Response to the Reviewers comments for manuscript egusphere-2025-3263

The authors would like to thank the two reviewers for the time spent reviewing our work and for their valuable suggestions. We have attentively addressed all the raised issues and have produced a revised version of the manuscript. Please find below our answers (shown in blue color) to each of the reviewers' comments . The line numbers indicated in our response refer to the "track changes" version of the new version of our manuscript.

Reviewer 1

The authors present simulation results of the radiance reflected at TOA using the spherical Monte Carlo code SMART-G, and explore the bias of using the plane-parallel assumption instead of the spherical shell assumption. They also explore the dependence of this bias on various parameters such as viewing and illumination geometry, surface albedo, wavelength, and aerosol (layer) type.

Below I state my three major concerns which should be addressed before publication:

Reviewer Comment 1.1 — In the abstract, Table 9, and the conclusion, the authors conclude a weak dependence of the above-mentioned bias on wavelength and insignificant dependencies on the surface albedo and aerosol properties. However, those effects are explored at a VZA of 45 degrees only, while most significant sphericity effects may be expected at the largest VZAs (which the authors also show for the default settings in their Fig. 1).

This is problematic for the manuscript, because the authors claim novelty of their work based on the large viewing zenith angle range (relevant for geostationary satellites as compared to polar orbiting satellites). The conclusion of the bias dependencies thus may be subjective to the choice of VZA. The editor already suggested the VZA of 45 degrees, but I would like to ask the authors to include simulations at a VZA of 60 degrees (representative for Mid Europe, see e.g. Fig. 1 of Masiello et al. 2015, https://amt.copernicus.org/articles/8/2981/2015/amt-8-2981-2015.pdf), which may possibly change the conclusions of the paper.

Reply: Thank you for this insightful comment, which ties in with comment 1.19 from the first reviewer and comment 2.1 from the second reviewer. Here we make a joint response addressing these three remarks. Initially, the impact of high VZA was only investigated in the first experiment described in Section 4.1.1 because we wanted to investigate the sphericity effects caused by high SZA and those happening at high VZA values separately. For the other experiments, we therefore chose to focus on a varying SZA (with fixed VZA) to investigate the limitations of PPA during the day, from sunrise to sunset, for a given location on the geostationary grid. However, we acknowledge the interest of considering other higher VZA configurations to better explore the dependence of sphericity effects on radiative parameters.

Reviewer 1 mentions our conclusions regarding the insignificant dependencies of surface albedo and aerosol properties on sphericity effects, while Reviewer 2 draws attention specifically to the atmospheric composition experiment. We have now run additional experiments with higher VZA as it was asked by both reviewers. In order to limit the number of additional figures, we decided to focus on the parameters for which we concluded on their non-dependency to sphericity effects (i.e., surface albedo, atmospheric composition and AOD). Additional figures (Fig. A to Fig. I) can be found at the end of this document. In particular, Figures A, B and C show the simulated TOA reflectances and the relative error induced by the sphericity effects depending on surface albedo respectively for VZA=60°, 70° and 80°. Similarly, Figures G, H and I explore the dependence on AOD respectively for VZA=60°, 70° and 80°, while Figures D, E and F focus on the dependence on the atmospheric profile respectively for VZA=60°, 70° and 80°.

Figures A, G and D, varying respectively the surface albedo, AOD and atmospheric profile for a VZA = 60°, resemble the ones presented in the manuscript at VZA = 45° (Figures 5, 7 and 11) and therefore lead to similar conclusions as in the original manuscript. However, Fig. B and Fig. C, obtained for VZA = 70° and VZA = 80° respectively, show a dependence on surface albedo of the relative error caused by the use of the PPA, even for moderate SZA values (<70°). A similar conclusion can be drawn from Fig. I, which shows that for VZA=80°, even for moderate SZA values, the relative error induced by the sphericity effects depends on AOD, while Fig. H, with VZA=70°, only shows a weak dependence of the relative error on AOD. Regarding the atmospheric profiles, only Fig. F, obtained for a VZA = 80°, shows a weak dependence of the relative error induced by the sphericity effects to the choice of atmospheric profile, whereas for VZA = 70° (Fig. E) this dependence is not observed and therefore considered to be negligible.

The following actions were taken to report these additional results in the new version of the manuscript. First, we included the figures corresponding to VZA = 80° for the experiments for which we concluded to a non-dependence of the sphericity effects in the initial version of the manuscript (Fig. C, F and I of this response are included as Fig. 6, 8 and 12 in the manuscript). This allows us to clarify our point, which now reads that a varying surface albedo, atmospheric profile or aerosol layer does not significantly affect the error caused by the use of PPA for moderate VZA values. Also, in the new version of the manuscript, we say that the choice of these parameters will affect the sphericity effects for high VZA values such as 80° (or 70° for surface albedo). All these precisions were added in the manuscript (see lines 389-393, 420-422, 490-492) in addition to the new figures mentioned above. Section 3.3 presenting our experimental setup was modified accordingly (see Table 2 and lines 265-266). Furthermore, Table 10 was modified according to the new conclusions. We also updated the "Discussion" section by explaining how the characterization of the sphericity effects made in this study allowed us to decide that our future correction method will only depend on wavelength and geometry because (i) they are the only parameters presenting a dependence throughout the entire day (even for moderate SZA) and (ii) they are easier to account for (see lines 514-515, 517-518, 521 and 528-531).

Reviewer Comment 1.2 — Another claimed novelty by the authors of their sphericity analysis is the inclusion of near-infrared (NIR) wavelengths where gaseous absorption plays a role. In Sect. 4.2, they attribute the relatively large errors at the longer wavelengths for SZA > 80 degrees to gaseous absorption, however, no evidence is provided. Could those errors possibly also be related to the wavelength dependence of the Rayleigh scattering efficiency in combination with sphericity effects at large SZA? I would like to ask the authors to include simulation results at a long wavelength, but without gaseous absorption, to prove that gaseous absorption is indeed responsible. This would improve the physical understanding of the simulation results.

Reply: Please find Fig. J, which features the same simulation results as the ones presented in Sect. 4.2 (and Fig. 4) of the manuscript, but after removing all absorbing processes in the atmosphere. One can see that removing gas absorption diminishes the relative error induced by the sphericity effects in all channels. However, this reduction is particularly important in longer wavelength channels NIR 2.2 and NIR 1.6, where the relative error stays very close to zero for all SZA values. These results suggest that the larger errors observed for these channels in Section 4.2 and Fig. 4 are indeed caused by gas absorption.

This new figure was included in the manuscript in Appendix C as Fig. C1, in addition to the argumentation above (see lines 622-633). A reference to Appendix C has been added in Sect. 4.2 for readers seeking further understanding (see line 363).

Reviewer Comment 1.3 — In Sect. 2.3., an explanation is provided for the cause of the bias of the PPA results compared to those of the SSA, for large SZA, and for large VZA. The authors state that (1) "high SZA causes an overestimation of the optical path in PPA, leading to an overestimation of the attenuation of the solar beam in the atmosphere, causing the PPA to induce a negative bias in TOA radiance estimation" and (2) "high VZA leads to an overestimation of the illuminated volume and Rayleigh scattering, creating a positive bias induced by the PPA in the simulated TOA radiances". However, from Adams and Kattawar (1978), I understand that in a spherical atmosphere (1) with increasing SZA, less photons reach the (black) surface, increasing the reflected radiance w.r.t. the PPA (indeed, in the twilight zone, at a SZA of 90 degrees and slightly larger, there is still signal due to horizontal scattering) and (2) with increasing VZA, the optical paths through the atmosphere along the line of sight from the detector are shorter (for VZA defined at TOA, with an optical path approaching 0 for VZA= 90 deg), yielding smaller radiances for SSA w.r.t. PPA. Although the signs of the biases are correct here, I would like the authors to reconsider the explanation of the cause of those biases, because it is key for the physical understanding of the simulations results in this paper. Including a sketch could be helpful. In addition, please cite the relevant literature here where those insights were introduced, for the readers seeking further explanation.

Reply: The explanation of the cause of the biases for large SZA and VZA has been enhanced and additional arguments (e.g. from the geometry perspective) have been added to improve the physical understanding as suggested (see lines 154-176 for more details).

Other comments:

Reviewer Comment 1.4 — The authors state that pseudo-spherical approximations are not feasible in operational algorithms, however, I think many satellite algorithms use the pseudo-spherical approximation. Do the authors have an example of an algorithm that cannot use the pseudo-spherical approximation due to computational constraints. Could the authors include the remaining error of the pseudo-spherical approximation? This would greatly improve the relevance of the paper.

Reply: While we are aware of the existence of pseudo-spherical approximations (see our literature overview in Section 2.2), these solutions are unfortunately not foreseen in the EUMETSAT's LSA-SAF operational applications (eg. Juncu et al., 2022) because their generally higher computational cost goes against the highly demanding near-real-time constraints (see lines 63-65 and 148-149). This is especially the case in applications using geostationary data (with tens of millions of pixels to process in less than 10 minutes in the case of FCI) in which any increase in the processing time of the in-line radiative transfer calculations made for data inversion can become prohibitive. This is why PPA-based radiative transfer codes, with significantly faster performances, are still used in several near-real-time geostationary operational retrieval algorithms other than those used in the LSA-SAF (eg. Zawadzka and Markowicz, 2014; NWC SAF cloud product processors ATBD 2021; CM SAF cloud physical products

ATBD 2022; Ceamanos et al., 2023), but also for polar orbiting satellite observation analysis (eg. Mason et al., 2023). We therefore believe that our study can be of interest for all these applications.

