

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.

#### **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 an 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.

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 they 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., CO<sub>2</sub>, Ozone, H<sub>2</sub>O, 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.

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  $Z \sim D^6$ , 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 exemplifying 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”.

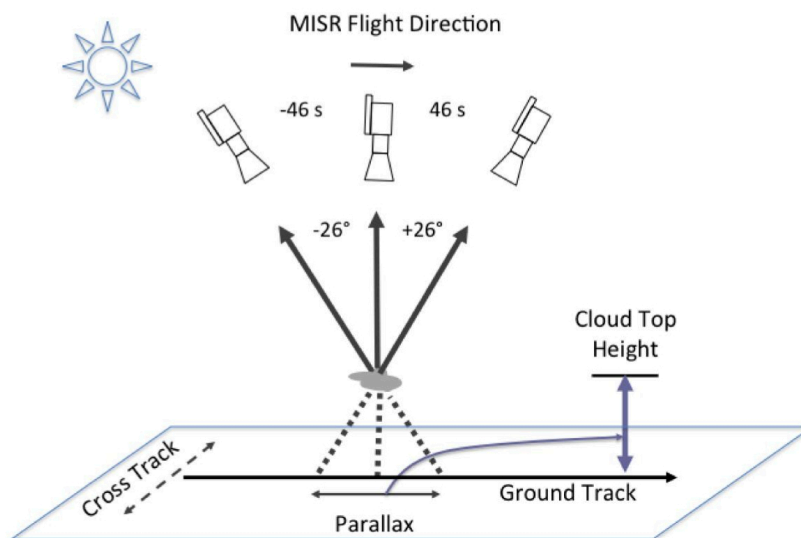


Figure C1: Diagram showing the parallax effect of MISR. This figure is adapted from Wu and Vayaka [2013]’s Fig. 1.

- 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?

Since the slant-view yields  $\sim 1/2$  of the nadir view IWP as seen from Fig. 7a and 7c, the average ice cloud width should be at least twice the average cloud thickness mass-wise. As the integrated IWPs from various slant-views differ up to 20% of that between the slantwise and nadir views (Fig. 7b and 7d), it is necessary for clouds to be sporadically present.

The main text has been reorganized for clarification.

- 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 5<sup>th</sup> 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?

Because that the colored lines ( $\Delta IWP = IWP|_{S\text{-view}} - IWP|_{N\text{-view}}$ ) in Fig. 7b and 7d (now Fig. 8) are nearly always positive across all latitudes and no matter what view angle number was selected. Note that S-view here has different definition with the space-view. Here, S-view starts from  $z=5$  km upward and looking to the south direction. So does the meaning of N-view. Therefore, the “nearly always” positive difference is not contradictory to our previous findings from the space-view.

- 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  $\Delta IWP$  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  $\Delta IWP$  (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 than 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 cloud 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 ( $\Delta$  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  $\Delta IWP = IWP|_{S-view} - IWP|_{N-view}$  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  $\Delta IWP=0$ , which would fall exactly at  $z=17$  km or  $z=5$  km.

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

Sorry that the caption of Fig. 7 (now Fig. 8) was not clear. It has been re-organized. Now the caption reads:

"Left panels: latitudinal distribution of  $\Delta$ IWP between southward-looking view and nadir (solid lines), northward-looking view and nadir (dashed lines with the same color of solid lines). Right panels: latitudinal distribution of  $\Delta$ IWP between southward-looking and northward-looking views (solid color lines) integrated from 5 km to 19 km. Top panels for DJF and bottom panels for JJA means. The black solid line is the mean IWP at nadir. Note that the Southward-looking view means looking upward to the south direction with the base starting from 5 km (i.e., opposite to satellite-based viewing geometry)."

### 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:

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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.

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