Response to reviewers' comments:

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. (3) Fig. 8 (previously Fig. 7) was replaced. A coding error was discovered after submission of the first round response, and now the entire paragraph (page 12, Line 15 – page 13, Line 13) was rewritten. As a result, line numbers and some responses to the reviewer's comments should be changed accordingly. They are now shown in red in the revised response letter. Thanks for your understanding!

Comments from Reviewer#1:

The article illustrates the existence of a systematic vertical asymmetry of clouds depending on the geographical region and latitude. The authors argue that not taking this asymmetry into account may lead to systematic uncertainties in ice water path (IWP) retrievals in the order of 5–20 % depending on the regions (mostly close to regions just north and south of the ITCZ). The degree of cloud-slanting is computed by comparing the difference between the IWP from integrating ice water content (IWC) at an inclination angle from south to north (forward and down in the satellite path) minus the IWP from integrating IWC at the reciprocal inclination angle from north to south (backward and down in the satellite path). This is illustrated in figure 1 in the article. This paper mostly relies on the CloudSat RO IWC dataset for this assessment.

The conclusion is that this uncertainty aspect may be important for retrievals from limb-sounding measurements of ice clouds, retrievals from high scan angles and low resolution models, which mostly use a maximum random cloud overlap assumption within each grid box.

This is only part of the conclusion, or the implications of the importance of this work to improve future satellite retrievals and/or model cloud physics scheme development. The major conclusion is that upper-troposphere ice cloud in the nature has a systematic meridional tilt in the tropics, which has never been

studied before. This paper is a science and observation oriented work, and the scientific discoveries are the real focus.

According to my assessment the overall point is conveyed that convective clouds appear to “climatologically” slant polewards at the edge of convective regions, but for me, many arguments are unclearly written and apparently not sufficiently justified. The main problem with the paper is that the results presented are unclearly described and much more care must be taken to explain their line of thought and to better motivate the very strong statements made. There are many unclear sentences. I suggest finding a colleague with a english-speaking background, to read through the article and highlight to the authors which sentences are unclear and help to reformulate them so that the message comes across clear enough so the point can be made.

We thank the suggestion from this reviewer. As a matter of fact, the third author of this paper is a native speaker and has been teaching college students for almost 15 years. The original manuscript was thoroughly edited before submission. We believe that English and grammars should not be a big issue that causes any difficulty in understanding the content. Rather, we admit that the logic may be jumping at some places, and some statements may be too strong. We have carefully edited the manuscript according to the inputs from the two reviewers. We truly hope that the revised manuscript is clear enough to follow. We highly appreciate Reviewer#1’s insightful comments on improving the readability of the paper.

Scientifically, I also believe it is essential to tie “cloud slanting” to the wind fields, partly to prove the point and, most importantly, to make the results applicable. For instance, if it is true that there is a systematic tilt in the clouds, how can the modellers correct for this?

As was explained in the response to the Reviewer#1’s first major comment, the major contribution of this paper is that this is the first observational evidence showing that UT clouds in the tropics are systematically tilted. Mean meridional wind fields are also shown in Fig. 4 to tie the observed cloud slantwise tilting to the general circulation. Please note that Fig. 3 and 4 have been re-plotted to enhance the figure quality. Hopefully, the wind arrows can now be seen more clearly.

In this first step of work, we simply would like to point out that the current GCMs using the “maximum-random” overlapping scheme globally may have additional biases or larger uncertainties in regions we observe systematic cloud tilt. It is beyond the scope of this study of giving a solution to the modeler.

The choice of datasets also appears strange to me. Why choose both CloudSat and DARDAR? They are very similar datasets since the are based on measurements from the same instrument. For IWP, it is expected that the two datasets will be quite similar as long as the clouds are not thin.

Firstly, DARDAR is a joint retrieval of CloudSat, Calipso and MODIS measurements. It contains much more ample ice cloud details compared with

CloudSat. DARDAR is indeed not an independent observation. Some conclusions (e.g., their consistencies validate ….) have been tuned down in the revised manuscript. Secondly, DARDAR and CloudSat disagree with each other on the tilt direction of the lower level ice clouds, while WRF simulation results support the CloudSat observation.

The paper states that this uncertainty "invalidates" the plane parallel assumption used in most IWP retrievals from passive instruments. This is a very strong statement which is not explained in the paper.

By definition, "plane-parallel atmosphere" means that the atmospheric compositions (e.g., CO2, Ozone, H2O, cloud) and characteristics (e.g., Temperature) should be horizontally homogeneous. In such case, any property from nadir-view can be immediately converted to any slantwise view by simply multiplying the cosine of zenith angle. That's apparently not a good assumption for highly inhomogeneous cloud field.