Some pseudo-spherical approaches consist in fast PPA calculations of some radiative terms, while other terms (such as Rayleigh scattering or single scattering processes) are computed in a more accurate spherical mode (eg. Momoi et al., 2024,). Again, this is not compatible with the near-real-time constraints of some applications as ours. Hence, this study was conducted to have a precise knowledge of the radiative dependencies of the sphericity effects, which will eventually allow us to build a method to compensate for the PPA-induced errors in our surface retrieval algorithm.

For all these reasons, and while we keep alert on the development of faster pseudo-spherical approximations, the use of these methods is out of the scope of our current work as explained lines 63-65 and 562-565. Hence, we decided against including the remaining error of such approximation in our paper as suggested by the reviewer, although we agree on the significance of such results for other studies.

Reviewer Comment 1.5 — Figure 1: Why are two satellites and two suns shown? This may be confusing, because also two systems are shown: PPA and SSA. I suggest removing one sun and one satellite in the sketch. I also suggest adding multiple layers to the sketch, as they are mentioned in the first sentence of Sect. 2.2.

Reply: We have removed the sun and sensor corresponding to low SZA and VZA from Fig. 1 in the manuscript. Although we understand your suggestion, we chose not to add multiple layers to the sketch as we believe that it would become too much complex and therefore more difficult to understand.

Reviewer Comment 1.6 — Line 150: "Rayleigh scattering is the main radiance-inducing atmospheric process in the short visible spectrum, corresponding to the first measuring channels of most geostationary satellite images. Therefore, the difference between the PPA and SSA geometries will primarily affect Rayleigh scattering." This is poor logical reasoning: if phenomenon A is dominant, it is not guaranteed that phenomenon B primarily affects A. Please rephrase.

Reply: Thank you for this insightful comment, which highlights some inaccuracies in the explanation of our reasoning. The reason we expect Rayleigh scattering to be strongly affected by the sphericity effects in shortwave observations is that the difference between a plane-parallel assumption and a spherical shell assumption is the length of the optical path traveled by the radiation. This passage was modified accordingly in the manuscript (lines 154-157).

Reviewer Comment 1.7 — Line 207: Please explain more clearly in one sentence what REPTRAN does.

Reply: Additional details were added in lines 227-231.

Reviewer Comment 1.8 — Line 212: "Since absorption cannot be neglected in the considered long visible and near-infrared channels." Please state the relevant gases here (probably H2O). Can absorption be neglected at the shorter VIS channels? What about O3?

Reply: The shortest channel available on FCI, VIS 0.4, is indeed almost not impacted by gas absorption since its spectral response does not encompass any important atmospheric gas absorption band. However, other spectral channels corresponding to longer wavelengths encounter non-negligible absorbing bands, as one can see in Table 1 of our manuscript. (for more details, see Fig. 5 and 6 from Gasteiger et al., 2014). The information of the absorbing gases in each considered FCI channel is now available in Table 1 in Sect. 3.3 of our manuscript.

Reviewer Comment 1.9 — Sect 3.3: Please include the wavelength ranges of the channels, and the indicate per channel which absorbing gases play a role, because this information is relevant to interpret the simulation results. A table could be helpful.

Reply: This information has been included in a new table in the manuscript (see Table 1 in Sect. 3.3).

Reviewer Comment 1.10 — Line 257: "One should note the negative sign before SZA values in Fig .2 has no physical nor mathematical meaning in this manuscript..." Please remove the minus signs in all figures and add 'RAA = 0 deg' and 'RAA = 180 deg' labels to the figures.

Reply: All negative signs have been removed from all SZA-axis labels in all figures. Similarly, we added the precision of the RAA value in each plot. Hence, the sentence quoted by the reviewer above has been deleted (lines 294-296).

Reviewer Comment 1.11 — Line 262: "Indeed, when VZA increases the illuminated volume grows" Do you mean "the observed volume"?

Reply: We are indeed referring to the "illuminated volume observed by the satellite sensor". This has been clarified in the text (line 299).

Reviewer Comment 1.12 — Line 265: "... PPA leads to an overestimation of the solar beam attenuation in Rayleigh dominated wavelengths." Please reconsider this explanation.

Reply: This explanation refers to the explanation given in Sect. 2.3, which has been clarified and extended in the new manuscript (lines 154-176).

Reviewer Comment 1.13 — Line 269: "the relative error (in absolute values)" may be confusing. Please explain or remove '(in absolute values)'.

Reply: This has been modified in the manuscript to "the absolute relative error" (line 309).

Reviewer Comment 1.14 — Sect. 4.2: Please mention what absorbing gases are relevant at the considered wavelengths. Please note that gaseous absorption (by O3) also occurs at shorter (VIS) wavelengths, at high altitudes, while H2O mainly absorbs at lower altitudes. Does the altitude dependence of the gas abundances affect the sphericity impact (since at large SZA and VZA, the reflected radiances is more sensitive to higher atmospheric layers)?

Reply: When looking at Fig. 4 in the manuscript, channels VIS 0.5 and VIS 0.6 show the highest PPA-induced relative errors for high SZA values. Now, when looking at the additional Fig. J at the end of this document, which shows the results of the same experiment but without gas absorption, the relative errors are greatly reduced for VIS 0.5 and VIS 0.6 (eg. from over 75% with gas absorption to less than 45% without gas absorption, for VIS 0.6 at SZA=89° and VZA=45°). However, if we look at VIS 0.8, the error reduction is smaller (from 25% with gas absorption to a bit less than 23% without gas absorption, at SZA=89° and VZA=45°). Knowing that O3 is the main absorbing gas in VIS 0.6 and VIS 0.5, while VIS 0.8 is mainly affected by H2O absorption (see Table 1 in the manuscript), it seems that spectral channels affected by absorption at higher altitudes are more impacted by the sphericity effects than channels concerned by low-altitude absorbing gases. However, this is only an assumption, and additional tests would be necessary to confirm this. We believe that this point is not central to our study, and therefore we have decided not to perform this investigation in the frame of the current manuscript.

Reviewer Comment 1.15 — Figure 3c could be considered redundant and can be removed, because is the combination of Figs. 3a and 3b. The combination is also covered in the main text.

Reply: We disagree about the redundancy of Figure 3c and decided to keep it because it is key to understand where the number of "36% of FCI observations over a whole year are affected by sphericity effects" comes from (line 338). Please consider that this key number is discussed in the abstract and the conclusions of the manuscript.

Reviewer Comment 1.16 — Sect. 4.4: The authors explore the effect of different atmospheric profiles but also mention that 'fluctuations in Rayleigh optical depth between atmospheric profiles are below 1%'. How do the input profiles differ? Please explain and if relevant, include a figure of the profiles.

Reply: The selected AFGL atmospheric profiles feature different altitude-temperature and altitude-mixing ratios for several molecules such as H2O, O3 or CH4. This precision has been added in the manuscript (line 403), and a reference to informative plots of some characteristics of these profiles is now given in lines 409-411 in case a reader wishes to obtain more information.

Reviewer Comment 1.17 — Editing suggestions:

Title: '... geostationary satellites observations... ' → '... geostationary satellite observations ...', remove the s.

line 1: '... allow a continuous ...' → '... allow continuous'

line 2: 'performances'→ 'performance'

line 15: 'measuring wavelength' → 'measurement wavelength'

line 59: 'well known' → 'well-known'

line 60: Please use a more logical start of a new paragraph, e.g.: 'An example of an algorithm that uses the PPA is... '

line 67: '... in the specific of geostationary sensors, which for example provide ...' →
'... in the application to geostationary sensors, which provide ...'

Line 84: 'The Earth consists in a near-spherical 3-D system,' Change 'consists in' to 'consists of' or rephrase.

Line 91; 'a scattered radiation being scattered' → 'radiation being scattered'

Line 94: 'identifying the invariances', I suggest rephrasing and use another work than 'invariances'.

Line 99: 'all the atmospheric layers as infinite parallel planets' → 'all the atmospheric layer boundaries at infinite parallel horizontal planes'

Line 101: Do you mean viewing or solar zenith and azimuth angles here? Please specify.

Line 234: 'consists in' → 'consists of' or rephrase

Line 249: 'simulations results' → 'simulation results'
Line 340: 'which makes sense considering' → 'because'
Line 390: 'does corresponds to' → 'corresponds to'

Reply: All these suggestions have been considered and necessary corrections have been made to the manuscript.

Additional comment:

Reviewer Comment 1.18 — Line 344: "... suggesting that the Earth's sphericity effects may slightly depend on surface albedo.".

The simulation in Sect. 4.3 were done for the VIS 0.6 channel.

At shorter wavelengths, the surface reflection can enhance the molecular scattering, via repeated reflections between atmosphere and surface.

What would be the Earth's sphericity impact dependence on surface albedo in the VIS 0.4 channel?

Reply:

Please find in this response document Fig. K, which features the same simulation results than Section 4.3 (Fig. 5) but for channel VIS 0.4. One can see that varying surface albedo does not have a significant impact on the relative error induced by the sphericity effects. Although surface reflection can indeed enhance molecular scattering, the surface signal is too weak in the blue wavelength for the albedo to have a notable impact.

