Authors response to the RC2 referee's comments on "Insights into 3D cloud radiative transfer for OCO-2" by Steven Massie et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-323-RC2, 2023.

We appreciate very much the helpful RC2 review comments. The indented lines contain our responses to the referee's comments, which we have put in italics. Line numbers of revisions are indicated by (e.g. N806) in the revised paper.

This paper provides information on the errors three-dimensional radiative processes cause in carbon-dioxide retrievals by the OCO-2 mission. The methodology is appropriate and, although I found a few segments a bit difficult to read, the presentation is generally good. Even so, I do recommend some important changes, mainly to make the study more complete and clearer. My detailed comments are as follows.

Main comments

Line 392: Increasing the threshold from 0.3 to 0.6 appears quite arbitrary. Given a such arbitrary increase, it seems unnecessary to even bother determining the initial candidate threshold value of 0.3; it would be easier to just say that a threshold of 0.6 was chosen because that threshold (and the results it yielded) seemed reasonable in some respect (explaining in what respect the 0.6 value seemed to work well).

Yes, the increase from 0.3 to 0.6 is arbitrary, and the revised text (N806) in Appendix A) now states this is the case. Since the ratio of cloud shadow to cloud brightening events was surprisingly low, the increase from 0.3 to 0.6 was done to produce a conservative estimate of the cloud shadow frequency. We feel it is useful to mention the factor of 2 adjustment. The conservative adjustment still yielded a low ratio of the number of cloud shadow events to cloud brightening events.

Lines 238-249, 373, and 644, plus Figures 8 & 9 and maybe other parts of the manuscript: While the manuscript examines a variety of parameters that influence the impact of 3D radiative processes in XCO2 retrievals, it does not discuss a key parameter: cloud optical depth. This is a critically important parameter, as previous studies (not dealing with XCO2 but with other aspects of 3D processes) showed that thicker clouds cast darker shadows and scatter more sunlight into surrounding regions. In other words, optically thicker clouds cause stronger 3D effects. The paper should discuss how cloud optical depth affects the magnitude of 1D/3D ratios and, in a wider sense, the impact of 3D effects on XCO2 retrievals. This includes specifying the cloud optical depth each time some other scene parameters are specified (e.g., for the bar clouds). In complex scenes, statistical parameters such as the scene average optical depth or the optical depth of nearby clouds should be specified.

The revised paper now includes (Fig. 4e which is added to the original figure) 1D /3D curves for cloud liquid water content of 0.1, 0.3, and 0.6 gm / m^3. While perturbations in a variable (such as LWC or surface reflectance) will noticeably change the 1D and 3D radiances, the perturbations in the 1D / 3D ratio are frequently small. Since OCO2 Lite files have few retrievals influenced by cloud shadowing events, darker shadows play a minor role (N460) in

the archived OCO-2 files.

Section 7: While this section provides a thorough overview of the findings, it would be very helpful to add a brief overall take-home message. This take-home message should include the implications and/or prospects—for example, that based on the new findings, what (if anything) the authors believe we still need to learn or do about 3D effects in XCO2 retrievals.

The last paragraph in the revised paper (N778) now states "Future work includes the development of a quick parameterization of 1D / 3D ratios as a function of aerosol and cloud optical depth, given an arbitrary geospatial distribution of clouds. This work will examine a wider range of parameters such as cloud height, aerosol height, aerosol composition, in addition to an examination of scenes not covered in this paper, such as brighter surfaces."

In response to the Reviewer's 1 suggestion, a new Sect. 7 (N659) discusses calculations of zonal averages of 3D cloud radiative effect biases as a function of latitude for bcXCO2 over ocean and land, and illustrates how the biases can be used to mitigate 3D cloud effect biases.

Section 7: It would help to discuss how the new results relate to the results of earlier studies. For example, it would help to note whether there is any significant discrepancy or reassuring agreement between new and earlier results, and to highlight the instances in which in which the new study adds the most important new information to prior knowledge.

The revised paper now includes this paragraph (N768) in the Conclusions:

"While Massie et al. (2021) focused on comparisons of bcXCO2 and TCCON, the analysis of 275 times more bcXCO2 data between 2015-2018 (without reference to TCCON data) *enabled calculations of 3D cloud effect biases as a function of latitude*. *The biases are larger in the southern hemisphere.* This is possibly due to the fact that there are fewer TCCON observations in the southern hemisphere. The magnitude of the 3D cloud effect biases discussed in Massie et al. (2021) and this paper are similar in size, with QF1 biases generally larger than QF0 biases. Since the post-retrieval bias correction process exclusively uses QF0 data, and dP and dPfrac variables, which are correlated with nearest cloud distance (Massie et al. 2021), it is expected that the QF0 biases will be small. The post-retrieval bias correction process *indirectly* accounts for 3D effects, but Fig. 16 and the Table 5 entries indicate that 3D cloud effect biases remain in the Lite file data."

