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Interactive comment on "Potential improvements in global carbon flux estimates from a network of laser heterodyne radiometer measurements of column carbon dioxide" *by* Paul I. Palmer et al.

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The model section of this study provides useful information concerning how our capabilities of estimating CO2 fluxes could be improved by further extending our observational capabilities. The problem I have with this paper is due to the fact that it does not perform this examination on the grounds of a hypothetical network of selectable quality, but explicitly refers to a network comprised of a certain type of existing device (mini-LHR instruments). If such a reference is chosen, it seems essential to me to include a reliable characterization of the actual performance achieved by this kind of device. The noise error of a single measurement recorded with a single unit of a network is not at

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all the critical issue when investigating what gain could be induced by adding such a network. The impact of systematic errors (between units: site-to-site biases, and correlated errors for a selected unit: drifts) is in my opinion not adequately addressed in the paper. A proper characterization of the error budget seems an essential prerequisite to this study as the authors explicitly refer to a certain kind of existing instrumentation (similar requirements would result from the claim that a mini-LHR network would be useful for satellite validation).

Therefore, the announced instrumental study should be published before the submitted paper in order to provide a reference. A longterm side-by-side comparison of one unit with a TCCON spectrometer (spanning at least one annual cycle) would be a minimum requirement. (A previous version of the manuscript handled under AMTD manuscript number 2017-368 provided slightly more information and presented actual mini-LHR spectra, so this current version of the manuscript seems to me a further degradation of the previous presentation.)

Moreover, the impression that no adequate investigation at all is attempted by the authors for establishing a reliable error budget is further consolidated by the fact that even a simple estimation of the total error budget based on plausible assumptions concerning instrument performance and a-priori knowledge of the atmospheric state is not provided. Note that TCCON uses the co-observed column of molecular oxygen for generating column-averaged dry-air mole fractions. This step is useful not only for reducing the error propagation of instrumental imperfections, but also to reduce other detrimental impacts, as e.g. errors in the assumed atmospheric temperature and ground pressure - I would therefore expect a less favourable error budget for the mini-LHR instrument.

In summary, I would require to profoundly strengthen the part on instrument characterization and overall error budget of the proposed network (or publish these aspects before the presented kind of study), and to apply the resulting correlated measurement errors (drifts, airmass-dependent effects, site-to-site biases, etc.) for achieving a real-

istic estimate of the potential improvements. I do not recommend a publication of this work in its current shape.

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