We thank both reviewers for the careful review of the paper. Below, the reviewer comments have been reproduced. Our responses follow each comment in the red text.

Response to Reviewer 1.

Page 2, 1st paragraph section 2.1, line 124. It is now known that the OCO-2 polarizer is rotated parallel to the principal plane (i.e. 90° to the pre-launch design orientation) due to a fabrication error. It would be good to state the operational orientation, and note that this was a fabrication error. One page 9, line 883, change the I and Q math to reflect the post-launch reality.

We agree that this is valuable information. A footnote has been added to this section, describing the polarization for the operational measurement.

Page 3, line 200. In regard to Figure 1, were SHDOM calculations carried out at all of the OCO- 2 wavelengths (at high spectral resolution) or for a subset, followed by a mapping technique to assemble the 1016 point / band spectra? It is not clear that SHDOM is the individual radiative transfer model used at the 0.01 cm^{-1} resolution.

The SHDOM calculations were carried out on the high resolution grid, using the optimally sampled, sparse subset. This section was expanded to clarify that information.

Page 4, line 287. Does the 30 second compute time refer to the operational OCO-2 computer at JPL, Colorado State University, or the University of Wisconsin? The compute time (if on a slower machine) could give a false impression of the operational setting. Please clarify (e.g. state that the runs were on the JPLOCO-2 operational computer).

The timing information was valid for workstations at UW. A sentence was added with this information, along with the CPU model, for reference.

Page 4, line 366. What is meant by "reduced" χ^2?

The reduced chi^2 is the chi^2 value divided by the number of degrees of freedom, which has an expected value of ~ 1. This definition of reduced chi^2 was added to the text.

Page 5, line 418. Can SHDOM be run using an open boundary condition to avoid the wrap- around problem? If this is not the case, tell the reader that the periodic boundary condition is a specific feature of SHDOM.

SHDOM can be run in either mode. A sentence was added.

Page 5, lines 435 - 436 and Figure 4. There is confusion between x-positions in Figure 4 and the SRF positions 0, 5,10, 15, and 20. A rephrasing "A total of 25 SRF centered positions are used. Positions 0, 5, 10, 15, and 20 (i.e. SRF centered positions at 0, .5, 1,

1.5 and 2.0 km for 100 m grid cells) are shown in Figure 4" is suggested. In Figure 4, "SRF at positions 0, 5, 10, 15, and 20 (panels from left to right)" could also help avoid ambiguity.

These sentences were rewritten to improve clarity.

Page 6, line 536-538. The sentence "so by comparing the clear sky bias at the same time, the 3D radiative transfer effect can be isolated" is not clear. The clear sky bias is compared to what? Which bias is referred to? Too much is said in too little words. Please expand the sentence and clarify.

This section was rewritten to improve clarity.

Page 6, line 552. There are missing numbers in the phrase "The gap in observations around SRF position is due to the ABP" after the word "position".

Changed to "… SRF position 10 …"

Technical corrections Line 159. Clarify "and the values" to "and the fluorescence variable values".

The suggested change was made.

Table 1. The text on page 4 (line 277) states that prior and variances are listed in the Table, but I only see prior values. A rephrasing to "The assumed prior values (i.e. optical depths, pressure heights and thickness values) are listed in Table 1" is suggested.

The prior mean and standard deviations were listed in Table I, but the presentation was unclear. The table layout was updated to hopefully improve its clarity.

Page 5, line 425. Typo "little extinction of the solar radiation".

Corrected.

Page 6, line 564. Change to "to compute an accurate X_{CO2} value for all cases in this scenario".

The suggested change was made.

Page 7, line 662. There are missing words after "for" in the phrase "posterior covariance for the from the"

Changed to "… is an example posterior covariance from the optimal estimation algorithm."

Figure 7 caption, last line. Change to "each boxplot is indicated at the bottom"

The suggested change was made.

Figure 14 caption, third line. Typo. Change to “the soil surface albedo.”

Corrected.

Response to Reviewer 2.

The only concern I have is that the study is limited to (more or less) a single low-level cloud that fills a fraction of the OCO-2 footprint. To what extent are the conclusions of general value for XCO2 errors induced by 3D radiative transfer effects? How relevant are the error estimates for a global ensemble of OCO-2 soundings? I acknowledge that it would require a lot of effort to extend the study to a realistic ensemble of cloud optical and geometric properties, surface properties, and solar-viewing geometries. This is probably beyond the scope of the paper. Despite this limitation, the employed methods are robust and might prove a starting point for further investigations.

