Authors’ Response to Referee Comment #1

Dear Lu Zhang,

Thank you very much for this detailed review of our manuscript. Please find below our point-by-point responses and proposed manuscript changes (in blue) to your comments, which helped a lot to improve the manuscript.

Major changes include the following:

- Improvements in the methodology by using a bilinear interpolation, which improves the results especially for small altitude variations, but changes most of the figures in the manuscript
- We added all regions and more statistics to Table 3
- We added a recommendation on how to deal with retrievals that retrieve the surface pressure in the NIR
- Improvements in the method description and a more cautious approach in describing the results, e.g. with respect to scattering

In addition, we found an error in Figs. 21 and 22 where we stated that GOSAT had a spatial sampling size on the order of 0.5 x 0.5 km^2, which we corrected to approx. 10 x 10 km^2.

Kind regards and on behalf of all authors,
Michael Weimer

Review report on “ Importance of subpixel Earth surface reflectance and altitude for atmospheric trace gas retrievals from passive satellite instruments”

The manuscript is generally well structured. The theoretical motivation is clear, and the self-consistent FOCAL-CO2M simulations provide useful evidence that the proposed correction can substantially reduce XCO2 biases in heterogeneous terrain. The paper has clear potential for publication. However, several aspects require further clarification or additional analysis before publication. In particular, the validity range of the first-order approximation, the representative of Sentinel-2 bands for CO2M bands

Response: We thank you for all these comments and will get back to each point separately in our responses below.

Major comments

1. The physical meaning of “albedo-weighted altitude” should be stated more carefully

In a real satellite measurement the contribution of each subpixel to the detector signal is not determined by surface reflectance alone. It also depends on:

the instrument point spread function or spatial response function; viewing and illumination geometry; surface BRDF ....

I recommend that the authors make this limitation clearer in the abstract, introduction, and conclusion. The current wording may give the impression that albedo weighting alone is generally sufficient, whereas the derivation assumes a non-scattering atmosphere, Lambert surface, homogeneous spatial response, and small altitude variability.

Response: We agree with this comment and made the following changes to the manuscript:

Abstract: Line 9: "[…] by using a linearized theoretical analysis at one wavelength with many assumptions including a non-scattering atmosphere and neglecting the influence of different illumination of the detector pixel (the point spread function) and effects of the bidirectional reflectance distribution function.

Introduction: We introduce the word "albedo" at its first occurrence in line 67 as an abbreviation of the surface reflectance in the respective wavelength band. In addition, we add the following sentence after line 76:
"Here, as in the remaining part of this study, we neglect effects due to the point spread function or the bidirectional reflectance distribution function."

In the conclusions, we already clearly state that: "We also assumed that each subpixel contributes equally to the signal on the detector, while, in reality, the illumination depends on the point spread function and might be different in the center and at the edges of the detector pixel." (line 684) and based on the reviewer's comment we added "and the bidirectional reflectance distribution function" to the point spread function.

In addition, we added the main assumptions of the theoretical derivation as mentioned by the reviewer to the conclusions in line 679:
"Apart from the approximations done in the theoretical derivation (e.g. non-scattering isothermal atmosphere, Lambertian surface and small subpixel altitude variability), […]"

2 The validity range of the first-order Taylor approximation needs clearer quantification

The theoretical derivation starts from a nonlinear expression for the effective altitude and then applies a first-order Taylor expansion. This is reasonable, but its applicability depends on the magnitude of subpixel altitude variability and albedo contrast.

The Mont Blanc case clearly shows that the proposed method substantially reduces the bias, but residual errors can still reach about ±2.5 ppm, which is well above the CO2M systematic error requirement. This suggests that the first-order approximation may not be adequate in highly complex mountainous terrain.

- The authors should quantify the validity range more explicitly.
- For what values of subpixel altitude standard deviation σ_z is the method reliable?
- At what altitude second-order terms become important?
- How does the approximation error depend on albedo contrast?
- Does the threshold depend on background altitude, SZA, VZA, or scattering conditions?

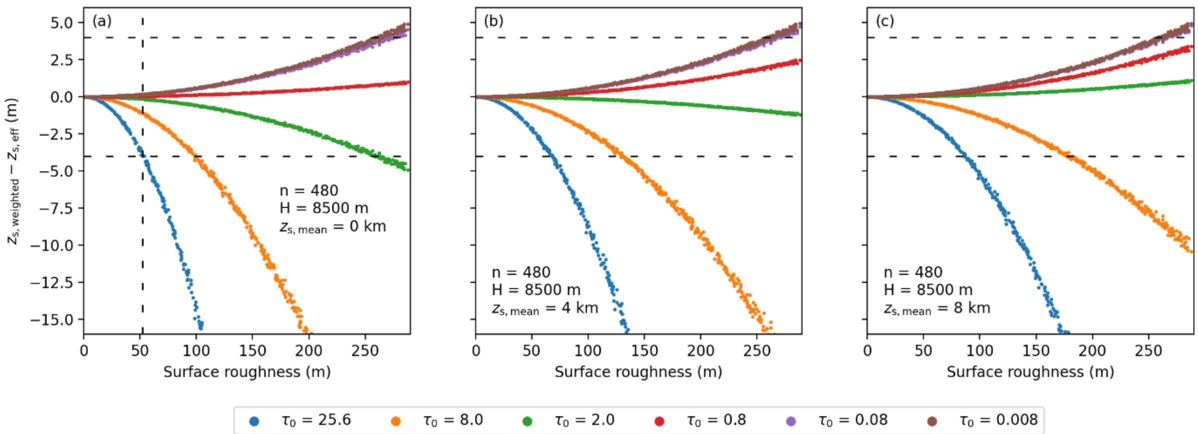
Response: [Note to the editor: This is the same response as for Major Point 2 by Referee #3] We'd like to thank the reviewer for this comment and their ideas about which kind of analysis is needed for this. It is clear that there is a limitation to the Taylor expansion of first order. In the following, we will explain why an analysis as requested by the reviewer is impossible in the real application. In summary, there are two major points we would like to make: (1) We do not know how the retrieval weights single wavelengths, especially not in advance of the retrieval itself, and (2) our results show that, although the approximation of first order is not precise at these altitudes and for these surface roughness regimes, it still leads to smaller errors compared to the unweighted average. Therefore, *the results in this study suggest that it is still better to use the albedo-weighted average of the surface altitude in these challenging regions, where the approximation leads to remnant biases.*

What we can do, however, is an empirical analysis after the retrieval to investigate where the large XCO2 biases actually come from. The dependence on the surface roughness (standard deviation of the subpixel altitudes within the satellite spatial sample) is already shown in the manuscript where it can be seen that there are limitations related to this parameter. We find that the albedo-weighted average of the surface altitude can be used up to a surface roughness of about 300 m. We undertook further investigations on the limitations related to the albedo and found that the largest XCO2 biases are connected to spatial samples with an average SWIR1 albedo being smaller than 0.07, which additionally depends on the atmospheric scattering. Such small albedo values will lead to a smaller sensitivity to the CO2 concentration because most of the signal arriving at the detector does not reach the surface anymore and actually comes from atmospheric scattering. This makes these types of conditions more challenging for the retrieval of XCO2. This is the case even when the subpixel altitudes are constant for the spatial sample. In summary, this demonstrates that the large XCO2 bias most probably results from the challenging conditions for the retrieval (e.g. low surface reflectance of a surface covered by snow) rather than due to the first-order approximation itself.

