Report #2 Referee#1 (David Mitchell)

Checklist for reviewers
1) Scientific significance
Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)?
Outstanding **Excellent** Good Fair Low
2) Scientific quality
Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?
Outstanding Excellent **Good** Fair Low
3) Presentation quality
Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)?
Outstanding Excellent Good **Fair** Low
For final publication, the manuscript should be
accepted as is
accepted subject to technical corrections
accepted subject to minor revisions
reconsidered after major revisions
rejected

Were a revised manuscript to be sent for another round of reviews:
I would be willing to review the revised manuscript.
I would not be willing to review the revised manuscript.

Suggestions for revision or reasons for rejection
(visible to the public if the article is accepted and published)
GENERAL COMMENTS:

The manuscript has been greatly improved and, in general, the authors have done a great job in response to the review comments I have made. However, they mention Section 2.4 titled "Approximation of radiative transfer in the thermal infrared", which somehow was not included in the revised version of this manuscript. This section is critical since it describes how radiation transfer is treated in the thermal infrared (TIR) spectrum; it is needed to properly understand the TIR results of this sensitivity study. This manuscript should not be published without it.

SPECIFIC COMMENTS:

1. On comment #7 from 1st review:
Equation 13: Is this equation used in libRadtran? If not, what is the point in mentioning it? Cloud property input to libRadtran consists of IWC and re, suggesting the zero-scattering approximation might be used for TIR hemispheric fluxes: $\varepsilon = 1 - \exp(-5\tau_{abs}/3)$ where ε is cloud emissivity and τ_{abs} is the cloud absorption optical depth. Please indicate whether ε is calculated in libRadtran, and how it is calculated if applicable.

Author response: The DISORT solver in libradtran (Buras et al 2011) calculates scattering in the TIR on basis of the bulk-scattering properties of ice crystals, analog to the solar wavelength range. Thus, the zero-scattering approximation is not used in the simulations. Equation 13 was added to the manuscript to provide guidance for the reader. To avoid misinterpretation the equation is brought into context and is expanded to section "2.4 Approximation of radiative transfer in the thermal-infrared", to incorporate suggestions from other Reviewers.

Referee comment for 2nd review: The author response above is puzzling since the Referee is finding no section 2.4 titled "Approximation of radiative transfer in the thermal-infrared" in the revised manuscript nor in the track-changes version (the diff file) of the manuscript. In the current revised manuscript, there is no discussion of how RT in the thermal infrared (TIR) is dealt with, which is critical for a RT sensitivity study presenting results in both the solar and TIR. Since the authors mention Sect. 2.4 in their response having the title "Approximation of radiative transfer in the thermal-infrared", it appears that this section was mistakenly omitted from the manuscript. The manuscript should not be published without this section.

Please see the answer to the following comment.

2. On comment #8 from 1st review:
Lines 209 – 213 and Eq. 14: Eqn. (14) appears flawed since, in principle, there should be an emissivity term (ε) for both the surface and the ice cloud. But since typically ε ≈ 1 at the surface, does ε in (14) correspond only to the ice cloud? If so, it would be incorrect to multiply it by Tsfc4 (which Eq. 14 does). Later, ΔFtir is shown for IWC, re, and ice crystal shape, so it appears that ε refers to the ice cloud and therefore ε < 1, but how then does ε depend on IWC, re and ice particle shape? The dependence of ΔFtir on cloud properties is a complete black-box mystery and this needs to be explained.

Author response: As mentioned in our reply to comment 7, a dedicated section for TIR RT was added to the manuscript. It is primarily based on the TIR RT approximation given by Corti and Peter (2009). Equation 14 is now replaced by Eq. 20. Major steps to derive Eq. 20 are given in the manuscript; details can be found in Corti and Peter (2009).

Referee comment for 2nd review: Same as above regarding comment #1.

The section "Approximation of radiative transfer in the thermal-infrared" is re-introduced. The section provides a brief outline of TIR radiative transfer that helps to understand and interpret the results related to the TIR radiative effect. We would like to direct the Referee to the diff file to avoid copying the entire section into the point-by-point response.

