The diurnal cycle and temperature dependence of crystal shapes in ice clouds from satellite lidar polarized measurements, proposed to publication in ACP by V. Noel, H. Chepfer, C. Barthe and J. Yorks

In addition to the changes outlined below, we also replaced the previous Fig. 1 (which showed the latitude variation of the repartition of particle shapes) by the previous Fig. A1 (which showed the same thing but only considering CATS measurements obtained around 01:00AM local time), since the point of the figure is to provide an idea of the agreement between CATS and CALIPSO measurements from Sato and Okamoto (2023) which were obtained at 01:00 AM. The figure in question is now called Figure 2 in the revision. The bibliography has considerably expanded following the inclusion of a discussion section (new Sect. 4) following a comment from reviewer 1.

Reply to comment from reviewer 1

Original comments from Reviewer 1 are shown in blue, our replies are shown in black.

This paper applies to the CATS observations a cloud-particle-shape partitioning framework based on lidar depolarization ratio, originally developed by Okamoto et al. (2019) and previously applied to CALIPSO observations by Sato and Okamoto (2023). The main interest of this study is that the CATS observations from the ISS offer a unique opportunity to document the global diurnal cycle of particle types observed by a spaceborne lidar.

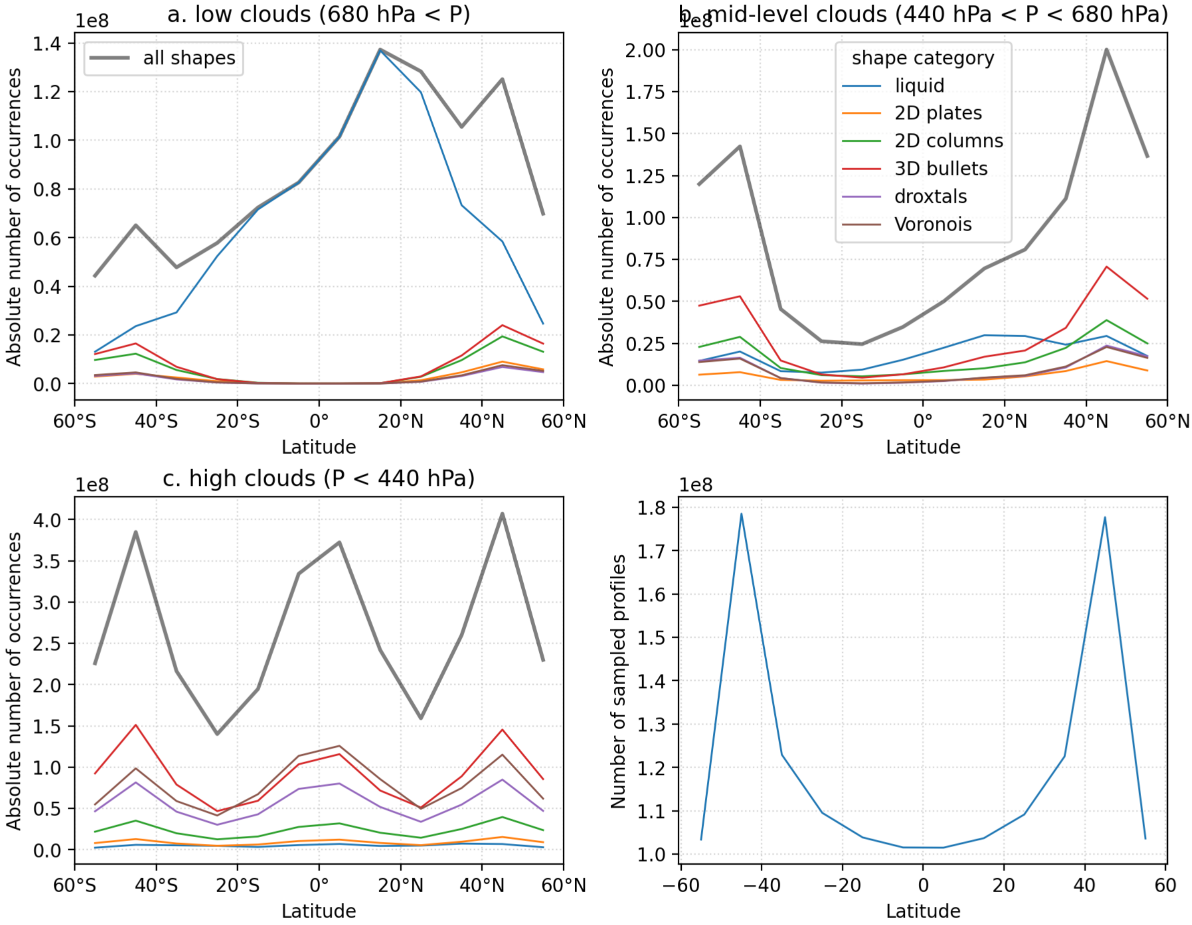
We thank the reviewer for taking the time to give feedback on our work.

MAJOR COMMENT 1

1.1 While the topic is relevant, the choices made by the authors in presenting the results make the key information difficult to interpret. All results are shown as fractional partitions rather than absolute occurrences of cloud-particle shapes. There may be valid reasons for this choice, but the authors do not provide any justification.

