Answers to the First Reviewer's Review of "A Comparative Analysis of In-Situ Measurements of High Altitude Cirrus in the Tropics", by Cairo et al., MS No.: egusphere-2023-112.

The Authors thank the Reviewer for the careful analysis of the manuscript, which led to a reconsideration and expansion of sections not sufficiently clear and/or exhaustive. Below are the replies to the reviewer's comments, and indications of additions, modifications or subtractions to the text under discussion. We report the reviewer's comments in italics, our responses in roman, and the text added to the manuscript in roman blue.

- *The discussion of instruments covers pages 5-8, and the analysis methods between 9 and 12, which I think is excessive, given that the written text is 20 pages in total. And there is virtually no discussion of the clouds sampled-the article is more of an instrument comparison study. There's a little discussion (a few sentences) just before the conclusions section but should have much more detail earlier in the manuscript. A discussion of earlier measurements in similar types of cloud conditions is warranted.*

In the revised manuscript we have tried to reduce the discussion of instruments and method of analysis. In fact, the manuscript was written with the intention of limiting ourselves to an instrumental comparison, and not to an in-depth discussion of the type of clouds encountered and the morphologies of their cloud particles. That can be found in other papers presented in the special issue *StratoClim stratospheric and upper tropospheric processes for better climate predictions* (ACP/AMT inter-journal) (Krämer et al. 2020, Lamraoui, F. et al, 2022; Khaykin et al., 2022). References to previous measures were inserted in the text (see lines 57-63) and in particular two articles (Baumgardner et al., 2017; Kramer et al, 2020) are an in-depth review of previous measurements in cirrus. We agree on the need for a description of the type of clouds observed, for this reason we have inserted in the revision of the manuscript, following line 76, the following text:

Most of the measurements during StratoClim were performed at temperatures $\lesssim$ 205 K, corresponding to potential temperatures $\gtrsim$ 355 K and altitudes $\gtrsim$ 14 km, i.e., in the TTL. Krämer et al. (2020) reports a description of the clouds observed during the campaign. The first part of the campaign period suffered from very rare cloud passes at elevated altitudes, with comparatively low cloud particle number concentrations (below 1 cm^{-3}). In fact, during this period, the vast majority of clouds were encountered during ascent from or during approach to the Kathmandu airport. On 29 and 31 July, and on 02 August, most of the clouds were come across at pressure level of ~400 hPa (and higher) during ascent and descent, with cloud particle concentrations ranging from 100 to 1000 cm^{-3}. The second campaign period (flights on 04, 06, 08, and 10 August 2017) provided extended fields of cirrus clouds of convective origin, with elevated particle densities and broad size distributions covering almost the entire detection size range of the different particle probes. The cloud particle measurements, mostly carried out over the southern slopes of the Himalayas captured high ice water content up to 2400 ppmv and ice particle aggregates exceeding 700 µm in size. The observed ice particles were mainly of liquid origin, with only a small amount formed in situ. ERA5 reanalysis corroborates the presence of high IWC detrained from deep-convective. A microphysical modeling study by Lamraoui et al. (2023) focuses on the flight of the 10 August, but its results can probably also be applied to the other cases of convectively generated cirrus measured during the second part of the campaign. The study predicts ice habits and reproduces the observed IWC, ice number concentration, and bimodal ice particle size distribution. The lower range of particle sizes is mostly represented by planar and columnar habits, while the upper range is dominated by aggregates with sizes between 600 and 800 µm. The study suggests that most of measured ice particles are of liquid origin with only a small amount formed in situ. These latter are associated with low values of IWC and number concentration, which makes them less influential in regulating the IWC which is, on the contrary, substantially influenced by planar ice particles of liquid origin. The difference in ice number concentration across habits can be up to 4 orders of magnitude, with aggregates occurring in much smaller numbers.

- *2. There are numerous acronyms used throughout the article. A table of the names associated with acronyms is warranted.*

A table of acronyms has been added to the revised manuscript.

