

Interactive comment on “Comparisons of the Orbiting Carbon Observatory-2 (OCO-2) X_{CO₂} measurements with TCCON” by Debra Wunch et al.

Anonymous Referee #1

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This study evaluates the quality of the OCO-2 observations (specifically the OCO-2 v7Br retrievals) by comparing it to the TCCON dataset. The paper presents an assessment of all the three operation modes for OCO-2. While the target mode assessments are critical to monitor and evaluate OCO-2 operations (and determine the global constant scaling factor), from the perspective of the science community, it is the nadir and the glint mode assessments that are high-priority and most critical.

The authors are well aware that this manuscript will become “THE OCO-2 validation paper” that will be referred to again and again over the lifetime of this mission. The manuscript is loosely written leaving several statements open to misinterpretation. This can have serious consequences regarding interpretation of the quality of observations

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from space-based platforms (both current and future missions). Given the importance of this manuscript, I suggest the authors make a stronger effort to justify critical numbers pertaining to biases in the OCO-2 data, associated statements, choices they have made with respect to data filtering, etc.. This manuscript should provide the carbon science community with a robust assessment of the quality of the XCO₂ estimates from OCO-2 and also guide the community on how to best use this novel and unique dataset.

MAJOR COMMENTS

(1) Switching OCO-2 data streams between Section 3 and Section 4: In Section 3, the authors used the full OCO-2 v7Br with a set of manual filters (loosely consistent with $WL \leq 15$). In Section 4, the authors use the ‘bias-corrected’ OCO-2 data from the lite files with $WL \leq 11$. Why? This was a huge disappointment - as a science user of the OCO-2 data, based on the analyses presented here, I cannot evaluate the relative quality of the nadir v. glint v. target data. What complicates matter further is that to determine the constant scaling parameter for bias correcting the nadir and glint data, the target data are used. This should be clearly stated in Section 4, i.e., point out the link with the discussion on Page 7. The science community recognizes that we do not have an infinite and unlimited number of validation data to work with. But the authors should keep the OCO-2 data set and the data filtering criteria same for both Section 3 and Section 4. Section 4 should have two parts – one with the dataset same as Section 3 and one with the bias-corrected, lower WL (i.e., higher quality) data that is currently discussed in Pages 9-10.

(2) Page 9, Lines 277-278: This to me is the most important summary line in the entire manuscript, and the authors need to justify it. The OCO-2 data used in this analysis is already bias-corrected. First this line should read – “differences between bias-corrected OCO-2 and TCCON are all less than . . .”. One may refer to this as the residual biases but the way this line is phrased is misleading. Second, somewhere in the text the authors also need to explain how they came up with the 0.5 ppm number

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– it is based off the last row in Table 3, which itself takes into account all the TCCON sites, i.e., in an average sense. But the range of differences is large across the TCCON sites. Can the authors provide an uncertainty bound on this “average” bias number, for e.g., 0.5 ppm \pm ??? Once the authors address #1 above, then this number will be revised – I expect that with WL \leq 15, both N and the Bias will increase in Table 3. But this will be extremely valuable information for the science community – with varying WL cutoffs, how does the OCO-2 data compare to TCCON?

(3) Estimate OCO-2 errors/biases under varying surface properties: The discussion of TCCON sites in Sections 2.1 and 3.2 seems to indicate that different sites can be grouped together into specific “surface classes” (i.e., albedo). Have the authors attempted to generate statistics of OCO-2’s performance based on the albedo around a TCCON site? For example, a figure similar to Figure 8 but broken up by albedo/surface topography and for different seasons may be highly informative. Such an assessment will allow the inverse modeling community to adjust the errors they specify on the OCO-2 data by season, location, etc. Again note similar to #2 – the authors should make an attempt to address potential science questions that are of interest and relevance to the community. The manuscript in its current form does not do that.

(4) Page 5, Lines 117-119: It is unclear what the authors mean that data from other target sites will help assess bias. Isn’t bias already being addressed in this manuscript? Are data from any of these other target sites available? How do they compare to the OCO-2 nadir and glint mode data?

(5) Given that O’Dell et al. [2016] is not yet available (on AMT, or elsewhere to the best of my knowledge), the authors should provide a bit more description on the bias correction procedure or refer the reader to the OCO-2 technical documents. Mandrake et al. [2015] doesn’t cover the recent version of bias correction algorithm that is in place. Again the authors need to be aware that this manuscript will be read by the bigger Earth Sciences community in general, and not just the core OCO-2 community.