In case the reviewer is not familiar with the "plane-parallel bias" in cloud property retrievals, the reviewer is kindly referred to Cahalan, R. and his colleagues' publications on such a topic, some of which are listed in the reference list appended with this comment response.

Coming back to the specific environment here in this paper to evaluate whether it's appropriate to use such a phrase, we deleted it and re-wrote the paragraph due to a coding error discovered later on in producing Fig. 8 (previously Fig. 7). We apologize for the mistake. Basically we found that the converging tilt in mid-troposphere can largely cancelling the effect of diverging tilt in the upper troposphere. Therefore, for ground instrument and nadir (near-nadir) observations that can penetrate the total column, "plane-parallel atmosphere" might be safe at most places. But still, this assumption is used for layer by layer calculations of radiation transfer in a lot of cases. For those cases, it is not a good assumption for cloudy sky.

Furthermore, the possible uncertainty of 5-20% due to cloud tilt is not alarmingly large from an observational point of view since, even in the CloudSat retrievals, the errors based on simulations are at least 40 % for some assumed particle microphysics (Austin et. al. 2009). Considering the additional uncertainties induced by assuming one ice particle distribution over another along with the Radar measurements hypersensitivity to large particles in radar retrievals because of Rayleigh scattering Z ~ D6, and more uncertainties, the 40 % estimate is likely too low. More likely the random errors are around 100%, give or take. For passive IWP retrievals there is an additional large uncertainty from not knowing the vertical distribution of clouds.

Firstly, Austin et al. [2009] claimed that CloudSat IWC retrieval error was at most 40%, not at least. Therefore, 5-20% is an alarming value to raise concern. Secondly, this is the first research that shows that ice cloud tilt is systematic rather than random.

2 Specific comments

- page 24917, line 9 :: “irregular visible outlooks to internal banded mass/energy structures.” I don’t understand this sentence

  We mean that the cloud bulk shape (i.e., outlook) is visibly irregular, and the cloud internal mass is also inhomogeneous, often exampling banded structures (as can be seen from Fig. 1).

  Now the sentence has been rewritten as “*Cloud 3D effects manifest themselves as multiple forms: the bulk outlook is visibly irregular, and the internal mass structures are also inhomogeneous.*”

- page 24917, line 10 ::“These detailed structures are often not fully resolved in satellite observations due to large sampling footprint size and, subsequently, neglected in GCMs” What satellite observations are you referring to? MODIS and the AVHRR-based datasets have footprint sizes comparable your reference dataset, CloudSat RO. The biggest problem is the lack of information on the vertical structure of clouds from these passive instruments. What do you mean by the 3D effects being neglected by models as a consequence?

  We apologize that our original statement was too generous and didn’t specify the causality. Your suggestion is very valuable. The sentence has been rewritten as “*The detailed cloud vertical structures are difficult to be resolved in passive satellite observations. Subsequently, they are either neglected or significantly simplified in GCMs*”.

- “However, studies have shown that this parameter has large geographical and temporal variations around the globe, which invalidated the prevailing assumption in GCMs.” Tone down this statement. Going so far as to say that regional variations in cloud overlap “invalidates” the overall overlap assumptions of basically all climate models requires more sentences to convince the reader.

  We fully agree with your comment. The new sentence is now written as “*However,…, which implied that the prevailing assumption in GCMs needed to be improved and could be constrained by satellite observations.*”

- Introduction: A description of what is meant by tropics in this study is missing (e.g. latitude bounds)

  “Tropics” is defined as [30S, 30N]. The boundary has been clarified in the abstract and the introduction sections.

- Page 24918, line 22: Avoid links in the paper as they will break over time.

  Thanks for the suggestion. Now the web link has been moved to the footnote.

- Page 24919, line 1 :: There are more uncertainties in the CloudSat RO dataset that should be mentioned (see above). At least the “official” 40% uncertainty should be mentioned.

  Austin et al. [2009] (see added reference) claimed that the uncertainty is less than 40%. Could you give us a reference that explicitly claims the “official” uncertainty level? Thanks.

  A sentence has been added in the paragraph to mention the uncertainty in the retrieval.

- Page 24920, lines 28–29 :: You are referring to figure 4 before it is introduced. At this point, not even figs 2 and 3 have not been mentioned yet. Maybe see over the order of the figures

  Suggestion accepted. This sentence has been deleted.

