Response to Reviewer #1

Title: The Education and Research 3D Radiative Transfer Toolbox (EaR^{3}T) – Towards the mitigation of 3D Bias in Airborne and Spaceborne Passive Imagery Cloud Retrievals

Authors: Chen et al.

Recommendation:
Major revision

Summary:
The authors of this manuscript developed a modularized Python package EaR^{3}T which automates the process of 3D radiative transfer calculation. They illustrated the broad range of applications of this 3D-RT package by showing four examples of 3D radiance simulation and cloud retrievals.

The work is solid and requires tremendous effort. The package developed by the authors is also very useful and has significant potential for 3D-RT-related applications.

However, I found that the current structure of this manuscript is difficult to follow. This is because there are too many low-level technical details in the first 4 sections, which could severely distract readers from the primary scientific findings of this manuscript (See my major comments).

Also, there are a few places where more explanations and clarifications are needed (see my major comments). Considering all this, I recommend a major revision for this paper.

R: Thank you for your comments.

Major comments:

Comments on the paper structure:
1. Though I understand that it takes tremendous effort to develop this package, I found the description in Section 2.1 Overview (especially L180-229 and Table 1) is too technical (not very scientific-related). It is better to move this part into an appendix and focus on the four applications showing the advantage of using 3D RT model in radiance simulation and cloud retrievals, the scientific part of this manuscript.

R: Thank you for your comments. We agree and moved the tables and technical descriptions about the input and output parameters into Appendix A1 (Page 35, Line 870).

2. The same comments to the Section "3. EaR^3T Procedures" (L400-434), L488-509. You can create a separate manual for your package, but in the text, you might want to focus on the scientific part. Only discuss the input/output sources for your applications. For example, some description of Table 2 is good enough.

R: We modified Section 3 – 1) the code walk-through in Section 3 was moved into Appendix A2 (Page 39, Line 919) and 2) the procedure description originally in Section 2 was moved into Section 3 (Page 16, Line 398). At the end of Section 3, we cross referenced Appendix A.

Comments on the results:
L541: Figure 5. It might be better to indicate the cloud and clear-sky regions in Figure 5. Also, show the RGB image here for more easy pairing your results with the cloud and clear-sky areas of the RGB image. For the EaR^3T IPA calculation, what is your input? Column gas and temperature profiles or still 3-D gas and temperature fields?

R: We added Figure 5b (Page 18, Line 463) in the same format as Figure 2 but for monochromatic IPA radiance calculations to provide context of the entire domain. For both IPA and 3D radiance calculations, the input column gas and temperature profiles are 1D profile, which only contains vertical variability and assumes atmospheric gases and thermodynamic parameters are horizontally homogeneous. All the radiation related input parameters are now provided in Appendix A1 (Table A1, Page 38, Line 906) for transparency. 3D atmospheric gas and temperature fields are supported but not used in any of the applications shown in the paper. We plan to build support for longwave in the near-future, where 3D temperature field will play an important role in determining 3D cloud radiative effects in the longwave.

L544: "In the cloudy regions", where are exactly cloudy regions? It seems to be not mentioned in the previous context.
R: We added text clarification (see Page 19, Line 470) as well as Figure 5b (newly added, Page 18, Line 463) for providing context for the domain.

L534: In this context, you attributed the biased clear-sky 3D-RT radiance bias to the surface reflectance (red). But could the diffuse radiance from the nearby clouds contribute to your biased simulations at the clear-sky regions? If true, how do you determine which components contribute more to the bias radiance?
R: What you described is exactly what we tried to explain in the paper. 3D effects contain two parts – brightening of clear-sky regions (or optically very thin clouds) near clouds as they are net photon "recipients" and darkening clouds themselves (if clouds are optically thick) as they are net photon "donors". At the OCO-2 footprints of the domain, the model input surface albedo is directly taken from the OCO-2 retrieved surface reflectance under clear-sky conditions without considering the brightening 3D effects. Since we saw an increase of the radiance from IPA to 3D calculations (see more explanation in our response to the next comment), we argue that the OCO-2 derived surface reflectance is indeed too high because of the cloud vicinity effect. One potentially confounding factor that we did not consider in our manuscript is that we did not include aerosols that can alter the results. To your question about whether we can determine which component contributes more, the answer is no we cannot with the simple showcase in the paper. However, it is possible if we extend this case to multi-spectral and multi-angle and even with sub-orbital observations from aircraft. Our next following paper will use this strategy for approaching radiation closure.