3. Figure 4d: The dot-dash curve showing the absolute difference in ΔF between plates and aggregates appears flawed for IWC > 0.02 g/m3, assuming Fig. 4a is correct. Perhaps I have overlooked something, but in Fig. 4a for θ = 30° and re = 25 μm (dot-dashed), ΔFsol appears fairly constant between plates and aggregates for IWC > 0.02 g/m3, indicating that their absolute difference in Fig. 4d should be approximately

constant for IWC > 0.02 g/m3 (with the dot-dash line being approximately horizontal). If there is such an error, this will affect Fig. 4f as well. The other curves look reasonable, as well as the curves in Fig. 4g.
I now see that Fig. 4a and Fig. 3b are different, although they should be the same if I understand correctly. The curves plotted in Fig. 3b appear consistent with those in Fig. 4d, suggesting that Fig. 4a is flawed.

The Referee correctly noted that Fig.4a was flawed. The plot has been corrected and replaced, and is now consistent with Fig. 3.

4. Lines 391-395: Manfred Wendish wrote a paper on this topic in JAS(?) around 2008 I'm guessing.

The Referee is right. An explanation and the following reference were added: Wendisch, M. / Pilewskie, P. / Pommier, J. / Howard, S. / Yang, P. / Heymsfield, A. J. / Schmitt, C. G. / Baumgardner, D. / Mayer, B., Impact of cirrus crystal shape on solar spectral irradiance: A case study for subtropical cirrus, 2005, J. Geophys. Res. Atmos. , Vol. 110, No. D3

This is supported by earlier observations and simulations for example by Wendisch et al (2005).

5. Lines 398-399: The decreasing order at re = 5 μm (droxtals, plates, aggregates) changes when re is larger to droxtals, aggregates and plates in Fig. 4b.

The Referee is correct. The sentence has been corrected.

With increasing crystal size the order changes to droxtal, aggregates, and plates, and the absolute values of ΔF_{TIR} *decrease.*

6. Lines 410-11: "relative differences exceed the absolute value by a factor of 10." How is this evident from the two plots where one is unitless and the other has units?

The Referee is right. The sentence has been phrased incorrectly and has been modified. The new sentence reads:

The largest relative deviations are found for the optically thinnest clouds, where ΔF_{net} *is generally small. In these cases of optically thin clouds consisting of the smallest crystals (*r_{eff} *= 5 μm), the relative deviations exceed the relative difference for optically thick clouds with the same crystal size by a factor of 10.*

7. I did not have time to carefully review the sections that came after Sect. 3.1, and the authors are encouraged to do so due to the above comments pertaining to Sect. 3.1 (#s 3 – 6) and the technical comments below.

We have taken note of the comment and have thoroughly reviewed the remaining part of the manuscript for errors. We would like to direct the Referee to the pdf diff file.

TECHNICAL COMMENTS: Line numbers correspond to the revised manuscript.

1. Line 90: The net RE given by => The net RE is given by?
The Referee is right and the sentence has been corrected.

2. Lines 93-94: Redundant portion of sentence.
The redundant section has been removed.

3. Line 103: Although the meaning of TIR might be inferred from lines 91-92, it is customary to explicitly state its meaning, like "The thermal infrared radiances (TIR) include …"
We agree with the Referee and now explain the abbreviation.

4. Line 175: Are you sure you want Λ = − 1/(a·b) since this would make the exponent in (4) positive?
The minus sign has been removed.

5. Equation 10: Since you are approximating τice for solar radiation only, this equation can be further simplified by noting Qe ≈ 2.
The Referee is right. We added Qe ≈ 2 and simply the equation further.

6. Line 212: "The altitude of 1500 k was selected" => The altitude of 1500 m was selected?
The typo was corrected and the unit was changed to: 1500 m

7. Line 378-9: Mitchell (1996) => Mitchell (2002)?
The Referee is right. The citation has been corrected.

8. Line 389: 50 W m-2 looks reasonable for plates at re = 5 μm, but I think this discussion is relating droxtals to aggregates, in which case the number looks closer to 25 W m-2 for IWC = 0.024 g m-3.
The Referee is right. The values has been correct to 27 W m^{-2}.

Report #2 Referee#2 (Andreas Macke)

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For final publication, the manuscript should be
accepted as is
accepted subject to technical corrections
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reconsidered after major revisions
rejected

Were a revised manuscript to be sent for another round of reviews:
I would be willing to review the revised manuscript.
I would not be willing to review the revised manuscript.

Suggestions for revision or reasons for rejection
(visible to the public if the article is accepted and published)
I still think that the consideration of a water cloud under the cirrus cloud is rather arbitrary and basically physically accounted for by the variation of the radiation quantities surface albedo and temperature. I found the arguments for keeping this cloud configuration not fully convincing. I leave the decision on this to the editor.