This comment also triggered us to improve the method for cases where the altitude variability is small. In the published preprint, we used a nearest neighbor interpolation to determine the surface altitude and related meteorological profiles, which essentially limits the accuracy of the method to the vertical spacing of the database, which is set to 4 m, i.e. an approximate ΔXCO2 of 0.16 ppm. We now apply a bilinear interpolation (in the altitude-albedo space) for the mean and effective altitudes. This does not change the main conclusions, but the variability of ΔXCO2 is now smaller. This changes all related figures in the manuscript and supplementary material (Figs. 4 – 6 , 9, 11, 13, 15 – 22, A1, S2, S3, S5 – S8) and the post-processing equation. We additionally added the following description of the interpolation to line 303 of the EGUsphere version of the manuscript:

"[…] and interpolate the meteorological profiles to their corresponding average albedo and the average or albedo-weighted altitudes using a bilinear interpolation in the albedo-altitude space of the database of spectra."

Regarding the reviewers' actual request, we respond as follows. In the initial draft of the manuscript, we had included the following figure:



It shows more or less exactly what is required by the reviewer's comment at first glance. The x-axis shows the surface roughness (standard deviation of the subpixel altitudes), and the y-axis is the difference between the weighted average surface altitude (see Eq. 23) and that from Eq. 11. These equations depend on the mean surface altitude which is different in each panel (i.e. 0 km in panel (a) , *4 km in panel (b) and 8 km in panel (c)) and on the atmosphere optical thickness at altitude 0 m, τ_0, from Eq. 11. To generate the subpixel variation we used a uniform random distribution of z values ranging from ±1, ±2, …., up to ±500 m, and we then calculate the surface roughness and add the respective point to the plot above. The number of subpixels is set to 480 and the subpixel albedo values are taken from a uniform random distribution of values between 0 and 1.*

The plots show that the error of using the Taylor expansion of first order strongly depends on the value of τ_0, and to a lesser degree on the mean altitude. For a constant τ_0, the error increases in one direction, either positive or negative, and in the case of one wavelength, as assumed in the theoretical derivation in Sect. 2, this error can be directly derived from this figure.

As the τ_0 depends on the cross section, it depends on the wavelength, pressure, temperature and H_2O concentration. The spectra measured by CO2M in the future and by current satellite instruments, such as OCO-2 and OCO-3, are not only at one wavelength but rather at hundreds to thousands of wavelengths, which are used to derive information on XCO2. At each of these wavelengths, the τ_0 will be different and the values shown in the figure cover the whole range of τ_0 values estimated from the simulated spectra used in this study. This would make the effective surface altitude dependent on each single wavelength, where we do not have surface reflectance data. In addition, it is not clear which value of τ_0 is representative for the whole spectral window. To address this issue, we would have to know which wavelength has the highest weight in the retrieval, but in advance of applying the retrieval. In addition, the τ_0 value depends on the CO2 concentration in the atmosphere itself, which is the main quantity to be retrieved by the retrieval and is not known in advance of the retrieval. As a result, we cannot assess the range of validity of the first-order Taylor expansion using this figure. At each wavelength, the error in z due to the first-order approximation will be different in the order of magnitude and even in the sign. One could consider the strictest limit, denoted by the dashed vertical line at surface roughness of 55 m related to the Copernicus DEM accuracy of 4 m, but our results demonstrate that the methodology is applicable to a far larger range of surface roughness values (see Figs. 17 and 18). This is not a satisfying conclusion, of course. If the reviewer has further thoughts on how this τ_0, which is representative for the whole wavelength band, can be estimated, we would be happy to test this. We would like to highlight that due to this specific issue, we increased the complexity of the experiments in Sect. 4.1 step by step, to test how far this one-wavelength equation is applicable in the multi-wavelength retrieval algorithm. Consequently, we added the following sentence to its introduction in Sect. 4 (line 310 in the preprint):

"The complexity of these experiment is increased incrementally to test whether the one-wavelength model of Sect. 2 is applicable to the actual multi-wavelength retrieval algorithm."

Although biases on the order of 2.5 ppm remain for the Mont Blanc region, it can be seen that the differences remain a factor of 4 smaller compared to the unweighted average. This implies that using this new method will also improve the results under such conditions. We consider that this high-altitude mountain region pushes the retrieval and the methodology to its limits (which was the intention) and that these data will most probably be filtered out during the retrieval process using real data in the end, as a result of other terrain- and surface type-related retrieval errors.

As this discussion is not yet included in the manuscript, we replaced the sentence in line 195 by the following paragraph:
"The question remains under which conditions this first-order Taylor approximation is valid. However, an evaluation using Eq. (11) and (23) will not help in the real application because the derivation is based on a one-wavelength retrieval while real retrievals use many wavelengths of different wavelength bands to retrieve the greenhouse gas concentration. We analyzed the parameter τ_0 for all wavelengths used in the retrieval for CO2M, see Sect. 3.3, and found that it covers four orders of

magnitude ranging from values between 0.008 and 25.6. This leads to errors due to this first-order approximation also differing by several orders of magnitude (not shown). As it cannot be evaluated in advance of the retrieval which will be the dominant error cause, this derivation is a demonstration that the albedo-weighted average of the surface altitude is a direct result of the assumptions made in this section, but its value for applications in actual retrievals is yet to be shown. This is why we start with examples with well-defined conditions increasing the complexity of the underlying data and finish with applications using real subpixel albedo and altitude data in the first part of the results section (Sect. 4)."

In addition, we replaced the sentences in lines 438 – 440 by the following sentence to emphasize the problem with the Mont Blanc region:
"In addition, it was estimated that larger surface roughness will lead to larger errors when applying the first-order approximation using Eq. (23). Therefore, we expect high mountain regions, such as around the Mont Blanc, the highest mountain of the European Alps, to show remaining biases even after applying the albedo-weighted average of the surface altitude. This region is chosen to assess how large this effect is when pushing the methodology and the retrieval towards the edge of their applicability."

3 The use of Sentinel-2 bands as proxies for CO2M bands is ok?

For SWIR2, the mismatch is even more significant: CO2M covers approximately 1990–2095 nm, while Sentinel-2 B12 covers roughly 2100–2280 nm. Snow, ice, vegetation, dry soil, rock, and mining surfaces may have substantial spectral differences across this interval.