Last comment:

Reviewer Comment 1.19 — Besides VZA = 60 degrees relevant for Europe, please also explore the sphericity effect dependencies on scene parameters at VZA = 80 degrees, which is an exploration of the most extreme geometries encountered for geostationary satellites (see also you Fig. 3).

Reply: Please see the joint response for comments 1.1, 1.19 and 2.1 in our first response of this document.

I am curious to see those results. Good luck with the revision!

Reply: Thank you again for your insightful comments that helped us improve the quality of this paper!

Reviewer 2

This manuscript explores the impacts of using the plane parallel geometry on radiative transfer simulations with solar and viewing angles relevant for geostationary satellites observing at visible and short infrared wavelengths. Radiative transfer simulations using a Monte Carlo algorithm are compared, using the plane parallel assumption or the spherical shell assumption. The impact of the different geometries, wavelengths, surface albedo and atmospheric components is analyzed.

This work is very interesting and such an analysis is important for the correct understanding and use of the new instrument data, such as FCI and more recently IRS (in the thermal IR range, not considered here).

However, I have some major comments to be addressed before final publication.

General comments

Reviewer Comment 2.1 — 1) Except for Experiment 1, all others were run at VZA of 45°. However, experiment 1 clearly showed that VZA impacts the PPA for high angles, and considering those high angles is the main specificity of this work. Therefore I think that all experiments should be run also for high VZA values, and I would expect all parameters to have a different impact on the PPA correction for such high angles as they do for high SZA, and maybe the final conclusions will be different. This is a very important point, as currently the final conclusion state that the correction should depend only on angles and wavelength, while maybe the atmospheric composition also plays a non-negligible role for high VZA.

Reply: Please find at the beginning of this document our joint response for reviewer 1's comments 1.1 and 1.19 and your comment 2.1 here.

Reviewer Comment 2.2 — 2) Please discuss what is considered a major / significant impact on the calculated radiance / reflectance / TOA intensity. The current status is inconsistent along the paper. Line 286 mentions 1% (and a very short discussion of this number is in the conclusion, but should come also much earlier), but then in the next lines and section 4.1.1 the cutoff is set at SZA 70° and VZA 70° , while looking at Table 2 the numbers do not match (e.g. SZA 70-80° lead to errors larger than 1% only when VZA is larger than 75°, which is already in the cutoff for VZA anyway). Then line 322 "significant biases" are discussed, for SZA larger than 70° in all channels, but from table 3 the error remains way below 1% in the 70-80° SZA range, for all channels. So the "significant biases" in this section relate to another cutoff and this is not explained or justified. Same again for the surface albedo section... And again in the conclusion.

Reply: Thank you for this insightful remark, which indeed highlights a confusing wording in the manuscript from our end. In fact, we chose two criteria to characterize the bias induced by the use of PPA as "significant": (i) the abrupt increase in the absolute relative error and (ii) an absolute relative error greater than 1% and (ii) .

The first criterion is quite intuitive. As we can see in all plots displayed in the "Results" section (Sect. 4), the PPA-induced relative error abruptly increases beyond a certain SZA value (usually around 60°-70°, as mentioned in line 314). This happens when the sphericity effect induced by SZA becomes predominant over the opposite-sign effect induced by VZA, as explained in lines (311-319). Since the relative error increases sharply from this cut-off angle on, we consider that a compensation for the sphericity effects should start at this threshold value, especially if we want to avoid artificial diurnal variations of our future AOD/surface reflectance retrievals.

The second criterion is more quantitative, and is justified by the precision required to enable accurate AOD retrieval (i.e., the first step to surface reflectance inversion) even in unfavorable conditions when aerosol sensitivity is low. A threshold value of 1% error is often used in the radiative transfer community to determine when simulation biases become significant. This is the case of the REPTRAN method (Gasteiger et al., 2014), for example, which determines the number of representative wavelengths to model the absorption of a given instrument channel as the minimum number of monochromatic simulations (each one at a different "representative" wavelength) that makes the simulation error lower than 1% with respect to reference line-by-line simulations.

To clarify this point before discussing the results in Sect. 4, we added the definition of these two criteria in a new Sect. 3.4 entitled "Definition of PPA-induced bias". Afterwards, the analysis of each result presented in Section 4 has been also modified accordingly, as well as the discussion in Sect. 5.

Reviewer Comment 2.3 — 3) The presentation of the concept of correction for sphericity effects must be changed. For the moment, it is always (mostly in introduction, discussion and and conclusion) presented as a correction to apply on the observation / data, while the correction must be done at the radiative transfer level, or after it. The observations themselves should never be corrected.

Reply: We apologize for this repeated misuse of language. Indeed, we wanted to refer to the correction (or compensation) for neglecting the sphericity effects in the PPA radiative transfer scheme. This has been corrected throughout the manuscript (lines 19-20, 81, 525-526, 530-531, 553, 557, 560-564, 576, 581).

Reviewer Comment 2.4 — 4) Section 4.5: it took me a while to realize that the aerosol properties were taken exactly as in the OPAC aerosol mixed types, which contain not only the optical properties but also the vertical profile and AOD; please provide a clearer and more complete description in the manuscript

Reply: Precisions have been added in lines 431-432.

Specific comments

Reviewer Comment 2.5 — lines 46-48 (also comes in line 54): please explain what this atmospheric correction means

Reply: This has been explained in line 49.

Reviewer Comment 2.6 — lines 66-68 and 161-163: please provide references for those earlier studies. They are given later, when this previous literature is discussed (starting line 170) but should already be listed the first times earlier studies are mentioned. Also, a quick search gave me this very recent publication https://doi.org/10.3390/atmos16080977 addressing similar issues - I know it was published after submission of this manuscript, but it would be interesting to mention it and highlight what is different in the current study (and also in the results analysis section).

Reply: As suggested, references have been added in lines 71, 73 and 180. Thank you for pointing out the publication from Gu et al. However, a direct comparison with our work is not straightforward due to differences in the objectives and methodological framework adopted in that study. For this reason, we decided against including it as a reference in our new manuscript.

Reviewer Comment 2.7 — Line 262: what means "the illuminated volume"? Do you mean the optical path length?

Reply: Not exactly, here we are referring to the "illuminated volume observed by the sensor". This precision has been added in line 299.

Reviewer Comment 2.8 — Figure 2 / section 4.1: For the sake of completeness, the non-symmetry of the SZA dependence should be explained (only once, not for each of the next figures)

Reply: This explanation has been added in lines 300-302.

Reviewer Comment 2.9 — Tables 2 and next: is the SZA range considered for both positive and "negative" SZA?

Reply: Yes, we considered both azimuthal planes in the mean error calculation. According to reviewer 1's comment 1.10, and to improve the clarity of our manuscript, we have removed the minus signs from all SZA axes and added the corresponding RAA value instead.

Reviewer Comment 2.10 — Line 313: please avoid imprecise phrasing such as "extremely close to zero" (replace by under . . . , for example)

Reply: This has been replaced by "respectively below 1% and 2%" (line 355).

Reviewer Comment 2.11 — Line 376: please be more specific about the "opposite physical properties" of the two aerosol types. It is not only important to know if particles are fine or coarse, but also their absorptive properties, maybe also the particle shape

Reply: Explanation on some relevant properties of the considered aerosol types have been added in lines 433-435.

Reviewer Comment 2.12 — Lines 382-383: please explain "the peaked phase function of such coarse particles that cause convergence issues when using the local estimate technique"

Reply: For coarse aerosols, the phase function is so peaked forward that with the local estimate method (mentioned line 245 of our manuscript), there are very rare contributions with a very strong Monte Carlo weight, which greatly increases the variance and does not allow for good statistical convergence.

This phenomenon is explained in Buras and Mayer (2011) (already cited in our manuscript) : "When using the local estimate technique (described e.g. by Marchuk et al. 1980) the strong forward peaks of the scattering phase function lead to rare events that contribute significantly to the total result, also known as "spikes". The accuracy of the result is then no longer determined by the number of simulated photons, but by the number of spikes having occurred, which can make the calculation unfeasible." Figure 4 in Buras and Mayer (2011) clearly shows the sudden change in variance due to these "spikes".

Reviewer Comment 2.13 — Figure 8 is not the right one or the legend is not correct... Legend is like Figure 7, but the plots are different.

Reply: We are sorry for this mistake, which has been corrected. Both the plots and legend are now right.

Reviewer Comment 2.14 — Line 453-4: please list the relevant wavelengths

Reply: This information has been added in line 517.

Reviewer Comment 2.15 — Table 9: Please be slightly more specific than "yes" and add "cutoff" angles / wavelengths here

Reply: Precisions have been added in Table 10 on the parameters whose variation slightly impacts the PPA-induced error. For zenith angles and wavelength there is however no cut-off value since their variation impacts the sphericity effects in all configurations.

Reviewer Comment 2.16 — Line 503: I don't really understand what is written; to my understanding, errors are in radiative transfer calculations under the PPA assumption

Reply: You are right, hence the mentioned correction should be applied to the inversion of FCI data and not to the data itself. This has been clarified (see lines 560-562).

Reviewer Comment 2.17 — Lines 508-510: those results do not apply to any geo weather satellite: there are also such instruments measuring at thermal IR wavelengths, for which another study would be needed

Reply: You are totally right, and we have therefore reformulated this conclusion to be more accurate by mentioning only visible and near-infrared imaging geostationary instruments (line 583).

