

Reply to Reviewer #2

Major Comments

1. Reorganizing the data and methodology section

As this manuscript handles a variety of datasets, Sections 2.1 to 2.6 introduce each of them. However, the detailed information necessary to reproduce the research is not sufficiently provided, for example, dataset names, versions, and variable names in the dataset. In addition to the introduction of products, it is necessary to precisely highlight what are used in this study. To help understanding the use of these products, it may be worthwhile to add a schematic diagram presenting the research strategy (e.g. what variable of which product is compared against what variable of which product). Since Sections 2.7 to 2.10 offers rich details of the methodology, they could form an independent Methodology section. A part of the list in the end of introduction (Lines 102-111) could be move to this new Methodology section to provide the overview of the technical challenges and relative position of them in the entire research framework.

We thank the reviewer for this constructive and valuable suggestion, which has significantly improved the structure and reproducibility of the manuscript. In response to the comment, we have restructured our approach into a dedicated Methodology section, new Section 3 (spanning old Sections 2.6 to 2.10). At the beginning of this section, we have added a flowchart (Fig. 2) that visually details all data products, their respective versions, and their validation and comparison pathways. Furthermore, we have explicitly included the exact variable names for the primary surface irradiance components (`flux_down_direct_solar_{1d,3d}_surface_all_sky` and `flux_down_diffuse_solar_{1d,3d}_surface_all_sky`). While we have highlighted these key variables in the text, we have chosen not to explicitly list every variable name to avoid cluttering the text, as the names are lengthy but largely self-explanatory. Regarding the suggestion to move a portion of the objective list from the end of the introduction to the new methodology section, we have carefully considered this but prefer to retain it in the introduction. Keeping it there ensures that readers receive an overview of the study's goals and framework early on in the manuscript.

2. Collecting discussion that is scattered around in Results and Conclusion sections

The Result section presents lots of data, sometimes together with the interpretation and expert's guess. Conclusion section is relatively long, with some comments that has a value as discussion. Collecting all interpretation, hypothesis, and discussion to form a new Discussion section would make the Results section easier to follow.

We thank the reviewer for this constructive suggestion regarding the organization of our results and discussion. In response, we have renamed Section 3 to "Results and Discussion" and integrated the scattered discussion points previously found in the conclusions directly into this section. The main changes are:

1. Implementation of the SCA performance evaluation under cloud-free conditions in new Section 4.1.1 (Lines 505–509): *"Another reason could be the degradation of the SCA's performance with increasing distance from the satellite's ground track. Indeed, for distances below 25km, the 7 resulting EarthCARE overpasses are in close agreement with BSRN observations ($MBE = -4.5 \text{ W m}^{-2}$, $RMSE = 12.0 \text{ W m}^{-2}$), while the 12 overpasses that are further than 25km away from the ground station exhibit a significant negative bias ($MBE = -25.8 \text{ W m}^{-2}$) and much larger discrepancies ($RMSE = 50.7 \text{ W m}^{-2}$)."*

2. Discussion of the direct/diffuse definition of EarthCARE/BSRN in new Section 4.1.1 (Lines 530–536): *“This discrepancy might be attributed to differing definitions of direct and diffuse irradiance between EarthCARE calculations and BSRN. While BSRN measures direct irradiance using a pyrhelometer with a 5° field of view, the EarthCARE ACM-RT BA baseline data analyzed in this study used a direct component defined to be downwelling solar radiation in the Monte Carlo calculation that reached the surface without undergoing any scattering events. This could introduce differences particularly regarding the circumsolar contribution, as near-forward scattered photons are registered as direct by the ground instrument but classified as diffuse by the RTM.”*
3. Comparison between CAMS and BSRN RMSE in new Section 4.2 (Lines 579–581): *The EarthCARE RMSE values against CAMS are substantially lower across all three methodologies compared to the BSRN evaluation, likely due to the improved collocation and more consistent methodology between the datasets.*
4. Discussion on the change in aerosol inputs between baseline BA and BC in new Section 4.3 (Lines 749–752): *“In contrast to the ACM-RT baseline BA, the subsequent baseline BC takes this underperformance of the CAPTIVATE processor into account and utilizes aerosol properties derived from single-instrument products for radiative transfer simulations, rather than the synergistic CAPTIVATE product.”*

However, we opted to maintain a combined Results and Discussion section rather than creating a separate section. Given the wide range of data products and intercomparisons presented, keeping the interpretation closely coupled with the corresponding results helps maintain a clear, cohesive narrative for the reader, whereas separating them could make tracking the performance of each specific dataset more challenging.