Clearly state in the conclusion that the current results rely on the approximation of CO2M band reflectance using Sentinel-2 bands.

This directly affects the albedo-weighted surface altitude, especially in scenarios involving snow-covered mountains, vegetation boundaries, bare rock, and mining areas.

Response: We are aware of this mismatch, which was already described in the manuscript. The wavelength ranges are compared explicitly in Table 1 in the manuscript. In addition, we would like to highlight that in both Eq. 11 and its approximation (Eq. 23), the albedo only appears as a weight for sums. Therefore, the absolute value of the albedo is not as important as long as its variability can be scaled to the respective wavelength band. As the albedo of the S2 channels B11 and B12 (which are much farther apart than the difference to the respective CO2M wavelength band) are well correlated, this seems to be a good assumption for CO2M. However, this cannot be tested without actual surface reflectance data in the fit windows used for CO2M. We agree that this was not mentioned in the conclusions and added these sentences after the sentence in line 682, which we hope clarify and sufficiently highlight this issue:

"Furthermore, the Sentinel-2 spectral bands do not exactly match the wavelength bands measured by CO2M. However, the available bands and their data sets cannot be changed. Therefore, this adds to the uncertainty in using Copernicus Sentinel-2 data as a proxy for the real surface reflectance at the CO2M wavelength bands."

Minor/technique comments:

1. The manuscript uses "albedo" and "surface reflectance" . Since Sentinel-2 provides surface reflectance and FOCAL retrieves Lambertian-equivalent albedo polynomial coefficients, it would be useful to clarify how these quantities are related in this study.

Reponse: In short: They are the same. In the introduction, we point out that we use "albedo" in this study as an abbreviation for the surface reflectance of a specific wavelength band. Generally, albedo refers to the reflectivity of a surface, usually as a broad-band quantity in the visible spectrum. Surface reflectance is the wavelength-dependent reflectivity, so the same quantity as albedo, but not only in the visible, which is why we abbreviated it here to albedo. The FOCAL algorithm retrieves the reflectance parameters for each fit window, which we call albedo parameters for the same reasons. We replaced the corresponding sentence of line 269 by:

"FOCAL-CO2M estimates the surface reflectance for each wavelength band (NIR, SWIR1, SWIR2) separately with a polynomial to account for non-linear effects, which are abbreviated as albedo polynomial coefficients in this study."

2. In Eq. (13): the starting point for the summation should be 1 rather than i?
   Response: Thanks, we corrected it.

3. In Eq. (16):It would be better for the subscripts to be consistent with the surrounding context.
   Response: We think that this comment is about an inconsistent use of subscripts "I" and "j". As "I" has always been used within sums, but the derivative in Eq. (16) is done for a specific subpixel altitude, we decided to use another subscript "j" here. We agree that this has not been introduced at this point, which is why we replaced the sentence in line 174 by:

   "so that the derivative of $z_{s,eff}$ with respect to one subpixel $z_{s,j}$ can be written as:"

4. At some points, ΔXCO2 appears to denote a correction term; else where it denotes the deviation from the true input value of 400 ppm. Please check it
   Response: In the whole study, ΔXCO2 always refers to the deviation from the true input of 400 ppm. The correction that is suggested in the results emulates the deviations from the true input of 400 ppm, which makes it a ΔXCO2, too. We've already added the subscript "fit" where it is a fit to the actual ΔXCO2 of the retrieval, therefore we think that this should be clear enough.

Authors’ Response to Referee #2

Dear Robert Nelson,

We’d like to thank you for this review with many very interesting follow-up thoughts. Thank you also for starting to apply the correction to real OCO-3 data which is very much appreciated. Please find below our point-by-point responses and proposed manuscript changes (in blue) to your comments.

Major changes include:

- Improvements in the methodology by using a bilinear interpolation, which improves the results especially for small altitude variations, but changes most of the figures in the manuscript
- We added all regions and more statistics to Table 3
- We added a recommendation how deal with retrievals that retrieve the surface pressure in the NIR
- Improvements in the method description and a more cautious approach in describing the results, e.g. with respect to scattering

In addition, we found an error in Figs. 21 and 22 where we stated that GOSAT had a spatial sampling size on the order of 0.5 x 0.5 km^2, which we corrected to approx. 10 x 10 km^2.

Kind regards and on behalf of all authors,
Michael Weimer

General comments:

The manuscript entitled, “Importance of subpixel Earth surface reflectance and altitude for atmospheric trace gas retrievals from passive satellite instruments” investigates the impact of subpixel albedo and altitude heterogeneity using real albedo and altitude datasets combined with simulated NIR/SWIR observations designed to estimate XCO2. The problem is that typical XCO2 retrieval algorithms assume the altitude of a given footprint is simply the mean altitude of the DEM (e.g., Copernicus GLO-90) when in reality it is weighted by the surface brightness of the scene. They combine Sentinel-2 30 m albedo data with the Copernicus 90 m DEM and the FOCAL GHG retrieval to investigate the magnitude of this effect. They first look at an idealized case, then move on to real-world cases (the Belchatow power plant, Black Forest, Mont Blanc, and Germany). Deviations from the true XCO2 can be several ppm depending on the scene, but it can be mostly corrected by using the albedo-weighted altitude within the retrieval itself. They also developed two flavors of a post-processing correction, which are almost as effective.

Overall, the manuscript is well-written, and I recommend publication after my minor comments are addressed.

Response: We thank Robert Nelson for his detailed review and recommendations. Please find our responses to the specific comments below.

P4 L94: “It should be noted that there are approaches to simultaneously retrieve the surface pressure using the NIR band of GHG satellites”

A few prominent retrieval algorithms retrieve surface pressure. This manuscript could benefit from a brief description on a potential correction method, which would make this research more applicable to those retrieval teams. Because XCO2 is the CO2 column divided by the O2 column (dry air assuming we know the O2 concentration), I think you can simply correct the CO2 column using either the WCO2 or SCO2 dz and correct the O2 column using the NIR dz. You could then convert the dzs to dps using the hydrostatic approximation and combine these corrections to adjust XCO2.

Response: We agree that it is beneficial to add such a recommendation to the manuscript, but there is one major issue we see with this approach: As the O2 and CO2 columns are derived from different bands, they (in general) have different surface altitude / surface pressure values when weighting with the subpixel albedo. For instance, at Belchatow we found that a higher altitude is needed in the NIR, but a lower altitude is needed in the SWIR bands. That means that the O2 column includes a different number of particles than that corresponding to the CO2 column. We think that this will lead to a mismatch and rather recommend a combined correction for XCO2 or even better to use the albedo-weighted average of the surface altitude / surface pressure directly in the retrieval as suggested in the current manuscript. On the other hand, it should be easy to convert the weighted z values in the post-processing equation to pressure differences. We highlight this recommendation in the revised manuscript at the end of Sect. 4.4:

"Some retrieval algorithm retrieve the surface pressure as part of the retrieval.