Minor comments / technical / language

Reviewer Comment 2.18 — line 35: slightly rephrase to make clear what became operational in 2024 and was renamed Meteosat-12 (only FCI or the whole satellite?)

line 43: its → their ?

equations 1,3,4: maybe consider to avoid starting an equation with the minus sign? although correct, I think it is not very common to write it like that

Figure 1: e missing in plane parallel assumption in the figure; in the legend, rephrase "height of the atmosphere"

Line 192: for the estimation OF land surface parameters

Line 270: VZA>=75°

Line 332: I think 'around' should be removed

Line 344: "may slightly depend on surface albedo" is a weird phrasing, as we know there is a dependence, and the question is how large / significant it is

Line 352: surface of albedo → surface albedo

Section 4.4: Maybe you could show those different profiles? They are easy to find, but it could be nice for the reader to easily see how much they change

Line 390: correspond (without the s)

Reply: All these suggestions have been considered and necessary corrections have been made to the manuscript.

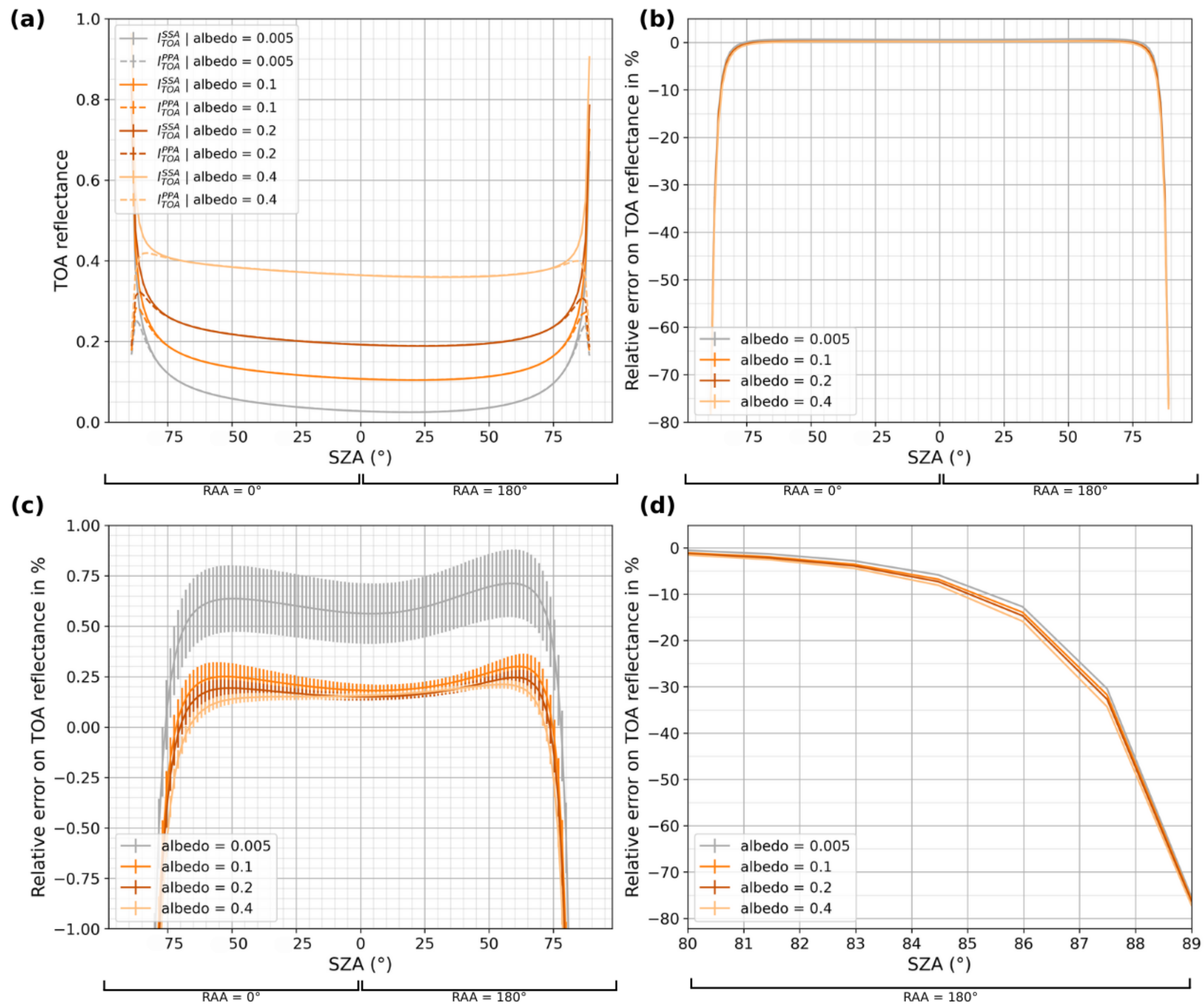

Figure A: Comparison of PPA and SSA simulations for several realistic continental surface albedo values for channel VIS 0.6 as a function of SZA, in a US standard aerosol-free atmosphere, with **VZA = 60°** and in the 0°-180° relative azimuthal plane, with 1e7 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -1 % and 1 %. (d) Same plot zoomed-in for SZA over 80°.

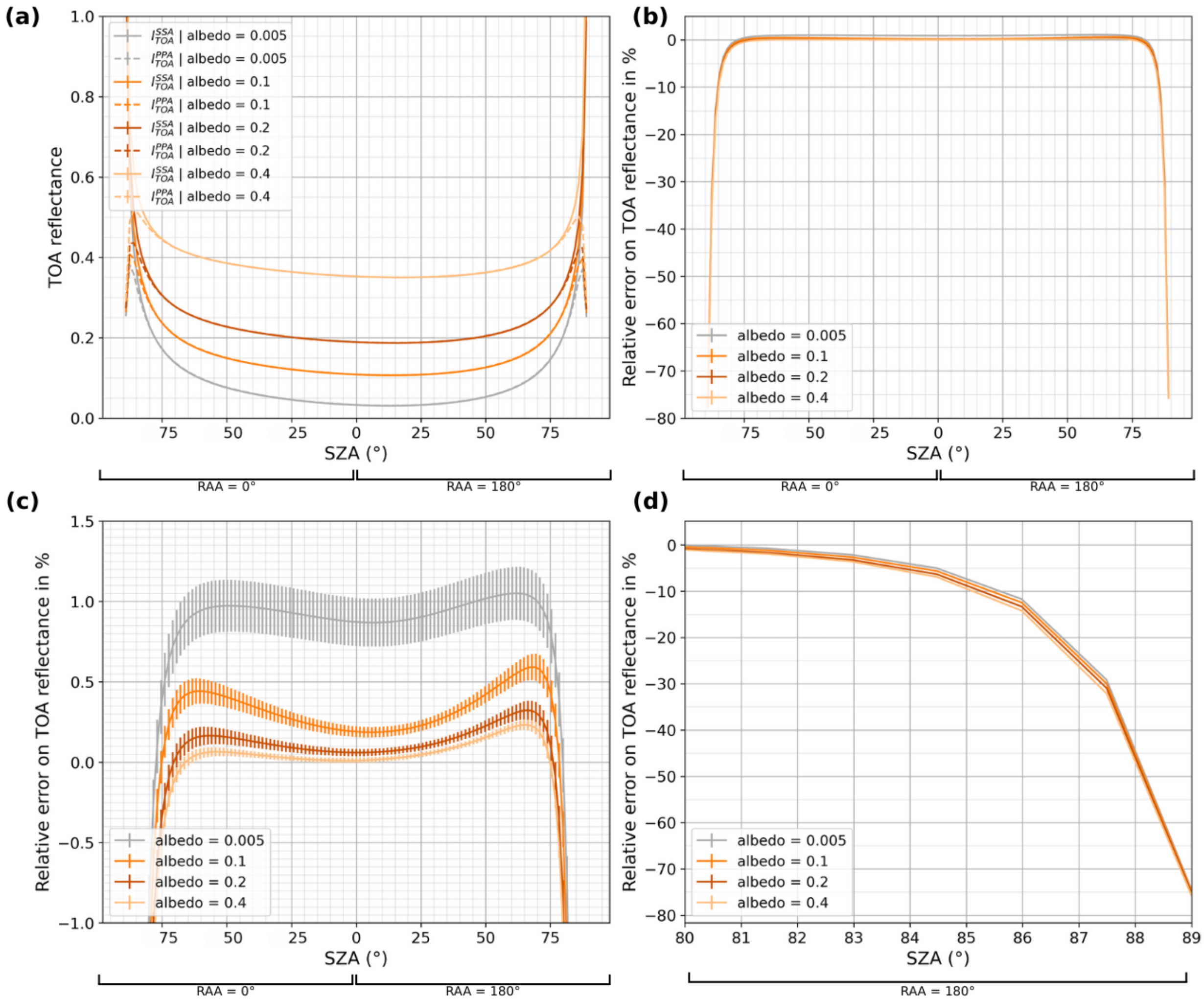

Figure B: Comparison of PPA and SSA simulations for several realistic continental surface albedo values for channel VIS 0.6 as a function of SZA, in a US standard aerosol-free atmosphere, with **VZA = 70°** and in the 0°-180° relative azimuthal plane, with 1e7 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -1 % and 1 %. (d) Same plot zoomed-in for SZA over 80°.