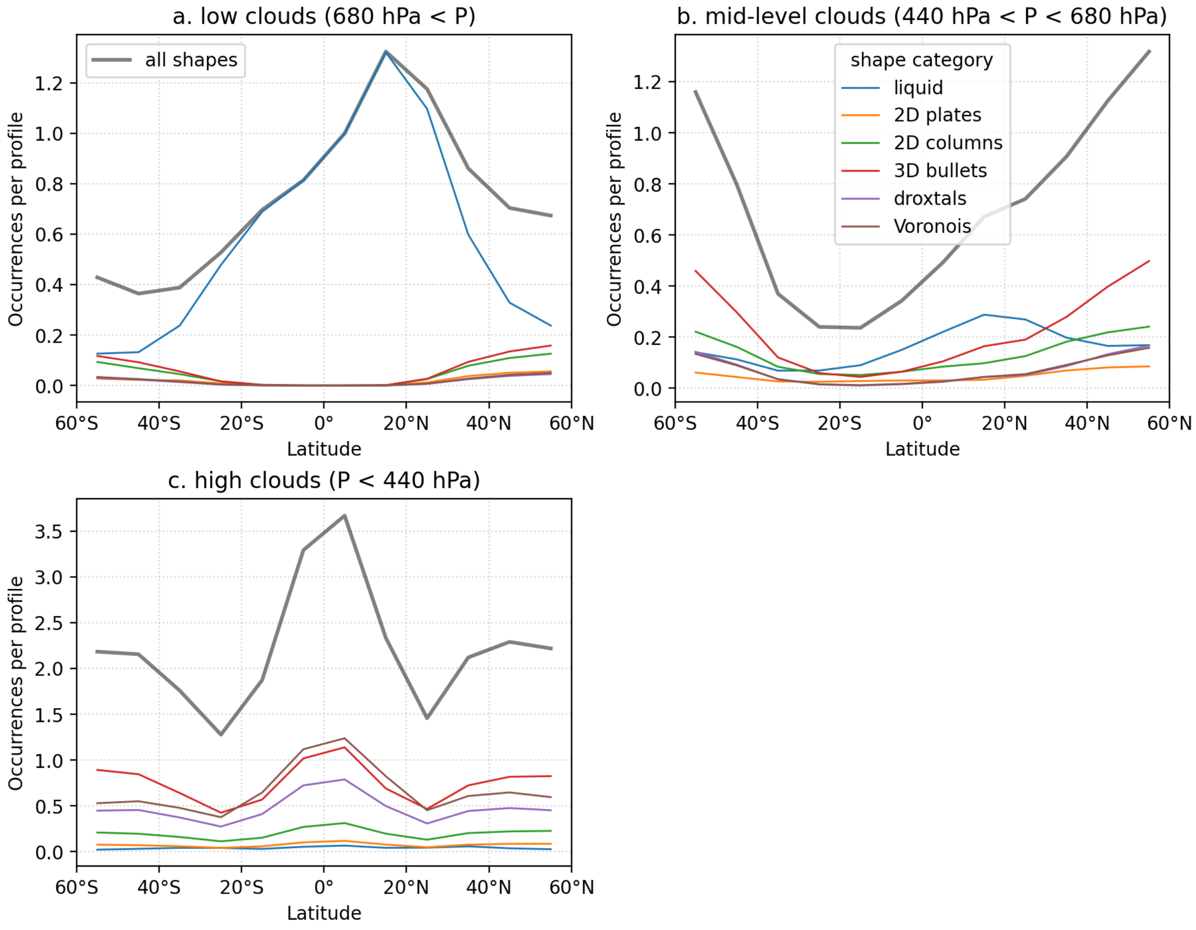
We agree with the reviewer that in the submitted paper we did not justify why we were presenting results as a partitioning of particle shapes within cold clouds, instead of presenting absolute occurrences. Below we attempt to clarify this point in two steps.

First, we tried to look directly at the absolute number of cloud particle occurrences, as suggested by the reviewer. The figure below shows the absolute number of occurrences of cloud particles by latitude for each particle type (colors), in low clouds (top left), mid-level clouds (top right), and high clouds (bottom left). The bottom right panel shows the number of profiles that were overall sampled by CATS by latitude. All occurrences were counted in 10° latitude bins. The grey lines show the total absolute number of cloud particle occurrences (ie the sum of all color lines), these lines were shown in Fig. 1d (bottom right panel) in the original submission.



Due to the orbital characteristics of the CATS instrument (operating from the ISS), the number of sampled profiles increases significantly at higher latitudes (bottom right). This directly impacts the zonal evolution of cloud particle absolute number of occurrences, most notably via a sharp increase near 55°N and 55°S - particularly noticeable for mid-level and high-level clouds (b and c). As a consequence, the absolute number of occurrences of cloud particle by particle type (color lines) is also affected by the same strong increase from low to high latitudes. For instance, the number of occurrences of 3D bullets in mid-level clouds (red line, top right) is multiplied by ~7 from 0° latitude to 55°N: this increase is directly driven by the increase in instrumental sampling, and is not representative of any geophysical behavior. For this reason, we felt that showing the absolute number of occurrences of particle types would give a biased idea of their zonal variation.

Second, we went further by trying to look at the number of occurrences of cloud type detections per profile. Although it is not absolute, this metric has the advantage of cancelling out the importance of the sampling effect, so it might be a better choice than the absolute number of occurrences. The figure below shows the zonal evolution of the number per profile of occurrences of cloud detections of a given type, again in low-level, mid-level and high clouds. The grey line shows the total number of cloud particles per profile.



Compared to the previous figure, the increase in sampling at high latitudes is no longer affecting the results. However, now the results by particle type (colors) are by definition constrained by the total number of cloud detections per profile (grey), and follow its evolution. This number depends 1) on the fraction of profiles that contain a cloud (in other words, the cloud amount), and 2) on the geometrical thickness of the sampled clouds (thicker clouds will lead to more cloud points). Thus, the number of occurrences per profile of a given particle type will be strongly modulated by the cloud amount and geometrical thickness of the considered cloud population in a specific latitude/temperature range. Attempts to document the variability of the number of occurrences per profile of a cloud particle type (with temperature, latitude, etc.) will have to be interpreted in light of the variation of cloud amount and geometrical thickness across the same range, and discussing the relative importance of particle types will not be straightforward. For instance, in high clouds (bottom left) the increase from 20°S to 0° latitude of the number of occurrences per profile of most particle types is without a doubt due to the large increase in the amount and thickness of high-altitude clouds between subsidence-dominated regions and convection-dominated regions. As this increase happens for all particle types, we need to abstract it away to conclude anything about particle types.