- *3. Another point: Given that your PSD instruments only measured up to 960 microns yet you are sampling convective outflow cirrus, is this a problem? Do you have any particle that are in the largest size bin?*

There are indeed occasions when even the largest dimensional bins reveal the presence of particles. However, these occurrences do not seem to significantly influence the calculation of the backscatter ratio, as shown in Figure 16 which illustrates the buildup of the particle backscattering coefficient with respect to the particle radius, for all the particle size distribution under study. From this figure it can be seen that is mainly the dimensional range between 100 and 300 μm in radius that influences the final value of the backscattering. A similar consideration can also apply to the Surface Area Density of the particulate matter. We also performed similar calculations for the Volume Density, arriving at similar results. We can therefore state that the main contribution to the bulk microphysical parameters of the cloud comes from particles between 200 and 400 μm in radius. We recall here that the estimate of the IWC presented in our work mainly derives from hygrometric measurements, and only on two occasions was it estimated from the size distribution of the particulate matter (line 341). We also noted in the manuscript that an extensive assessment of the methodology to retrieve IWC from the two hygrometers and from the PSDs, as well as a comparison between the two approaches, has been carried out in Afchine et al. (2018) who demonstrated the equivalence of the two methods.

Furthermore, from PSD presented in the literature (e.g. Tian et al, 2010; Lawson et al., 2019) we expect a reduction of 2-4 orders of magnitude in the concentration of particles larger than 500 μm in radius, compared to those in the range 100-300 mm in radius.

Finally, A PIP also was concurrently operated with the other cloud probes. The data demonstrate that the number densities for particles larger than 960 μm were often four orders of magnitude or more below the concentrations reported by other instruments, with only very few occasions where this ratio was reduced to three orders of magnitude. We are therefore confident of the relative non-influence of the particulate with linear dimensions greater than one μm, in determining the value of the backscattering. An extremely conservative estimate of the contribution to the backscattering coming from the particulate with a diameter greater than 960 μm does not exceed a few percentage units in rare worst cases. So, in order to keep the computations manageable and the paper compact, we stayed away from introducing the PIP instrument together with the corresponding measurements.

- *4. Lines 398-400. It's a very smart idea to compare backscattersonde to the lidar data*

We thank the reviewer for this kind remark.

- *5. Figure 4. Can you change the units to something that might be more meteorologically oriented, for example m-1? This is very much the case for Figure 7-11, and ones that follow.*

There is no general consensus on how to represent the measurement units of the backscattering coefficient, which in lidar practice is indifferently indicated in m^{-1} sr^{-1}, $km^{-1}sr^{-1}$ and also, albeit less frequently, in Mm^{-1} sr^{-1}. We will comply with the reviewer's suggestion by indicating it in m^{-1} sr^{-1} here and elsewhere.

- *6. Section 4.3, Figures 7-1q. I feel strongly that the units for backscattering coefficient in the figures km-1 sr-1 should be something that modelers, etc could use. These should be put in standard cloud physics units. Also, IWC should be in g/m3. This would facilitate comparison with other studies (for example, IWC in Figure 11 to Thornberry Thornberry, T. D., A. W. Rollins, M. A. Avery, S. Woods, R. P. Lawson,T. V. Bui, and R.-S. Gao (2017), Ice water content-extinction relationships and effective diameter for TTL cirrus derived from in situ measurements during ATTREX 2014, J. Geophys. Res. Atmos., 122, 4494–4507, doi:10.1002/.*

We now comply with the reviewer's suggestion by indicating in all our figures the backscattering coefficient in m^{-1} sr^{-1}, and IWC in g m^{-3}. We thank the reviewer for pointing out Thornberry et al., 2017. This has led us to a revision of the way we present the comparison of IWC determinations with respect to the extinction. Figure A9 has been suppressed while a new comparison figure has been inserted in the text, with several IWC-σ regressions, illustrated in a new table.