- Page 24921, lines 9–10 :: “The parallax issue is mostly solved by this assumption through large sample integration.” I don’t understand this, please elaborate in the text what is meant
  Fig. C1 (Fig.1 from Wu and Vayaka [2013]) shown below should help explain the “parallax issue” if it’s new to the reviewer. We also include this paper in the reference list in case some readers are not familiar with this concept. The basic concept is that slantwise view of a cloud would project the cloud location to a wrong place (i.e., the two slantwise dashed lines would project the cloud to a wrong location). Only when you know the cloud top height that you could correct this parallax effect induced registration bias. You are also referred to Fig. 1 of Marchand et al. [2007, JGR] paper for more illustrations.
  In our paper, if the slantwise integration path pair starts from the top of the layer of interest (i.e., 17 km for the upper troposphere), the same cloud would be registered to two different locations separated by 2*(17-11)=12 km. Since cloud top within the layer varies, it’s reasonable to assume that the average cloud mass center is at the middle of the layer (14 km), and starts the integration path pair from the middle of the layer (Fig. 1c in our paper). We apologize that the “grey bar” in the original text should be changed to “black box”.

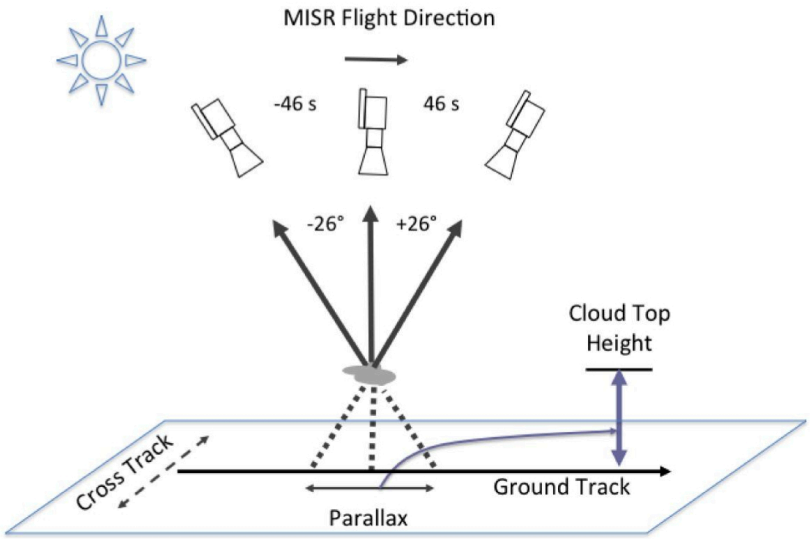

Figure C1: Diagram showing the parallax effect of MISR. This figure is adapted from Wu and Vayaka [2013]'s Fig. 1.

- Page 24921, line 21 :: " beat down the noise and distill the complex cloud information" I would tone this down. The ice cloud measurements are very, very coarse from the limb-sounder so I don't know if averaging 20 profiles will distil complex cloud information

  We apologize that this explanation was wrong. The correct explanation of using 20-point averaging is that "*By averaging the 20 saturated radiance measurements at the bottom of each scan, we can treat the averaged radiance as those measured from the slant views by a nadir sounder rather than from a limb column, which help distill the complex cloud information [Wu and Eckermann (2008)]* ".

- Page 24921, lines 26–27:: "Hence, it cannot be used as an independent observational evidence but rather as a supplement." Why is not MLS considered an independent dataset compared to CloudSat RO? Granted that the uncertainties from MLS IWP are very large and the dataset might not be ideal for assessing cloud tilt, but it is quite independent from CloudSat I'd say. The DARDAR and CloudSat RO datasets on the other hand are dependent datasets. Maybe you don't mean dependent?

  MLS obs. is indeed independent with CloudSat. The largest difficulty is that TB difference between ascending and descending orbits contain cloud diurnal information, which is not removable by any means using MLS only. Therefore, the evidence we saw from MLS, although highly agreeable with CloudSat, cannot be used as a direct, "independent" support to the results we found from CloudSat.

  Now we rephrased the sentence as "*the analysis results using MLS observation have to be interpreted with a lot of caution. Details will be discussed in section 4.*"

- Page 24922, lines 25–26:: “The broad consistency between CloudSat and DARDAR analysis results validate the robustness of our findings.” As mentioned earlier, these datasets are not independent

  We agree with the reviewer that DARDAR and CloudSat are not independent. Please notice that we didn’t claim anywhere in the paper that they were independent. The DARDAR analysis results were originally planned to be shown only in the appendix, but the editor suggested to include all in the main text as the DARDAR results were supportive and DARDAR was more or less different from CloudSat, especially when thin cloud was present. At the lower level, DARDAR data have in general better capability to resolve precipitating cloud (see our response to the next question).