Therefore, I recommend publication after considering the mostly minor comments below.

Specific comments

Overall: In the view of my concern above, I recommend that the discussion covers some words on how relevant the findings presented are for the OCO mission. Biases found here are on the order of 0.5 ppm to 5 ppm which is in the significant range that could be detrimental to OCO's goals if the errors occurred regionally correlated.

We agree – this study by itself doesn't directly estimate biases for the overall mission, because we do not have a quantitative estimate of the frequency of subscale clouds that would match the presented scenarios. We think such a study would be very valuable, but it is beyond the scope of the present paper. A paragraph was added to the conclusions to elaborate on this point.

p11548,l4 (and several other places): "plane parallel" While plane parallel is certainly a standard term describing the type of employed radiative transfer model, it is actually not really the plane-parallel assumption that is tested. It is rather the assumption of a horizontally homogeneous atmosphere. This should be mentioned at least once in manuscript.

The horizontally homogeneous atmosphere assumption is mentioned in the introduction.

p11554,l7: were > where

Corrected.

p11558,l22: There is also clouds with thicknesses 0.8 and 1.2km.

This section was expanded to better explain the cloud shapes.

p11562,l13: "0.8 km" Is this the thickness of the cloud?

This section was expanded to better explain the cloud shapes. The clouds are oblate spheroids, all have 0.6 km thickness, but the horizontal dimensions vary.

p11564,l9 and onward: In my opinion it would be better to not discuss both, albedo and relative errors in %. One is meant percentage points, the other real percentage. How about using fractions for albedo (throughout the paper)?

We agree, and changed the references to albedo in the text and the figures to be the unitless albedo fractions.

p11565,l22: remove "and"

Corrected.

p11565,section 5.2.4: Discussion should focus on the correlation of actually observed XCO2 and pressure biases, not on the calculated statistical covarince. The latter largely depends on what the state vector variables are. If these are really XCO2 and pressure (and not partial columns or similar) I wonder why the calculated covariance is small.

We added some additional explanation here to distinguish between the statistical covariance, output from the OE algorithm, and the correlation between the bias errors within each scenario. The statistical covariances are the full column XCO2 and surface pressure.

The covariance between XCO2 and surface pressure may be smaller than expected since the ACOS retrieval algorithm is actually retrieving a 20 layer vertical profile of CO2 mole fraction, with the XCO2 computed from the retrieved profile. A change in retrieved surface pressure will change the thickness of only the bottom layer (see O'Dell 2012 ATM, from our reference list). By examining the posterior correlation between the retrieved surface pressure and the CO2 profile, we see that the CO2 mole fractions in the lowest 3-4 layers are negatively correlated with the surface pressure. The upper atmosphere layers are mostly uncorrelated with the surface pressure, or even show weak positive correlations. The net result is a weak overall correlation between surface pressure and XCO2.

p11566, Fig.14: It would be interesting to see the a priori scattering parameters and to discuss how the retrievals deviate from the prior.

This was added to Figure 14 on the right side.

p11566,l22: ABO2 > ABP ?

Corrected.

Table 1: Pressure units missing.

The pressure values are normalized to the surface pressure, which is mentioned in section 3.1 Physical Optimal Estimation Retrieval. The table caption was updated to make it more clear.

Fig.3: What does reflectance mean exactly here? It does not seem to be the ratio of

backscattered continuum radiance to solar irradiance.

The reflectance was computed using what we believe is the standard relationship:

$$\rho(\lambda) = \frac{\pi L(\lambda)}{I_S(\lambda)\cos\theta_S}$$

Here L, is the observed spectral radiance, I_S, is the TOA solar irradiance, and θ_S is the solar zenith.

Fig.4: Is the SZA=35 deg case illuminated from the opposite azimuth than the SZA=60 deg case or why is the shadowing to opposite sides?

This is due to the periodic boundary condition in SHDOM. Note the figure caption has a corrected typo here – the second to last sentence now says “In the bottom row…”

Fig.14: soid > soil

Corrected.