Another point is that the "occurrences per profile" metric is not very easy to interpret quantitatively (what does "1.34 occurrence per profile" mean?). Since a cloud occurrence corresponds to a fixed vertical height (240 m), this metric could be converted into an average cloud thickness per profile. This would be, in our point of view, the closest we could get to an absolute occurrence of cloud particle types as suggested by the reviewer, while providing a physical quantity similar to cloud thickness. It would however be affected by variations of cloud amount (cloud-free profiles would bring the number closer to zero), and thus not much easier to interpret: saying, for example, that there are 300 m per profile of 2D columns on average between 10°N and 20°N is not easily applicable information.

In any case, showing the average number of cloud particle type (as above) or cloud type thickness per profile would have focused at least part of the discussion on variations of cloud properties, while our intent was to focus on the variation of the dominant particle type within cold clouds. For this reason, we decided to abstract away both the cloud amount and thickness, by showing the fraction of cloud points of a given shape type. Sato and Okamoto (2023) choose the same representation, probably for the same reasons.

To sum up, in order to focus on the variation of the particle shape repartition, we decided to get rid of the impact of sampling, cloud amount and thickness by focusing on the repartition of particle types within cloud points. In essence, our goal was to document, given the presence of a cold and optically thin cloud, the probability that particles within would be of a given shape depending on temperature, latitude, etc. Such a value could be compared with shape repartition from e.g. model simulations.

Following this comment from the reviewer, we have updated the article to make our intent clearer, and included a justification for choosing the shape partitioning metric at the beginning of Sect. 3.

1.2 As a consequence, several of their interpretations become confusing, especially when discussing the diurnal cycle (Fig. 3) and the so-called “diurnal fraction anomaly amplitude” (Fig. 4). For instance, statements such as “Particles with strong daily cycles include 2D columns and plates at cold temperatures” (line 263) are hard to assess, because it is unclear whether these particle types actually exhibit a diurnal cycle in absolute occurrence, or whether the variations seen in fractional partition simply reflect diurnal changes in other categories (e.g., droxtals or Voronois), or vice versa.

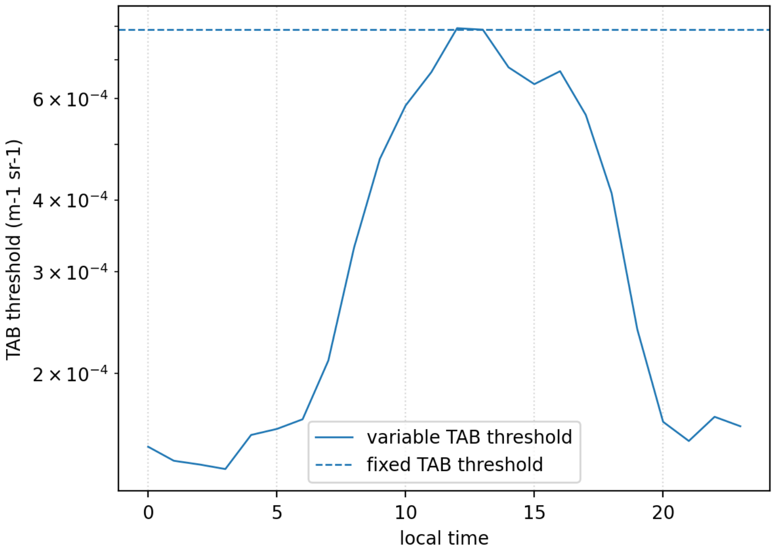
We thank the reviewer for clearly pointing out that our original submission confused the distribution of particle types and their repartition. While the distribution of a given particle type (with latitude, temperature, etc.) is independent from the distribution of the other particle types, the partition of any single particle type will be affected by the repartition of all particle types. The original submission confused the two, leading to incorrect interpretations. We have updated the manuscript to clearly define both quantities and tried to remove all points of confusion by making it clear that the discussion always focuses on the fractional repartition. This comment also helped us clarify our intent in the manuscript.

MAJOR COMMENT 2

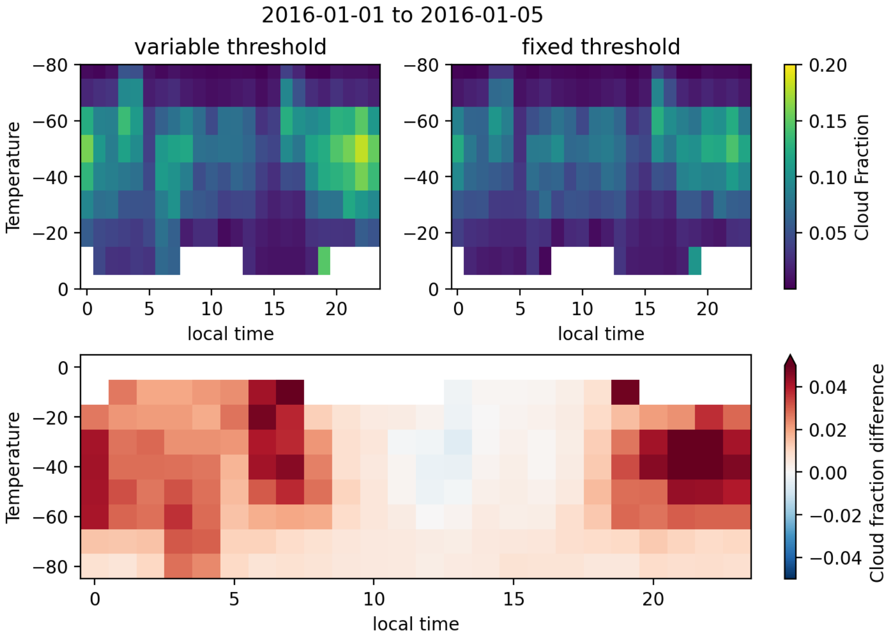
In addition, the study does not address uncertainties. Since the classification relies entirely on depolarization-ratio thresholds, it would be important to demonstrate that the reported diurnal cycles are not artifacts arising from daytime–nighttime differences in signal retrieval. Moreover, variations could also arise from changes in detection sensitivity in the TAB when applying the Hagihara et al. (2010) cloud detection methodology.