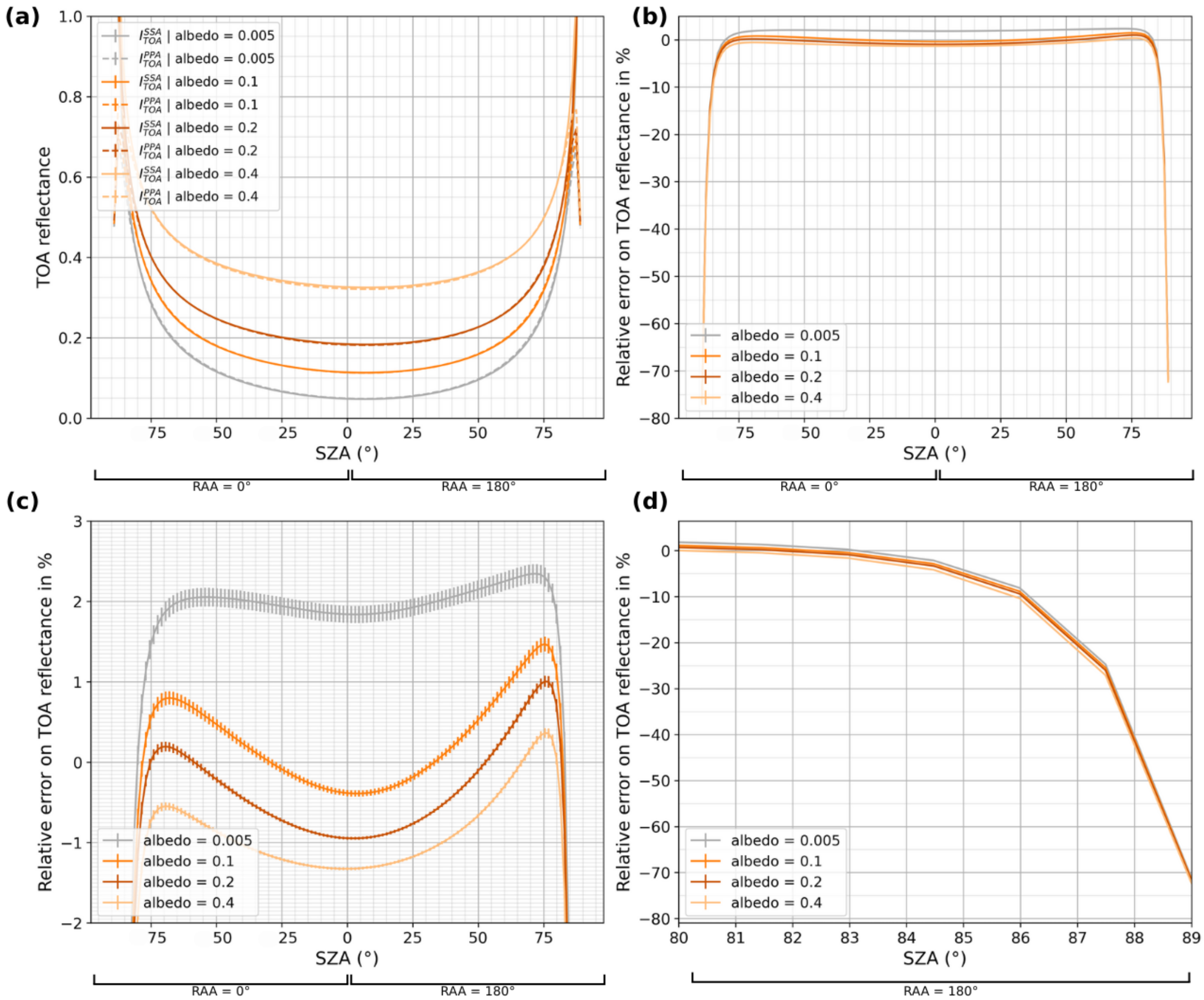

Figure C: Comparison of PPA and SSA simulations for several realistic continental surface albedo values for channel VIS 0.6 as a function of SZA, in a US standard aerosol-free atmosphere, with **VZA = 80°** and in the 0°-180° relative azimuthal plane, with 1e7 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -2 % and 3 %. (d) Same plot zoomed-in for SZA over 80°.

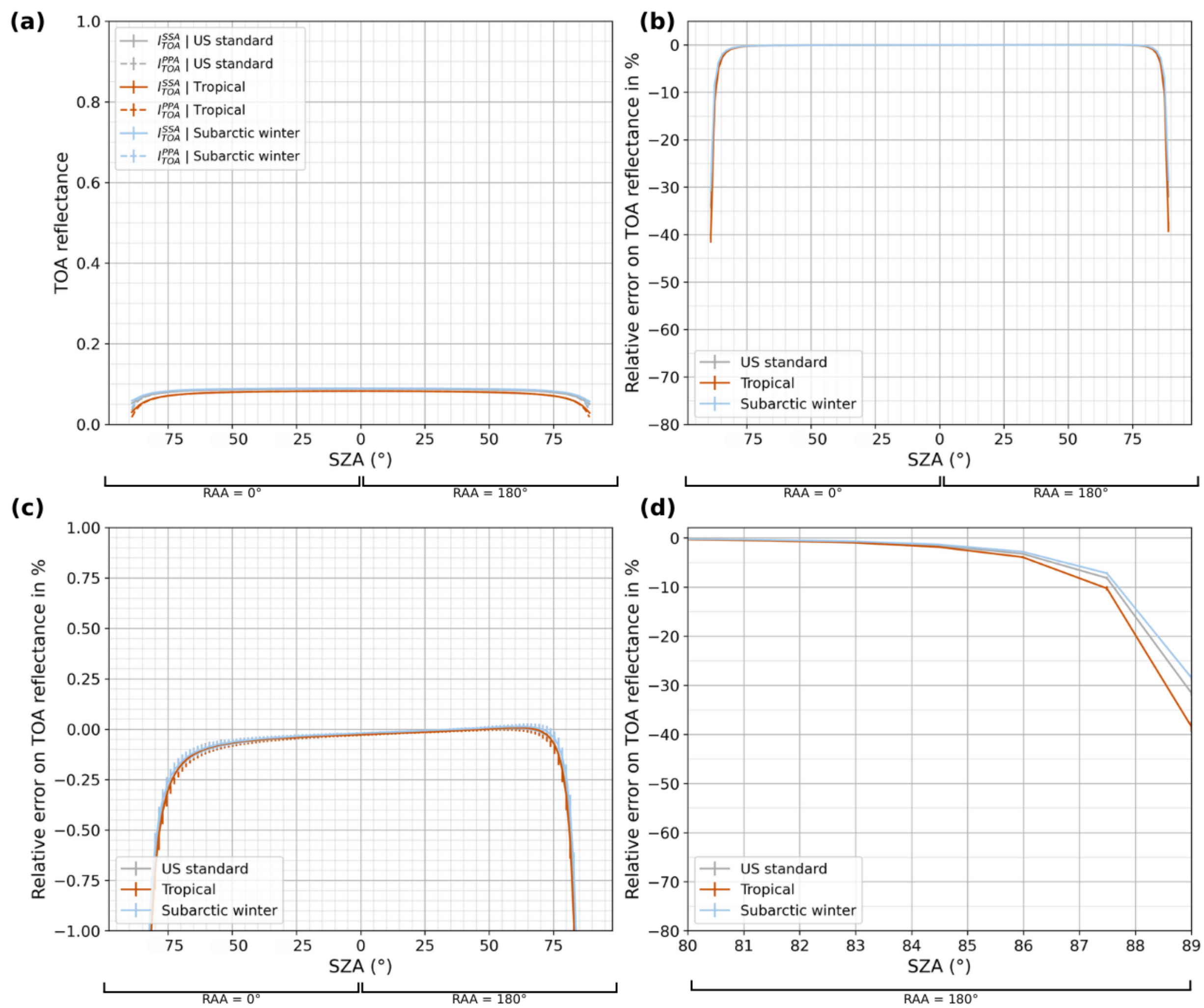

Figure D: Comparison of PPA and SSA simulations for several atmospheric profiles as a function of SZA, for channel NIR 2.2 in a aerosol-free atmosphere with a Lambertian surface of albedo 0.1, with **VZA = 60°** in the principal relative azimuthal plane, with 1e7 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -1 % and 1 %. (d) Same plot zoomed-in for SZA over 80°.

