Reply to Reviewer#1:

We sincerely thank the reviewer for providing his/her valuable comments and suggestions. We especially appreciate your suggestion on reordering the flow of writing to make the logic go more smoothly, as well as pointing out an important reference that we overlooked. Our replies to the questions will be shown in blue below.

Instead of discussing how the observed polarization signals can be used for further study and how this can further affect the accuracy of IWP retrievals in detail (which would be rather significant for future study), it only analyzed the observation results and only mentioned that 30% error will be caused if polarization is neglected both in the abstract and in the conclusion part. How exactly can the observation of PD improve the accuracy of IWP retrievals, how will PD be used? The results mentioned here are not convincing at all.

This point is closely related to one of the lead author's previous paper on using 157 GHz brightness temperature to retrieve column-wised ice water path (Gong and Wu, 2014). For the referee's convenience, the related figure is attached here:

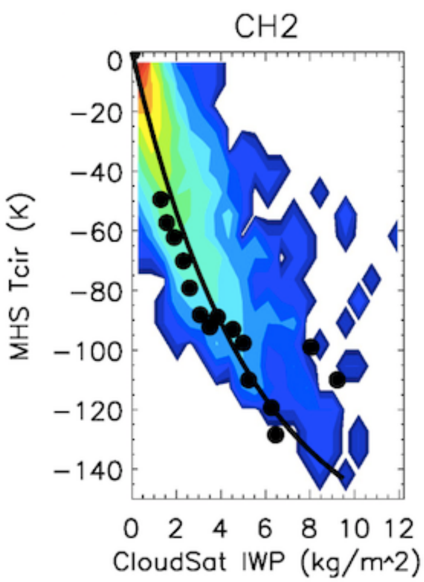

Figure R1: Two-dimensional probability density function to show the empirical relationship of column-wised Ice Water Path (IWP) and 157 GHz cloud-induced brightness temperature (Tcir) relationship. This empirical relationship is generated from collocated and coincident CloudSat IWP and Microwave Humidity Sounder (MHS) Tcir at near-nadir-view (scan angle between $-5°$ *and* $5°$*) measurements in the tropics (*$[25°S, 25°N]$*) collected during June 2006 to March 2011. The peak showing the largest possibility is shown as the black curve. This figure is adapted from Gong and Wu, 2014, Fig. 3a. Tcir is defined as measured TB minus the clear-sky radiance (Tccr), where Tccr is calculated using the Community Radiative Transfer Model (CRTM) by inputting MERRA atmospheric profile without cloud layers.*

We assume that we could reach a very similar curve for GMI's 166 GHz channel. Under this assumption, we can see that for anvil clouds (i.e., medium thick), in general Tcir falls roughly between -40 to -80 K, which corresponds to the steepest drop in the slope of the black curve in Fig. R1. Meanwhile, anvil cloud also possesses the largest PD. So a 10 K PD can easily result in 33% difference in IWP retrieval if Tcir is measured as -50K for the H-pol channel (corresponding to IWP=2 kg/m2) vs. -40K for the V-pol channel (corresponding to IWP=1.5 kg/m2). This is where our "30% of uncertainty in IWP retrieval" came from.

As this point is beyond the main ideas we intend to discuss and convey to the readers, we just mention it very briefly by the end of the main content to bring up one of the many reasons that understanding PD is important. We realize now that some readers might be interested to know

more, so we add a few sentences to clarify this point in the revised manuscript now, but readers are referred to Gong and Wu (2014) for all the subtle details.
(It's now read "Last but not the least, the observed PD-TB relationship has an important implication for cloud ice retrieval. Gong and Wu (2014) used an empirical IWP - TB relationship derived from CloudSat-MHS (Microwave Humidity Sounder) measurements for the IWP retrieval, where they found this relationship is nearly linear for medium thick ice cloud (i.e., anvils). The observed PD value range in this study therefore can be translated into a 30% IWP retrieval error if polarization is neglected. This is a very rough estimate that warrants a thorough evaluation in the future. ").

The structure of the paper is confusing. The authors described the observational data used in the study and analyzed the data, and then suddenly jumped to RT model description. Following the observational statistics in Section 3, another simple model was built up in Section 4. Basically, the paper was organized from data analysis to model description in Section 2, back to data statistics in Section 3, and then discussed with models again in Section 4. It seems to me that this is a bit chaotic.

