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Atmospheric Measurement Techniques Discussions

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.

Anonymous Referee #2

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The authors have performed Observing System Simulation Experiments (OSSEs) to evaluate the potential of a new network of mini-LHR instruments to reduce uncertainty in carbon flux estimates. The mini-LHR is a low-cost instrument that can be installed at AERONET sites globally, potentially leading to a significant expansion of the surface observing network. The OSSEs in the manuscript showed that with only 50 mini-LHR sites, well located around the globe, it would be possible to greatly reduce CO2 flux uncertainties. The paper is well written, and the suggestion of deploying the mini-LHR in tandem with the sun photometers at the AERONET sites is an excellent idea. However, I cannot recommend the paper for publication in its present form. I believe that additional OSSE work, as described below, is needed before the paper would be

acceptable for publication.

Main Comments

1. My first main concern is that the OSSEs do not account for spatially and temporally varying systematic errors in the synthetic data. This would have been an acceptable OSSE study a decade ago, but experience with GOSAT and OCO-2 data has shown that systematic errors are the main challenge with using XCO2 data for flux inversions. Indeed, this was noted by the authors on Page 2, where they stated that for GOSAT and OCO-2 "poorly characterized systematic errors compromise the accuracy of their data (Wunch et al., 2017) and limit the utility of such datasets for inferring surface flux distributions (Basu et al., 2013)." In light of this, I don't see how the authors can neglect systematics errors in their OSSEs. I am sure that the authors are aware of the numerous published OSSEs that were conducted before the launch of GOSAT and OCO that argued that future satellite observations of CO2 will significantly reduce flux uncertainties. Unfortunately, many of those OSSEs did not realistically look at the impact of systematic errors on the flux inversions. The authors must address this in their OSSEs before this study can be considered acceptable for publication.

2. My second main concern is with the use of TCCON as a benchmark for the OSSEs. It has been shown that TCCON can provide useful information on the carbon cycle, but the network was designed mainly for satellite validation. If the focus of this manuscript is on the "potential improvements in global carbon flux estimates" associated with the mini-LHR network, the issue should be examined in the context of the added value of the mini-LHR network given the exiting in situ and satellite observing systems that provide observations used in flux inversions. TCCON data are rarely used for flux inversions. At a minimum, the authors should have included the in situ surface network (the flask and quasi-continuous sites) in their OSSEs. However, I would prefer to see a comparison involving the in situ network and OCO-2 with the mini-LHR network.

3. Another concern that I have with the OSSEs here is that the same model is used

for the nature run as for the assimilation. A challenge with CO2 flux inversions is that we don't unknown how model errors will be manifested in the estimated fluxes given the information content of the data. Using the same model to produce the synthetic data and for the assimilation creates an overly optimistic scenario. I would encourage the authors to use output from another model, using different meteorological fields, to produce their synthetic data. This will provide a more rigorous OSSE and is now standard OSSE practice (See Hoffman and Atlas, Future Observing System Simulation Experiments, BAMS, Vol 9, 1601-1616, 2016).

4. The authors acknowledge that cloud cover is an issue, i.e. data are collected "throughout the day during sunlight hours when clouds are not present." How was that accounted for in the OSSEs? Did they use the MERRA cloud fields to simulate data loss due to cloud cover? It is unclear if this was done. Capturing this well is important for contrasting the regional improvements in the flux estimates as data loss will be worse in some regions than others, and will vary seasonally.

Minor Comments

1. Page 2, line 13: define mini-LHR.

2. Page 3, line 9: it should be "result in a" and "will be the".

3. Page 4, lines 19 and 20: Is it MERRA or MERRA-2?

4. Page 4, line 28: Some words are missing here: "to get calculate spectra".

5. Page 6, line 24: No, this is not a rigorous test of the data. Please see main Comments 1, 3, and 4 above.

6. Page 7, line 18: How reasonable is this 5% error? On what it is based? The assumed a priori error will influence the estimated DOF. An overestimate of the a priori error will result in artificially large DOFs.

7. Page 8, line 3: Instrument biases really must be included in the OSSEs (see main

comment).

8. Page 9, lines 2-4: What is the reference for this statement? It is not clear to me from what was presented that the performance shown here rivals the in situ network over North America and outperforms it in the tropics. This is why I would like to see the in situ data included in the OSSEs (see main Comment 2).

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