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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Differences of the inverted terrestrial ecosystem carbon flux between using GOSAT and OCO-2 XCO_2 retrievals" *by* Hengmao Wang et al.

Anonymous Referee #2

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Review of "Differences of the inverted terrestrial ecosystem carbon flux between using GOSAT and OCO-2 XCO2 retrievals" by Wang et al

This manuscript presents the results from a numerical experiment in which two inverse estimates of land carbon uptake were made, driven by two different satellite retrievals. One of them results from OCO-2 spectra, while the other one comes from GOSAT. The inverse system is based on GEOS-CHEM and a 4D-VAR method, and spans the year 2015 completely. Posterior fluxes are evaluated by comparing against CT2016 fluxes, and against a set of flask observations, as well as TCCON XCO2 retrievals. The authors conclude that the inversion brings fluxes in closer agreement with all three of

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these, and differences between the two flux estimates are discussed in the text. Overall, the manuscript is easy to read and organized logically, and sufficient information is presented to allow the reader to appreciate the results.

What is missing from the current manuscript mostly is scientific depth. The experiment conducted is relatively straightforward, and the text at many points falls into long repetitions of numbers presented already in figures and tables. The differences are highlighted, but what drives these differences, what they imply for the use of these satellite data, and what to learn from the comparisons remains unclear. This does not invalidate the substantial effort, but it brings into question whether a publication like this should be considered scientific literature, or a technical report. I will leave this for the editor to judge.

But even for a technical report, I find the manuscript as presented currently incomplete. The demonstration of smaller biases relative to TCCON and flask observations, and the incidental agreement with CT2016, or GCP, or a set of Asian inversions, brings me to hypothesize that the improvements are not due to the use of the spatially explicit satellite data, but simply a manifestation of a better global total land sink compared to the prior. This can be tested using the poor-man's-inversion first described by Chevallier et al., (2010), in which a global residual land sink (for example that from GCP) is projected onto the land biosphere following the pattern of Net Primary Production. This benchmark is more difficult to beat than a prior from CT2016, as it inherently is globally unbiased and follows patterns of vegetation activity. Improvements beyond those in a poor-man's-inversion due to the use of satellite data would imply that spatial patterns can indeed be estimated from such satellite data, and thus make this manuscript worth reading. Finally, the use of CT2016 as benchmark for a non-satellite inversion seems illogical to me, and should be replaced by a flask-only inversion using the same system as used for the other inversions.

Without these two additions, I feel that this manuscript is not ready for publication in ACP, either as a technical report or as a scientific paper.

A long list of further remarks, and points that require further explanation and discussion comes inside the annotated PDF that accompanies this review.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-1175/acp-2018-1175-RC2-supplement.pdf

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