Equation (30) can be converted to include pressure differences by first converting the $z_{s,weighted,i}$ values to their differences to the mean altitude $\Delta z_{s,weighted,i}$ and subtracting the coefficient values from $z_{s,mean}$ in Eq. (30):

$\Delta XCO_{2,\,fit} = -\,7.0\times10^{-6}\ \Delta z_{s,weighted,NIR} + 1.0\times10^{-2}\ \Delta z_{s,weighted,SWIR1} + 3.2\times10^{-2}\ \Delta z_{s,weighted,SWIR2}$

$+\ 1.2\times10^{-3}\ \sigma_z + 5.1\times10^{-5}\ z_{s,mean}$

Each Δz in this equation can be converted to a pressure difference using the hydrostatic approximation for pressure:

$\Delta z = -\,H\,\Delta \ln(p)$."

It has to be kept in mind that when retrieving GHGs we are not really interested in the O2 column in the NIR band, but the dry air column that is representative for CO2, i.e. the dry air column with the highest weight in the SWIR bands. Therefore, assuming that the surface pressure is retrieved only from the NIR band, it could also be possible to retrieve the surface pressure in the NIR and then subtract the difference of the effective altitudes between NIR and SWIR converted to the pressure difference to get the surface pressure for the respective bands which can then be used to derive XCO2. We added this after the new paragraph above:

"When retrieving the surface pressure in the NIR band, only, another possible way to get the dry-air column representative for the CO_2 column would be to use the retrieved NIR surface pressure and use the difference of the effective surface altitudes in the NIR and SWIR, converted to the pressure difference, to calculate the surface pressure in the SWIR. This new surface pressure can then be used to determine the dry air column which can be used to compute XCO_2."

I went ahead and did this for a real OCO-3 SAM over Belchatow on 18 October 2024 (OCO-3 ACOS v11.0 Lite raw XCO2 data, using ~6 months of HLS S2 data averaged together to get sufficient albedo coverage) and got results very similar to your simulations:

[see figure in the original Referee comment]

OCO-3's footprints are 1.6x2.2 km2, slightly smaller than CO2Ms. You can actually see a ring around the edge of the central mine, because this effect only occurs where there are altitude gradients.

Response: We highly appreciate this first application to real satellite data and it is great to see that similar results can be found for this OCO-3 SAM. We are currently working on the application to real satellite data, which is out of the scope of this current study. Our recommendation to improve the coverage of the albedo data, however, would be to do a multi-annual mean for a specific month rather than averaging six consecutive months in order to conserve the annual cycle of the surface reflectance (due to seasonal vegetation changes etc.). See also our response to your comment to P30 L 576 below.

P35 L655: "the subpixel albedo composition is usually measured with another instrument and might be provided at a later time."

It might be worth further discussion on the nuances of ingesting non-simultaneous high-resolution albedo measurements. In a desert, for example, S2 could make a single overpass a week later and you could probably use that albedo data to make the XCO2 correction. Over Poland, however, you might have to average 6+ months of S2 data to get enough spatial coverage.

Response: We agree and added the following sentences to line 728: "In addition, the spatial coverage of the S2 data is important because it depends on cloud-free and low-aerosol scenes. Therefore, single overpasses close to the XCO_2 measurement time might be sufficient e.g. in desert regions whereas averages over months might be needed over regions with many cloudy days."

P30 L576: "However, when assuming that we can get a good estimate of the surface albedo using climatologies, it might be better to use the albedo-weighted surface altitude directly as input to the retrieval"

I suspect this is mostly true, even if it takes several months of averaging albedo data to get sufficient coverage. It would be interesting to look at how long it takes to get sufficient albedo coverage from S2 for certain locations and how much the albedo can change during this time, but this is beyond the scope of your work.

Response: We started to create such a climatology on a monthly basis, but agree that this is beyond the scope of this study to be presented here. Our experience is consistent with your statement: It takes several months to get a complete coverage in the mid-latitudes. There is a significant change in albedo over time depending on the seasonal vegetation change, which we intend to show in a follow-up publication. For Belchatow, for instance, we see changes of the SWIR2 albedo in the surroundings of the coal mining field between 0.05 (January) and 0.30 (July), which might also be affected by snow cover.

P27 L535: "However, the surface albedo can change over time, e.g. due to snow cover"

Would this be an issue for real CO2M observations? Typically, snow-covered soundings are of poor quality and are screened out. When creating an albedo climatology, you'd also want to remove snow/ice pixels.

Response: There is no dedicated filtering for snow/ice in FOCAL-CO2M and it might be difficult to estimate in advance of the retrieval whether a subpixel region is covered by snow. It is, however, indeed likely that the snow-covered spatial samples have a poor quality, e.g. due to their low albedo in the SWIR, and are filtered during the retrieval post-processing. Because there is no dedicated filtering, we decided to keep the snow pixels in the S2 data, but we remove data with high aerosol load (Fmask > 192) or with cloud (Fmask = 2), adjacent to clouds (Fmask = 4) and cloud shadow

(Fmask = 8) contamination. In addition, we removed cases with extremely high albedo (larger than 2.0) or lower than zero in any band. We added this information to the manuscript after line 233 in the description of S2:

"The HLS data are provided with a Fmask for quality assessment. In each downloaded S2 tile, the data contaminated with high aerosol load (Fmask > 192), clouds (Fmask = 2), adjacent to clouds (Fmask = 4) or cloud shadows (Fmask = 8) are removed. In addition, each tile is filtered for high surface reflectance (α > 2.0) or reflectance lower than zero in any band. We deliberately keep the snow data, although they might have poor quality and would be filtered out, because it might be difficult to estimate in advance if a spatial sample is partly covered by snow and because there is no explicit filter for snow in the FOCAL-CO2M retrieval algorithm."

Specific comments:

P9 L239 "Note that the time and viewing geometry are different between S2 and CO2M which is not relevant for the simulated data here, but could lead to errors in later applications, e.g. due to different shadows of the mountains at the time of measurement.":

Other (rare) cases might be where it rains (and darkens the surface) over half the pixel during the elapsed time. Or there's snow present during the first observation but melts before the second!

Response: Thank you for these additional cases, which we added to this line in the manuscript: "[…], e.g. due to […] time of measurement, if rain darkens part of the surface in the time between the two measurements, or if snow falls or melts in the meantime."

P11 L285: "we define the region of interest by using a S2 tile provided by the HLS dataset, average the S2 timeseries of cloud-free surface reflectance measurements using the data of the year 2019"

Does this mean you only applied the "Cloud" mask in Fmask for the HLS S2 data? Was there a reason you didn't also apply the other masks, like "Cloud shadow"? Also see my previous point about removing snow/ice scenes.