  We fully agree with the reviewer that this statement was too strong. We now retreat back a bit. The word “validate” has now been replaced by “show”.

- Page 24924, lines 10–13 :: “IWC itself cannot reveal the entire cloud mass/shape structure in the lower level as liquid and mixed-phase clouds dominate the lower level (e.g., see the round-up at the bottom of Fig. 1a).” What does “round up” mean? And does the figure really illustrate this problem as stated?

  “Round-up” is an inaccurate word to be used here, which should be replaced by “rounded bottom”. What we mean is that CloudSat radar signal tends to easily be saturated at heavily precipitating scenes (e.g., the two deep convective clouds in Fig. 1a between 9N and 10N, compared with Fig. 2a). Now the sentence has been changed to “*e.g., the rounded bottom of deep convective clouds of Fig. 1a between 9◦N and 10◦N*”. We also replaced “round-up” used in a later paragraph of the text.

- Page 24924, line 19:: “we will show using the WRF simulations that CloudSat results might be more reasonable.” Show that CloudSat is more reasonable than what, DARDAR?

  Yes. At lower level, CloudSat result indicates that the ice clouds should tilt inward while DARDAR result is contradictory. Although we know that CloudSat has saturation issue with heavily precipitating cloud, WRF simulation agrees with CloudSat analysis result nevertheless.

- Page 24926, line 5:: “The “upward and inward” mid-level ice cloud mass” What do you mean by “upward and inward”?

  This paragraph discusses simulation results shown in Fig. 5. Since the lower-level signs (bottom panels of Fig. 5 from CloudSat and from WRF) are completely opposite to upper-level clouds (top panels of Fig. 5), the systematic tilt direction of lower-level ice cloud should also be opposite, and we explained it through mass continuity (convergence at lower level and divergence at upper level). We agree with the reviewer that “upward and inward” is not an accurate nor proper phrase to describe such a

phenomenon. We replaced it with “converging” instead. Also, this sentence has been rewritten into a paragraph to hopefully state the feature clearer.

“In the middle troposphere, most ice clouds are convective cumulus. Some of previous case studies suggested that the tilt of convective core within a convective system could experience a life cycle of downwind, upright and upwind with respect to the local wind shear (Weisman and Rotunno (2004), Lane and Moncrieff (2010)). By far, the climatological characteristic of the vertical orientation of deep convective cumulus has not been well studied nor understood. According to Fig. 5d observed by CloudSat and Fig. 5e simulated by WRF D03 experiment, both of which show generally opposite patterns to the UT ice clouds, we can reach the conclusion that the mid-level ice cloud mass tends to exhibit a ”converging” signature on a climatological mean.”

- Page 24926, line 28 :: “This indicates that on average ice clouds are slim and sporadic.” How do you reach that conclusion?
  This paragraph has been rewritten (Line 15, page 12 to Line 13, page 13).
- Page 24926, line 29 :: ““Plane-parallel atmosphere” assumption is constantly violated when ice cloud is present” This very strong statement is not explained. If this is so, you need convincing arguments.

  Please refer to the response to the 5th major comments.
- Page 24927, line 1 :: “nearly always” Is this globally valid?

  That’s what Fig. 7b and 7d tells us (please note that Fig. 7 has been moved to Fig. 8, while we still use the old figure number). We apologize if the explanation of the dashed lines of Fig. 7a and 7c were confusing. Now with the re-organized explanation in the figure caption, we hope the reviewer could vividly see the difference between solid and dash lines.
- Page 24927, line 2 :: “more integrated ice cloud mass than the northward-view based on the CloudSat observation.” What do you mean?

  This paragraph has been rewritten (Line 15, page 12 to Line 13, page 13).
- Page 24927, lines 5–8 :: “This result is not contradictory to our finding on the systematic cloud tilt, since firstly the integration path here extends through the entire troposphere above the freezing level, and secondly the reference point is at the ground.” How is this different from integrating CloudSat IWC?

  Please see my previous explanation of the difference between ground-view and space-view.