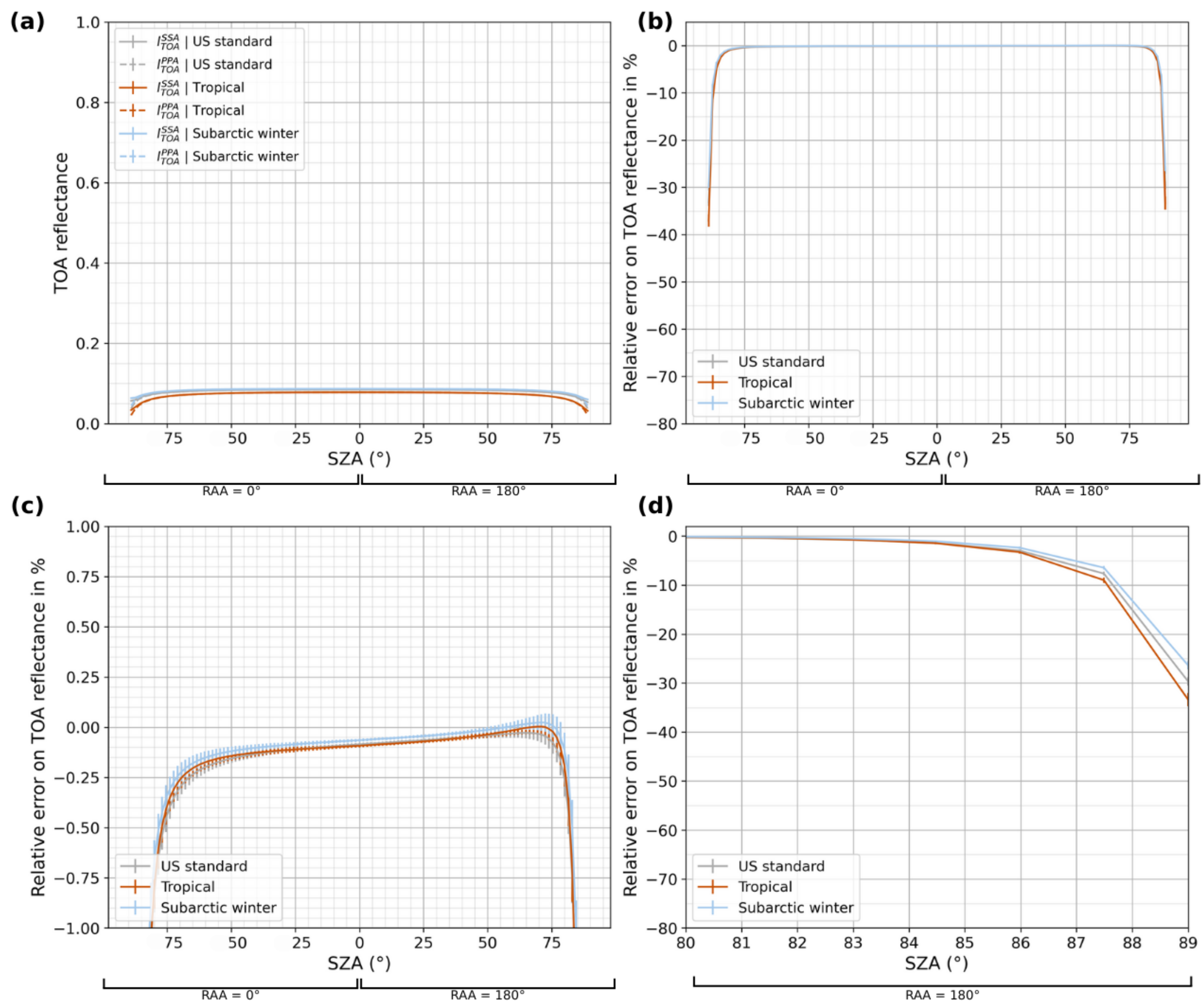

Figure E: Comparison of PPA and SSA simulations for several atmospheric profiles as a function of SZA, for channel NIR 2.2 in a aerosol-free atmosphere with a Lambertian surface of albedo 0.1, with **VZA = 70°** in the principal relative azimuthal plane, with 1e7 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -1 % and 1 %. (d) Same plot zoomed-in for SZA over 80°.

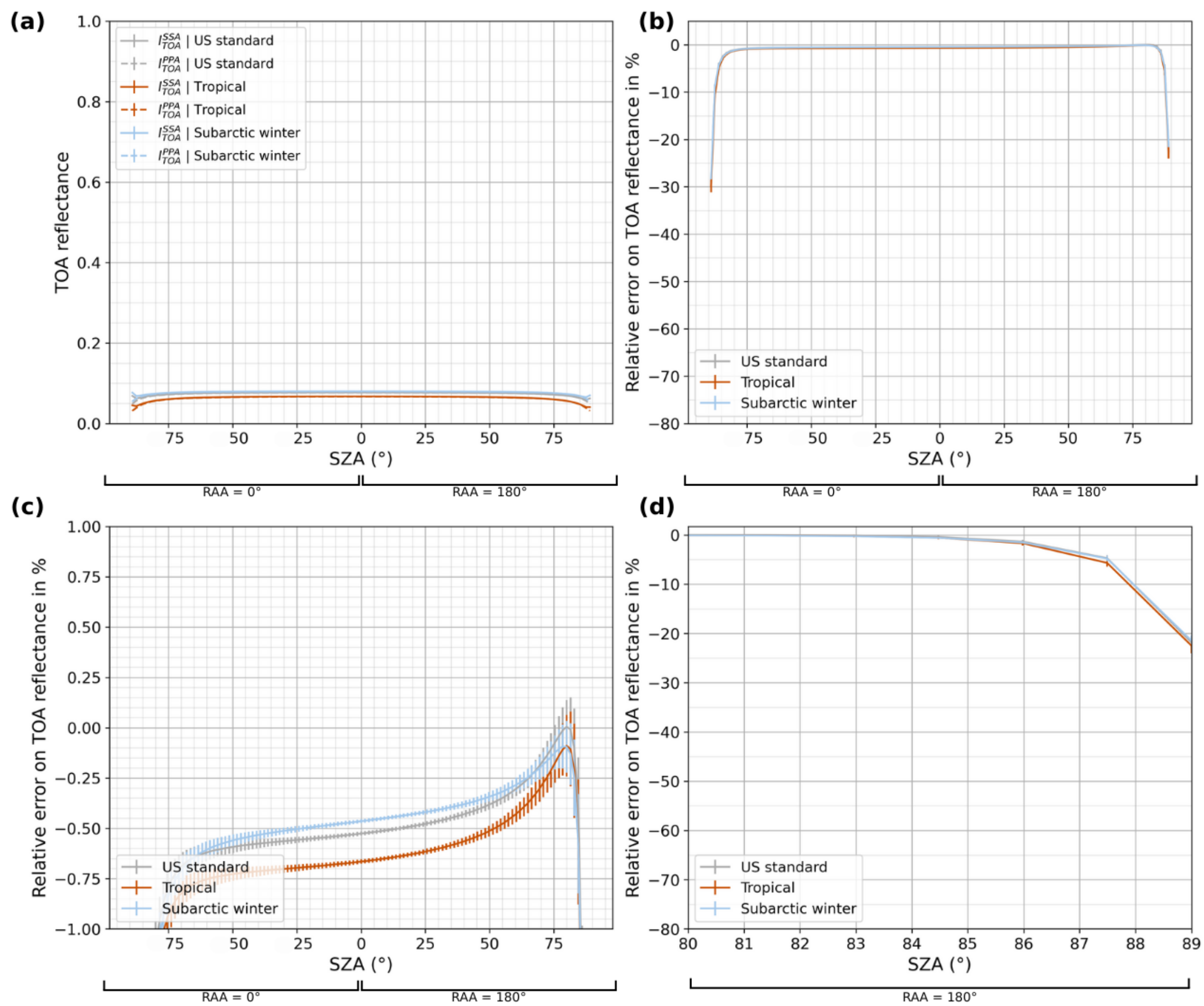

Figure F: Comparison of PPA and SSA simulations for several atmospheric profiles as a function of SZA, for channel NIR 2.2 in a aerosol-free atmosphere with a Lambertian surface of albedo 0.1, with **VZA = 80°** in the principal relative azimuthal plane, with 1e7 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -1 % and 1 %. (d) Same plot zoomed-in for SZA over 80°.