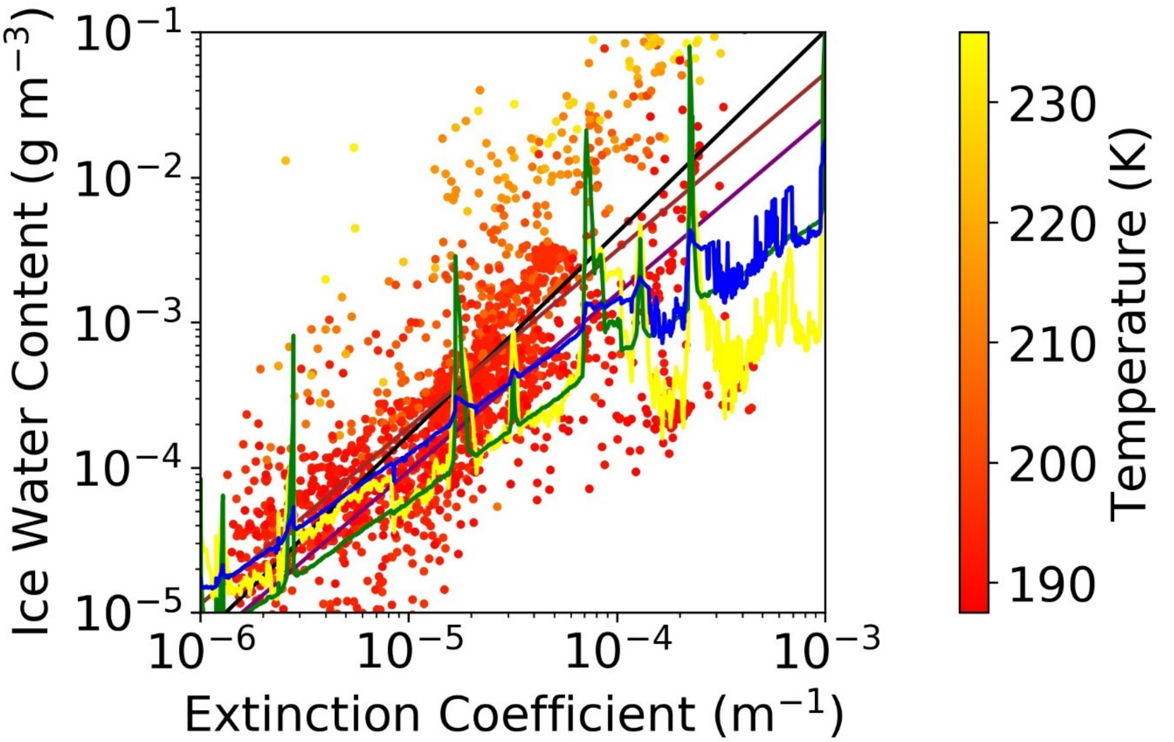

Figure 18: Scatterplot of measured IWC vs estimated extinction s=30b. The solid lines represent regressions from i. the present work, black; ii. Heymsfield et al. (2005), purple; iii. Avery et al., (2012), brown; Heymsfield et al. (2014), (a) yellow; (b) green; Thornberry et al. (2017), blue. Experimental points are color-coded in temperature of the observation.

IWC[g m^{-3}] - σ[m^{-1}] parametrization		
Reference	Functional form	T range
Heymsfield et al. 2005	IWC=119*$\sigma^{1.22}$	198-263 K
Avery et al., 2012	IWC=238*$\sigma^{1.22}$	
Heymsfield et al. 2014 (a)	IWC=a*σ^{b} a=0.00532*(T[°C]-183)$^{2.55}$ b=1.31*exp(0.0047*(T-273))	188-270 K
Heymsfield et al. 2014 (b)	IWC=σ(0.91/3)*91744*exp(0.177*(T-273)) IWC=σ(0.91/3)*83.3*exp(0.0184*(T-273))	202-217 K 188-202K
Thornberry et al., 2017	IWC=σ∗(0.92/3)*(40+0.53*(T-192)) IWC=σ∗(0.92/3)*(12+28*exp(0.65*(T-192)))	192-207 K 185-192 K

Present work	IWC=1552*$\sigma^{1.39}$	

Table 2: IWC-σ Parameterizations (adapted from Thornburry et al., 2017)

To update the text and present the figure, lines 571-584 have been deleted and the following text has been inserted:

Several studies have provided an estimate of the dependence of the IWC on lidar extinction (Heymsfield et al.; 2005, Avery et al., 2012; Heymsfield et al, 2014; Thornberry et al., 2017). They are based on in situ measurements of IWC and PSD, the latter used to provide an estimate of the lidar extinction from optical modeling of the cloud particles.