  We now include a new diagram in Fig. 7 to explain the groundbased view.
- Page 24927, lines 11-14 :: “Another possibility, which is more likely to happen, is that the “bottom round-up” effect near the freezing level of

CloudSat IWC retrieval may significantly skew the overall ice cloud mass distribution." I don't understand this statement at all

We mean the CloudSat signal saturation near the freezing level for heavily precipitating scenes may introduce a significant dry bias for the calculated IWP and ΔIWP if we integrate upward from 5 km. Note that CloudSat IWC retrieval is not only for non-precipitating ice cloud, but also includes precipitating frozen particles. The "ice cloud tilt" concept in the lower level and in the ground-view study includes cloud and precipitating frozen particles as a whole.

- Page 24928, lines 19–21 :: "Clearly, neglecting systematic cloud tilt in satellite retrieval can result in additional biases especially for limb sensors (e.g., Microwave Limb Sounder), nadir sensors at slantwise view-angles (e.g., AIRS, MODIS)" The maximum scan angle for MODIS is a bit more than 50 degrees if I recall correctly, i.e., much less than 77 degrees used to test the cloud-slant-problem, and the furthest off-nadir footprint is "only" a few kilometers across, i.e. a fair bit less than the length of the "curtain" used to find ΔIWP (if I understand figure 1c correctly). Therefore, at worst, the error introduced by not taking the cloud slanting into account will lead to less error then the 5 –20 % found in this study. I'm not convinced that this is a problem for MODIS/ AVHRR. I could be missing something here, please convince me.

  You are absolutely correct. Ignoring the systematic cloud tilt in the meridional direction would result at most 20% of retrieval error of ice cloud mass, as we concluded from this paper (stated in the abstract and the conclusion section). However, the errors would likely to be larger at the zonal direction (estimated to be up to 50%), as we suggested in our previous two papers listed in the reference list of the manuscript (Gong and Wu, 2011, GRL; Gong and Wu, 2013a, JGR). MODIS is a cross-track scanner, so it would be impacted. I'm personally not familiar with AVHRR, so I would not comment on that instrument. Moreover, this is the first finding that could tilt could be impactful to cloud retrievals. It's worth mentioning the potential issue of retrieval algorithm that ignores this effect, even the impact would be small compared with some other dominant factors.

- Figure caption 1: "The blue curves whose zero values are centered around the 5 and 17 km vertical level illustrate the ice water path differences (Δ IWP) derived from the algorithm demonstrated in the diagram" What do the blue lines mean? Zero difference in what? The text in the article didn't help me either.

  We apologize that the figure caption was not clear enough to the reviewer. The blue curves are $\Delta IWP=IWP|_{S\text{-}view}-IWP|_{N\text{-}view}$ calculated for the upper-troposphere (11-17 km) and middle-troposphere (5-11 km) separately. The former result is shown as the blue curve at z=17 km, and the latter

result is shown as the blue curve at z=5 km. Therefore, "zero value" means that ΔIWP=0, which would fall exactly at z=17 km or z=5 km.

• Figure 7: The dashed lines don't show up in the legend

Fig. 7 (now Fig. 8) has been replaced, and the figure caption has been re-written.

3 Technical comments

- I decided to not dig into technical details as much of the text needs rewording for clarification

We truly hope that the revised manuscript is now clarified enough for the reviewer to take the next step. Thanks. We deeply appreciate your help on improving the readability of our paper.

References:

Cahalan, R. F., D. Silberstein, and J. B. Snider, Liquid water path and plane-parallel albedo bias during ASTEX, J. Atmos. Sci.,52, 3002–3012, 1995.

Marchand, R. T., T. P. Ackerman, and C. Moroney (2007), An assessment of Multiangle Imaging Spectroradiometer (MISR) stereo-derived cloud top heights and cloud top winds using ground-based radar, lidar, and microwave radiometers, J. Geophys. Res., 112, D06204, doi:10.1029/2006JD007091

Rozwadowska, A., and R. F. Cahalan, Plane-parallel biases computed from inhomogeneous Arctic clouds and sea ice, J. Geophys. Res., 107(D19), 4384, doi:10.1029/2002JD002092, 2002.

Wu, D. L. and M. J. Kayava (2013), CGMS working paper global wind measurements from earth orbit – atmospheric motion vectors and development of Doppler Lidar Systems, CGMS-41 NASA-WP-05.

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. Fig. 7 (now Fig. 8) has also been replaced due to a coding error discovered later on. The corresponding contents were rewritten to accommodate the new features shown in Fig. 7 (now Fig. 8).

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

It has been removed. See answer to previous question.

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.

Fig. 7 (now Fig. 8) has been replaced. We add a new label of mean IWP and we intend to keep the original single y-axis to show up that the total column integration is trivial compared to the mean.

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

Thanks. Now only JJA is shown as DJF reaches the same conclusion. We add the description in the figure caption.

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.