3. Shortening conclusions and associating claims therein to results

The Conclusions section counts nearly 100 lines, exceeding 1/8 of the main manuscript texts. Some repetition of important results is necessary, but the current Conclusions section develops a new discussion with unrepresented data and omit a part of presented results (e.g. IR validation). By introducing the Methodology section that describe the overall research architecture, the Conclusions section would become naturally shorter. It is also necessary to highlight each and every claim in the Conclusions section to results.

We thank the reviewer for this helpful observation. In line with the previous comment, the Conclusions section has been reduced from ~ 90 lines to ~ 75 lines by relocating discussion points and unrepresented data into the Results and Discussion section. Various statements have been strengthened by providing a direct link to the results. The new Conclusions section should therefore be clearer, more concise and better aligned with the structure of the Results and Discussion section.

Specific Comments

Line 4: “However, due to the advanced technologies and retrieval approaches”

Authors reason the need of validation by the novelty of the mission, but the validation is essential for any indirect measurements. Operational meteorological satellite missions invest tremendous efforts in calibration and validation even though the technology is mature. I suggest that authors simply remove this sentence because it is not necessary.

We thank the reviewer for this comment. We agree that validation is essential for any satellite-derived product, regardless of the maturity of the technology, and our intention was not to suggest otherwise. However, we believe that a brief statement on the motivation for validation is useful for the reader at this point in the text. We have therefore rephrased the sentence so that the need for validation is no longer attributed to the advanced technologies used in EarthCARE (Lines 4–6):

“As for any satellite mission, the products of each EarthCARE instrument, as well as their synergistic products, must be validated against independent reference measurements.”

Line 33: “averaging around 1361 Wm⁻²”

When averaging globally and daily (both day and night), the incoming solar flux is about 340 Wm⁻², which is the value used to compute the energy imbalance. I request authors to develop the reason why the TOA irradiance is presented here. Otherwise, simply referring to the value by Stephens et al. 2012 or Loeb and Wielicki 2016 would be sufficient.

We agree that distinguishing between the global/diurnal mean solar flux and local instantaneous irradiance is essential for clarity. While the diurnally and globally averaged flux ($\approx 340 \text{ W m}^{-2}$) governs global energy budget studies, validation studies of EarthCARE’s instantaneous, localized radiative transfer retrievals are bounded by the total solar irradiance at normal incidence ($\approx 1361 \text{ W m}^{-2}$). To clarify why this value is presented in our manuscript, we have expanded the text (Lines 36–40) to explicitly detail this distinction and included the reference to Stephens et al. (2012):

” The level of radiative forcing is governed by the top of atmosphere (TOA) solar (global) irradiance in the 0.2–4.0 μm spectral region. While the diurnally and globally averaged TOA solar flux driving the global energy budget is approximately 340 W m^{-2} (Stephens et al., 2012), local instantaneous radiative transfer simulations are bounded by the total solar irradiance at normal incidence, typically averaging around 1361 W m^{-2} (Prša et al., 2016; Schmutz, 2021).”

Line 48: “high-temporal-resolution”

Though depending on the swath of the sensor and the latitude of target location, a polar-orbiting satellite imager revisits the same location on the ground only twice a day. Authors should provide the reason why they consider this is high temporal resolution.

We thank the reviewer for this remark. We agree that for near-polar orbiting satellites, temporal resolution is limited. However, our statement specifically refers to geostationary satellites rather than polar-orbiting platforms. Geostationary imagers provide continuous, high-temporal-resolution observations over fixed domains (in the case of CAMS with 15-minute temporal resolution). To ensure this distinction is clear, the text explicitly specifies geostationary imagers (Lines 55–57):

” Alternatively, imagers on geostationary satellites provide high-temporal-resolution horizontal information about clouds that can be used as input alongside assimilated atmospheric composition data in RTMs.”