And was there a reason you used all of 2019? Looking at the HLS S2 data over Belchatow, there are some strange high albedo values on the edges of the pits during more recent years.

Response: We used 2019 because it is the first full year with two satellites for S2 (better coverage), which is then closest to the Copernicus DEM data which are "acquired during the TanDEM-X mission between 2011 and 2015" (Copernicus Programme, 2026). We added this information to line 209 of the manuscript for Copernicus DEM:

"[…] better than 4 m and is derived from satellite measurements in the time range between 2011 and 2015"

and to line 285 for the S2 data:

"The year 2019 is used because it is the first full year of measurements of S2 with two satellites (better coverage) closest to the acquisition time in the CopDEM data."

Thank you also for pointing out that we neglected to describe the Fmask values we used for S2. As described above as response to P27 L535, we also added this information to the manuscript.

References:
Copernicus Programme: Copernicus Digital Elevation Model, 90 m resolution (GLO-90) [dataset], https://doi.org/10.5270/ESA-c5d3d65, accessed on 26-Feb-2026, 2026.

Fig. 5: it's interesting that for small area fractions at 8 km, using the albedo weighted surface altitude for NIR+SWIR1 gives slightly worse errors than not using any albedo weighting.

Response: Yes, this is true and the reason is probably that when using the average in the SWIR1 band, where the altitude-dependence is reversed compared to using the average only in SWIR2 and where consequently the altitude 8 km has the smallest error, leading to a smaller total error at 8 km. It is not exactly the case but the total error in the first row can be seen as a linear combination of the errors of the three following rows. We added the following sentence to line 361 of the manuscript:

"In addition, it can be seen that the error at 8 km is larger when applying the average only in the SWIR2 band compared to the average in all bands, which shows that the error in the first row is a linear combination of the error in all bands."

In addition, there was a small error in the description in the next line where we replaced "last" by "first row".

Fig. 13: when using the albedo-weighted average surface altitude directly in FOCAL, you still get differences up to +/- 2.5 ppm, which means you would still try to filter out much of this data, correct?

Response: Yes, this is true and we are aware that this region pushes the retrieval to its limits and should be seen rather as a theoretical case than a case actually occurring in retrievals with real data. To highlight this, we replaced "Nevertheless" in line 441 of the manuscript by:

"Although these data probably have to be filtered"

In addition, we added the following sentence to the end of the Mont Blanc section:

"On the other hand, it is clear that this estimated bias up to ±2.5 ppm is larger than the accuracy requirement for CO2M so that they will likely be filtered out in retrievals with real satellite data."

Fig. 15: for the full Germany case, by my eye it looks like higher altitudes generally have more forests, which are darker in all bands. As you mention in the Black Forest case, this is especially concerning because it could result in XCO2 errors manifesting as CO2 fluxes.

Response: It is true that the mountains in Germany are mainly covered by forests which is typical for mountain ranges with moderate peak altitudes (~1500 m) in the mid-latitudes. In addition, there is agriculture and small towns in the valleys, which usually have higher surface reflectance in both the NIR and SWIR compared to the forests at higher altitudes. We decided to add this to line 418 in the manuscript:

"Agricultural activity and small towns lead to generally higher albedo values in the valleys of the Black Forest compared to the forest regions at higher altitudes."

Sec. 4.3: "This fraction is close to zero until σ_z = 20 m":

Can you give a rough estimate of what fraction of Earth's surface, for CO2M-sized pixels, has σ_z <= 20 m? This would give context for the magnitude of the problem and inform existing science teams on how much of their data they would have to reprocess if they implemented this new altitude parameterization.

Response: We evaluated 11 days (corresponding to one repeat cycle) of simulated orbits of one CO2M satellite with the result that globally about 80 % of all samples over land have surface roughness less than 20 m. This means that about 20 % of the land data would have to be reprocessed. We added this information to this sentence in L. 527:

"[This fraction is close to zero until σ_z = 20 m] (corresponding to about 20 % of global CO2M spatial samples over land) […]"

Note, however, that the 0.5 ppm requirement used here for accuracy includes many components, including that of "heterogeneous scenes" (Buchwitz et al., 2020), which is on the order of 0.2 ppm, so that an even higher fraction of land data would be affected. Therefore, we added this sentence to the conclusions (line 650):

"We note that although 0.5 ppm was used in this study, the actual error budget for heterogeneous scenes and surface pressure is only part of the total accuracy and on the order of 0.2 ppm (Buchwitz et al., 2020)."

Buchwitz, M., S. Noël, M. Reuter, H. Bovensmann, A. Richter, J. Landgraf, P. Veefkind, and H. Bösch (2020): Study on Consolidating Requirements and Error Budget for CO2 Monitoring Mission (CO2M-REB): Error Budgets and Performance for anthropogenic CO2 Monitoring Mission (CO2M), Tech. rep., https://www.iup.uni-bremen.de/carbon_ghg/CO2M-REB_TNs/CO2M-REB_TN-3000_v2p2.pdf

Sec. 4.5: "Overall, the application as direct input to the retrieval removes the topographic structures from the retrieval results, but seems to increase the variability over homogeneous scenes, where the average altitude was already a good assumption."

Can you expand on the reasoning behind this? It's hard to tell that there's increased XCO2 variability in northern Germany in Fig. 15(f)

Repsonse: Thank you for this comment. We realized that this larger variability is due to the fact that we used a nearest neighbor interpolation for estimating the meteorological profiles from the spectrum database, which limits the analysis to an accuracy in altitude of 4 m, or approximately 0.16 ppm in XCO2. We replaced this by a bilinear interpolation in albedo and altitude with respect to its nearest neighbors in the database which reduces the variability with albedo-weighted altitude for these homogeneous cases. This changes all related figures in the manuscript and the supplementary material (Figs. 4 – 6 , 9, 11, 13, 15 – 22, A1, S2, S3, S5 – S8) and the post-processing equation. We additionally added the following description of the interpolation to line 303 of the EGUsphere version of the manuscript:

"[…] and interpolate the meteorological profiles to their corresponding average albedo and average or albedo-weighted altitudes using a bilinear interpolation in the albedo-altitude space of the spectra database."

Furthermore, we removed the part of the sentence cited in the reviewer's comment after "but seems to increase […]".

Sec. 5: "One main assumption was that the altitude dependence of the dry-air column-averaged mole fraction in the lowest parts of the atmosphere, where the profile is cut or extended, is small."

And, to a lesser degree, that extending/cutting the meteorological profiles is a valid assumption.

Response: We added exactly this wording to this sentence.

Sec. 5: "especially relevant for greenhouse gases."

And especially for CO2, of course.

Response: We included "CO2 and other" after the "for" in this sentence.

Technical comments:

P1 L5: typical defined as "column-averaged dry-air mole fraction of carbon dioxide"
Response: Corrected.

P2 L36: "to Earth's surface"
Response: Corrected.