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MINOR COMMENT

(1) Page 2, Lines 5-7: Specify that these numbers are valid for a selected subset of bias-corrected and screened OCO-2 data, i.e., even after bias correction, a WL filter of 11 was applied. Or report here the statistics for $WL \leq 15$ (or $QF = 0$).

(2) Page 2, Lines 25-26: The utility of XCO₂ lies in the fact that we use it to infer surface fluxes of CO₂ (a minor nitpicky point). Maybe just rephrase this sentence.

(3) Page 3, Line 39: Add the word data after TCCON

(4) Page 3, Line 40: Replace the word “measurements” with observation modes

(5) Page 3, Line 48: This is a rather loose statement. Many factors contribute to the CO₂ seasonal cycle, one of which is the boreal forest. Kindly rephrase this statement or end it at the “. . . northern hemisphere”.

(6) Page 3, Line 57: Replace “be measured” with “measure”

(7) Page 3, Lines 61: Replace “was” with present tense – kindly check the verb forms throughout the manuscript to make it more appealing to the reader.

(8) Page 4, Line 74: Replace the word “measurement” with “region”

(9) Page 4, Line 76-78: Unclear. Why can't the variability be real? Especially if a weather front is passing through carrying dirty anthropogenic plumes. The authors need to specify caveats associated with this statement, and conditions under which variability in XCO₂ can be considered an artifact.

(10) Page 4, Line 104-106: Please be more descriptive of the exact surface properties or albedo conditions at these sites. Line 104-105 currently reads like a nursery rhyme.

(11) Section 2.1: The authors can choose to add a column in Table 1 called ‘Notes’ or ‘Site Description’. By adding information in Table 1, they can and should cut out a lot of the details from this section. Given that the authors have not covered all the TCCON

sites, or all seasons at all sites, it is unclear why specific sites have been discussed. This entire section should be rephrased and re-structured. Kindly understand that the readers' time is valuable and provide information that is necessary and relevant. For example, how is the population of specific cities necessary to interpret any of the results in this manuscript (Page 5, Lines 113-116)?

(12) Page 6, Line 144: This is the first time that the phrase “warn level” has been used. What does this mean? Please provide a description and point to appropriate references.

(13) Page 6, Line 148: It is unclear what the authors mean by – “limitations in the information content of the measurements” and how it causes systematic biases.

(14) Page 6, Line 152: Add the term algorithm or procedures after “bias corrections”.

(15) Page 6, Lines 152-157: The authors may want to number the three key biases as (a), (b), (c), and discuss them in the order they are numbered.

(16) Page 6, Line 170: The authors should provide more details here for the average reader – how can examining data near coastlines provide an estimate of biases? Or refer to the OCO-2 technical documents.

(17) Page 6, Lines 176-177: This is another example of a poorly written statement, which can be easily misinterpreted. Generating high-quality OCO-2 data is crucial for obtaining surface flux estimates with reasonable accuracy. I do not understand why the authors want to make this consistent with “the state of the art inversions of surface in situ data”. I believe the authors are implying a statement about the quality of the XCO₂ estimates and the need to bring them on an equal quality level with the surface in situ data – I don't see why inversions and fluxes are brought into the mix here. Kindly rephrase.

(18) Page 7, Lines 191-204: Can the authors comment on the significance of their calculated correlation coefficients? In fact this is more relevant for Section 4 (i.e., Table

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3), where in several cases $N < 10$ at an individual TCCON site. The authors need to proceed with caution when calculating statistics using a low number of samples. And that should be acknowledged as a caveat.

(19) Page 9, Lines 281-283: I do not understand why the correlation coefficient is impacted because the XCO₂ variability is lower. It may impact the RMS but I do not understand the rationale behind the impact on R². Can the authors clarify their reasoning here?

(20) Page 10, Lines 294-295: This statement should come with a set of caveats about the way the data were selected and/or filtered. See earlier comments.

(21) Figure 8: The positive differences observed at Reunion Island stands out in this graph. Can the authors comment on why such large differences are being observed at this location? Such large differences do not show up in Figure A1(r) or the statistics reported in Table 3.

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