We highly appreciate the reviewers' insightful and helpful comments on our manuscript.

(1) Many sentences of the manuscript have been carefully rewritten or re-organized to enhance the logic flow and make the statements stricter in a proper tone.
(2) WRF nested domain map is now included as Fig. 5 in the revised manuscript. A new diagram (Fig. 7 in the revised manuscript) has been included as well to better illustrate the methodology difference between the two views (space-view vs. ground-view). All most all the original figures have been improved of the quality and enhance the main messages they are supposed to convey to the readers (mainly according to Reviewer#2's suggestions).

We also would like to correct some typos/mistakes we made in the original manuscript: (1) to explain the parallax effect, we used the "grey bar" in Fig. 1c, which we actually meant to be the "black box"; (2) the reason for using 20-point averaging to process MLS radiance was wrong. The correct reason is now highlighted in red characters.

Comments from Reviewer#2:

The authors present excellent research that fits very well in ACP. The research is important and well-performed. However, the presentation could be significantly improved. Therefore, I recommend publication with minor revisions.

We are grateful to the reviewer#2 for your appreciation of the value of our work. We thank you very much for providing many detailed suggestions and comments, most of which are excellent suggestions and we adopted.

Please find specific comments below.

Page 24919, line 9: The authors mention the vertical levels of 5 km and 9 km. They argue that 5 km is roughly the freezing level. This is mostly true in the tropics, but judging from Figure 1b, the work is not limited to the tropics. Is this limit still valid? Secondly, how do the authors arrive at the 9 km boundary?

This paper is about tropical ice cloud, as indicated in the title. Although we applied the same layer definition to the mid-latitude, we don't see a significant systematic tilt signal out from the analysis. That may occur due to the improper setting of 5 km and 17 km as the layer boundaries, as indicated by the reviewer here. We would love to perform the analysis for the mid-latitude in the future with different layer settings. But since this paper is focused on the tropics, the current threshold would not raise a big concern.

As for "9 km", that is not the boundary we define to separate middle and upper troposphere, but rather an altitude above which CloudSat IWC result is trustworthy (Protat et al., 2009). Please refer to Line 26 of Page 4.

Page 24919, line 15: The authors interpret asymmetries as northward/poleward. Close to the equator, the ground track of Cloudsat is close to perpendicular to the equator (8◦ angle with the poleward direction). To interpret to what degree

asymmetries can be directly translated to north-south asymmetries as opposed to east-west imbalances, it would be instrumental to have a simple diagram showing the angle between the Cloudsat groundtrack as a function of latitude. This can clear any doubts as to the attribution of observed asymmetries to north-south as opposed to east-west phenomena.

Thanks for your suggestion. We explained in the main text (Line 13, page 24922; or in the revised version, Line 3-5, page 8) that the dominant signal should have been come from the north-south direction as the ascending and descending maps are very similar to each other. If the signal was from east-west direction, the two maps should look opposite in sign due to the non-perpendicular orbit track with respect to the equator.

Page 24919, line 15: The northward tilt is not immediately obvious. It appears the methodology to calculate this is outlined only in page 24920, up to line 18. The methodology to arrive at the blue line should be explained before the northward tilt is described.

I may miss your point here: do you mean that the northward tilt is not immediately obvious from the CloudSat cross-section image in Fig. 1b? Do you suggest to remove the north half of the image that could result an amplified south half of the image? The frontal clouds look to me all consistently tilt northward from the cross-section to me.

Page 24920, line 8: With "randomly" distributed, I suppose the authors mean randomly distributed according to a uniform random distribution. If ice is randomly distributed according to a non-uniform random distribution, the observation that the different paths would yield the same IWP may no longer hold.

Yes, we mean uniform random distribution. I can imagine that IWC tends to increase with decreasing altitude for UT ice cloud, but I can't see an apparent reason that the horizontal distribution obeying non-uniform random distribution. Now we add "in the horizontal direction" after "randomly distributed" to make this assumption stricter.

Page 24920, line 15: The authors name the number of 77◦, but explain only further down at line 24 how they arrived at this number. Please explain the calculation of the view-angles before giving this number.

77◦ comes from the original ratio of CloudSat horizontal (1.1 km) and vertical (0.25 km) resolutions, which is shown in Fig. 1c. We mentioned in the caption of Fig. 1 that the horizontal scale is squeezed to ¼ of the origin length, otherwise, the panels would look too squashed. The caption of Fig. 1 is not rewritten for clarification (highlighted in red).

Page 24921, line 2: Here, the authors say interpolation was not conducted. However, on page 24920, line 25, the authors say the IWC profile is initially interpolated. I'm confused. What did the authors do?