I would suggest doing the following experiment: conduct a simulation over a large clear-sky region (so no diffuse radiance from nearby clouds) to see if the clear-sky 3D-RT still overestimates the radiance. If so, you can attribute the bias to the surface reflectance. Now, I cannot determine this because I'm not sure how large is your clear-sky region in Figure 5.

R: A new simulation is not needed as we have IPA calculations (blue in Figure 5a). From the IPA calculations, we can see that within the clear-sky regions (e.g., latitude range of [38.05°, 38.3°]), the radiance simulations are roughly in agreement with the observations. As stated in the previous response, the bias can be traced back to the surface reflectance contaminated by 3D cloud radiative effects in the raw OCO-2 observations (referred to as stage 1), which were used for surface reflectance retrieval without any correction for the 3D effects. Next, we perform 3D calculations (red) and the radiance simulation goes even higher than the observations (stage 2), indicating an enhanced 3D effects when radiation is allowed to scatter from clouds into clear-sky. Such increase high bias from stage 1 to stage 2 corroborates our assumption that the bias resides in the surface reflectance when it was contaminated by 3D effects at stage 1. Of course, the aerosols, which are not considered in the RT, can play an important role and potentially alter the results. Thus, we changed our wording by adding "probably" (Page 18, Line 454).

L582-584: "Since the MODIS reflectance is not self-consistent…of COT". Here you have implicitly assumed that Era3T calculation is the truth. It would be good to discuss the input of IPA calculation, especially the different input components to EaR^{3}T IPA and a standard plane-parallel 1D RT model, since those different components contribute to the simulation difference here. It will be self-consistent if you use the same plane-parallel 1D RT model to retrieve those cloud parameters.

R: The ground truth we are relying on is the MODIS (and OCO-2) observed radiance, not EaR^{3}T's radiance calculations. We agree that oversimplifying 1D RT (for example, when using the two-stream approximation as done in the original version of this paper), or using erroneous inputs to either 1D or 3D calculations can introduce errors. What we mean by radiance self-consistency is the following: We first map the radiance to cloud products (COT etc.) in a very similar manner as done in the heritage retrieval, except that it leads to products at a higher spatial resolution than provided in the operational L2 product (matching the resolution of the L1B radiances). In the first step, we then run forward calculations from these retrievals with 1D RT (EaR^{3}T-IPA), and compare these calculations with the original radiance observations. Provided that no systematic errors were made in these calculations, they should agree with the original observations because IPA is essentially the inverse operation to the original retrieval. Examples of these calculations are shown in Figure A4a and A4b (newly added, Page 47, Line 1107). Any deviation from the 1:1 line here indicates errors in the input cloud or surface properties, or errors in the radiative transfer itself. In the case of Figure A4a, for example, the calculated reflectances on the lower end (clear sky) are sometimes lower than the measurements. However, overall, the scatter around the 1:1 line is negligible. In sum, when using IPA as the forward model, the calculated radiances are largely in agreement with the observations as they should be. However, in reality, 3D-RT is at work in nature. If the retrieved L2 properties are correct, then it should be the radiances derived from forward calculations in 3D-RT, not 1D-RT that reproduce the measured radiances. MODIS (or OCO-2) radiances are not self-consistent when 3D-RT does not reproduce the original observations. Lack of self-consistency can be attributed to two factors: (1) 3D effects as described (2) any errors in the input fields that were already detected by comparing IPA calculations with the observations. The 3D effects can be isolated by comparing them against the IPA baseline. In our examples (Figure 7 and 13b against the newly added IPA baseline in Figures A4a and b), the 3D effect dominates the radiance inconsistency by far. The 3D bias in radiances (Figure 7) can be as large as 40% for optically thick clouds (reflectance greater than 0.3), whereas there is no systematic bias in the IPA (Figure A4a). Thus, using the radiance self-consistency to evaluate 3D biases is justifiable.

