

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

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Dear Dr Hase,

The holiday season delayed our reply to Dr Feist. We believe that this reply, now published, may address some of your concerns. Your comments regarding the assessment of the accuracy of XCO₂ are very well aligned with our own roadmap for the next steps in the project, as outlined in the conclusion. We would like to reiterate that the primary focus of the manuscript is the description and performance assessment of the instrumental part of the measurement system. As such, only the analysis of instrumental error propagation has been so far performed. Since the instrumental performance is shown to be good (\sim few times below the ultimate shot noise limit), the next step in the

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maturation of the technique consists of addressing the issues you raised, in relation to uncertainty propagation of the geophysical inputs, to provide a full error budget on XCO₂ measurements. To this aim, we have ~ 14 months of recorded data to be analysed. This is a significant amount of effort related to the development of our retrieval software (still currently in its infancy), in addition to cross-validation against well established, mature, instrumentation, and funding has been sought to deliver this follow-on work.

Comment 1 on the passive solar tracker and field deployability

In our manuscript, we have not given a figure quantifying the overall precision or accuracy of the solar tracking system assembly once deployed, since no solar disk imager was installed to allow us to do so. The various figures we give correspond to hardware specifications of the individual components (motorized stages), published precision estimates for the Sun position algorithm, modelled estimates for displacements over the course of a single acquisition, and lab-based pointing precision assessments of the assembled tracker, after mirror alignment but before deployment. Nevertheless, we found the system to qualitatively perform largely as anticipated, in as much as few adjustments are generally necessary over the time span of several hours of continued operation to keep pointing at the Sun. Of course, this is not a quantification of pointing accuracy at angles smaller than the arc subtended by the Sun disc, which is needed for retrieval error assessment. It is also not currently sufficient for autonomous and unsupervised operation. The presented research breadboard system is severely constrained by the building layout. The implementation of a solar disk imager was not practical (the distance between solar tracker and instrument is too long). We are aware that quantitative pointing accuracy data are needed to carry out the error budget already mentioned above. The field deployable version of the system currently being engineered will integrate a solar disk imager and the corresponding pointing analysis software with two objectives: 1) quantitative assessment of a passive pointing mechanism, and if required 2) active feedback for automated correction. Detailed assessment

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of the tracking performance of such a system will follow once available, together with considerations about repeatability and campaign implications. As mentioned in the manuscript, the algorithm at the core of the tracker's control software can easily be upgraded as necessary, e.g. to include refraction corrections for high-latitude operation, or a slow component of control feedback.

Comment 2 on the degrees of freedom (DFS)

We have clarified the DFS in our response to comment 3 of SC1. We ought to clarify this more explicitly in the manuscript by mentioning the partial DFS of the individual retrieved quantities, making up the overall DFS. Retrieval sensitivity to spectroscopic parameters relates to bias analysis and geophysical input uncertainties. For all the input parameters, sensitivity matrices (D matrix in Rodgers terminology) shall provide the quantitative assessment; and spectroscopic parameters will be included. One of the advantages of using a narrow spectral window and a single transition is that spectroscopic parameters uncertainty propagation is more straightforward to analyse and control. Currently we haven't touched upon this particular task, which is part of the follow-on error budget. For the preliminary retrieval shown, data are taken directly from HITRAN 2012, and the specific parameters, as reported in HITRAN, are

- for CO₂: line position: 952.880849 \pm <0.00001 cm⁻¹ (1) intensity: 1.900e-23 cm⁻¹/molec.cm⁻² \pm >20% (2) air-broadened half-width: 0.0793 cm⁻¹/atm \pm <1% (3) self-broadened half-width: 0.109 cm⁻¹/atm \pm <1% (4) temperature-dependence for air-broadening: 0.71 \pm <1% (5) air pressure-induced line shift: -0.002100 \pm <0.00001 cm⁻¹/atm (6)

- for H₂O: line position: 953.367430 \pm <0.0001 cm⁻¹ (7) intensity: 4.801e-24 cm⁻¹/molec.cm⁻² \pm 5-10% (8) air-broadened half-width: 0.0406 cm⁻¹/atm \pm 5-10% (9) self-broadened half-width: 0.235 cm⁻¹/atm \pm 2-5% (10) temperature-dependence for air-broadening: 0.39 \pm 10-20% (11) air pressure-induced line shift: -0.004630 cm⁻¹/atm \pm <0.001 cm⁻¹ (12)

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So clearly there errors can be large and dominant as you suggest. Spectroscopic parameters accuracy will have to be significantly improved for the chose transitions. With an under-constrained problem, we agree that spurious profile oscillations are highly likely, and we have been facing such as well. Even before thinking of long-term measurement of real data, work on improved a priori constraints and vertical grid optimization have to be conducted and evaluated.

Comment 3 on errors associated with imperfectly known temperature profiles

We agree that the temperature sensitivity of the measurement needs to be looked into and analysed. We refer to our reply to comment 2 as well as the introductory paragraph to this reply. At this stage, the OSS has been developed to assess instrumental errors only (p. 7 l. 13) and determines how close to a perfect LHR the research system operates. We do not claim operational readiness but rather a first step in maturing the approach towards this goal. Biases due to errors in geophysical inputs, including the temperature profiles, are not included on purpose, and are subject to follow-on work (p. 19 l. 25). Our plan consists of using temperature (and pressure) data generated as space- and time-interpolated profiles from operational NWP analyses, with associated uncertainties (which are typically hard to quantify and/or obtain). These uncertainties will need propagating into the uncertainties of the retrieved CO₂ product(s). Whilst we agree that temperature variations in the PBL are frequently well beyond 10K, the uncertainty associated with interpolated 3h or 6h analyses (or higher-resolved forecast data) can be assumed to be much less, in particular throughout the troposphere. The sensitivity analysis should also provide valuable information informing on an optimal measurement protocol to be used.

Comment 4 on cross-comparison and validation

Further to the above, we re-emphasize that validation forms an integral part of follow-on work. We have collected data over a full year and (as far is possible in often cloudy Britain) over diurnal cycles. Corresponding assessments of cyclic variability will be

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included in that work. Our research instrument remains available for future tests and inter-comparisons, particularly with our on-site FTS that is currently being upgraded towards TCCON requirements. The GOSAT monthly mean data point comparison was included as a coarse independent cross-check (sanity check) rather than as a comprehensive validation argument. We could emphasize this further in the manuscript if it is felt to be not clear enough. The use of modelled data will also be considered in follow-on work, either for comparison, as you suggest, but also in order to evolve our retrieval software with more rigorous a priori specification.

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