The interpolation was done only vertically for each CloudSat IWC profile, since the retrieved profile is not necessarily on a 0.25 km vertical grid but roughly

nearby the grid. We didn't conduct any slantwise interpolation as to compute the slantwise IWP, but staged by one horizontal grid each time (Fig. 1c) for a "mock slantwise view". Hence, interpolation would not cause an artifact for the ΔIWP computation.

Page 24921, line 4: The authors talk about tropical ice clouds. However, the maps clearly extend to the extratopics (how far? 35◦ or so?), and figures 1b and 2b clearly do not relate to the tropics. Is this method still valid?

The method should be still valid, but the vertical layers should be defined with separate ranges in the mid-latitude as the reviewer also suggested previously. The difficulty here is to smoothly transit the layer boundaries from a tropical atmosphere to an extra-tropical atmosphere. Besides, the vertical extension of convection is very different between winter and summer in the mid-latitudes. Convections in the summer mid-latitude more or less behave similarly to their companions in the tropics, and we can see similar "diverging" feature at the north and south peripheries of mid-latitude convective center.

Based on the above reasons, we limit the discussion of mid-latitude features to the minimal in this paper, and only touched features in the summer mid-latitude upper-troposphere as the results are consistent with those findings in the tropics.

Page 24921, line 5: "...extend form 5 to 17 km...", please show this or give a citation.

A citation of Wu et al. [2009] is added.

Page 24922, line 1: "differentiating" is not the right word here, you are (as I understand it) taking the difference between slant northward and slant southward. Differentiating would yield the vertical gradient of IWP or so.

Thanks. "Differentiating" has been replaced by "differencing".

Page 24922, line 11-15: Here the authors address that the Cloudsat orbital track is not perpendicular to the equator. Could you please elaborate a bit more on how this affects interpretation of the results? I think you are treating it too quickly here.

The sentence has been re-written as follows:

"Given the fact that CloudSats orbit is not strictly perpendicular to the equator (82◦ angle at the equator), any signal from the zonal direction projected to the orbit track would be opposite sign between the ascending and descending orbits. Therefore, the highly consistent geographic patterns between the day (ascending) and night (descending) imply that the signals should mainly originate from the meridional direction rather than the zonal direction."

Page 24922, line 19: Please explain in a quantitative manner how you reached the number 13◦

Because 90◦ − 77◦ = 13◦. The computation step is now added.

Page 24924, lines 12-17: I think it would be valuable to show some of these results for 5-11 km. Does Figure 5 show this? Then the authors should refer to Figure 5 from the text here.

Yes, Fig. 5b shows part of the map. Now we include in the text that “part of which will be shown in Fig. 5b”. Please also note that “upward and inward (or equatorward)” has been changed to “equatorward” or “converging” according to the Reviewer#1’s suggestion.

Page 24924, lines 17-18: What is the level of statistical significance that the authors describe as "barely significant"?

95% confidence level.

Page 24925, lines 1-4: Please briefly explain what is meant by primary, secondary, and innermost domain, as not all readers may be familiar with those terms.

A sentence has been included: *“Each nested domain is driven along the lateral boundary conditions supplied by the parent domain with coarse resolution.”*

Page 24925, lines 5-6: Please indicate this domain on a map.

A new figure has been included as Fig. 5 to show the domain map.

Page 24926, line 9: Why do you integrate "upward"? Why is the geometry of integrating from 5 to 11 km different in integrating from 11 to 5 km? The integrated IWP does not depend on the direction of integration.

A new diagram (Fig. 7) is now included to illustrate the difference between ground-based view and space view.

Page 24926, line 20: Authors go from Figure 5 to 7 (and later 6). Please fix the order of the figures.

Agree with you. Thanks. The figure order has been swapped between Fig. 6 (now Fig. 9) and Fig. 7 (now Fig. 8).

Page 24926, line 20: Are these results now from CloudSat, DARDAR, or WRF?

No. Results shown in Fig. 7 (now Fig. 8) are computed solely from CloudSat. It’s clarified now in the text.

IWP has almost the same magnitude, but opposite sign, as nadir IWP. That would mean slant IWP is close to 0. Clearly I’m misunderstanding something? Why does Figure 7 (left panels) look the way it does?

Sorry for a mistake we had in the panel title of Fig. 7 (now Fig. 8). IWP differences in the left panels were computed by subtracting slantwise IWP from nadir IWP. The figure title and corresponding text have been corrected.

Page 24927, line 4-5: Could you quantify this relation with an equation?

An equation has been included in the parentheses after the “cosine law” (highlighted in red).

Page 24927, line 8: Please explain a bit more on the two completely different definitions of delta-IWP used in this paper, preferably in the methodology section. It's not really clear to me. Surely if cloud mass is tilted, we should see tilt both from above and from below? This could use an illustration, similar or added to Figure 1.