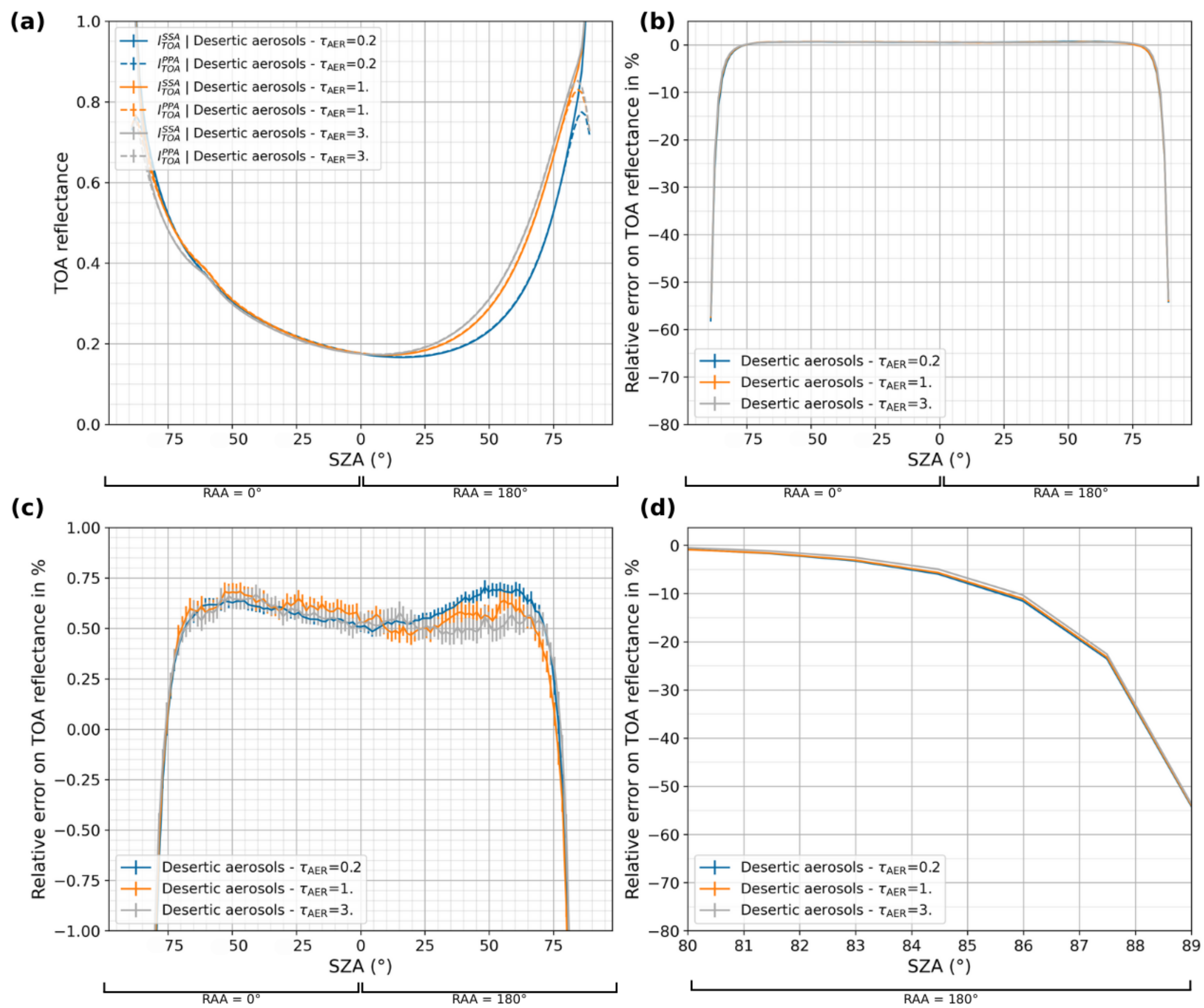

Figure G: Comparison of PPA and SSA simulations for several AOD values as a function of SZA, for channel VIS 0.4 in a US standard atmosphere with a Lambertian surface of albedo 0.1, with **VZA = 60°** in the principal relative azimuthal plane, with 1e8 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -1 % and 1 %. (d) Same plot zoomed-in for SZA over 80°.

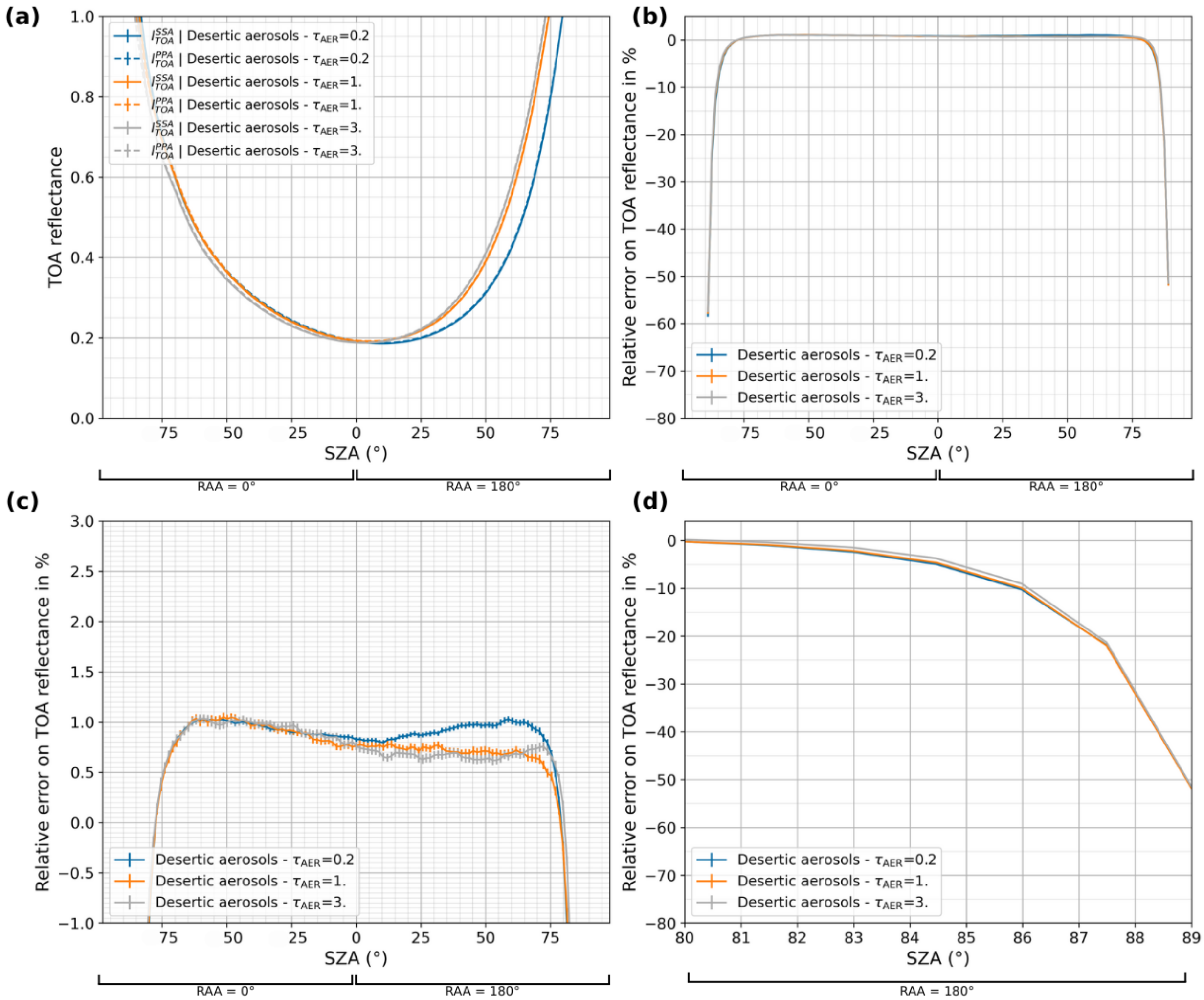

Figure H: Comparison of PPA and SSA simulations for several AOD values as a function of SZA, for channel VIS 0.4 in a US standard atmosphere with a Lambertian surface of albedo 0.1, with **VZA = 70°** in the principal relative azimuthal plane, with 1e8 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -1 % and 1 %. (d) Same plot zoomed-in for SZA over 80°.

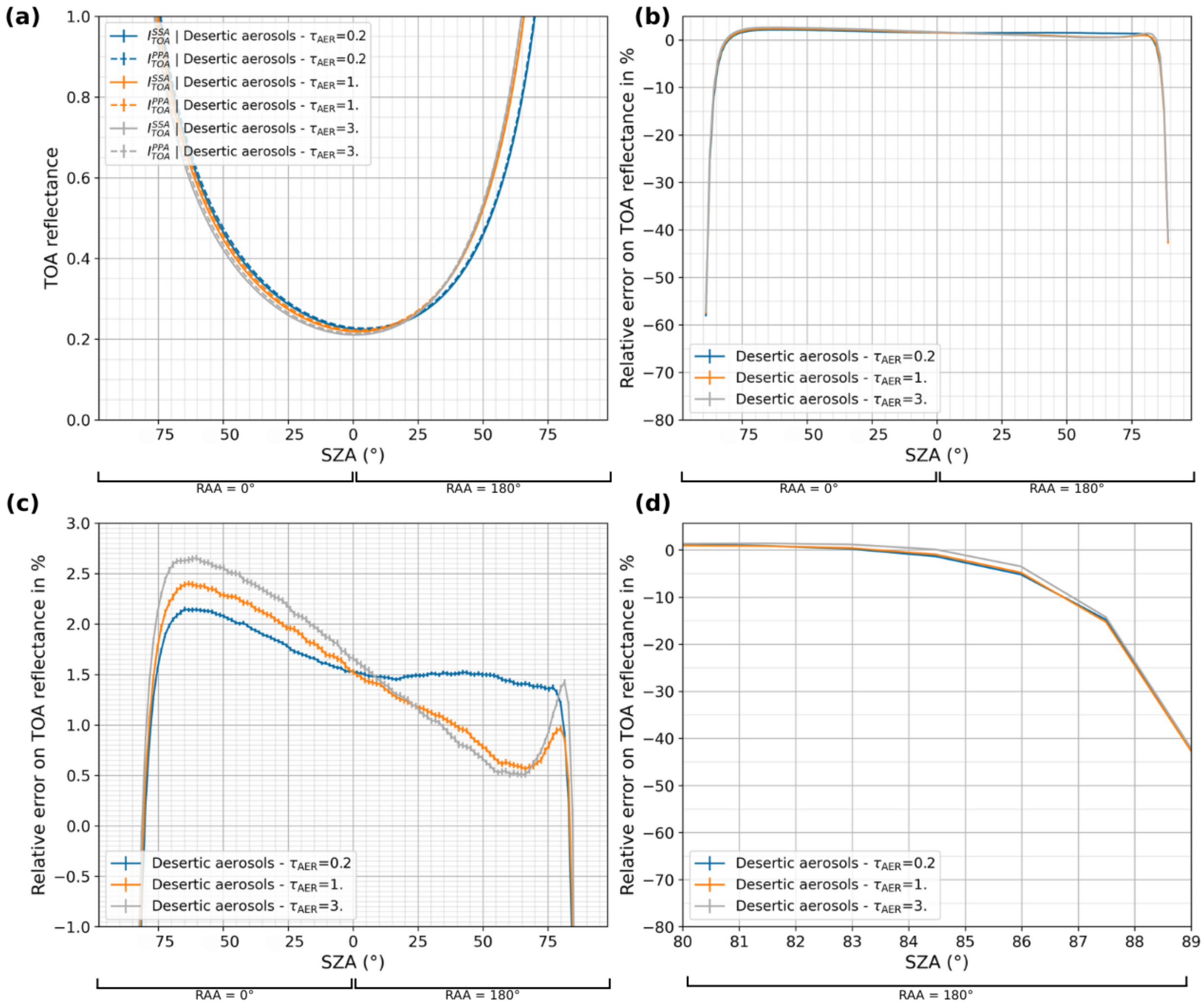

Figure I: Comparison of PPA and SSA simulations for several AOD values as a function of SZA, for channel VIS 0.4 in a US standard atmosphere with a Lambertian surface of albedo 0.1, with **VZA = 80°** in the principal relative azimuthal plane, with 1e8 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -1 % and 3 %. (d) Same plot zoomed-in for SZA over 80°.