P11 L285: "an S2 tile"
Response: Corrected.

P11 L286: "for the 30-m pixels"
Response: Corrected.

Fig. 4 caption: "Because some of the panels have values that are close to zero"
Response: Corrected.

P15 L344: "The non-monotonous values within one fraction are a result"
Response: Corrected.

Figs. 6 & 16: might be worth putting the standard deviation of dXCO2 for each panel
Response: Thank you for this suggestion which we applied and which immediately shows the improvements using the albedo-weighted average of the surface altitude. We added this to both figure captions:
"The σ values correspond to the standard deviations of the XCO_2 *values (in ppm)."*

Furthermore, we added this text to line 366 related to the description of Fig. 6:
"This is also reflected in the standard deviations for both methods: With the unweighted average, the standard deviation is 0.9 ppm whereas it is 0.01 ppm when applying the albedo-weighted average of the surface altitude."

Related to Fig. 16, we added this for the Belchatow example (line 492):
"[…] , which also reduces the standard deviation from 0.12 ppm to 0.05 ppm."

And this for the Black Forest example (line 497):
"The standard deviations are reduced from 0.41 ppm} to 0.05 ppm."

And for the Mont Blanc example (line 501):
"[…], resulting in a reduction of the standard deviation from 1.78 ppm to 0.39 ppm."

Finally, we added this to the description of the example in Germany (line 514):
"[…] with a reduction of the standard deviation from 0.52 ppm to 0.09 ppm."

P17 L364: "compared"
Response: Corrected.

Fig. 6 caption: "and the shaded ranges"
Response: Corrected.

P17 L374: "structure of Earth's"
Response: Corrected.

P17 L375: "real numbers of subpixels"
Response: Corrected.

P18 L385: “one of the largest point sources”?
Response: We corrected this to “strongest”.

P20 L425: “The surface altitude differences are generally larger in the SWIR bands than in the NIR band.” Repeated information?
Response: Yes, we removed this sentence.

P22 L440: “or other differences”
Response: Corrected.

Fig. 16: coincidence that (b) and (c) have the same N? Or typo?
Response: The Sentinel-2 tiles have approximately the same size. (a) was a zoom into one S2 tile, therefore fewer data. (b) and (c) were generated for a whole S2 tile, and (d) are many S2 tiles. Therefore, the number of CO2M spatial samples in (b) and (c) are the same. We do not account for CO2M spatial samples at the edge between two S2 tiles in this study which is also part of the explanation why the numbers of spatial samples in (b) and (c) are the same.

Fig 19(b): z should be “zs,mean” for consistency
Response: Corrected.

P31 L594: clarify what “complete dataset” means
Response: We added “the dataset including spatial samples in Germany, the Alps and the Pyrenees as in the previous examples”.

Authors’ Response to Referee #3

Dear Referee,

Thank you very much for this detailed review of our manuscript, which is very much appreciated. Please find below our point-by-point responses and proposed manuscript changes (in blue) to your comments, which clearly helped to improve the manuscript.

Major changes include the following:

- Improvements in the methodology by using a bilinear interpolation, which improves the results especially for small altitude variations, but changes most of the figures in the manuscript
- We added all regions and more statistics to Table 3
- We added a recommendation on how to deal with retrievals that retrieve the surface pressure in the NIR
- Improvements in the method description and a more cautious approach in describing the results, e.g. with respect to scattering

In addition, we found an error in Figs. 21 and 22 where we stated that GOSAT had a spatial sampling size on the order of 0.5 x 0.5 km^2, which we corrected to approx. 10 x 10 km^2.

Kind regards and on behalf of all authors,
Michael Weimer

This manuscript investigates an important and practically relevant error source in passive satellite retrievals of greenhouse-gas column abundances. Biases in XCO_2 caused by correlated sub-pixel variability in surface reflectance and surface altitude. The authors argue that the conventional use of an unweighted mean surface altitude within a satellite ground pixel can misrepresent the effective light path when brighter sub-pixel areas are preferentially located at higher or lower elevations. To address this issue, they derive an albedo-weighted effective surface altitude and test the concept using Copernicus Sentinel-2/HLS surface reflectance, Copernicus DEM data, and the FOCAL-CO2M retrieval framework. The manuscript applies the method to idealized two-part pixels and to several real-world examples. The study also proposes an empirical post-processing correction and discusses implications for surface-roughness filtering and spatial-sample size.

I recommend minor revision before the manuscript can be considered for publication.

Response: We thank the reviewer for their detailed review and respond to each point in our responses below.

Major comments

1. The manuscript suggests that the albedo-weighted surface altitude should be used in trace-gas retrievals and that it can reduce biases below future mission requirements. This is a potentially important conclusion, but the evidence presented is not yet sufficient to support it in an operational sense.

The study uses FOCAL-CO2M to generate simulated spectra and then retrieves XCO_2 with FOCAL-CO2M using either unweighted or albedo-weighted altitude input. The authors also state that no radiance noise is added, although a realistic uncertainty based on signal-to-noise ratio is used in the inversion.

I recommend that the authors revise the abstract, conclusions, and relevant parts of Section 4 to make clear that the demonstrated improvement applies to self-consistent FOCAL-CO2M simulations under controlled assumptions. Statements such as "below the requirements for future satellite missions" should be qualified, particularly because the Mont Blanc example still shows substantial residual errors even after applying the albedo-weighted altitude. A more appropriate conclusion would be that the method substantially reduces one specific component of terrain-related XCO_2 bias and may be valuable for future retrieval development, but that its operational benefit remains to be demonstrated under realistic measurement and forward-model uncertainties.

Response: We agree that the conclusion to use this in operational products is yet to be demonstrated and made the following adaptions to the manuscript:

Abstract: We removed the statement mentioned by the reviewer. In addition, we added "in these self-consistent experiments" to the last sentence.

Conclusions: Line 697: We included "that there are strong indications" to this sentence. We include "The results using the self-consistent setup suggest that accounting […]" in front of the sentence starting on line 700 and we add the conclusion as suggested by the reviewer to the end of the conclusions: "The method substantially reduces one specific component of terrain-related XCO_2 bias and may be valuable for future retrieval development, but its operational benefit remains to be demonstrated under realistic measurement and forward-model uncertainties which is the subject for a future study."

2. In the theoretical derivation in Section 2, the authors derive an exact effective altitude expression under a simplified Lambert-Beer framework and then apply a first-order Taylor expansion around the mean surface altitude. This leads to the albedo-weighted surface altitude. The derivation is elegant and physically intuitive, but the validity of the first-order approximation is not sufficiently quantified.

The manuscript states that the expansion assumes the deviations of individual sub-pixel altitudes from the mean altitude are small enough. However, later applications include cases with large sub-pixel altitude variations, including mountainous terrain and surface-roughness bins extending to several hundred meters. In such cases, second-order and higher-order terms may not be negligible. The Mont Blanc case appears to be an example where the albedo-weighted approach still leaves residual XCO_2 errors of order ppm, suggesting that the first-order approximation or other simplified assumptions break down for strongly heterogeneous high-mountain scenes.