We would like to comment on this advice with the following arguments.

One argument for an additional cloud layer is that the cloud albedo is spectrally different from the surface albedo as we assume a constant surface albedo for all wavelength. By including the liquid water cloud layer, we achieve consistency between the surface, the liquid cloud and the ice cloud temperatures, which would not be the case by simply varying the surface temperature.

Report #2 Referee #3 (anonymous)

Checklist for reviewers
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Outstanding Excellent Good **Fair** Low
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Outstanding Excellent Good **Fair** Low
3) Presentation quality
Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)?
Outstanding Excellent Good **Fair** Low
For final publication, the manuscript should be
accepted as is
accepted subject to technical corrections
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I would be willing to review the revised manuscript.
I would not be willing to review the revised manuscript.

Suggestions for revision or reasons for rejection
(visible to the public if the article is accepted and published)
The authors have addressed several points raised in the previous review. Yet, two key issues that were previously raised have not been appropriately addressed. Additionally, there are several other remaining issues that need to be resolved before the publication can proceed. In light of this, I strongly recommend consulting with both the libRadtran team and the authors of the reference studies (cf. (A)) in order to effectively address and resolve these outstanding concerns.

(A) The manuscript is missing a (quantitative and more detailed) discussion of the results with previous studies which are mentioned in the introduction (Fu and Liou (1993), Yang et al. 2010, Zhang et al. 1999, Mitchell et al. 2011, Schumann 2012, Meerkötter et al. 1999). It is stated that the presented study builds upon and is motivated by Meerkötter et al. 1999. Even the central Figure 2 is adapted from this study as stated in the figure caption. References to this study and the other central publication from Schumann et al. 2012 have been added in response to the previous review, but remain all qualitative (cf. lines 349, 412, 582). A clear motivation is missing why a new study is needed and what the improvements or new insights are compared to previous results.

General comment on the more quantitative comparison:

The requested more quantitative comparison with the cited literature is complicated or impossible as the data sets used in these studies are not publicly available. Only the parameterizations by Schumann et al (2012) might be used to re-create the fir from Schumann et al (2012) that could be compare it with our results. This was partly done in the Community comment (https://doi.org/10.5194/egusphere-2023-155-CC1) by Schumann et al., which showed good agreement between our simulations and the parameterization. However, I was not our intention to replicate or recreate previous studies. The motivation for this study was to provide a parameter-based sensitivity study and an publicly available data set that can be used for various potential applications.

We followed the suggestion of the Referee and provided a more quantitative comparison where possible. This comparison is limited to a few sections as our simulated clouds deviate from the cloud setup in Meerkoetter et al. (1999), particularly the geometric thickness. Even Meerkoetter et al. (1999) states that:
"Our results depend also strongly on the assumed IWC, the geometrical thickness of the contrails, and the particle sizes of the contrails, all together controlling the optical depth of the contrails. Contrails could have a factor of 3 smaller IWC than assumed in this study (see Fig. 1), may be geometrically thicker by a factor of 3 (Freudenthaler et al., 1996; Sassen, 1997), and the particle size is known, at best, to a factor of 2, implying an uncertainty in optical depth of about factor 4 (based on the square of the sum of the individual uncertainty factors). The contrail cover value is estimated to be uncertain by a factor of about 2. Hence, the given amount of radiative forcing by contrails is uncertain by a factor of about 5, mainly because of uncertain contrail cover and optical depth values."

1. Line 349: "The presented analysis of solar, TIR, and net ΔF sensitivity on the selected input parameters generally agrees with the results from Meerkötter et al. (1999). We found differences in the importance of the parameters, which are explained by the fact that our simulations span a larger and different parameter range, for example in reff and Tsrf. In addition, the sensitivity analysis in Fig. 2 is sensitive to the selection of the reference cloud."

Concerning Line 349:
Selecting parameters from our parameter space that are most similar to the cloud case (A) from Meerkoetter et al (1999) yield a change in the importance of the parameter. While we found R_{eff} to be more important than IWC for dF_{sol} in our study, selected parameter ranges similar to Meerkoetter et al (1999) changed the order of importance of the parameters, with IWC becoming more important than R_{eff}.