A new paragraph (2nd to last paragraph of the Introduction) is added to the original text (N142) . This discusses how the current goes beyond the content of Massie et al. (2021).

Other comments about substance:

Lines 272-273: The main text or the caption of Fig. 4 should clarify that the figure only shows results for the "cloud brightening" case shown in Fig. 3 (that is, near the right-side cloud). It might also help to include results for the "cloud shadowing" case or to mention briefly how they behave.

The revised caption of Fig. 4 states that the curves pertain to the cloud brightening situation.

The cloud shadowing case situation was graphed in the same style as Fig. 4 and discussed in the revised paper (N350).

Lines 318-321: It could be good to briefly mention that—and, if possible, why—the pressure perturbations reach a maximum around a vertical optical depth of 2, after which they decrease again and reach zero at 4.

Additional code improvements were implemented to produce a new Fig. 5, in order to better answer the Reviewer's question. The improvements deal with a routine that does an improved Lagrange interpolation of ABSCO data in order to specify the ABSCO cross sections associated with each of the SHDOM model altitude levels. In the new Fig. 5, the pressure perturbations do not reach zero at optical depth of 4 in the SCO2 band in the revised paper.

Though not mentioned in the revised text, the derivatives are expected to vary as a function of optical depth since as optical depth increases, the weighting function altitude of the maximum contribution to the top of atmosphere radiance increases in altitude, and doppler contributions increase relative to pressure broadening contributions.

The intended focus of the paper is stated by the following paragraph added to the revised paper (N411):

"The wavelengths selected in Fig. 5 are representative. A different set of wavelengths would produce derivatives, especially for the pressure and CO_2 derivatives in the SCO2 band at optical depths greater than two, that differ from those shown in Fig. 5. The key point of Fig. 5 is that the pressure and CO_2 derivatives are negative, ranging from 0% to -1%, and are of similar absolute size to the 3D radiance perturbations, which vary from 0% to 3%, for an observation 4 km from the nearest cloud. Figs. 4 and 5 are the only figures in this paper that have information that relates to non-continuum wavelengths."

Lines 371-372: It is not clear to me how "The prevalence of cloud brightening versus cloud shadowing effects for the Amazon scene is revealed in Table 2.". I can see the fraction of shadowed pixels, but the manuscript should clarify (a) whether retrievals are available for all shadowed pixels, and (b) whether brightening occurs for all pixels that are not shadowed.

The revised text now states (N460) "Of the 589 successful retrievals for the Table 2 Amazon 150622 case, only eight retrievals (1.3%) are associated with shadows, and the other retrievals (100% – 1.3% = 98.7%) are associated with cloud brightening, assuming that all cloud heights are 8 km in vertical extent." in order to clarify the text.

Line 373: Does MODIS or OCO-2 indicate which of these cloud heights is closest to what was observed?

MODIS data can be used on a scene-by-scene basis to specify cloud top heights. This was used for the 36 scenes to generate the input data to SHDOM. For the global analysis we needed to use NASA Giovanni statistics. OCO-2 does not determine the locations of clouds nor cloud top heights. Line 415 of the original paper now states (N468):

"Cloud heights, however, are less than 8 km. Application of NASA Giovanni (https://giovanni.gsfc.nasa.gov/giovanni/) analysis of MODIS MYD08 data files yields histograms (not shown) of cloud top temperatures and pressure means which correspond to cloud top heights between 1 and 2 km for the 150622 and 160622 cases, and heights between 2 and 3 km for the 2016 Amazon and Pacific yearly averages."

Lines 383-384: The causality (that is, the reason for the word "since") should be clarified in "It is necessary to consider two dozen latitude bins since some bins are fully cloudy, and some

bins have relatively few clouds.". Alternatively, the wording should be changed by removing "since".

The sentence has been rewritten in the revised text (N797):
"Some of the latitude bins have too few clouds, and are excluded since it is of interest to determine clear radiances in the vicinity of clouds. A $Clear_{ave}$ radiance average is calculated from the $Clear_{bin}$ averages when the percentage of clear flags for a latitude bin is greater than 50%."

Lines 430-434: It appears that in Figure 8 there are a lot of points that have QF1 data even very far to the north from clouds. Is there perhaps a reason not related to clouds that causes more QF1 (and less QF0) retrievals in the areas that happen to be on the north side of the clouds? (Perhaps different wind conditions or stronger glint?

A visual examination of 12 graphs (similar to Fig. 7) only showed one other ocean glint scene with more QF1 data points north than south of the main clouds (so the new Fig. 7 behavior is not general). The revised text now states (N485):

"This situation is not, however, generally the case, since a visual examination of Figs. similar to Fig. 7 for the other ocean glint scenes listed in Table 3 did not show this behavior. An examination of the NASA Worldview imagery for the Fig. 7 scene did indicate that there are more very small "cloud remnants" north of the main cloud region with a very clear region south of the main cloud. The visual examination of the 12 scenes does indicate that QF1 data points are consistently closer to clouds than the QF0 data points".