These IWC-σ relationships could be compared with our IWC estimates based on β, if a suitable extinction-to backscatter ratio (a.k.a. Lidar Ratio) LR is chosen. Unfortunately, LR can vary from 10 to 40 sr in tropical cirrus clouds (Chen et al., 2002) thus making the comparison somewhat arbitrary. Using a LR = 30 sr as a most probable value (Balmes etal., 2019) and posing σ = LR*β we can correlate our IWC measurements with σ. Figure 16 is therefore the analogue of figure 11, where this time IWC is reparametrized as a function of σ.
The same figure shows the analytical relationship obtained in this work (solid black line), with those present in the literature, shown in table 2. Although all parameterizations capture the IWC-σ trend, and align with each other in the lower range of data variability, the result of our study is in agreement only with Avery et al. (2012) while it diverges from the other parameterizations, more severely for those that depend on the temperature. This especially in the upper range of data variability. It should be noted that in this range, the data themselves have also a greater dispersion.
We want to underline the limits of this comparison: in the case of the present study, they are caused by having chosen, rather arbitrarily, the same LR value for all the clouds observed, and in the case of the other parametrizations they are caused by having used an indirect determination of the extinction, calculated from the PSDs. It would be very interesting to have simultaneous in situ observations of backscattering, extinction and IWC available in the future.

- *7. Lines 524-525. The greatest ambiguity in the results of the comparison is linked to the choice of particles' morphology. Perhaps you should temper this because the mass dimensional relationship and the masses of the small particles are not compared directly to FLASH.*

There is an ambiguity due to an imprecise formulation of our sentence, which in effect only comments on the part of the study involved in the comparison of the observed versus the calculated backscattering. We have rephrased the sentence (524-525) as follows:

… it is clear from our simulations that the greatest ambiguity in the results of the observed versus the calculated comparison is linked to…

- *8. The relative independence of β from R_{mean} and R_{eff} confirms N_{ice} as the main parameter governing the cirrus scattering properties at optical wavelengths. Does this result also fall out of the analytical relationships assuming gamma PSD and quasi-spherical ice particles?*

Gamma distributions can be used to represent the number distribution function n(r), often used in GCMs to for the size distribution for each class of hydro-meteors in the model (e.g., ice, snow, and rain) (Gettelman et al., 2010) and it is given by:

$$n(r) = ar^{\alpha}e^{-br}$$

This distribution has a mode radius of $r_m = \frac{\alpha}{b}$, while the particle concentration is expressed by $N_0 = ab^{-\alpha-1}\alpha!$ (Grainger, 2017).

In our study we demonstrated how most of the backscattering is built up in the particle range 100-300 μm (see fig. 16), where the size parameter $\frac{2\pi r}{\lambda}$ of the particle is much greater than 10 and this result suggests to place oneself in the geometrical optics approximation, setting the backscattering efficiency to be unitary, so as to place in the calculation of the backscattering coefficient:

$$\beta \cong \int_0^{\infty} \pi r^2 n(r)\, dr$$

(this is the integral form of our eq. (11)). Moreover, when α is an integer, the moments of the gamma distribution have a simple analytical expression:

$$m_i = ab^{-\alpha-1-i}\Gamma(\alpha+i+1) = ab^{-\alpha-1-i}(\alpha+1)!$$

so as to have:

$$\beta \cong \pi ab^{-\alpha-1}\alpha!\, b^{-2}(\alpha+1)(\alpha+2) = N_0\pi b^{-2}(\alpha+1)(\alpha+2) \sim N_0\left(\frac{\alpha}{b}\right)^2 = N_0 r_m^2$$

So β turns out to be linear in N_0, but also approximately quadratic in r_m.

We arrive at a similar result using a lognormal instead of the gamma distribution for the PSD

$$n(r) = \frac{N_0}{\sqrt{2\pi}} \frac{1}{r\ln(s)} e^{\left[-\frac{(\ln(r)-\ln(r_m))^2}{2ln^2(s)}\right]}$$

with identical approximations:

$$\beta \cong \pi N_0 r_m^2 e^{2ln^2(s)} \sim N_0 r_m^2$$

(Grainger, 2017). Thus a dependence on the square of the modal radius – and hence of other similar parameters linked to it, as the mean or the effective radius - is indeed to be expected, as the physical intuition would also suggest. In our case, the variability of such radius is of the order of a factor 2 (refer to figure 8 and 9) while the variability of the particle concentration extends for five orders of magnitude, and for this reason it turns out to be the main factor driving the backscattering variability.