We originally followed a traditional template that puts data/model/methodology in Section 2, and results in Section 3. But as the reviewer suggested, since an example has already been given and discussed in the data description parts (Section 2.1 and 2.2), the logic flow is interrupted if we continue on Section 2.3 with model description. We now move Section 2.3 to Section 4.1, and add a paragraph at the end of the new Section 4.1 to connect the context ("In the next section, we will proceed the model explanation from an extremely simplified two-layer model. By computing the layer-by-layer radiative transfer with including the AR concept, we can reproduce the bell-curve with reasonable range of PD values. Then, the more sophisticated RTMs described above will be employed for further simulating and understanding the observed PD - TB characteristics.").

At the end of Section 2.3, the authors introduced the concept of AR and defined it as the ratio of V/H scattering coefficients. Then the authors mentioned in the following sections that this parameter AR is equivalent to what is mentioned in Davis et al. 2005. However, the AR in Davis at al. 2005 describes the shape of ice particles (the ratio of the long axis to short axis of spheroids), this is totally different from the AR defined in this paper (Section 2.3 and 4.1). The authors didn't discuss in detail (1) how this AR in the paper is affected by particle microphysics at different frequencies (habit, orientation, size and so on); (2) Why AR is independent of height, considering the complex atmospheric conditions. To understand your conclusions in the following sections, the authors should also present what the simulated value of AR is for different particles shapes/orientation/other microphysics.
I do believe the particle habit is related to the V/H scattering coefficients, but this is not the only factor. The orientation of ice particle, which was mentioned several times in the paper but

not discussed here at all, is another important factor, which is related to AR defined in the paper.
We totally agree with the reviewer (and thank you for bringing this point up) that the definition of AR in this paper is not clearly tied to a direct microphysical meaning, but rather a column-wised average ratio between τ_V and τ_H. Therefore, every microphysical characteristic along the line of sight that impacts the optical depth would also impact the value of AR, which includes but not limited to the particle habit, orientation and size projected to the line of sight. We now add a sentence immediately after first introducing the AR concept by stating that "As one can see from the definition, AR is a function of all microphysical property that plays a role in determining the optical depth along the line-of-sight (LOS), including particle size, orientation, habit, etc." Furthermore, we also restated when we cite Davis et al. (2005) that the definition of AR is not exactly the same between ours and theirs.

Having said that, one of the most important assumptions we made throughout the highly simplified, illustration-purposed two-layer model is that every microphysical property is homogeneous within the ice cloud layer. With that assumption, AR is equivalent to the actual axial ratio of the ice particle projected to the GPM viewing plane. Of course this is not likely the case in reality, and varies a lot case by case. But as we found later in Fig. 8, the best-fit AR varies only in a narrow range of 1.2-1.4. Considering that 89, 166 and 640 GHz channels are sensitive to completely different parts of the ice cloud layer, such a narrow range of "best-fit" value of AR strongly indicates that the homogeneity assumption along the line-of-sight is actually not bad at all in a statistical view.

Please note that in the original manuscript, when we first define "AR", we explicitly explained that "we vary the AR value but keep the rest model input parameters (e.g., D_{me}, IWC profile, etc.) unchanged. This is equivalent to the particle AR effect in which horizontally-oriented particles tend to create a stronger scattering for the H-pol radiation than for the V-pol". We think this statement itself (i.e., our definition of AR is equivalent to the particle AR effect) is not wrong under very stringent conditions, which are clearly stated in the context.

Section 4.1 introduced a simple model to explain the "bell" relation. However, as I men- tioned above, I don't understand the AR values (Equation 4 should be (T1-Tj)*(tau_2h- tau_2v) ???). Why is AR in the range of 1.1-1.3? Why is Tj-T1 roughly constant? (P13, L4). To me, Tj depends on the location of ice cloud and T1 is related to the near surface temperature. Both Tj and T1 are varying with atmospheric conditions. And basically the assumption of constant Tj-T1 is not true as the authors mentioned in the paper.
Firstly, some typos have been corrected (mixing between V & H in subscriptions). AR=1.1, 1.2, 1.3 for the simulations to generate Fig. 7 are just some examples we chose, because the main purpose of this conceptual model is to reproduce the "bell" curve through using the concept of AR, the idea of which is then applied to the more sophisticated CRM to try to mimic the observation and to identify the "best-fit" AR.