A major difference between Meerkoetter et al (1999) and our study is the cloud geometric thickness. Meerkoetter et al (1999) used an IWP of 4.4 g m^{-2} and an IWC of 0.021 g m^{-3}. Assuming a homogeneous distribution of IWC, this leads to an approximate cloud geometric thickness of 210 m. In our case the simulations have been performed for a cloud geometric thickness of 1000 m. Matching IWC and reff of our simulations to yield a similar cloud optical thickness of 0.52 is only valid for the solar wavelength range. The cloud geometric thickness will influence the results in the TIR wavelength range and subsequently in the net cirrus radiative effect.

2. Line 412: "The analysis of all simulations shows that the shape assumption has only second-order implications on the RE compared to other parameters like IWC or reff (see Fig 2), which agrees with Meerkötter et al. (1999)."

The qualitative description is extended by a more quantitative one. The section has been extended as follows:

The analysis of all simulations shows that the crystal shape assumption on the cirrus RE is small compared to other parameters particularly IWC or r_{eff} (see Fig 2). However, we found a larger variability in ΔF_{sol} and the resulting ΔF_{net}, i.e., whether a contrail has a net warming or cooling effect compared to ΔF_{tir}. For the defined reference consisting of aggregates, a ΔF_{sol} of -50.2 W m^{-2} was simulated, while for plates and droxtals values of ΔF_{sol} of −8.6 and −44.3 W m^{-2} were obtained, respectively. The impact of the crystal shape is less pronounced in the TIR wavelength range with ΔFt_{ir} of 46, 44.5, and 48.9 W m^{-2} for aggregates, plates, and droxtals, respectively. The variation of ΔF_{sol} propagates into ΔF_{net} with −4.2, 35.9 and 4.6 W m^{-2} for aggregate, plates, and droxtals, respectively. Based on the presented simulations, we found larger maximum variations in ΔF_{sol}, ΔF_{tir}, and ΔF_{net} of 41.6, 4.4, and 40 W m^{-2}, respectively, compared to Meerkötter et al. (1999). They found variations of ΔF_{sol}, ΔF_{tir}, and ΔF_{net} of 2, 6, and 7 W m^{-2}, respectively. The difference are explained by the selected reference (Meerkötter et al., 1999). However, selecting cloud parameters similar to the reference cloud of Meerkötter et al. (1999), we still found larger maximum variations ΔF_{sol}, ΔF_{tir}, and ΔF_{net} of 17.3, 4.2, and 17.9 W m^{-2}, respectively. This is attributed to the remaining differences among the selected reference values.

3. Line 582: "While Meerkötter et al. (1999) showed that solar, TIR, and net ΔF are only slightly sensitive to changes in dz – under the premise of a constant ice water path (IWP) – the present simulations indicate that the effects on ΔFtir and particularly ΔFnet have to be considered."

Concerning line 582:
The section was extended and we quantify the effects from varying d*z.* The following sentences were modified and added:

This partly agrees with the findings from Meerkötter et al. (1999) who showed that solar, TIR, and net ΔF are only slightly sensitive to changes in dz with solar, TIR, and net ΔF below 2 W m^{-2}, under the premise of a constant ice water path (IWP). The presented simulations indicate ΔFsol of 2 W m^{-2}, which are comparable to Meerkötter et al. (1999), but we found slightly higher ΔFtir and ΔFnet of 4.5 and 3.1 W m^{-2}, respectively.

(B) A clear statement of the intended use of the dataset together with assumptions made for the radiative transfer simulations and their impact on the accuracy of the results is missing. What is the intended use case of this dataset beyond the presented sensitivity study? What are possible applications or scientific questions that can be answered with the data?
The following statement was added in response to the previous review (line 76): "The data set might be coupled with cloud microphysical models, e.g, the Contrail Cirrus Prediction Tool (CoCiP) from Schumann (2012), to estimate the RE of the simulated

contrails." If this is the intended use case, please provide more details on how CoCiP works and how it can be coupled with this dataset.

The look-up-table is intended to estimate the contrail radiative effect based on the eight-dimensional parameter space. Therefore, CoCiP was mentioned as a potential user-case example. However, to avoid to be too prescriptive, we rephrased the manuscript and provide a more generic example as we hesitate to imply or determine the usage of the data to the users. Quite the opposite. We are interested in new applications that we cannot think of. In the light of this we argue that the look-up-table can be coupled together with models of any complexity as long as the model output agrees with the dimensions of the data set.

The text is rephrased as following:

The look-up-table could in fact be coupled with models of any complexity, as long as they simulate the dimensions of the data set, namely: solar zenith angle, ice cloud temperature, surface albedo, ice water content, surface temperature, ice crystal effective radius, and liquid water cloud optical thickness.