We agree with the reviewer that that daytime-nighttime differences in detection sensitivity and signal retrieval could affect the presented results. This is particularly true in the present paper as the amount of solar noise in CATS data will vary significantly as a function of the local time of observation (while most studies based on spaceborne lidar data generally use nighttime-only measurements to maintain the noise levels to a minimum). To address this comment, we looked at 1) how the daytime changes in cloud detection sensitivity affect the obtained cloud cover and 2) how the daytime changes in signal quality affect the retrieved diurnal cycles.

For the first point, we compared results obtained using the cloud detection methodology used in the paper (in which the TAB threshold used for cloud detection depends on the noise level in the lower stratosphere), with results obtained with a fixed TAB threshold. Investigating the cloud detection threshold (figure below) shows that when applying the Hagihara et al. (2010) cloud detection methodology to the CATS 1064nm TAB, nighttime sensitivity (without solar noise) is ~5.5 times better than daytime sensitivity. We set the fixed TAB threshold to the one for maximal solar noise (i.e. around noon).

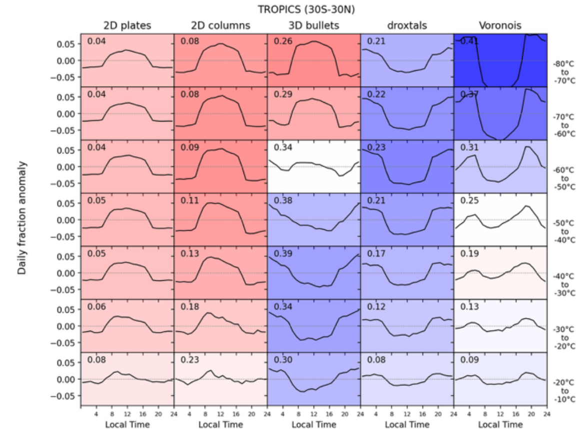
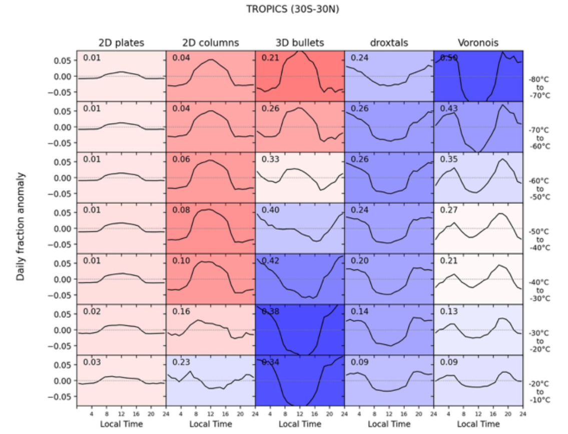
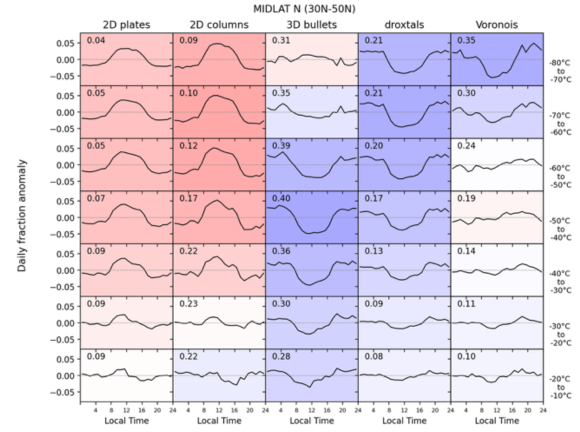
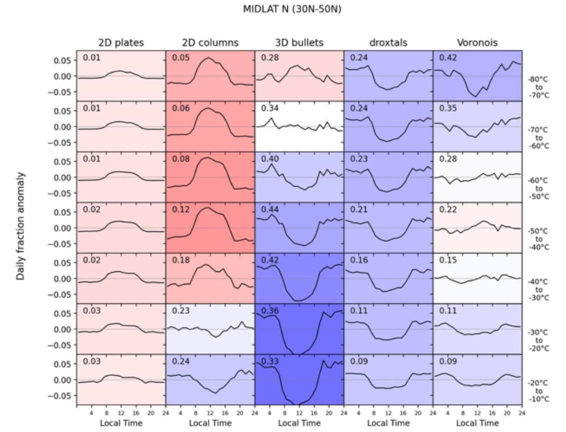
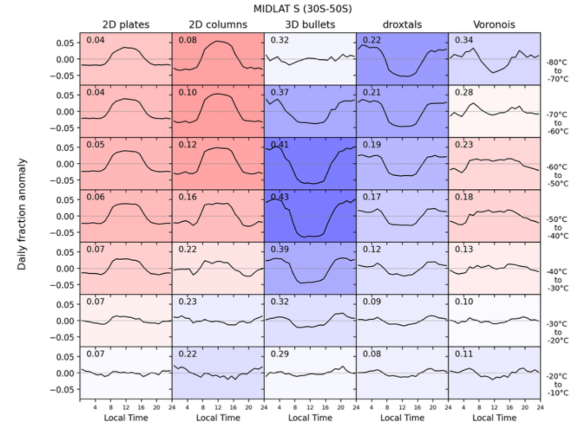
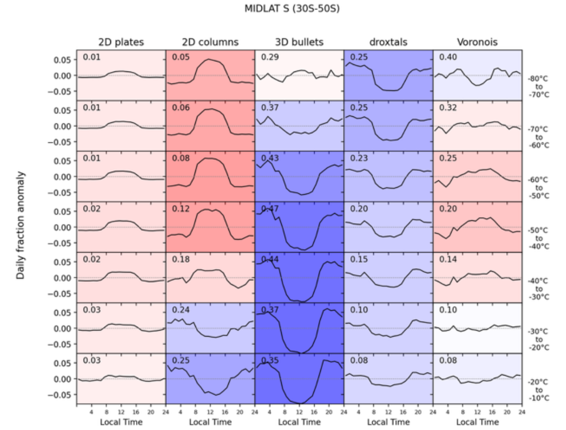


