Reply to Reviewer #2 (Andreas Macke)
(Referee comment on "Radiative effect by cirrus cloud and contrails – A comprehensive sensitivity study" by Kevin Wolf et al., EGUsphere, https://doi.org/10.5194/egusphere-2023-155-RC2, 2023)

We thank the Reviewer Andreas Macke for the time he spent on the manuscript. The comments helped to improve the manuscript, but more importantly spurred us into repeating our calculations with (1) a completely revised libradtran configuration to ensure that we use state-of-the-art parametrization; and (2) much extended parameter ranges to be better representative of cirrus and contrails. The discussion in the manuscript has been revised to reflect the new calculations and analyses. In the following, the Reviewer's comments and the corresponding responses are listed. The page and line references given by the Reviewer relate to the manuscript in discussion. Numbers given from our side relate to the revised manuscript.

For better legibility, the Reviewer's comments are highlighted in **bold** and changes in the manuscript are in *italic*.

General remarks:
The manuscript describes an impressive sensitivity study (very nicely summarized in Figure 2, indeed!)) on the importance of the governing physical parameters of cirrus clouds and contrails on their radiative effects in the climate system. Numerous studies on the influence of various parameters already exist, but not on this scale presented here. The authors also largely correctly refer to the previous literature, but I would have liked to see a somewhat more quantitative presentation here. A table roughly summarizing the parameter variations and effects on the radiation effect of previous work could be helpful.

I understand that even 94,000 radiative transfer simulations cannot cover all cases of real-world clouds and illumination geometries. The authors should therefore make somewhat more prominent (not just at the end of the manuscript) which assumptions in their calculations constrain the phase space. This seems particularly important to me because, while the authors commendably make their data available for further radiative effect studies, there is then a danger that it will be used without further questioning. For example, ice clouds generally have a distinctive vertical structure of crystal sizes and shapes, which affects both solar reflectivity and thermal emission. Horizontal crystal orientation - as often observed - also has an effect, as does 3D radiative transfer for optically thicker ice clouds. Similarly, a crystal size distribution is always also a crystal shape distribution, so distinguishing clouds consisting of only one crystal shape is somewhat unrealistic. It is not for nothing that Baum et al. (2005) combined size and shape distributions to obtain more realistic optical properties. I realize that one cannot account for all of this in a large sensitivity study, but limitations should be clearly pointed out.

Some results are quite obvious, e.g. that the solar cooling effect is determined by the albedo differences of cloud and ground, and the warming effect by the temperature differences of cloud and ground. It is also not necessary to point out several times that

the solar parameters do not affect the terrestrial radiation effects and vice versa.

The study of a water cloud underlying the ice cloud seems somewhat contrived to me, see the specific references below.

Would it somehow be possible to reduce the number of figures, e.g. take only those whose results are referred to in the summary at the end? See also my comments below.

These general remarks are addressed below when they are repeated in the specific remarks.

Specific remarks:

line 54-56: I agree that liquid water clouds have simpler microphysics. However, this simplification is perhaps surpassed by the problem of 3D radiative transfer in such clouds.
Therefore, I would not say that radiative transfer in cirrus clouds is more complex.

The sentence was rephrased to specify that we refer to the direct interaction of radiation and particle.

> *“To estimate the radiative impact of a cloud as well as related potential uncertainties and sensitivities, RT simulations represent a helpful tool. While the atmospheric RT in liquid water clouds composed of spherical cloud droplets can rely on geometric optics or Mie-scattering theory (Mie,1908; van de Hulst, 1981), RT simulations of ice particles are made complicated by the non-spherical shape and the interaction with the incoming radiation, i.e., through their single-scattering phase function. The single-scattering phase function, for example, has to be determined by computationally-expensive methods, like ray tracing (Bi et al., 2014), Monte Carlo simulations (Macke et al., 1996a, b), or the T-matrix method (Mishchenko, 2020).“*

68-69: Why distinguish between sensitivity to size and to size distribution?

The Reviewer is right. Zhang directly refers to ice crystal size distribution, while Mitchell refers to the effective radius, which is determined by the ice crystal size distribution. Therefore, both studies investigate the impact of the ice crystal size distribution on the radiative forcing / effect of cirrus clouds. The sentence has been rephrased.

> *“The effect of the ice crystal size distribution on cloud radiative forcing / effect was analyzed, for example, by Zhang et al. (1999) or Mitchell et al. (2011).”*

104-105: ...but then you need to show/cite that 2d or 3d variability is not a driving parameter. And the present work is not even 1d (vertically resolved), but 0d (plane-parallel homogeneous).

Thank you for this helpful comment. In line with the comments from Reviewer 3, the potential uncertainties due to 3D scattering effects and heterogeneous clouds are now mentioned and referenced in the sections “Introduction” and “Radiative transfer simulation set-up”. Due to the length of the new paragraph in the Introduction we would like to direct the Reviewer to the diff file. The second paragraph is given below.