We do not fully understand why there is a cross-latitude positive value of ΔIWP from the ground-based view, but we think the discrepancy between the ground-based (now illustrated by Fig. 5) and space views (Fig. 1c) can be explained by the fact that the former is total column integration, and the latter is separately computed from two layers. Instead of thoroughly explain the feature seen from the ground-based view, we'd rather raise it as an open question for future exploration.

Page 24927, lines 21-26: One more problem that is not explained is why the sign is different at 5 - 11 km (Figure 5)

Excellent suggestion. Thanks. Now a sentence has been included:

"At 5-11 km, Hadley circulation computed from the reanalysis wind is weakly divergent. Therefore, the possible 5-11 km ice cloud equator ward tilt cannot attributed to the general circulation, either."

Page 24928, line 13: How would this explain the "upward and inward" tilt?

For each single convective cloud, 5 – 11 km could still be weakly converging, or the tilt of convective core may still be determined by the lower-level wind shear below 5 km. We add the assumption now: "…assuming the slantwise orientation of the convective core is determined by lower level wind below 5 km".

Page 24935, Figure 1a, 1b: it would be helpful to add the date to these figures, in a somewhat easier notation than "day of year".

Suggestion adopted.

Page 24935, Figure 1c: The way it is currently drawn, it appears that the slant paths are longer than the nadir path. Instead of drawing a diagonal through the northward and southward views, it would be more correct to draw a vortical line in the middle of each of the green and yellow squares. Then it would become more apparent that the slant path has the same length as the nadir path.

Sorry, we didn't get your point. Do you mean that we should delete the two slantwise line-of-sight arrow lines?

Page 24937, Figure 3: The maps would be somewhat easier to place if the authors would draw lines of longitude and latitude, and label those at the edges of the map. The continents are highly deformed so the user might need to puzzle a bit before realising what area of the world is shown. It is also very relevant where the equator is. Secondly, the authors might want to consider a more appropriate map projection (any pseudocylindrical projection would do)

Thanks. Suggestion is taken.

Page 24937, Figure 3: The contour lines are not very obvious, and the gradient of the contour lines is unclear. I would recommend the authors to explore a clearer way to visualise both IWP and the tilting thereof.

Thanks. The figures are indeed very busy. We now enhance the coastal lines and add longitude and latitude grids to hopefully make the figures clearer.

Page 24939, Figure 5: Same comment as earlier. Please add lines of longitude/latitude and/or tick marks, and show more clearly what contour corresponds to what value of IWP.

Thanks. Suggestion is taken.

Page 24941, Figure 7: Please add ticks to the right y-axis, indicating the values of the absolute IWP. It seems strange to use the same axis to indicate the difference and the absolute value.

A label is now added near the black line (absolute IWP). Since they have the same value range and unit, putting only one y-axis would be more straightforward to show that the difference is on the order of the absolute value.

Page 24941, Figure 7: Please add the seasons in the figure caption.

Thanks. Suggestion adopted.

Editorial comments:

Page 24916, line 9: Replace "little" by "less"

Thanks. Replaced.

Page 24917, line 1: Replace "require" by "requires"

Thanks. Replaced.

Page 24918, line 26: Add "the" before "CloudSat"

Thanks. Added.

Page 24919, line 8: Add "the" before "CloudSat"

Thanks. Added.

Page 24919, line 12: Replace "showed" by "show"

Thanks. Changed.

Page 24919, line 23: Replace "good" by "well"

Thanks. Changed.

Page 24920, line 28: Replace "resulted" by "resulting"

This sentence has been deleted based on the suggestion by Reviewer#1.

Page 24924, line 12: Remove "the lower level" (duplication)

The second “the lower” has been replaced by “that”.

Page 24925, line 15-16: I would suggest to write: "...interpolated to 250 metre vertical and 1 km vertical resolution". As short as now and more readable.

Thanks. Changed.

Page 24925, lines 17-20: Swap those lines around, they are confusing now.

Deleted. Only 77 deg view angle result was computed from WRF simulation results.

Page 24927, line 25: Replace "slopping" by "sloping" Page 24928, line 3: Remove "that" Page 24928, line 6: Replace "structures" by "structure"

Thanks. All replaced.

Page 24935, Figure 1: Please make sure all labels are large enough to be readable

Labels have been enlarged for Fig. 1. Thanks for your suggestion.

General: In many places, the authors use the compact notation such as "a positive (negative) value means it tilts northward (southward)". I think it would benefit readability to describe this more verbosely, i.e. "a positive value means it tilts northward, whereas a negative value means it tilts southward" or "...means the opposite" or so.

Suggestion adopted at the new manuscript: Page 6, Line 12; Page 11, Line 17-18; Page 12, Line 20.