L790: Figure 13 shows you are using a Two-stream approximation. Have you tried larger stream numbers (at least 4 streams)? For your CNN trained on EaR^3T-based 3D Radiance field, how many streams do you use? It will be a fair comparison only if these two use the same number of streams.

R: Thank you for your comments. We agreed that two-stream approximation can lead to artifacts in the 3D effects we demonstrated in the paper as it is only a good proxy for irradiance but not radiance. To address this, we changed the IPA cloud retrieval method from two-stream approximation to IPA reflectance to COT (cloud optical thickness) mapping (described in Appendix C2, Page 45, Line 1073) obtained from the same radiative transfer process we used in the paper. This way the IPA consistency is ensured, and any biases exist in the 3D radiance self-consistency check stem from 3D effects. The two-stream approximation (or higher stream) is simplified analytical solution for 1D-RT, which uses plane-parallel assumption and independent pixel approximation. While the 3D radiance simulations from EaR^3T use MCARaTS as 3D-RT solver, which uses Monte-Carlo method to output simulation results based on photon statistics, no plane-parallel assumption or independent pixel approximation is involved, which better depicts the reality of nature than the 1D-RT. There were a few simplifications we made in our preliminary CNN, e.g., surface albedo of 0, Henyey-Greenstein phase function (g=0.85) for clouds etc. (details are discussed in the revised manuscript as well as Nataraja et al., 2022) that we plan to improve in the near-future. This will be elaborated more in the upcoming publications. Nevertheless, even this CNN out-performs the heritage IPA retrieval.

Minor comments:
L41: “In contrast to isolated case studies in the past, EaR^{3}T… irradiance.”: This claim seems misleading. Even the RT calculations with the plane-parallel RT models are made independently for each pixel, they are usually verified against over large regions as long as we have observations. Also, they are verified against multiple sources. I would recommend deleting this claim from your abstract.
R: We agree with this statement. However, we are not criticizing the plane-parallel approach here, nor are we questioning that many studies do a large amount of data aggregation. What we are trying to express here is that the automation capability allows us to perform massive RT 3D calculations for an entire campaign as opposed to limited case studies in the past (e.g., from our own papers) that often focused on single legs. For this reason, we decided to keep this statement. We did delete “isolated” to make this clear (see Page 2, Line 42).

L77: “Once the CNNs are trained”: remove “the”
R: We think “the” should be here as “CNNs” was described in previous sentence and here we are referring to that specific “CNNs”.

L107: “cloud fields with minimal user input”: Please rephrase this sentence. It would be better to emphasize how it automates the whole 3D-RT calculations instead of saying “with minimal user input”. Readers can have different interpretations of “With minimal user input”. One unpleasant interpretation is to use this tool as a black box.
R: We rephrased the statement to the following (also see Page 5, Line 126):
“It can be operated in two ways– 1) with minimal user input, where certain RT parameters are bypassed through default settings, for quick radiation conceptual analysis; 2) with detailed RT parameters setup by user for radiation closure purpose.”

L148-149: “The code, along…”: You can move this sentence to the Section of “Data and Code Availability”, as required by EGU journals.
R: We moved the text to the Data and Code Availability Section (See Page 50, Line 1162).

L248: Figure 2: Are those circles representing OCO-2 showing their actual spatial resolution, or just an illustration, since those footprints in the figure are not distorted and shown as circles?
R: The circles can only indicate the location of the OCO-2 footprints, NOT the spatial resolution. We added clarification in the caption of Figure 2 (See Page 10, Line 244).

L306-307: Move the data source of OCO-2 data to the Section of “Data and Code Availability”
R: We moved the OCO-2 data source to the Data and Code Availability Section (see Page 50, Line 1162).