Line 74: “However, significant discrepancies were identified in the depolarization ratios”

The depolarization ratio is not mentioned anywhere other than this line in this manuscript. I don’t think this sentence is necessary for this paper, though informative.

We agree, this sentence is tangential to our focus on surface irradiance validation. We have removed the sentence from the manuscript accordingly.

Line 106: “cloud modification factor approach”

This is somewhat confusing because this approach is nowhere defined in the introduction. If the authors consider this approach is their original approach, it should be mentioned as such. Otherwise, it’s preferred to present the previous relevant studies.

We agree with the reviewer that introducing the term “cloud modification factor” in the Introduction without prior context could be confusing. To maintain clarity in the introduction, we have rephrased the sentence to focus directly on the classification objective (differentiating sky conditions), reserving the cloud modification factor introduction for the Methodology section (Line 115):

” Differentiate between cloud-free, partly cloudy and overcast conditions.”

Line 107-108: “quantify the across-track extension algorithm’s performance”

No reference is mentioned to quantify the performance of the algorithm. Do authors imply, “Evaluate both 1D and 3D EarthCARE products against BSRN data to quantify the across-track extension algorithm’s impact”?

We thank the reviewer for this helpful comment. We agree that mentioning the across-track extension algorithm here introduces unnecessary confusion, as our main objective at this point is to compare the performance of EarthCARE’s 1D and 3D radiative transfer models. While the across-track extension algorithm is inherently part of generating the 3D domain (and is used to define the same assessment domain for the 1D evaluation in this study), evaluating its specific performance is not a primary objective of this work. We have therefore removed the reference to the across-track extension algorithm from this sentence (Line 117):

” Evaluate differences between EarthCARE’s one-dimensional and three-dimensional models.”

Line 110: “differences observed at the TOA”

It would be helpful to specify what measured quantity the authors are focusing on.

We agree with the reviewer and have updated the sentence to explicitly specify that the evaluation focuses on TOA upward radiative fluxes (Lines 119–120):

” Assess whether surface irradiance discrepancies (relative to BSRN/CAMS) correlate with TOA upward flux differences relative to the BBR instrument.”

Line 122: “ACM-CAP and ACM-COM”

This subsection could be easier to understand if it provides the overall relation between datasets (ACM-CAP, ACM-COM) and the required inputs to ACM-RT. Currently, the title “ACM-CAP and ACM-COM” implies that both are prerequisite for the ACM-RT processing, while at the last sentence reads “an alternative set of cloud, aerosol, and precipitation profiles”, implying that ACM-CAP could replace ACM-COM. As this is in the “data and methods” section, it is also necessary to describe which profile is used (ACM-CAP or ACM-COM one) in this study. If this manuscript doesn’t directly use ACM-CAP or ACM-COM products, there is no need to make a section to describe whole details.

We thank the reviewer for this constructive comment. We agree that introducing both ACM-CAP and ACM-COM as product inputs without explicit context could cause confusion. In ACM-COM, both RT input profiles are stored, from the CAPTIVATE algorithm (ACM-CAP product) and composite algorithm. The CAPTIVATE retrievals are EarthCARE’s “best estimates”, representing synergistic use of the observations made by CPR, MSI, and ATLID, while the composite algorithm utilizes single-sensor measurements to provide an alternative set of cloud and aerosol profiles. To make this distinction more clear, we changed in some instances ACM-CAP to CAPTIVATE in the paragraph and added the following concluding sentence at the end (Lines 145–147):

” Consequently, either CAPTIVATE or composite profiles can serve as RTM inputs, with performance being evaluated in each baseline to select the optimal approach.”