To clarify this point, we have deleted lines 443-445 and inserted the following text:

With simple analytical calculations on various types of functional forms for the PSD (gamma, lognormal, etc.), and in the spherical ice approximation, it is easy to demonstrate that a dependence on the square of the modal radius – and hence of other similar parameters linked to it, as the mean or the effective radius - as well as by the total number of particles, is indeed to be expected for b, i.e. $\beta \sim N_0 r_m^2$, as the physical intuition would also suggest. In our case, such a dependency on r_m, which varies by a factor of 2, is masked by the much wider variability of N_0, which varies over five orders of magnitude.

Minor Comments:

I have numerous minor comments that should be considered in the revision of the manuscript.

Line 3. in view to > with the goal of connecting

Corrected in the new version of the manuscript.

7. Hymalaian to Himalayan

Corrected in the new version of the manuscript.

14? What do you mean by "can be set"

The computed backscattering coefficient can be brought into good agreement…

26: Cirrus at higher altitudes. Regarding your statements about cirrus, it would be good to use the AMS Glossary of Meteorology definition.

Lines 25-26 have been substituted with:

Cirrus are high clouds existing between -35° and -85°C, composed of ice crystals, of micron to millimeter size (Lynch et al. 2002), that are fairly widely dispersed, usually resulting in relative transparency and whiteness and often producing halo phenomena not observed with other cloud forms (AMS, 2023).

85. properties. This is particularly…

Corrected in the new version of the manuscript.

98. Why not use the Self-Similar Rayleigh-Gans Approximation (Hogan and Westbrook, 2014), or DDSCAT?

As far as we know, the Self-Similar Rayleigh-Gans Approximation (SSRGA) is mostly used to compute the scattering from aggregated ice particles and snowflakes in the microwave and millimeter parts of the spectrum. For such kind of particle shapes the soft sphere/spheroid approximation tends to significantly underestimate scattering. In our case, given the supposed prevailing morphology of the diffusers and the wavelength used, we preferred a different approach for the scattering.

Discrete Dipole Approximation (DDA) could have been an adequate choice, and DDSCAT a good implementation of it. Indeed, the program supports calculations for a variety of target geometries (e.g., ellipsoids, regular tetrahedra, rectangular solids, finite cylinders, hexagonal prisms, etc.). However, is very computationally costly, and it is reported a lack of convergence for size parameters > 25 (Draine and Flatau, 2013)., much smaller than those encountered in our study.

118. developed by

Corrected in the new version of the manuscript.

265. This line should be part of paragraph on line 264.

Corrected in the new version of the manuscript.

285-330 Very nice, comprehensive calculations of how the aspect ratio affects the

backscattering efficiencies.

We thank the reviewer for this kind remark.

339. Could you include the m-D relation you use in the text?

Added in the new version of the manuscript (line 339):

… we have used the m–D relation described in Krämer et al. (2016): $m=a*D^b$ where a = 0.001902, b = 1.802 for D > 240 µm; a = 0.058000, b = 2.700 for D = 10–240 µm; ice crystals are spheres for D < 10 µm. The validity of the m-D relation is verified by Afchine et al. (2018) by comparison to thirteen others.

368-370 Very good determination of why the NIXE-CAPS data set was used.

We thank the reviewer for this kind remark.

Section 4.2. It's clever to use the backscatter model together with the measurements for find

the best AR.

We thank the reviewer for this kind remark.

385. Remove i

Corrected in the new version of the manuscript.

405. backscattering should not be capitalized

Corrected in the new version of the manuscript.

408. range is

Corrected in the new version of the manuscript.

440. as seems to do > as well as SAD and IWC

Corrected in the new version of the manuscript.

441. as hardly change. Please rephrase

Corrected in the new version of the manuscript.

475 and and

Corrected in the new version of the manuscript.

512. We remind. We note that...

Corrected in the new version of the manuscript.

Bibliography (related to the present answer and not previously quoted in the manuscript, new addition in the manuscript are in blue):

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