L388: “Only radiance data from the red channel were used in this paper.”: Is there any reason why you only use observations from 626nm band?
R: Yes, the reason is the CNN model used in this paper (App. 4) was trained on synthetic data with realistic radiance simulated at 600 nm. We selected the red channel of the camera since the wavelength (centered at ~626 nm) is close to what CNN was trained.

L410: Again, move the link to the Section of “Data and Code Availability”
R: We moved the links to the Data and Code Availability Section (see Page 50, Line 1162).

L478: “In addition to MCARaTS, planned solvers…”: Move this part to the “Summary and Conclusion” Section as future work.
R: We moved the text to the conclusion (Page 34, Line 862).

L607: “For technical references,”: change to “for the running time of simulation,” or something more specific.
R: Changed (see Page 22, Line 545).

Response to Reviewer #2

I am very satisfied with the authors' response to the reviews, both with the changes to the manuscript as well as the clarifications and explanations in the response documents.

R: Thank you for your constructive comments and your time and effort dedicated to the review. After we revised our manuscript to address your comments along with comments from another reviewer during the initial review process, we received a third review during the technical correction process that requires some additional changes including text modification and result update. Please note that the manuscript was therefore changed beyond the recommendations you made, described below.

Text modification:

- The table for EaR^3T output parameters (originally in Section 2) has been moved to Appendix A1;
- Some descriptions for EaR^3T procedures (originally in Section 2) have been moved to Section 3;
- The code walk-through for App. 1&2 (originally in Section 3) has been moved to Appendix A2 (newly created, Page 39, Line 919);
- The cloud detection method has been polished (Appendix C1, Page 44, Line 1036);
- Appendix C2 (Page 45, Line 1073) has been updated to the new IPA reflectance-to-COT (cloud optical thickness) mapping method;
- The parallax correction has been polished – now includes a "cloud crack" treatment (Appendix D1, Page 47, Line 1113).

Result update:

- We switched from the usage of surface reflectance provided by MYD09A1 to white-sky albedo provided by MCD43A3 for surface albedo parameterization due to the finding of MCD43A3 is more reliable;
- We updated the IPA method for retrieving COT_{IPA} based on cloud reflectance;
- Figures 3 – 7 and 11 – 14 have been updated.

I have only very few very minor comments:

- I'd appreciate if a cross-ref to Appendix A (not only to TabA.1 therein) is added in the manuscript (somewhere appropriate Sec1-3)

R: We cross-referenced Appendix A at the end of Section 3 (Page 17, L434).

- Sec2: with addition of Appendix B, the limitation "four of which are discussed in this paper" (L174) seems not necessary anymore (and contradictory to the following 5-items list). Also it would be better to use Apps1-4 or panels (a)-(d) from Fig1 instead of "the first four [applications]". L207f is redundant to the 5-items list above. There's no fifth column in Fig1 (L216) anymore; rather refer to it as panel (e).

R: We removed the obsolete text and changed the "fifth column" to "panel e" (Page 9, Line 217).

- Fix typesetting of formula on L733.
R: Corrected (Page 25, Line 627).

- Fig9: rather "blacked filled" (or similar) to refer to SPN-S (I was at first stupidly looking for a black line...)
R: Corrected (Page 25, Line 636).

- Fig12: maybe adjust colorbar range or colorbar map to more clearly pronounce the IPA-CNN diffs again. They were clearer in previous Fig version, now get somewhat lost on the benefit of diffs to the obs (I appreciate the addition of the obs very much, though!)
R: We changed the colormap (Page 29, Line 730).

- L970: one of the App1-5 is redundant.
R: Corrected (Page 35, Line 872).

- Fig6 discussion: Could be worth to add to the manuscript (from response to Reviewer #2) that errors from fixed-SZA assumption are negligible.
R: Added (Page 21, Line 532).

- Fig14 discussion: Could be worth to add to the manuscript the explanation of the seemingly counterintuitive conclusion (as given in response to Reviewer #2)
R: Added (Page 26, Line 660, and Page 31, Line 775).