(C) Technical issues regarding radiative transfer setup and results:
1. Effect of neglecting 3D radiative transfer:
a. While a discussion about 3D effects has been added, it is important to mention in the abstract and summary that this study is based on 1D radiative transfer.

We agree with the Referee and now mention that the simulations were performed with a 1D radiative transfer solver.

[...] In total, 283,500 plane-parallel radiative transfer simulations have been performed, not including three-dimensional scattering effects. Parameter ranges are select that are typically associated with natural cirrus and contrails. In addition, the effect of variations in the relative humidity profile and the ice cloud geometric thickness have been investigated for a sub-set of the simulations. The multi-dimensionality and complexity of the 8-dimensional parameter space makes it impractical to discuss all potential configurations in detail. Therefore, specific cases are selected and discussed. [...]

[...] The RT simulations were performed with a 1D solver (plane-parallel clouds) and 3D scattering effects were not considered despite the fact they become relevant become relevant for large solar zenith angles (θ > 70◦.) [...]

b. The study by Gounou and Hogan was not cited correctly: Line 624: "…found differences in contrail solar RE between 1D and 3D simulations of up to 40%. These values were found for extreme cases, e.g., large solar zenith angle (Sun close to the horizon)." Significant differences were found not only for extreme cases:
--> The summary states: "The horizontal photon transport increases the longwave RF of contrails by around 10%". In the shortwave "the horizontal photon transport weakly decreases the magnitude of the RF by around 5%" and up to 30% for solar zenith angles >70deg. "When we consider the net RF […] the effect of the horizontal photon transport becomes important." Please correct accordingly.

The section has been rephrased to the following:
[...] This study was followed by Gounou and Hogan (2007) and Forster et al. (2012), who used 3D Monte Carlo simulations and found differences in contrail solar RE between 1D and 3D simulations ranging from 5 to 40 %. The largest deviations were found for extreme cases, e.g., large solar zenith angle (Sun close to the horizon). With the Sun illuminating the contrail or cirrus from the side, extinction and absorption within the cloud increases and scattering at cloud sides becomes more important compared to an illumination from above. Enhanced scattering at cloud sides also increases the likelihood that photons get scattered back into space instead of being absorbed. Such effects are not captured by 1D RT simulations. Concerning the TIR wavelength range, Gounou and Hogan (2007) found that horizontal photons transport can increase contrail radiative effect by around 10 %, which has to be considered in the calculation of the contrail net radiative effect.

c. Line 645: “Only few studies are available on cirrus 3D effects, e.g., Hogan and Kew (2005); Gounou and Hogan (2007).” Should this sentence be deleted?

This sentence was accidentally kept in the manuscript and is now removed from the text.

2. Radiative transfer simulations with DISORT:
a. Line 105: DISORT (Buras et al. 2011) is not original author, please cite original author in addition:
Stamnes, K., Tsay, S.-C., Wiscombe, W., and Jayaweera, K.: Numerically stable algorithm for discrete–ordinate–method radiative transfer in multiple scattering and emitting layered media, Appl. Opt., 27, 2502–2509, 1988.

We thank the Referee for providing this reference and have added it to the manuscript.

b. The use of the solar and thermal spectral range is inconsistent throughout the manuscript. The introduction (line 38) mentions a range from 0.2-3.5 µm and 3.5-100 µm, respectively. Table 3: 0.3–3.5 µm (solar) & 3.5–75 µm (thermal-infrared). Why do the simulations not cover the full range up to 100 µm?

The exact wavelength range that is covered by the term ‘thermal-infrared’ is not exactly defined in the literature and can be debated. For example, Petty (2006)* describes the thermal infrared band as located between 4 and 50 µm, and the far IR band between 50 and 1000 µm. However, we acknowledged the commonly used agreement of the terrestrial radiation reaching up to 100 µm. But, as outlined in the manuscript, the wavelength range from 3.5 to 75 µm covers 99.3% of the 3.5 to 100 µm range and, in our opinion, can be regarded as equivalent.

(*) Petty, G. W.; A first course in Atmospheric Radiation, 2006, Sundoc Publishing

c. Line 112: Please explain why REPTRAN “coarse” mode is justified for this application and show that it provides sufficient resolution compared to “medium” and “fine” mode (if needed, consult with libRadtran team).
REPTRAN ‘coarse’ provides a spectral resolution of 15 cm^{-1}, which corresponds to $\Delta\lambda$ =0.41 nm (at 550 nm) and $\Delta\lambda$ =3.5 µm at (50 µm).

