Dear Reviewer #1 (RC2),

The authors would like to sincerely thank the reviewer for taking the time to evaluate this paper and provide useful, constructive feedback. We have made every effort to address the reviewer's comments and revise the paper accordingly. Detailed responses follow.

RC2: 'Comment on amt-2023-151', Anonymous Referee #1, 21 Sep 2023

> In this paper, Jacobs et al. evaluate the impact of different digital elevation models (DEMs) on XCO2 retrievals from the OCO-2 Instrument. By comparing different DEMs, including OCODEM used in the ACOS V10 algorithm, the NASADEM+ used in ACOS V11, and the Copernicus DEM used in ACOS V11.1, the authors conclude that the Copernicus DEM has better overall continuity and accuracy. The authors demonstrate that the differences in DEM have a significant impact on the bias-corrected XCO2 retrievals. With the use of the Copernicus DEM and updated quality control filtering, the ACOS V11.1 XCO2 retrievals show generally similar or improved accuracy and spatial continuity as compared with V10 and V11 retrievals. In addition, V11.1 also has increased the data volume that passes through the data quality filtering. This paper discusses an important input dataset for satellite trace gas retrievals. The results and recommendation from this study should be useful for both algorithm developers and data users. The paper is well organized, and the figures are mostly clear. However, I feel that in its current form, the paper is more tailored towards readers who are already familiar with (or have working knowledge in) the ACOS retrieval algorithm. It would be fine as a chapter within the ATBD or product readme file, as in those documents, the other chapters would have provided the necessary context. As a stand-alone paper, I found it rather difficult to follow. Some terms and parameters are casually introduced with little explanation. Some important conclusions are drawn without the necessary supporting evidence.

Response: Some additional details have been added to Sect. 2.1 and throughout the text. As before, we cite the OCO-2 Algorithm Theoretical Basis Document (ATBD) and several other relevant publications in Sect. 2.1 of the paper, which should point readers who are less familiar with OCO-2 operations and retrievals to useful and informative resources. For a full explanation of how the OCO-2 instrument and retrieval operate, the ATBD and other cited literature , it is not practical for us to reiterate everything in this paper. The most recent OCO-2 ATBD can be accessed at https://docserver.gesdisc.eosdis.nasa.gov/public/project/OCO/OCO_L2_ATBD.pdf. It should be sufficient to provide citations to literature that already discuss background information, allowing readers who need more details to seek it out within those cited works. This paper does present results that are relevant to other satellite missions and retrievals. Nearly all satellite-based retrievals of trace gases struggle with the accuracy of their surface pressure retrievals, or do not retrieve surface pressure at all, and in most cases a DEM is required to rescale surface pressure from a meteorological model or reanalysis product. Furthermore, even the meteorological models and reanalysis products used for prior surface

pressure estimates use some source of DEM data. It is important for many groups aside from only users of the OCO-2 product to consider the impacts of a poor choice of DEM on their data products, which should justify this paper as its own publication and not simply a chapter in the OCO-2 ATBD. In addition, to align more with terminology in Kiel et al. (2019), we have changed "dpfrac" to dP_{frac} be more in line with his definitions. Regarding the latter statement, It is unclear to which conclusions the reviewer is referring. We would need more specificity from the reviewer in order to properly address their concern.

Specific comments:

P2, L6

RC2 comment: can you provide a reference that the 14-year GOSAT data record is long enough to inform multi-decadal climate variations?

Response: Changed to "accumulated data records long enough to describe interannual climate variations and characterize seasonal cycles" and several citations are added.

P2, L25

RC2 comment: can the authors elaborate more on the tests that fix surface pressures? What are the other deleterious effects and why? This would be interesting, given that there are GHG retrieval algorithms that use assimilated surface pressures (rather than retrieving them).

Response: Although it would be interesting, elaborating on these tests within the paper would likely require at least an additional section, if not an entirely separate paper. We believe this is not a useful addition because the impacts of the DEM would persist regardless of whether the surface pressure is fixed at the prior or retrieved and bias corrected in post-processing. The wording has been changed to remove mention of the fixed surface pressure analysis and, instead, we discuss the prevalence of other satellite-based retrieval algorithms that either fix surface pressure at the prior or retrieve it and then have to bias correct their final product due to a correlation between surface pressure bias and Xgas bias.

P5, L5

RC2 comment: this part repeats what has already been introduced in page 2.