Setting a fixed TAB threshold will decrease the cloud detection sensitivity and will decrease the amount of optically thin clouds in the dataset (especially at night), but will make daytime and nighttime cloud detections consistent. We conducted this comparison over 5-days periods in each season but conclusions were quite similar – here we show results for January 2016 (Figure below).



These results show that using a detection threshold fixed to its maximal daytime value leads to a decrease of (mostly nighttime) cloud fraction by at most 0.05 – i.e. up to a 30% drop. This is quite a significant change in cloud fraction, even though diurnal cycles remain present (nighttime cloud fractions remain larger than daytime). Largest differences in cloud fraction are found in the -60°C to -40°C temperature range. These results show that the cloud dataset based on a variable threshold will during nighttime be more representative than during daytime of optically thin clouds in the particle shape repartition. These results are now mentioned in the text (first within the description of the cloud detection scheme and second in the conclusion).

For the second point (how the daytime changes in signal quality affect the diurnal cycle) we looked if using the fixed daytime TAB threshold (leading to the elimination of clouds with weakest signal from the cloud dataset, especially during nighttime) has an effect on the discussed diurnal cycles. The table below shows, on the left column, the diurnal evolution of the repartition of particle shapes using a variable threshold (as shown in the paper), and on the right column the same results using the fixed daytime threshold. All results are based on the analysis of the entire CATS dataset (from February 2015 to November 2017). As in the paper, the color of each box depends of whether the daily cycle maximum occurs during the daytime (red) or nighttime (blue), and the intensity of the color depends on the amplitude of the cycle.

Variable TAB cloud detection threshold	Fixed TAB cloud detection threshold
	
	
	

When switching from the variable TAB threshold (left column) to a fixed TAB threshold (right), the local time of daily maximum remains the same for all particle shapes in all temperature ranges (i.e. the colors of all boxes remain the same). Some daily cycles become more pronounced (i.e. the colors become more intense) -- this is most noticeably the case at warmest temperatures (-30°C to -10°C), where the nighttime repartition becomes more strongly dominated by 3D bullets and (in a smaller proportion) 2D columns. This happens in all latitude ranges. These changes imply the weak-signal nighttime clouds (removed from the studied dataset) using a fixed threshold inject at this temperature range considerably more 3D bullets in the shape mix of variable-threshold detections. Also noticeable is the flattening at temperatures colder than -30°C of the daily cycle for 2D plates (1st column). We interpret this change as meaning the weak-signal nighttime clouds removed from the dataset using a fixed detection threshold were classified in this temperature range as any particle type other than 2D plates in variable-threshold cloud detections. There is no significant change in the daily cycle of other particle types.

This comparison suggests that the daily evolution of the shape repartition is stable with the sensitivity to cloud detection, and that the retrieved daily amplitude could be under- or overestimated for specific particle shapes in the temperature ranges mentioned above. We have now included these findings in the text (end of Sect. 3.3).

MAJOR COMMENT 3

Finally, it would strengthen the paper to relate the reported diurnal variations in particle shapes to existing knowledge on cloud diurnal evolution and microphysical processes. Highlighting how the different particle shapes interact with radiation, and how the results presented here help improve our understanding of these interactions, would also enhance the overall impact of the study.

We agree with the reviewer that relating the retrieved diurnal variations to existing knowledge on microphysical processes would make the paper more interesting. In response to this comment, we have added a new Sect. 4 which discusses what processes could drive the diurnal cycles described therein. Literature on processes driving the microphysical properties of cloud change along the diurnal cycle is scarce, but we included several elements from observational and model studies that we hope help better interpret the variations in particle shape repartition and their possible implications for cloud lifecycle and impact.

I recommend that the manuscript undergo major revision to address the points outlined above. The topic is of significant interest, and the study has the potential to make a valuable contribution to the field once the issues related to presentation, interpretation, and uncertainties are adequately addressed.

Specific comments:

- The terms ‘3D’ and ‘2D’ ice particle types are not defined and may be unclear to the reader. Please clarify their meaning, or consider using clearer terminology such as ‘randomly oriented’ and ‘horizontally oriented’ particle types.

In response to this comment, the meaning of 2D and 3D has been explained in the text. We choose to retain this terminology by consistency with Sato and Okamoto (2023).