We still think that describing these algorithms is necessary in Section 2, especially as they are compared against each other in the aerosol optical depth evaluation. Nonetheless, we agree that it is necessary to describe which profile is used (ACM-CAP or ACM-COM) in this study. Therefore, a sentence was added in Section 3.2, Lines 313–314:

” In this baseline, cloud and aerosol products from the CAPTIVATE algorithm (see Sect. 2.1.1) were used as input in EarthCARE’s RTMs.”

Line 128: “allowing users”

I suppose this “users” mean the data product provider, not the data product users. The description of CAPTIVATE algorithm is useful to understand the ACM-CAP product, while some sentences are more of introductory nature, making them more appropriate in “introduction” section.

We thank the reviewer for this helpful comment. We agree that details on configurability address more data product providers rather than end-users. We have removed the configuration sentences and updated the paragraph to focus on the variational retrieval process and product outputs, while making them less of introductory nature (Lines 136–141):

” This algorithm uses a classic variational approach that minimizes the cost function using measurements of the three different instruments ATLID, CPR, and MSI. If the cost function converges, the resulting cloud, aerosol, and precipitation parameters of the profile and their retrieval errors are provided as product output (Mason et al., 2023).”

Line 148: “scene construction algorithm”

Is this different from “across-track extension algorithm” mentioned in Introduction? If so, please describe, if not, please keep the wording consistent throughout the manuscript.

We thank the reviewer for pointing this out. Yes, this refers to the same algorithm. To ensure consistent terminology throughout the manuscript, we have standardized all references to the “Scene Construction Algorithm” (SCA) in alignment with other EarthCARE literature.

Line 153-154: “cosine of ... should not exceed a certain amount”

I don’t geometrically understand what “the cosine of solar zenith angles and azimuth angles between sun and satellite tracking direction” means. Perhaps, authors meant as follows: the cosine of solar angles as well as the cosine of azimuth angles between sun and satellite directions should not exceed a certain amount. However, it is still confusing because a cosine exceeding a certain value means that two directions are close. Solar zenith angles close to nadir, or two azimuth angles close to each other are rather favorable condition, isn’t it?

We agree, the cosine should not be included in this sentence. We have updated the sentence accordingly (Lines 168–170):

” the differences in solar zenith angles as well as relative azimuth angles between sun and satellite directions should not exceed a certain amount.”

Line 167 and 168: “ $\pm 55^\circ$ forward”, “ $\pm 55^\circ$ backward”

I am confused by the negative in these expressions.

We agree that the $\pm$ symbol is redundant and confusing since the directions are already explicitly specified as forward and backward. We have removed the $\pm$ sign from these expressions.

Line 232: “AERONET”

This section may surprise readers because AERONET is not mentioned anywhere in abstract and introduction.

We agree that it may surprise readers. Therefore, we have now introduced AERONET alongside the other datasets in the introduction (Lines 108–110):

” Utilizing the BSRN and CAMS validation datasets, alongside additional information from EarthCARE’s BBR instrument and ground-based aerosol observations from the AErosol RObotic NETwork (AERONET), this study addresses the following objectives:”

Line 250: “measured value”, “modelled value”

It’s unclear what measure/modelled value can go into G. Perhaps, “downward shortwave flux value”?

We agree, it may be unclear what value can go into G. We thank the reviewer for the suggestion, and have updated the sentence the following (Lines 287–288):

”where G_{all_sky} is the measured downward surface GHI value under all-sky conditions while G_{clear_sky} is the modelled value assuming clear-sky conditions.”

Lines 273–279

This paragraph introduces EarthCARE dataset rather than the collocation approach. It could be placed elsewhere.

While this discussion touches on dataset availability, we believe it is well-suited here because it establishes the temporal scope and baseline selection specifically for this evaluation. However, we agree that the title of the section is not ideal. Therefore, we changed the title from *”Collocation approach between EarthCARE and BSRN”* to *”Approach for evaluating EarthCARE retrievals against BSRN observations”* (Line 309). The same was done for the corresponding CAMS section (Line 365).

Line 315: “SCA indices”

The subsection fails to describe what SCA indices are, and the difference from the quality index.

We agree that it may be confusing to readers what SCA indices are and their differences to the quality index. Therefore, we changed the term *”SCA indices”* to *”constructed data from the ACMB-3D product”* in this subsection (Lines 345 and 354) and throughout the manuscript.