I suggest the authors add a dedicated analysis comparing the exact effective altitude from their non-linear expression with the first-order albedo-weighted approximation. This should be done across a representative range of altitude variability, albedo contrast, background altitude, solar/viewing geometry, and optical depth. Such an analysis would allow the authors to define a validity domain, for example in terms of surface roughness, maximum sub-pixel altitude range, albedo contrast, or expected XCO_2 residual.

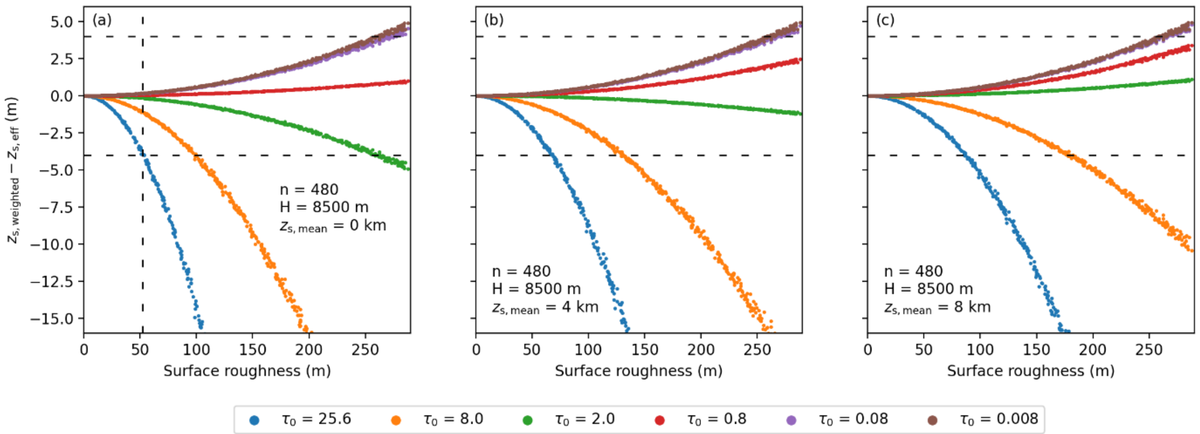
Response: [Note to the editor: This is the same response as for Major Point 2 by Referee #1] We'd like to thank the reviewer for this comment and their ideas about which kind of analysis is needed for this. It is clear that there is a limitation to the Taylor expansion of first order. In the following, we will explain why an analysis as requested by the reviewer is impossible in the real application. In summary, there are two major points we would like to make: (1) We do not know how the retrieval weights single wavelengths, especially not in advance of the retrieval itself, and (2) our results show *that, although the approximation of first order is not precise at these altitudes and for these surface roughness regimes, it still leads to smaller errors compared to the unweighted average. Therefore, the results in this study suggest that it is still better to use the albedo-weighted average of the surface altitude in these challenging regions, where the approximation leads to remnant biases.*

What we can do, however, is an empirical analysis after the retrieval to investigate where the large XCO2 biases actually come from. The dependence on the surface roughness (standard deviation of the subpixel altitudes within the satellite spatial sample) is already shown in the manuscript where it can be seen that there are limitations related to this parameter. We find that the albedo-weighted average of the surface altitude can be used up to a surface roughness of about 300 m. We undertook further investigations on the limitations related to the albedo and found that the largest XCO2 biases are connected to spatial samples with an average SWIR1 albedo being smaller than 0.07, which additionally depends on the atmospheric scattering. Such small albedo values will lead to a smaller sensitivity to the CO2 concentration because most of the signal arriving at the detector does not reach the surface anymore and actually comes from atmospheric scattering. This makes these types of conditions more challenging for the retrieval of XCO2. This is the case even when the subpixel altitudes are constant for the spatial sample. In summary, this demonstrates that the large XCO2 bias most probably results from the challenging conditions for the retrieval (e.g. low surface reflectance of a surface covered by snow) rather than due to the first-order approximation itself.

This comment also triggered us to improve the method for cases where the altitude variability is small. In the published preprint, we used a nearest neighbor interpolation to determine the surface altitude and related meteorological profiles, which essentially limits the accuracy of the method to the vertical spacing of the database, which is set to 4 m, i.e. an approximate ΔXCO2 of 0.16 ppm. We now apply a bilinear interpolation (in the altitude-albedo space) for the mean and effective altitudes. This does not change the main conclusions, but the variability of ΔXCO2 is now smaller. This changes all related figures in the manuscript and supplementary material (Figs. 4 – 6 , 9, 11, 13, 15 – 22, A1, S2, S3, S5 – S8) and the post-processing equation. We additionally added the following description of the interpolation to line 303 of the EGUsphere version of the manuscript:

"[…] and interpolate the meteorological profiles to their corresponding average albedo and the average or albedo-weighted altitudes using a bilinear interpolation in the albedo-altitude space of the database of spectra."

Regarding the reviewers' actual request, we respond as follows. In the initial draft of the manuscript, we had included the following figure:



It shows more or less exactly what is required by the reviewer's comment at first glance. The x-axis shows the surface roughness (standard deviation of the subpixel altitudes), and the y-axis is the *difference between the weighted average surface altitude (see Eq. 23) and that from Eq. 11. These equations depend on the mean surface altitude which is different in each panel (i.e. 0 km in panel (a) , 4 km in panel (b) and 8 km in panel (c)) and on the atmosphere optical thickness at altitude 0 m, τ_0, from Eq. 11. To generate the subpixel variation we used a uniform random distribution of z values ranging from ±1, ±2, …., up to ±500 m, and we then calculate the surface roughness and add the respective point to the plot above. The number of subpixels is set to 480 and the subpixel albedo values are taken from a uniform random distribution of values between 0 and 1.*

The plots show that the error of using the Taylor expansion of first order strongly depends on the value of τ_0, and to a lesser degree on the mean altitude. For a constant τ_0, the error increases in one direction, either positive or negative, and in the case of one wavelength, as assumed in the theoretical derivation in Sect. 2, this error can be directly derived from this figure.