We would like to direct the Referee to Fig. 3.7 on page 47 of the libRadtran Documentation (version 2.04). Figure 3.7 shows the different spectral resolutions of REPTRAN coarse, medium, and fine. As given in these examples, the resolution ‘coarse’ resolves the major features of the spectrum and, therefore, we argue that coarse is sufficient for broadband irradiance simulations.

We further argue that the ‘coarse’ resolution is sufficient for broadband irradiance applications while acknowledging that higher spectral resolutions are required for spectrally resolving radiance simulations. Furthermore, when calculating solar, TIR, and net radiative effect as differences between cloudy and cloud-free simulations, effects and potential errors from molecular absorption and due to the choice of spectral resolution from libRadtran partially compensate.

http://www.libradtran.org/doc/libRadtran.pdf (last access: June 28th, 2023)

d. Table 3 states “Molecular absorption: Fu and Liou (1992, 1993)”. This is inconsistent with the earlier statement of REPTRAN. Please correct/clarify.

The table has been corrected according to the Referee’s comment and we have replaced the former citation with the Gasteiger et al. (2014) reference.

e. Line 130: Surface temperatures Tsrf are set to −15.8◦C (subarctic winter), 15.05◦C (US standard), and 26.5◦C (tropical), respectively, to match the lower most temperature in the atmospheric profiles. Why is the additional selection of surface temperature necessary? The lowest temperature should coincide with surface level?

The sentence was rephrased for better clarity to the following:
[...] *Surface temperatures Tsrf of −15.95◦C (subarctic winter), 14.85◦C (US standard), and 26.55◦C (tropical) are defined in libRadtran by the lower most temperature in the APs.* [...]

f. Sample input file: why 257.2 K for surface temperature? This is not consistent with the parameters provided in the manuscript (e.g. Tab. 4).

The value in the table has been corrected to 257.2 K.

g. Line 134: “The cirrus cloud top temperatures T_cld,ice are selected to span the temperature range in which contrails and cirrus typically form (Krämer et al., 2020). Here we cover a range from 219 to 243 K. The resulting ice cloud top altitudes z_ice,CT are set to the altitude, where the temperature in the APs equals the desired T_cld,ice”. How are the cloud top altitudes computed? Linear interpolation? Please clarify.

Following the suggestion of the Referee a sentence was added to better explain how the altitude was determined.

[...] Here we cover a range from 219 to 243 K. The resulting ice cloud top altitudes zice,CT are set to the altitude, where the temperature in the APs equals the desired Tcld,ice. zice,CT was found by linear interpolation between the altitude and temperature levels. [...]

h. Table 1: Cirrus pressure/altitude do not match the values. The labels seem to be swapped. Consider reducing the information to the essential numbers to make the table easier to understand, e.g. provide temperatures in [K] only.

The labels have been swapped and moved in the table to appear directly over the respective values. The table should be more clear now. We would like to keep both units of K and ºC, as degree Celsius is frequently used in literature about cirrus observations, while libRadtran uses K.

i. Why does the provided uvspec input file compute results at several altitudes? It is unclear why this is necessary and might even slow down the computation.

For purposes not relevant for the study, the output was specified for multiple layers. To not confuse the reader the additional layers have been removed from the example script.

j. Please provide a sample input file for the thermal spectrum and cloud file as well. From the information provided in the manuscript it is not possible to reproduce the results.

We followed this remark and now provide an additional example input file for the thermal-infrared wavelength range. Input files for clouds can easily be created using the libRadtran manual and do not require an example. The idea of providing an example input script is to be transparent and to provide a guideline that might be used as a template by a reader, and not to provide a copy and paste ready model configuration.

3. Choice of ice crystal properties:
a. Please use the official description as provided in Yang et al. 2013 when referring to these ice crystal properties. It is not clear which ice crystal shapes and roughness levels are used in this study. Yang et al. 2013 provide each habit in 3 different roughness levels. The sample input file hints at the choice of "moderately rough aggregates of 8-element columns". Please double-check throughout the manuscript.

Aggregates consisting of '8-element-columns' were used from Yang et al. (2013). This has been clarified in the manuscript in Sec. 2.2 Radiative transfer simulation set-up. Later in the text the term 'aggregates' is used synonymously for '8-element-columns'.

b. Line 151: Forster and Mayer (2022): "…found that cirrus are frequently comprised of mixtures of rough-aggregates and plates". This reference is not correct, the abstract states: "mixtures of smooth and rough column aggregates". Please correct.