Line 380: “composite algorithm products (see Sect. 2.1.1)”

This expression doesn’t appear in Section 2.1.1. It could mean ACM-COM, but the explicit mention would be beneficial for the clarity.

We agree. In line with our updates regarding ACM-COM/ACM-CAP, we changed Section 2.1.1 so that it explicitly mentions the composite algorithm now.

Line 426: Table 1

The number of samples in each regime should be reported as in Table 2. Since the number of points in cloud-free and overcast conditions are limited, it may be beneficial to show median in addition to exclude the impact of outliers.

We thank the reviewer for this helpful comment. As suggested, we have added the sample counts (N_{points}) and median GHI for each cloud regime to Table 1 for more information. In addition, we added the following sentence to Lines 476–477:

”The median values (Tab. 1) differ significantly from the MBE for overcast and cloud-free conditions, illustrating the strong impact of individual overpasses on the mean bias for these low sample sizes.”

Lines 459–460: “combined effects of . . . are smallest when in close proximity”

More description would be beneficial since neither of median nor RMSE (or IQR) approach to zero with decreasing distance. The difference to Fig. 6d could be discussed to emphasize if there is an advantage of using ACMB-3D.

We thank the reviewer for this constructive comment. We have expanded the description to discuss the IQR trend and included an explicit comparison to the along-track analysis (new Fig. 5d) to demonstrate the benefit of the ACMB-3D across-track extension (Lines 509–515):

”Similarly, the distance dependence of all-sky GHI differences (Fig. 6d, e, f) confirms that differences and their variability are smaller in close proximity to EarthCARE’s nadir ground track, as evidenced by the larger interquartile ranges (IQRs) in the two outermost distance bins. Specifically, the outer two bins yield a combined IQR of 92.2 W m^{-2} , compared to 82.6 W m^{-2} for the inner three distance bins. Nevertheless, even this larger IQR of the outer bins remains smaller than that of the corresponding distance bins in the along-track analysis ($\text{IQR} = 117.0 \text{ W m}^{-2}$, Fig. 4d), highlighting the advantage of using the ACMB-3D across-track extension.”

Lines 461–485

This paragraph criticizes the radiative transfer model (RTM) used in the production of EarthCARE 1D and 3D products, but the data don’t show the flaw of the RTM itself. I suppose that authors meant the EarthCARE 1D and 3D products overall (including SCA and all relevant processing) rather than RTM. This comment applies to many other places, where RTM is used as the label of the EarthCARE products. It is necessary to reword all confusing usage of the term RTM.

We agree, saying that the 1D/3D RTMs have biases can be confusing as they bias might not come from the radiative transfer models themselves but rather from single-sensor measurements or other input in the workflow. Therefore, we changed EarthCARE 1D/3D RTM to 1D/3D product or 1D/3D estimates at the appropriate places in the manuscript.

Line 462: “This discrepancy is largely attributable to the suboptimal spatial collocation”

I could not find the data supporting this claim.

We thank the reviewer for pointing this out. To support this claim, we have expanded the text to explicitly reference the findings from the across-track extension evaluation over BSRN stations, which demonstrates that improved spatial collocation reduces the observed GHI discrepancies (Lines 519–520):

”As shown in the previous paragraph, this suboptimal spatial collocation compared to the across-track extension estimates over the station leads to higher differences in GHI.”

Line 475: “do not produce such significant deviation”

I encourage authors to present these results as it is the backbone of the expert’s guess about the discrepancy.

We agree with the reviewer that presenting these specific values strengthens the argument. We have now incorporated the explicit MBE values for both BHI and DHI for the 1D along-track average and the 1D extension over the assessment domain into the manuscript (Lines 539–543):

”Another potential reason is a systematic error within the 3D algorithm, as both the 1D RTM along-track average ($\text{DHI MBE} = -21.9 \text{ W m}^{-2}$ and $\text{BHI MBE} = 2.4 \text{ W m}^{-2}$) and its extension over the assessment domain ($\text{DHI MBE} = -22.2 \text{ W m}^{-2}$ and $\text{BHI MBE} = 3.5 \text{ W m}^{-2}$ using the SCA) do not produce such significant deviations from the BSRN observations despite having the same cloud and aerosol retrievals as input.”