Response: This has been updated to read as follows:

The OCO-2 instrument uses observed spectral radiances in the O2A band to retrieve estimates of surface pressure necessary for retrieving XCO2 (see Sect. 2.1 and 3.2).

Section 2.1

RC2 comment: can the authors clarify on the status of OCO-2 XCO2 products? Will V11 be released or only V11.1 be released?

Response: Both v11 and v11.1 have been released and are currently available on GES-DISC, but the default OCO-2 XCO_2 product is now v11.1, which we encourage users to use. This has been made clear in the paper.

P5, L25

RC2 comment: can you briefly explain the footprint bias correction (it is also in equation 6)?

Response: The multiplicative scaling is to ensure no global bias with respect to TCCON. The footprint correction is an additive offset of a few tenths of a ppm or less to ensure there is no mean difference amongst OCO-2's eight footprints. There are two citations provided in the paper that describe the footprint bias corrections in detail, as well as all other terms in the bias correction and how they are calculated. The details of the bias correction that are not thoroughly explained in the paper are left out because they are not particularly relevant to the analysis presented in this paper. See also Kiel et al. (2019).

Section 2.1.1

RC2 comment: could you please rewrite this section to help those who are less familiar with ACOS to better understand it?

Response: This section was moved to the appendix because it is incidental to the primary focus of this study, which is the digital elevation map and its impact on XCO2 and subsequent inferred CO2 fluxes. We only include the changes from v10 to v11 as background information for interested readers, which motivated the move to an appendix.

Equation 6

RC2 comment: where does "0.016" come from? One can guess from the figure, but it would be helpful if the authors can give some explanation.

Response: This appendix has been reworded for clarity in the revised manuscript. In particular, we now write:

"h2o_ratio_uncert_high_TCWV represents the asymptotic value of the uncertainty in h2o_ratio at high TCWV and is set to 0.016."

P6, L28

RC2 comment: can the authors give some quantitative results on how much more robust the new filtering is?

Response: This is shown in Fig. 9 and discussed in Sect. 4.4. We have added a sentence to Sect. 2.1 pointing to this.

P8, L1

RC2 comment: is the aggregation done to all DEMs or just OCODEM?

Response: Aggregations were done on all DEMs for the purposes of comparison, as described in Sect. 3.1, but only the OCODEM altitude were extracted from OCO-2 sounding retrievals. We were not able to obtain access to the original DEM data from the OCODEM, and therefore the OCODEM elevations have already been averaged over the sounding field of view before being aggregated. It also means that we only have OCODEM elevations for regions where there are OCO-2 soundings that pass preliminary screening and have a valid retrieval. We have revised the paper to clarify this.

P9, L13

RC2 comment: GMTED2010 is not defined or used elsewhere in the paper.

Response: Global multi-resolution terrain elevation data 2010 (GMTED2010) is the DEM used in TROPOMI retrievals prior to the most recent update to the Copernicus DEM. Mention of this DEM has been removed from the paper and its analysis is not relevant to this study. We have also made revisions in regard to our discussion of results from Hachmeister et al. (2022) and the DEM update in TROPOMI retrievals in response to comments by RC1.

Section 3.1

RC2 comment: it would be good to run V11 full physics algorithm for a small subset of OCO-2 data to confirm that you will get the same bias-corrected XCO2 as V11.1.

Response: Due to computational expense and lack of available human resources at JPL, it is not feasible to run the v11 L2FP on a subset of the data with the Copernicus DEM.

Section 4.5

RC2 comment: it could be useful to produce maps for Lauder, Pasadena, and Eureka that are similar to Figure 11 (even if just place them in the supplemental information).

Response: Some additional details on the target measurements at these three sites have been added to the supplemental materials and the relevant section of the supplement is referenced in the paper.

Figure 1

RC2 comment: fitting at low TCWV range appears to be of worse quality - how does this affect the corrected h2o_ratio and the overall results, given that significant improvement is seen over low TCWV?

Response: It's relative to "no correction at all". Clearly at low TCWV, there is a bias in h2o_ratio; even if there is uncertainty in the correction, it is much better than no correction. Plus, it is likely that other factors may also affect the clear-sky bias in h2o_ratio, such as surface albedo, signal-to-noise ratios, etc. But this is the largest such factor. Also, note that this figure has been moved to an appendix along with the relevant section, and is no longer Fig. 1 in the revised version of the paper.

Figure 5

RC2 comment: consider putting mean and standard deviation of delta-altitude in different panels so that the differences in the mean between NASADEM+ and Copernicus DEM are more obvious.

Response: This figure has been updated as suggested.