The sentence and this reference have been removed. The reference Forster et al (2022) was added to the previous sentence.

Three different ice crystal shapes, namely: i) '8–column–aggregates' (called 'aggregates' thereafter), agglomerations of 8–columnar ice crystals; ii) 'droxtals', almost spherical ice crystals; and iii) 'plates' are used. These three shapes are selected to represent different stages in the temporal evolution of contrails. Several airborne in situ measurement *campaigns that targeted cirrus and contrails imply that aggregates are the dominating ice crystal habit (Liu et al., 2014; Holz et al., 2016; Järvinen et al., 2018). For example, Järvinen et al. (2018) found that 61 to 81 % of the sampled ice crystals were aggregates with a rough surface. Such ice crystals are also assumed in current remote sensing applications of ice cloud, e.g., in the re-defined ice optical properties used by the Moderate Resolution Imaging Spectroradiometer (MODIS) Collection 6 product (Yang et al., 2013; Holz et al., 2016; Platnick et al., 2017; Forster and Mayer, 2022). Therefore, we selected 8–column–aggregates as the primary ice crystal habit. The second most observed habit are plate-like ice crystals (Holz et al., 2016; Forster et al., 2017; Järvinen et al., 2018), which are included in the simulations as a second shape. The 'droxtal' parameterization is selected to estimate ΔF of young contrails, which primarily consist of near-spherical ice crystals (Goodman et al., 1998; Lawson et al., 1998; Gayet et al., 2012). We emphasize that contrails can be comprised of other ice crystal shapes, like single columns, hollow columns, 3D bullet rosettes, or mixtures of these (Lawson et al., 1998; Baum et al., 2005a), but the simulated shapes cover the majority of observed cirrus situations. The utilized ice optical properties of the three selected shapes are based on the parameterization from Yang et al. (2013) that assume randomly oriented ice crystals with a 'moderate' surface roughness.*

c. Figure D1 shows phase functions for smooth ice crystals but in the text, "rough" crystals are mentioned. Please use the official description from Yang et al. 2013 and show the phase functions which are actually used to compute the look-up table.

As described in the previous comment, we used 8-column-aggregates, droxtals, and plates with moderate surface roughness in the simulations. This has been checked throughout the manuscript.

Three different ice crystal shapes, namely: 8--column--aggregates (called 'aggregates' thereafter), agglomerations of 8--columnar ice crystals; 'droxtals', almost spherical ice crystals; and 'plates' are used. [...]

4. Figure 2:

a. It is not clear how the variation of F_net, tir, sol in Figure 2 are achieved: are the remaining parameters fixed to the "reference case" or are they averaged as stated in line 281: "This strategy can be interpreted as a type of sub-sampling, by averaging all unfixed parameters to project ΔF onto the one-dimensional space."? Please clarify in the manuscript.

Please see the next response to comment b)

b. If the variation is investigated by "averaging all unfixed parameters", it is not clear what the role of the reference cloud is.

The Referee is right. The phrase "averaging all unfixed parameters" was incorrectly positioned, which caused the misinterpretation. The section was shifted to the beginning of subsection "Sensitivity on solar zenith angle and surface albedo", as the averaging of all unfixed parameters is applied to create Figure 5 and the following ones. The reference is only used to create and discuss Fig.2.

We have also refrained from the term ‘reference cloud’, but call it reference. The reference is defined by selecting the minimum or maximum of the parameter range, except for IWC. An intermediate IWC was selected to obtain a cloud optical thickness of 0.48 (at 550 nm) that is representative for contrails. Selecting an intermediate IWC was required as using either the minimum or maximum IWC would lead to almost no cloud (smallest IWC) or a cloud with an optical thickness well exceeding 30 being unrepresentative for contrail or cirrus. In contrast, all other parameters, like solar zenith angle, R_{eff}, or surface albedo, from their minimum to their maximum can occur for cirrus clouds.