- The 3D column category is not mentioned. In the method proposed by Sato and Okamoto (2023) and followed here, it appears that 3D columns as well as aggregates of rosettes and columns are included in the 3D bullet types. Please clarify whether your categories are intended to be exhaustive with respect to ice particle shapes, or whether some particle types were deliberately excluded. For example, under which category should dendrites be classified?

As noted by the reviewer, in the original submission we omitted to mention that the "3D bullet" category represented aggregates that include 3D columns and bullet rosettes (following the description from Sato and Okamoto, 2023). This is now mentioned in the text. Sato and Okamoto do not, as far as we can tell, mention dendrites. Although the classification means to be exhaustive (all ice cloud detections have to fall into exactly one category), it is not necessarily always possible to know in advance, given a specific ice crystal morphology, into which category it shall be classified, as the categories all rely on how the shape reacts to light.

- The classification relies solely on depolarization thresholds. Consequently, our confidence in the results depends directly on the uncertainty in the depolarization parameter retrieved by CATS. Please provide information on this uncertainty and discuss how it may affect the findings.

We agree with the reviewer's remark that confidence in the results depends directly on the confidence in the depolarization ratio. As recommended in the CATS product v3 release notes [1] we avoided granules with suspect depolarization ratios (Depol_Quality_Flag > 0), this is now mentioned in the text. Providing a full quantitative evaluation of CATS' depolarization ratio in our opinion falls outside the scope of the present paper. Our own experience with the CATS depolarization data suggests its variability and uncertainties are certainly comparable to those from the CALIPSO depolarization, used in Sato and Okamoto (2023), but a thorough evaluation of depolarization uncertainties would certainly help better understand the limitations of our conclusions. In the revision we now mention this point when discussing the limitations of our results.

[1] https://cats.gsfc.nasa.gov/media/docs/CATS_Release_Notes7.pdf

- I suggest including a few example lidar curtains showing the ice-type classification masks to help demonstrate the method and build confidence in its application to the CATS data.

Following the reviewer's suggestion, we have included two figures (a new Fig. 1 and a new Fig. A1 each with 4 panels) to illustrate the steps of the cloud detection and crystal shape classification method (Sect. 2.2).

- Figure 1 shows the partitioning of crystal shapes at each latitude, as indicated by the caption (“The sum of frequencies for all shapes at a given latitude is unity.”). If the authors wish to retain partitioning-fraction representation, I therefore suggest the following:

- Use a 100% stacked area chart, as in Fig. 2.
- We agree with the reviewer that stacked area charts are more appropriate when showing repartitions. However, our goal here was to remain as close as possible to the design choices from Fig. 2 in Sato and Okamoto (2023), in order to facilitate comparison. We hope the precisions added to the text about shape repartition are enough to limit reader confusion.
- Revise the caption from “Variation of crystal shape repartition with latitude” to “Partitioning of crystal shape categories with latitude”,
- We have modified the figure caption according to the reviewer's suggestion.
- Avoid discussing zonal ‘variations’ of the fraction of a single ice type, because such statements do not provide meaningful information on their own; the fraction of one category is inherently dependent on the occurrence of all other categories. For example:

    - Line 138: “liquid particles still follow a symmetric repartition” => May be misleading. Since the figure shows the partitioning among particle types, the apparent symmetry of one category is inherently dependent on the fractions of the other categories and does not necessarily reflect the actual distribution of liquid particles.
    - Lines 140–141: “the distributions of solid particles show a slight hemispheric asymmetry” => The plot does not show the distributions of particles, but their partitioning among particle types. Therefore, if the zonal distribution of one particle type is asymmetric, the zonal partitioning of all other particle types will automatically appear asymmetric, even if their actual distribution are not.
    - Line 143: “Within the Tropics, they are clearly less present in the southern hemisphere” => Misleading. A decrease in the fraction of one particle type does not necessarily imply that this type is less present; it may simply reflect changes in the occurrence of other particle types.
    - Lines 151–154: “A slight increase in liquid particles […] could be related to the local minimum of ice cloud amount […] which would lead to an increased influence of detection sensitivity.” => The conditional (‘could’) is unnecessary here because the plot represents partitioning. It is unclear what information this statement conveys.
    - Lines 154–155: “Considering now solid particles, their amount are all symmetric around the equator” => Idem.

- We agree with the reviewer that our initial description of the zonal differences in shape repartition could sometimes lead to confusion. We have updated that section to taking into account the sentences outlined above.

- I suggest including illustrations of the different ice shapes somewhere in the article. For example, above each column in Fig. 3.

This is a very good suggestion, and we agree that the paper would be more intuitive to understand if it included images of particle shapes, especially for unfamiliar shapes like Voronois or Droxtals. However, there is to our knowledge very little imagery documenting these particle shapes. We are not aware of in-situ studies providing imagery appropriate to document the visual aspect of e.g. Voronoi or Droxtal particles, alongside the lidar measurements of depolarization ratio required to categorize their shapes. Building such a dataset would provide important validation to the results shown here, in Sato and Okamoto (2023), and in other related works. Following this comment, Voronoi particles are described in more detail in Sect. 2.2, and the conclusion now mentions the need for gathering in-situ imagery able to document the visual aspect of all particle shapes (including Voronois and Droxtals) to provide a more direct assessment and interpretation of the Sato and Okamoto classification.

- Lines 73–74: “This averaging configuration provided appropriate signal quality for shape classification on CALIPSO data.” => Please provide evidence or justification to support this statement.

The argument was that since CALIPSO data and CATS data lead to consistent cloud detections at similar resolutions (as discussed by Sellito et al., 2020), their share similar cloud detection sensitivities and from there similar signal to noise ratios in clouds. We have updated the text in Sect. 2.2 to make this reasoning clearer.