> *“The RT simulations are performed with the 1D solver DISORT (Buras et al., 2011), which is part of libRadtran. Clouds are assumed to be horizontally uniform and lateral photon transport between columns is neglected, which is called the independent pixel approximation (IPA, Stephens et al., 1991; Cahalan et al., 1994). As the main objective of this study is to map the basic dependencies of ΔF on the driving parameters, we neglect any variability in the spatial ice water content (IWC) distribution that exists in cirrus (Minnis et al., 1999). We also restrict the simulations to fully cloud covered scenes. [...]”*

135-136: according to the title, the work is about cirrus and contrails. So, do the 3 shapes suffice for cirrus as well? Does the aspect ratio of the hexagonal particles varies with size?

Within this study we focused on three particle shapes that represent three stages of contrail development from almost spherical particles over plates – often used in remote sensing applications - to complex aggregates. We now provide a more detailed literature overview and some references that support the selected ice crystals shapes, which confirms that the selected shapes are representative for the majority of contrails and cirrus. Due to the length of the added section we direct the Reviewer to item 4 in section 2.2 ‘Radiative transfer setup’.

Table 3: I understand that some hard choices have to be made if one is to make sense of the parameter space of the physical properties of cirrus clouds. However, it seems to me that the range of only three cloud temperatures is very limited compared to the parameters that make up the optical thickness (IWC, r_{eff}). The cloud greenhouse effect is thus much more discretized than the albedo effect.

Similar to the selection of three particle shapes, the step size for each parameter had to be limited. Nevertheless, we extended the number of simulations. Now, five cirrus temperatures are simulated to better capture the effect changes in ice cloud temperature and altitude. Also the number of simulated solar zenith angles and effective radius were increased.

167: which r_min and r_max where chosen for the gamma size distribution?

According to Emde et al (2016), the bulk optical properties are calculated for r_{min} of 5 µm and r_{max} of 90 µm. This information is added to the manuscript.

> *“[…] For the gamma size distribution a minimum and maximum r_{eff} of 5 and 90 µm are selected. [...]”*

173: isn't the effect of an underlying cloud not somehow accounted for already by varying surface albedo and surface temperature?

We partly agree with the Reviewer. As similar comment concerning section 3.6 is raised below, we answer both comments below.

190-192 and eq. (11): This rearranging only work if r^3_vol is not a function of r. But I'd think that this parameter is very much a function of r.

The Reviewer is right. Incorporating additional Reviewer comments, the equation was modified. Equation 14 became Eq. 15 in the new manuscript. Please see the document and the latex diff file for the updated version.

220-221: what do you mean with "diagnosed by libradtran"? For a given size and shape, the extinction coefficient should be readily available, given that the extinction efficiency = 2 for large particles.

“Diagnosed’ is an inappropriate term and the sentence was rephrased. The values of tau is directly calculated by libRadtran and extracted from the verbose output.

> *“[…] The cloud optical thickness τ_{ice} at 550 nm wavelength, given in Fig. 1d–d, is directly calculated by libRadtran using optical properties from droxtals.“*

228: The term "observed" may be misleading as this is about modeling, not observations.

The sentence was rephrased.

> *“The inherent dependencies of […] ”*

Fig. 1: Since only theoretical relations between the dependent quantities N, IWC, r_{eff}, and tau are shown here, which are rather clear, one could omit this discussion and refer to a textbook on radiative transfer.

The intention of this subsection and figure was to provide a condensed overview of the basic dependencies that will help readers, who might not be familiar with radiative transfer and the interactions of N_{ice}, IWC, and r_{eff}, to better interpret the relations and figures that follow later in the paper. Therefore, we would strive to keep this section.

307: "To some extend" -> "For idealized hexagonal columns and plates"

Thank you for providing the more precise formulation. It is adopted in the manuscript together with the modifications considering other Reviewers comments. Please see the diff file for the revised section.

327-328: Macke and Großklaus is about rain drops :), you probably meant:
Macke A, Francis P-N, Mc Farquhar G-M, Kinne S (1998) The role of ice particle shapes and size distributions in the single scattering properties of cirrus clouds. Journal of Atmospheric Sciences 55 (17), 2874-2883.

The Reviewer is right and the citation was changed accordingly.

360-361. wrt the forward peak: The forward peak (0 degree scattering angle) is never directed upward. Are you refering to the forward scattering range?

The Reviewer is right. Here we do refer to the forward scattering range given by scattering angles <= 90 degree. The sentence was modified accordingly.

> *"[…] and iii) an increase in upward scattered radiation with increasing θ as the light rays get slanted and a larger fraction of radiation from the forward scattering range is directed upward."*

Figs. 5b and 6b can be omitted.

We partly agree with the Reviewer. While there is no impact of the solar zenith angle or the solar surface albedo on the TIR component, we would like to keep these plots to provide a systematic overview throughout all parameters and for symmetry with the discussion of the other parameters.

378: "IWC is the primary factor...": Not according to Fig 2 and your previous explanation that solar and terrestrial effects of IWC cancel out each other. Do I misunderstand something here?

The Reviewer is right the sentence is false and is now rephrased.