In addition, differences due to direct/diffuse definitions between ACM-RT 3D estimates and pyranometer observations have been tested and incorporated in the manuscript, Lines 536–539:

”To test this effect, collocated EarthCARE frames were reprocessed using a 5° view cone definition for direct irradiance. This adjustment slightly improved the 3D GHI biases, resulting in an MBE of

−41.9 W m^{−2} for BHI and 37.7 W m^{−2} for DHI. However, this geometrical refinement alone cannot fully account for the observed discrepancies.”

Line 684: “the analysis confirms that ...”

As pointed out in a previous comment (Lines 459-460), this statement would become more convincing by providing further description of results. At the current state, it could be considered as an overstatement.

In line with the previous comment (Lines 459-460), the IQR values have been added to make the statement more convincing (Lines 788-790):

” Furthermore, the analysis confirms that the SCA’s performance diminishes with increasing distance from the satellite’s ground track, with the combined IQR expanding from 82.6 W m^{−2} across the inner three distance bins to 92.2 W m^{−2} for the two outermost bins.”

Line 685: “our analysis of ACM-RT input data from CAPTIVATE”

I assume that this is based on the results presented in Section 3.3, where CAPTIVATE AOD is shown to be smaller than AERONET AOD. The manuscript would become more convincing to add that the reference is AERONET.

We thank the reviewer for this suggestion. However, as part of the broader restructuring in response to Major Comment 3, this specific sentence was removed from the manuscript.

Lines 693-695: “while the 12 overpasses ...”

This data is not presented anywhere in Result section. I request authors to provide appropriate description of all results that support conclusions.

We agree with the reviewer that these results should be introduced in the Results & Discussion chapter before supporting our conclusions. We have now integrated the results of this analysis into Section 4.1.1 (Lines 505-509):

” Another reason could be the degradation of the SCA’s performance with increasing distance from the satellite’s ground track. Indeed, for distances below 25km, the 7 resulting EarthCARE overpasses are in close agreement with BSRN observations ($MBE = -4.5 \text{ W m}^{-2}$, $RMSE = 12.0 \text{ W m}^{-2}$), while the 12 overpasses that are further than 25km away from the ground station exhibit a significant negative bias ($MBE = -25.8 \text{ W m}^{-2}$) and much larger discrepancies ($RMSE = 50.7 \text{ W m}^{-2}$). ”

Lines 707-726

This paragraph doesn’t follow the flow of Section 3.2, making it difficult to find the corresponding evidence of authors’ claims in Results section. I strongly encourage authors to reorganize either of this paragraph or Section 3.2 to make their logical flow consistent.

We thank the reviewer for this recommendation. We agree that the alignment between this discussion paragraph and the structure of new Section 4.2 could be improved to help readers trace the supporting evidence more easily. To establish a consistent logical flow, we have reorganized both the paragraph and Section 4.2, ensuring that the discussion points now follow the exact same structural sequence as the results presented in Section 4.2.

Line 707: “quality of the different collocation methodologies”

The second paragraph of Section 3.2 states that authors show the results for different “EarthCARE methodologies”, not really three different collocation methodologies. I suggest removing “collocation” from this line.

We agree, "collocation" has been removed from this sentence.

Lines 724-726: "For the direct and diffuse components ..."

I was not able to find the corresponding evidence in Results section.

We thank the reviewer for this observation. The statement originally intended to combine general trends across multiple evaluations (old Figures 6, 12, C2, and C3). However, because these figures rely on distinct samples, combining them into a generalized statement is methodologically inconsistent. To maintain alignment with the presented data and avoid unsupported claims, we have removed this sentence from the manuscript.

Lines 733 and 741:

Authors claim that the difference in AOD retrievals cannot fully explain the clear-sky irradiance bias in Line 733, while presenting a hope that the clear-sky irradiance discrepancies to be resolved in by switching the retrieval from CAPTIVATE to COM in Line 741. I got an impression that authors are not consistent in the same paragraph. More concise but insightful comments are necessary.