- Line 91: “Third, we identified ice clouds based on temperature (colder than -5°C) and the x parameter.” => I am confused. Do you restrict your analysis to clouds that meet these conditions? If so, do the clouds satisfying these criteria (which are intended to identify ice particles) still include only liquid particles in low-level tropical clouds (Fig. 1a)?

We thank the reviewer for pointing out inconsistencies between what was described in the text and how the analysis was conducted, which we can only attribute to confusion on our end. In response to this comment, we have made several important modifications to the revised article. First, we have updated the text in Sect. 2.2 to separate more clearly the consecutive steps of 1) cloud detection (based on TAB thresholding and spatial consistency), 2) cloud phase identification (based on x and temperature), 3) ice particle shape identification (based on depolarization ratio). In addition, this section now also includes a new Fig. 1 (suggested by a previous comment), which helps illustrate each step of the process. Finally, while the results on particle shape partitioning with latitude (Sect. 3.1) still include both cloud types (liquid and ice) as in the original submission to facilitate comparison with results from CALIPSO, further results (Sect. 3.2 and 3.3) now focus exclusively on ice particles. The text now mentions these points.

- What hypotheses do the authors propose to explain that “CATS-based results report fewer bullets near the equator” (lines 159–162)?

As the reviewer points out, near the equator Voronois dominate the CATS shape mix, while the CALIPSO shape mix is dominated by bullets. The depolarization ratio ranges of both these shapes are quite different, and separated by droxtals. We currently have no explanation for this difference, apart from differences in the observation period of the CATS and CALIPSO datasets. We have pointed that out in the text.

- Why does “The importance of 2D columns drops faster in the Tropics (Fig. 2b) compared to midlatitudes (Fig. 2a and 2c).” (line 185)?

We currently have no explanation for this finding.

- Lines 208–209: “amplitude as an indicator of the diurnal fluctuations of the importance of a given particle shape” (lines 208–209) => Although the sentence is logically correct, it is unclear what useful physical insight this ‘indicator’ is supposed to provide.

We meant to indicate that shapes with a high amplitude represent very different proportions of the shape mix in daytime and nighttime – ie they might dominate the mix in nighttime and be in the minority in daytime. We think that knowing a given particle shape is found often in ice cloud in daytime but not in nighttime could be useful to know when designing microphysical models or retrieval algorithms.

- Lines 217–219: “The DFA amplitude and the average fraction are not independent: large daily variations of fractions are only possible when the average fraction is important.” => I find this statement unclear, and it also appears to be mathematically incorrect. For example, imagine a situation where liquid particles are almost absent throughout the day except during one specific hour when all ice particles suddenly become liquid. In that case, the daily average liquid fraction would remain very small, yet the DFA amplitude could still reach 100%. While this example is exaggerated, it illustrates that a large amplitude does not require a large daily mean.

We thank the reviewer for providing a clear example of why the statement as written in the original submission was incorrect. We have rephrased the sentence to mean that in our results, large DFA amplitudes are usually associated with large average fractions. This suggests that situations like the one described by the reviewer not frequent in the dataset.

Technical corrections:

- Line 5: “Institut Polytechnique de Pairs” => “Institut Polytechnique de Paris”.

We thank the reviewer for noticing this error, which has been corrected.

- Line 12: “33 months of CATS spaceborne”.

At this point in the article the CATS acronym has not been defined yet, so we'd rather avoid using the term there. The sentence is still correct without mentioning the name of the instrument.

- Line 13: “We find that that” => Remove one “that”.

We thank the reviewer for noticing this error, which has been corrected in the revision.

- Lines 63–64: “These come from the MERRA-2 reanalysis” => Indicate the spatial and temporal resolutions.

The MERRA2 data is documented on a cartesian grid of 0.5° × 0.625° (lat-lon). Since the CATS product is provided on a horizontal grid which follows the trajectory of the ISS, changes in meteorological data in the CATS product do not necessarily occur on a fixed-step grid along the horizontal. In the V3.00 L1B product, meteorological ancillary profiles are on average distant of ~200 backscatter profiles (which are themselves 350m apart), so two meteorological profiles are separated by ~70 km. Along the vertical, meteorological profiles are provided on the same grid as CATS backscatter profiles (533 points). We have added these elements to the revision (Sect. 2.1).

- Line 69: Remove “1.1.1 Subsubsection (as Heading 3)”.

We thank the reviewer for noticing this error, which has been corrected in the revision.

- Line 72: “the CALIPSO Kyushu University (CALIPSO-KU) cloud product (Yoshida et al., 2010)”

Yoshida, R., Okamoto, H., Hagihara, Y., & Ishimoto, H. (2010). Global analysis of cloud phase and ice crystal orientation from Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observation (CALIPSO) data using attenuated backscattering and depolarization ratio. Journal of Geophysical Research: Atmospheres, 115(D4).

We thank the reviewer for selecting a more appropriate reference. This has been modified.

- Line 74: “Although CATS and CALIPSO operated at different wavelengths” => Please clarify, as both instruments actually operate at 532 nm and 1064 nm.

We thank the reviewer for spotting this incorrect writing. While CATS and CALIPSO were both at launch able to operate at 532 and 1064 nm, for technical reasons CATS operation quickly became 1064 nm only. Thus, the majority of CATS cloud detections were based on measurements at 1064 nm. Meanwhile, even though CALIPSO operated at 532 and 1064 nm, CALIPSO cloud products are mostly based on measurements at 532 nm. This distinction has been made clearer in the revised text.