As the τ_0 depends on the cross section, it depends on the wavelength, pressure, temperature and H_2O concentration. The spectra measured by CO2M in the future and by current satellite instruments, such as OCO-2 and OCO-3, are not only at one wavelength but rather at hundreds to thousands of wavelengths, which are used to derive information on XCO2. At each of these wavelengths, the τ_0 will be different and the values shown in the figure cover the whole range of τ_0 values estimated from the simulated spectra used in this study. This would make the effective surface altitude dependent on each single wavelength, where we do not have surface reflectance data. In addition, it is not clear which value of τ_0 is representative for the whole spectral window. To address this issue, we would have to know which wavelength has the highest weight in the retrieval, but in advance of applying the retrieval. In addition, the τ_0 value depends on the CO2 concentration in the atmosphere itself, which is the main quantity to be retrieved by the retrieval and is not known in advance of the retrieval. As a result, we cannot assess the range of validity of the first-order Taylor expansion using this figure. At each wavelength, the error in z due to the first-order approximation will be different in the order of magnitude and even in the sign. One could consider the strictest limit, denoted by the dashed vertical line at surface roughness of 55 m related to the Copernicus DEM accuracy of 4 m, but our results demonstrate that the methodology is applicable to a far larger range of surface roughness values (see Figs. 17 and 18). This is not a satisfying conclusion, of course. If the reviewer has further thoughts on how this τ_0, which is representative for the whole wavelength band, can be estimated, we would be happy to test this. We would like to highlight that due to this specific issue, we increased the complexity of the experiments in Sect. 4.1 step by step, to test how far this one-wavelength equation is applicable in the multi-wavelength retrieval algorithm. Consequently, we added the following sentence to its introduction in Sect. 4 (line 310 in the preprint):

"The complexity of these experiment is increased incrementally to test whether the one-wavelength model of Sect. 2 is applicable to the actual multi-wavelength retrieval algorithm."

Although biases on the order of 2.5 ppm remain for the Mont Blanc region, it can be seen that the differences remain a factor of 4 smaller compared to the unweighted average. This implies that using this new method will also improve the results under such conditions. We consider that this high-altitude mountain region pushes the retrieval and the methodology to its limits (which was the intention) and that these data will most probably be filtered out during the retrieval process using real data in the end, as a result of other terrain- and surface type-related retrieval errors.

As this discussion is not yet included in the manuscript, we replaced the sentence in line 195 by the following paragraph:
"The question remains under which conditions this first-order Taylor approximation is valid. However, an evaluation using Eq. (11) and (23) will not help in the real application because the derivation is based on a one-wavelength retrieval while real retrievals use many wavelengths of different

wavelength bands to retrieve the greenhouse gas concentration. We analyzed the parameter τ_0 for all wavelengths used in the retrieval for CO2M, see Sect. 3.3, and found that it covers four orders of magnitude ranging from values between 0.008 and 25.6. This leads to errors due to this first-order approximation also differing by several orders of magnitude (not shown). As it cannot be evaluated in advance of the retrieval which will be the dominant error cause, this derivation is a demonstration that the albedo-weighted average of the surface altitude is a direct result of the assumptions made in this section, but its value for applications in actual retrievals is yet to be shown. This is why we start with examples with well-defined conditions increasing the complexity of the underlying data and finish with applications using real subpixel albedo and altitude data in the first part of the results section (Sect. 4)."

In addition, we replaced the sentences in lines 438 – 440 by the following sentence to emphasize the problem with the Mont Blanc region:
"In addition, it was estimated that larger surface roughness will lead to larger errors when applying the first-order approximation using Eq. (23). Therefore, we expect high mountain regions, such as around the Mont Blanc, the highest mountain of the European Alps, to show remaining biases even after applying the albedo-weighted average of the surface altitude. This region is chosen to assess how large this effect is when pushing the methodology and the retrieval towards the edge of their applicability."

3. The paper discusses scattering using a small number of prescribed scattering scenarios. This is useful, but the range appears limited. In the appendix, the authors test no scattering and two scattering cases with different optical thickness and scattering-layer pressure. The resulting correlations suggest that the empirical correction remains broadly useful, but one of the scattering cases has much lower explanatory power than the others.

I suggest the authors avoid statements implying that the proposed correction is valid over a wide range of scattering conditions unless a broader sensitivity analysis is performed. A more complete test would vary aerosol optical depth, scattering-layer height, Ångström exponent, surface albedo level, solar zenith angle, and viewing geometry. At minimum, the authors should explicitly state that the scattering sensitivity is limited to the few prescribed scenarios tested in this study.

Response: We thank the reviewer for this comment and agree that tests of three scattering configurations might not be enough for this conclusion. It is not feasible, though, to do the complete analysis as suggested by the reviewer, as it requires computation of the database of spectra (computation time 10 hours) and of the 3 million CO2M spatial samples (~1 day incl. some parallelization) for every single scattering configuration. To test the range in all parameters mentioned by the reviewer would require 10^6 different setups. We therefore follow the reviewer's last suggestion and remove the first concluding sentence in line 715 and replace the paragraph in this section by:

"To summarize this section, Equation (30) is also applicable to cases without scattering in the atmosphere, but not if the scattering optical thickness is increased to τ_{sca} = 0.037. A more comprehensive study varying all scattering parameters would be needed to confirm that the equation is also applicable for a wide range of scattering regimes."

4. The manuscript presents several real-world examples with useful maps and qualitative discussion. A summary table would greatly improve readability and allow readers to compare the performance of the unweighted, albedo-weighted, and post-processed approaches.

Adding a table for each test region is recommended, which can include the number of valid spatial samples, mean bias, median bias, standard deviation, RMSE, etc. This table should include at least Bełchatów, the Black Forest, Mont Blanc, and Germany.

Such a table would also clarify where the method works very well and where it still leaves large residual errors.

Response: Following the suggestion by the reviewer, we extended Table 3 in the manuscript to show the values for all regions and the whole dataset as a summary. We note that the predicted ΔXCO_2 needed for the root-mean-square error is the constant value of 400 ppm, which means that the standard deviation and the RMSE are identical in this case. Therefore, we show only the number of spatial samples (all assumed to be valid, cf. also our explanation to point 2 of the reviewer), the mean and median bias, and the standard deviation for all regions and methods.

We replaced Table 3 in the manuscript by this new table and added the following paragraph describing the results after line 597:

"In addition, Table 3 shows statistics for all regions analyzed in this study. The median and mean biases are reduced for all regions, except for the Mont Blanc, where the mean bias increases from -0.08 ppm (unweighted) to 0.29 ppm (albedo-weighted) for the reasons discussed in the previous sections. The standard deviations are reduced for the albedo-weighted and post-processing method compared to using the unweighted average. The standard deviations using the post-processing are slightly larger than using an albedo-weighted average directly in the retrieval."

Minor comments

1. The abstract should explicitly state that the study is based on self-consistent simulations, not on real CO2M observations or independent satellite retrieval validation.
Response: We already mention in line 11, that we "create a variety of self-consistent experiments to test this theory". In order to avoid any confusion, we added "in these self-consistent experiments" to the last sentence in the abstract.

2. Line 75: The phrase "this examples shows" should be corrected to "this example shows".
Response: Corrected.

3. Line 81: The phrase "the first of three satellite" should be corrected to "the first of three satellites".
Response: Corrected.

4. In Eq. (13), the summation index appears to be written as (i=a). Please check whether this should be (i=1).
Response: Corrected to $i = 1$.