

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2022-323

Anonymous Referee #2

Referee comment on "Insights into 3D cloud radiative transfer effects for the Orbiting Carbon Observatory" by Steven T. Massie et al., Atmos. Meas. Tech. Discuss.,
<https://doi.org/10.5194/amt-2022-323-RC2>, 2023

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).

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.

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.

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.

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.

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.

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.

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

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”.

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?)

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

Comments about minor issues:

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

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

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.

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

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.

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

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

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

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

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

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.

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).

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.

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

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".

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 is very similar for land observations taken at nadir or at a glint direction).