The text, describing the reference, was rephrased in the following way:

[...] To reduce the multi-dimensionality, for each of the eight parameters a reference is defined by selecting either the minimum or maximum value from the parameter space. The reference parameters are selected to highlight the upper or lower range of each parameter and the spanned variation, and to define the reference for the fixed parameters. The reference parameters are given by $\theta = 0^{\circ}$, $T_{cld,ice} = 219$ K, $\alpha_{srf} = 0$, $T_{srf} = 299.7$ K, $r_{eff} = 85$ µm, and $\tau l_{iq} = 0$ (no liquid water cloud). For IWC we use an intermediate value of 0.024 g m^{-3} because together with a dz of 1000 m and an reff of 85 µm this leads to a τ_{ice} of 0.46 at 550 nm wavelength, which is representative for contrails and young cirrus (Iwabuchi et al., 2012). Otherwise selecting the minimum or maximum IWC in combination with reff of 85 µm would lead to high or low τ_{ice} that are not representative for contrails. For ice crystal shape, we select aggregates as the reference. We particularly emphasize that the defined references are not representative for any particular cloud situation, but are a useful point of comparison to assess the impact of a given parameter on the diversity of cloud RE.

c. The choice of parameters for the reference cloud is inconsistent: while for the IWC, a value “representative for contrails and young cirrus” (line 292) is selected, an effective radius of 85 µm seems quite extreme. A more representative choice would be 20 µm, for example.

85 µm is the maximal R_{eff} value that can be simulated with the default configuration of libRadtran. However, contrails and cirrus can be composed of ice crystals that are much larger. Sizes of up to several hundred µm have been observed. Therefore, 85 µm cannot be regarded as extreme.

d. Similar, a surface albedo of 0 (completely black surface) is not very realistic. For ocean a value of 0.2 would be more representative.

A value of 0.2 as the lower minimum and for open ocean is debatable. The albedo of open ocean, depending in wind conditions, can reach values of 0.08 or even lower. As pointed out, we wanted to cover the full (theoretical) albedo range from 0 to 1.

Literature that supports low sea surface albedo (from libRadtran user manual):
Cox, C. and Munk, W.: Measurement of the roughness of the sea surface from photographs of the sun’s glitter, Journal of the Optical Society of America, 44, 838–850, 1954a.

Cox, C. and Munk, W.: Statistics of the sea surface derived from sun glitter, Journal of Marine Research, 13, 198–227, 1954b.

Nakajima, T. and Tanaka, M.: Effect of wind-generated waves on the transfer of solar radiation in the atmosphere-ocean system, J. Quant. Spectrosc. Radiat. Transfer, 29, 521–537, 1983.

e. Fig. 2c reff 85 and 5 µm labels seems to be swapped. Please double-check.

The Referee is right. The labels have been corrected and the figure was replaced.

f. Compared to Meerkötter et al, the visualization here is dominated by the parameter range for reff, making it almost impossible to visually resolve variations in F_net, tir, sol for the remaining variables.

We partly agree with the Referee and elongated the figure to improve the legibility of the figure. However, the large bar from R_{eff} in relation to the other bars is also a direct indicator of the relevance of each parameter considering the typical parameter range. Consequently, the importance of each individual parameter, in relation to the others, is directly visible in the figure.

5. Provided dataset
a. Cloudy_cloudfree variable is defaulted to 9.96921e+36 for both values. A more intuitive variable name would be cloud_fraction with values 0 and 1, referring to clearsky and cloudy, respectively. The meaning of these values can be added to the attributes.

We follow the suggestion of the Referee and renamed the variable to cloud_fraction. The variable cloud_fraction is now correctly implemented in the data-set.

b. Tau: not specified at which wavelength, this should be added to the attributes.

It is not clear whether the Referee is concerned with the labeling of tau for the liquid or ice water cloud. However, the labeling of the data set was revised and the wavelength information (550 nm) was added to all ice and liquid water cloud optical thickness fields.

c. Why does tau have dimensions ('solar_zenith_angle', 'ice_cloud_temp', 'surface_albedo', 'ice_water_content', 'surface_temperature', 'crystal_effective_radius', 'optical_thickness_liquid_water_cloud')? It should only depend on IWC and effective radius, that way saving storage space.

For consistency with the size of the other data fields and for convenience in reading the data, the dimensions of the tau array have been kept. The full data set, containing three files with several thousands of entries, and a total size of 15 MB, is surely manageable in terms of storage space.

d. How is the optical thickness computed/derived? From libRadtran directly, or using the approximation provided by Eq. 10?

All values of cloud optical thickness were directly extracted from the libRadtran verbose files.

The ice cloud optical thickness τice at 550~nm wavelength is directly obtained from the libRadtran verbose output using optical properties of droxtals.