We thank the reviewer for pointing out this inconsistency. We agree that the previous phrasing created an apparent contradiction in our discussion. To resolve this, we have removed the speculative remark regarding switching the retrieval from CAPTIVATE to COM. Instead, we have updated this paragraph to focus on the newly identified solar zenith angle (SZA) specification issue within EarthCARE's ACM-RT baseline BA product (detailed in Sect. 4.5). This issue led to EarthCARE underestimating surface irradiance. Consequently, when accounting for this correction, the clear-sky irradiance comparison will likely align better with the spatial patterns observed in the AOD comparison. We believe this update in the text provides a more concise, consistent, and physically grounded explanation.

Technical Comments

Line 8: "reported in the EarthCARE ACM-RT product"

For the clarity, I suggest moving this clause immediately after the "global horizontal irradiance (GHI)", and changing "computed using 1D and 3D ..." to "that are computed using 1D and 3D ...".

We thank the reviewer for this helpful suggestion to improve readability. To enhance clarity and maintain a clean grammatical structure, we have split the statement into two separate sentences (Lines 6–9). In addition, we have added the period of the analysis (September 2024–November 2025):

"In this study, ground-based radiation observations from the Baseline Surface Radiation Network (BSRN) are used to validate surface global horizontal irradiance (GHI) reported in the EarthCARE ACM-RT product during an early phase of the mission (September 2024–November 2025). These irradiances are computed using one-dimensional (1D) and three-dimensional (3D) radiative transfer models (RTMs)."

Line 52: "CloudSAT"

The capitalization is "CloudSat".

Fixed.

Line 150 and others: "SCA algorithm"

When spelled out, this becomes "Scene Construction Algorithm algorithm", which is redundant. It is worth considering replacing them by simply "SCA".

Fixed throughout the manuscript.

Line 220-221 and 230-231: “Copernicus Atmosphere Monitoring Service”

The same section starts with acronym “CAMS” and uses the acronym throughout except these locations. In addition, the years are not consistent (2022, 2021, while 2026 in the second line of the section.)

We thank the reviewer for bringing this to our attention. These instances were specific data citations for the respective archived datasets rather than general text references to the service. To maintain consistency and eliminate confusion, we have updated the text to use the acronym “CAMS” throughout and formatted the dataset references with parenthetical citations (Lines 243–244 & 254–255):

“The corresponding datasets are publicly archived and accessible via the CAMS Atmosphere Data Store (ADS) (CAMS, 2022).”

Line 245: “these effects”

Perhaps, “their effects”? It’s difficult to interpret as: “these effects = influence”.

We thank the reviewer for the suggestion and have changed “these” to “their”.

Line 398: “extrapolation”

Is this interpolation instead?

Yes, it should be interpolation. We fixed the sentence accordingly.

Line 423-424: “BSRN measured slightly higher irradiance than EarthCARE, ...”

As this section is the EarthCARE validation against BSRN, it would be more straightforward to phrase as, “EarthCARE reports slightly lower irradiance than BSRN”. Likewise in Lines 441-442.

Fixed.

Line 425: “data sets”

To be consistent to elsewhere, “datasets” would be better.

Fixed.

Line 443: “regularly”

I suppose that authors meant “frequently” or “occasionally”.

Fixed to frequently.

Lines 487-488: “lower values in the performance metrics”

It could be misread as lower performance. I guess that authors meant as follows: Figure 9 demonstrates that EarthCARE 1D downward flux in LW deviates less than in SW from the BSRN measurements.

Fixed.

Line 488: “improvement”

“low discrepancy” would be more suitable. This is not an improvement (which implies the gained performance over time), but just the different physics as authors mention later.

Fixed.

Line 490: “physical behaviour”

Cloud doesn’t behave differently in SW and LW, but results in different radiative consequences. What about “physics behind the radiative effects”?

Fixed to *”physical mechanisms governing cloud radiative effects”*.