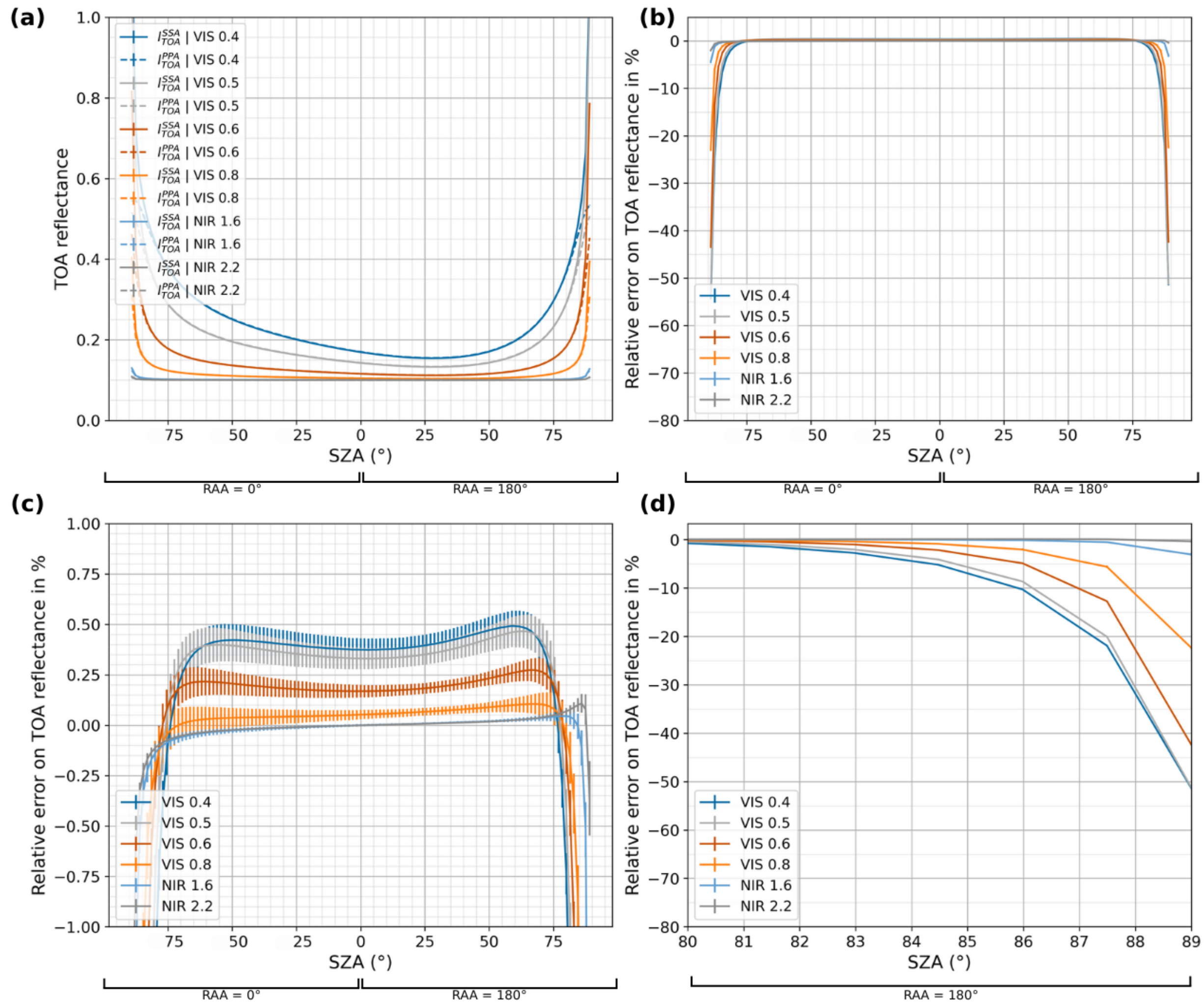

Figure J: Comparison of PPA and SSA simulations for several FCI VIS and NIR spectral channels as a function of SZA, with VZA = 45° in a US standard aerosol-free **non-absorbing** atmosphere with a Lambertian surface of albedo 0.1, in the principal relative azimuthal plane, with 1e7 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -1 % and 1 %. (d) Same plot zoomed-in for SZA over 80°.

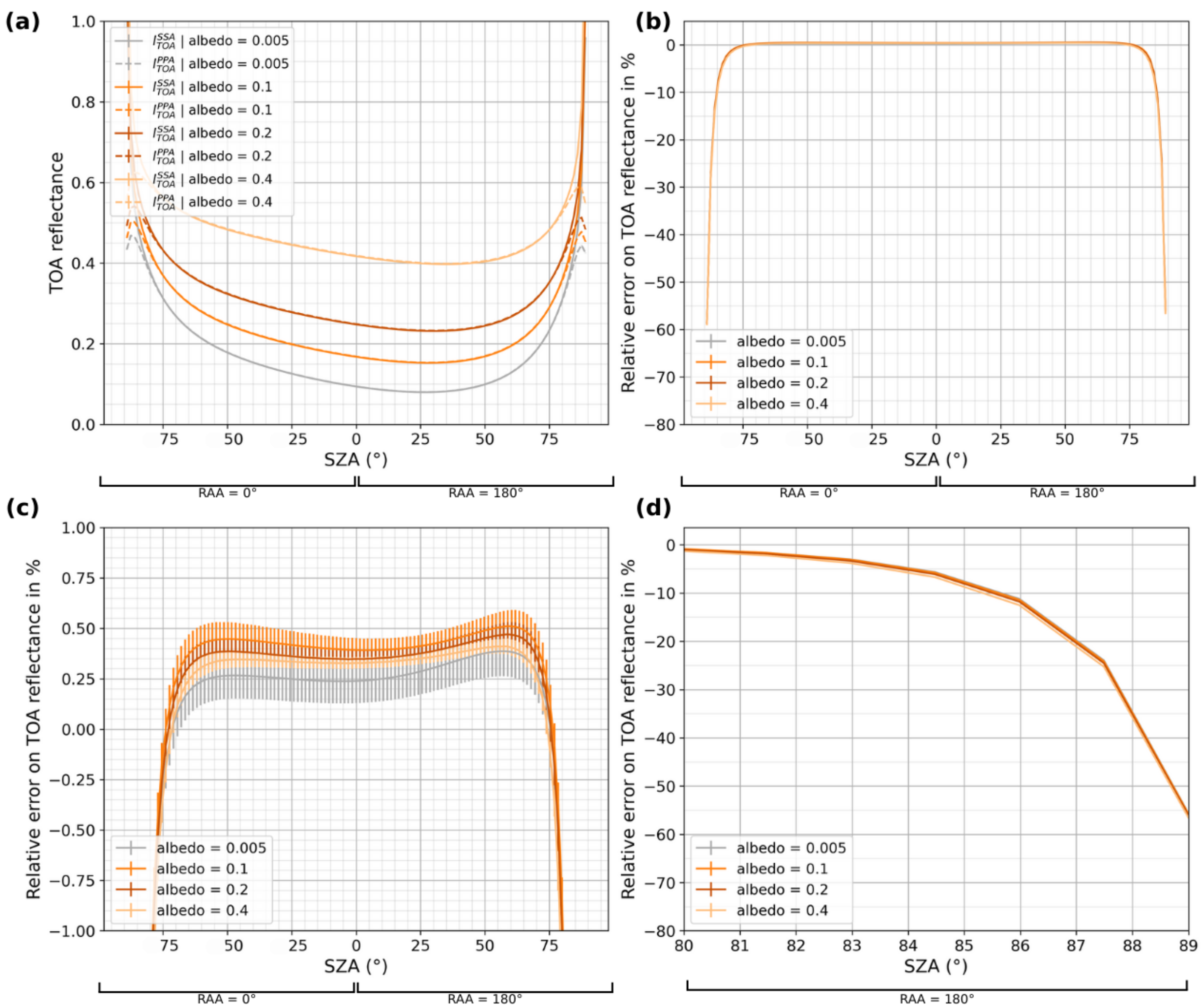

Figure K: Comparison of PPA and SSA simulations for several realistic continental surface albedo values for channel **VIS 0.4** as a function of SZA, in a US standard aerosol-free atmosphere, with VZA = 45° and in the 0°-180° relative azimuthal plane, with 1e7 photons. (a) Simulated TOA reflectances. (b) Calculated relative error between PPA and SSA simulated TOA reflectances. (c) Same plot zoomed-in between -1 % and 1 %. (d) Same plot zoomed-in for SZA over 80°.

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