> *[...] IWC is the second most influencing factor [...].*

389: "...photon path length ... has an almost negligible impact on the cloud RE in the solar and TIR.": photon path lengths in solar and thermal IR are not the same.
Did you specifically calculate the mean free path length at the thermal IR? Which wavelength? Water vapor or CO_2 absorption might also affect the path length.

The mean photon path length has not been calculated explicitly. We generally refer to the increased cloud optical thickness and to the fact that the cloud becomes more opaque in the solar and TIR wavelength range with increasing IWC. Following the comment, we rephrased the sentence**.**

> *"For ΔFtir the increase in IWC leads to an intensified warming effect (Fig. 8b). Again, this is caused by the increase in the total particle scattering and absorption cross-sections. Similar to ΔF_{sol}, the steepest increase appears for IWC < 0.012 g m^{-3}, while for larger IWC the medians approach an almost constant level and a further increase in IWC has only a limited effect on ΔF_{tir}."*

418 - 419: "indicates an increase in the sensitivity of ΔFsol, particularly with respect to reff": Wasn't that already obvious from Fig. 2?

We partly agree with the Reviewer. Nevertheless, Fig. 9 provides a more nuanced overview of F_{sol} on IWC and r_{eff} as there is an additional separation for α_{srf} and the solar zenith angle. The sentence was slightly rephrased to include the Reviewers comment.

"[…] ΔF_{sol}, particularly with respect to r_{eff} , as it is expected from Fig. 2"

3.4.2: The title is "Thermal IR", but the text below is about F_{net}

The Reviewer is right. Now, the section explains F_{tir} and F_{net}. The title was modified accordingly.

"Thermal-infrared and net radiative effect"

3.6: Again, I would think that the radiative boundary conditions that arise from an underlying cloud are covered by the variations in surface albedo and surface temperature, already.

We partly agree with the Reviewer that, from a radiative transfer perspective, a variation in the cloud optical thickness of the second cloud layer is already cover in the variation of the surface albedo or surface temperature. Nevertheless, we think that the discussion is beneficial for readers, who are not (yet) familiar with the topic of radiative transport and the interactions of radiation, surface, and clouds. In addition, including the second water layer in the provided data set allows to directly access simulated RF for combinations of surface properties and second cloud layer cloud optical thickness. For example, a user can directly access the cloud RF of a cloud over a surface with a certain surface albedo and second liquid water cloud (of certain COT). Without the second cloud layer, the user would be forced to transfer the COT of the liquid water cloud to an equivalent surface albedo. The transfer would have to be parameterized, which adds an additional uncertainty.

501-502: Of course, F_sol and Delta F_sol = 0 during night. But given this obvious day-night differences in the contributions of F_sol to F_net, wouldn't it not make more sense to study F_net for 24h means?

The Reviewer is right but this is beyond of the scope of this study in which we intended to investigate the basic dependencies of RT in ice clouds on the selected parameters.

509: "Delta F_sol is dominated by Delta F_tir": Typo? F_sol -> F_net?

The sentence was incorrectly phrased and is now modified.

"For all θ and the majority of the simulations, negative ΔF_{sol} is exceeded by positive ΔF_{tir} and leads to a positive median ΔF_{net} (warming)."

511-512: alpha_srf = 1 is rather unrealistic on this planet. So, I don't think that solar warming ever occurs.

The sentence was rephrased and generalized. The simulations showed a transition from a solar cooling to a solar warming between α_{srf} of 0.6 and 1.0. Since sea ice can have α_{srf} between 0.6 and 1.0, we argue that, under some circumstances, cirrus and contrails can have a solar warming and the figure is valuable.

[…] except for α_{srf} approaching 1 [...]

515: "the resulting net RE is a warming.": -> small.
The competition alone does not explain a warming or cooling.

The Reviewer is right and the sentence was rephrased. The resulting positive net RF is caused by the dominance of ΔF_{tir} over ΔF_{sol}.

> *“An increase in IWC intensifies the cooling in the solar and the heating in the TIR. As both effects compete against each other and ΔF_{tir} dominates ΔF_{sol}, the resulting net RE is a warming. An exception appears for largest IWC, where median ΔF_{net} is negative. Simultaneously, the increase in IWC causes an enhanced impact of the free parameters and associated uncertainties.”*

527: "Simultaneously, the TIR heating remains almost constant...": yes, because the cloud top temperature is fixed. The latter could also be subject to variations. In fact, brighter clouds often have larger vertical extend and are thus colder. I suggest to drop this "underlying cloud" study.

The fixed temperature results from the set-up of the simulations. Here we selected T_{cld} as the specified parameter and positioned the cloud depending on the atmospheric temperature profile. Alternatively, one could have fixed the altitude, e.g., 10 km, and vary T_{cld}. Fixing the cloud altitude would does not seem appropriate as the formation of clouds is primarily driven by temperature and the altitude varies depending on atmospheric profile and location.
Similar to the statement from above, we intent to keep the underlying cloud study. The reason was given in one of the previous answers.

528: infinite -> horizontally infinite

The sentence was modified.

> *“[…] the cloud RE of homogeneous, horizontally infinite ice cloud [...]”*