmass reference and pointing accuracy seems important to me. It contradicts common experience that a passive tracker works so precisely as claimed by the authors. The claim should be verified by presenting actual measurements of the solar position on a target during the day. Moreover, the authors should explain which steps needs to be taken (hardware and software) for reproducing this tracking quality if the instrument is duplicated or operated in a campaign mode. (2) A retrieval with 8 degrees of freedom (NDOF) would be an outstanding achievement. As investigated by the FTIR community so far, a pro-

file retrieval for CO2 is demanding (due to low variability of profile shape), even when attempting NDOF of about 2. I understand that the heterodyne approach might outperform the FTIR, but the obstacles met when it comes to profile retrieval are partly of extrinsic origin from the instrumental point of view. Is the spectroscopic description in such an excellent shape that we do not need to worry about spurious profile variability in the retrieval result as function of temperature, water vapor content, airmass, etc? It would be desirable to support the claim by demonstrating e.g. the detection of expected plausible profile variations during a measurement day based on retrievals of real data (increase of boundary layer thickness). (3) I think it is justified to start from an error estimation based on model simulations when pioneering a new observational approach. However, such an estimate should not be overly optimistic. The temperature error should definitely been taken into account, as it cannot be assumed that the temperature profile is known exactly - actually, the temperature uncertainty often is large, especially in the boundary layer variations well beyond 10 K can occur during an observation day (which implies sunny conditions and therefore high insolation). This temperature error might be a critical limitation for the proposed observational approach: it should be recalled that the O2 rationing applied by TCCON does not only compensate airmass errors but also partially compensates the temperature error in the resulting XCO2. (4) The comparison of an average XCO2 value over an extended observation period with a monthly mean of GOSAT observations is not a very useful demonstration. The bias between GOSAT and a mid-IR observation is probably dominated by the systematic error of the assumed linestrength of the CO2 line used for the observation. Instead, it seems crucial to me to demonstrate that the new sensor is capable of detecting the variability of XCO2. If the observational period is too short for detecting the annual cycle and if no data from a collocated reference sensor is available, if finally GOSAT observations are too sparse for a detailed intercomparison, still a comparison with modelled XCO2 data as provided by Copernicus / MACC would be possible.