Lines 630-631: It would be interesting to comment on why the land results for the two hemispheres differ from each other.

The hemispherical asymmetry is another surprising result in our calculations. We do not have a good speculative comment to offer. (V11 processing, in production, uses a different digital elevation map than V10 processing, but these effects kick at high northern latitudes. We feel most comfortable not offering a comment.)

Comments about minor issues

Line 28: The spaces should be deleted from "1 D" and "3 D".

Spaces are now deleted in the revised text.

Line 134: The word "depth" or "thickness" seems to be missing after "vertical optical".

Typo is corrected.

Line 200: The wording should probably be changed in "agreement with the observed reflectance", as there is not clear exactly what observed reflectance (i.e., actual OCO2 observation) is referred to. Also, the wording suggests that SHDOM simulations are adjusted and repeated until a simulation using a certain surface albedo yields the expected result.

The revised text now states (N231):

"The SHDOM calculations do not iterate for the surface reflectance. A constant Lambertian surface reflectance in each band for land observations is specified (hardwired as an input to SHDOM) based on the Lite file retrieved values. These values produce SHDOM 3D top of atmosphere reflectance in good agreement with the observed (archived Lite file) reflectance. For ocean glint observations the Mishchenko and Travis (1997) implementation of the Cox-Munk windspeed dependent surface reflectance formulation is used in the SHDOM calculations, with windspeed specified (hardwired as an input to SHDOM) based on the Lite file retrieval of the windspeed."

As mentioned above, a change in a variable such as surface reflectance will perturb the 1D and 3D radiances, but not so much perturb the 1D / 3D intensity ratios.

Line 208: For added clarity, I suggest inserting the word "vertically" in front of "constant".

Revised as suggested.

Lines 243-249: I suggest mentioning that cloud effects don't extend past 10 km (e.g., Fig. 2), which means that the periodic boundary conditions used by SHDOM don't cause clear- sky pixels near the left cloud to be impacted by photons that, after being scattered by the right-side cloud, move across the right edge of the scene and reappear at the left edge.

The revised text (N296) now states:
"From Fig. 2 it is apparent that 1D /3D ratios asymptote for a length scale of approximately 10 km. The periodic boundary conditions used by SHDOM therefore do not cause clear- sky pixels near the left cloud to be impacted by photons that, after being scattered by the right-side cloud, move across the right edge of the scene and reappear at the left edge. These considerations motivated our selection of the Fig1 geometry and selection of a 32 km by 32 km SHDOM grid."

Figures 2 and 3: As in Figure 4, the vertical axis label should include the words "intensity ratio".

Revised as suggested.

Figure 5: The vertical axis label should match the notation in Lines 302-303, with R(1D) instead of just 1D.

R(1D) is used in the revised figure.

Line 353: The word "zenith" should be inserted in front of "angle" once or even twice, and perhaps "nadir" could be deleted.

The revised text (N432) is now "The direction of the incident sunbeam is from the northwest at the solar zenith angle of 38°, while the OCO-2 sensor angle is 0°."

Figure 6: The caption could mention that north is at top.

Revised as suggested.

Lines 368-369: I recommend considering a switch from fractions to percentages, for example changing 0.60 to 60%, etc.

Revised as suggested throughout the text and Table 2.

Line 487 and Fig. 10: I guess the units for delta wind should be m/s instead of m. Also, similarly to the top right panel of Fig. 10, the bottom right and middle left panels could also include the units along the Y-axis.

Revised as suggested.

Figures 14 and 15: I recommend moving the ocean glint panels up so they become the top row. This would fit because ocean panels are discussed first (e.g., in Lines 534-537). The ocean results were also discussed first in Section 5 and were displayed first in Fig. 10 (ahead of Figs. 11 & 12). I also suggest clarifying the extent of the three latitude bins (I guess they go from -15 to -5, -5 to 5, and 5 to 15 degree).

Revised as suggested.

Lines 556 and 564: The text "as a function of nearest cloud distance" seems to be in error, as Figs. 16 and 17 do not seem to show anything as a function of distance to clouds.

These typos are corrected in the revised paper.

Table 4: In the top row, the two "degree signs" (°) are both placed for the South value; one of them should be moved to the North value.

Revised as suggested.

Line 605: For consistency with other parts of the sentence, "decreases" should be changed to "decreased"—or perhaps both "decreases" and "decreased" should be changed to "reduced".

The sentence is revised as suggested.

Lines 656-657: It should be mentioned whether this sentence is also true for land glint observations and not only for land nadir observations (if we assume that the distance to cloud s very similar for land observations taken at nadir or at a glint direction.

The new Fig. 11 has both land nadir and land glint curves. They are similar.