- Line 76: “the CALIPSO-KU averaging scheme” and remove “designed for CALIPSO”

We have updated the text according to this suggestion.

- Line 92: “ATB” => “TAB”.

We thank the reviewer for noticing this error, which has been corrected in the revision.

- Fig. 1d: Replace the y-axis max value by 1e8.

We thank the reviewer for noticing this. We have updated the figure and fixed the incorrect y-axis range.

- Lines 99–100: “bullets (including bullet rosettes and 3D aggregates)” => And 3D columns?

We have updated the text to make the shape categories more consistent with the definitions from Sato and Okamoto (2023). We thank the reviewer for pointing out this inconsistency.

- Line 129: “ice particles in those clouds are almost exclusively liquid” => Seems to defy a few laws of thermodynamics.

We thank the reviewer for noticing that we wrote a quite obviously nonsensical sentence. We hope the revision is free of such occurrences.

- Lines 169–170: “high clouds feature mostly 3D bullets and Voronois (the second one being significantly more frequent in the 20°S-20°N band)” => The comment in parentheses does not support the statement that “the CATS results shown here agree very well with the CALIPSO results”.

We agree with the reviewer that our assessment of the CATS vs CALIPSO comparison was too strongly worded and overestimated the agreement. In response to this comment, we have rephrased this passage.

- Line 194: “In daytime conditions (06:00-18:00 local time, not shown)” => Please add it to the appendix.

We agree with the reviewer that not showing this figure was a strange omission. We have now included it as Fig. A2 of the Appendix.

Reply to comment from reviewer 2

Original comments from Reviewer 2 are shown in blue, our replies are shown in black.

This paper uses the methodology of Sato and Okamoto (2023) that was applied to CALIPSO lidar (CALIOP) measurements to estimate the relative abundance of various ice particle shapes in clouds but now applies this methodology to the CATS (Cloud Aerosol Transport System) satellite lidar dataset for this same purpose. As a “sanity check”, consistency between the results from this new study and that of Sato and Okamoto (2023) was verified. Then the diurnal variation of ice particle shape was investigated for the first time, with quite interesting results. This paper is of high caliber and worthy of publication in ACP after minor revision by addressing the comments listed below. The paper is well organized and well written.

We thank the reviewer for taking the time to give feedback on our work.

Specific Comments

1. Section 2.2: As shown by Eq. 1 in Sato and Okamoto (2023), the lidar backscatter β is an integral product of the particle size distribution (PSD) and the particle’s mean backscattering cross-section (Cbk) where Cbk depends on particle size. Thus, β appears to be a measure of the PSD second moment, while the ice particle number concentration Ni denotes the 0th moment of the PSD. Is it safe to relate the fraction of a particle shape in this paper to the relative Ni of that particle shape in the clouds? That is, there may be a tendency for readers to interpret these results as a relative measure of Ni for each ice particle shape. Since the lidar depolarization ratio δ that is used to discriminate cloud particle shape is the ratio of two β values (for horizontal and vertical polarization), PSD effects should cancel, leaving just the depolarization effect. The statistics in this paper would thus be reporting the frequency of occurrence of δ corresponding to various cloud particle shape categories as defined in Sato and Okamoto (2023), where δ identifies the dominant shape sampled.
While in essence this is implied in Sect. 2.2 (and is evident in Sato and Okamoto), more of this information could be presented so that the reader can more clearly understand what the statistics in this paper actually mean.

The reviewer notes that when the lidar probes ice particles with different particle shapes, the importance in the measured backscatter signal will depend on the number of particles in each shape and on their size distributions. In other words, particle number concentrations being equal, larger particles will generate a larger backscatter coefficient. Meanwhile, since the depolarization ratio is a ratio of backscatters, in its retrieval the effect of particle size cancels out. This means the retrieved particle shape in a sampled volume is representative of the shape of the most numerous particles in that volume, with no regard to their size. We thank the reviewer for bringing up this valid concern that was not explicitly addressed in the original submission. We have added comments regarding this within Sect. 2.2 and at the beginning of Sect. 3.

2. The paper would be more interesting if it included images for the different ice particle shapes being evaluated. Voronois ice particles are especially important since many

readers may not be familiar with them, and several images may be justified due to their varied, complex shapes.

This is a very good suggestion, and we agree that the paper would be more intuitive to understand if it included images of particle shapes, especially for unfamiliar shapes like Voronois or Droxtals. However, there is to our knowledge very little imagery documenting these particle shapes. We are not aware of in-situ studies providing imagery appropriate to document the visual aspect of e.g. Voronoi or Droxtal particles, alongside the lidar measurements of depolarization ratio required to categorize their shapes. Building such a dataset would provide important validation to the results shown here, in Sato and Okamoto (2023), and in other related works. Following this comment, Voronoi particles are described in more detail in Sect. 2.2, and the conclusion now mentions the need for gathering in-situ imagery able to document the visual aspect of Voronoi and Droxtal particles to provide a more direct interpretation of the Sato and Okamoto classification.

3. Lines 251-252: Please cite Ken Sassen's work from the 1990's here. Ken was the first to relate lidar depolarization ratios to cloud particle shape as per my understanding, and he has many published papers on this topic.

We thank the reviewer for reminding us about the pioneering work done by K. Sassen. In the revised article the conclusion now includes two appropriate references authored by K. Sassen (1977 and 2001).

Technical Comments:

1. Line 69: This line contains "1.1.1 Subsection (as Heading 3)" and should be deleted.

We thank the reviewer for noticing this error, which has been corrected.

2. Line 92: ATB => TAB?

We thank the reviewer for noticing this error, which has been corrected.

3. Line 107: -80°S => -80°C ?

We thank the reviewer for noticing this error, which has been corrected.