T1-Tj essentially determines the spread of the starting point at the warmest side of the bell-curve. As one can see from the GMI observations in the tropics in Fig. 3 of the manuscript, the spread is roughly +/- 10K around 280K, so it is not a bad assumption of constant T1-Tj value. We also agree with the reviewer that it is not clearly stated in the main text about the reason we assume it is constant. We now have clarified it.

The authors claimed that the PD-TB relationship is independent of channel frequency (P13, L11-15). This conclusion is based on the assumption of constant AR in the atmosphere at different frequencies. However, this assumption wasn't proved to be true in this paper. As shown Figure 2, 3, and 5, the maximum PD corresponds to different TB and depends on channel frequency, i.e., the PD-TB relationship changes.

First of all, for 89 and 166 GHz, the analysis results shown in Fig. 3 and 5 are based on two and six months of all GMI data collected in the tropics, respectively, the sample sizes of which we believe are large enough to speak themselves out on a robust statistical sense (please note that Fig. 2 is generated only from a case study in Fig. 1, so it does not have any statistical implications whatsoever). Secondly, the PD peak amplitudes remain roughly the same (~10K) across different channel frequencies. As for the TB value where PD peaks, we admit that it is around 220K for 89 GHz, as opposed to 200K for 166 and 640 GHz. However, considering that 89 GHz contain so many surface polarization signals (e.g., the highly polarized branch in the warm TB side remains for 89 GHz even on land as shown in the top-right panel of Fig. 3), the warm side of the PD-TB relationship for 89 GHz essentially starts at a positive value, just like the Fig. 7b's situation. Please note that in this situation, the TB value where PD peaks also shifts to a warmer value (Fig. 7b). Lastly, here (P13, L11-15 in the original manuscript) we stated that "PD-TB" relationship is WEAKLY DEPENDENT on the channel frequency, not "independent". Therefore, we think the original statement is accurate and proper in tone, and we decide not to change the wording.

Also Figure 8 shows the dependence of PD-TB on AR at three different frequencies, which is not roughly 1.3 for all the frequencies at it showed.

OK, now I see what you mean. When we say PD-TB relationship is WEAKLY DEPENDENT on the channel frequency, we mean that given an AR value, the peak value of PD and where it peaks on the TB axis remain roughly unchanged (so we used the phrase "WEAKLY DEPENDENT") against frequencies. We do not by any means to implicate that this relationship is independent of AR.

We also noted that, during the revising period, Defer et al. [2014, JGR] found a somewhat smaller peak value of PD (~ 8K) at 89 GHz using the MADRAS instrument onboard the Megha-Tropiques mission, while their 157 GHz PD-TB relationship is very similar to what we found in the GMI 166 GHz. While the RTM simulations conducted in that paper concluded that PD increases with channel frequency, the authors also recognized that the simulated PD is very sensitive to particle size, density, etc. that we also found in the RT4 simulation. Therefore, RTM simulations from both of our study and Defer et al. [2014] could not lead to definitive, conclusive answers. More observations at higher-frequency channels like 640 GHz (such as ICI and our ongoing instrument development project of a polarized channel pair in the IR spectrum) are very much needed globally. This discussion has been included now in Section 5, 3rd paragraph.

P16, L1, How accurate is the BB flag? This has a significant effect on the conclusions. Section 4.3 didn't distinguish different precipitation types: whether rain or snow. When the authors can't distinguish the liquid/frozen precipitation, the results are still too rough. No BB could be snow precipitation as the authors mentioned. For snow precipitation the snow scattering is weak and 89 GHz channels can still "see" the ocean surface. Thus the PD at 89 GHz is strong. For rain with BB, the near surface is screened by BB and rain, and the BB has polarization. Thus it could result in a higher PD as observed above. The mechanism is still too complicated and not clearly interpreted here.

According to the ATBD of V1.4 L2 radar product, BB flag is quite reliable for the Ku-band. We also consulted Dr. Liang Liao in GPM team who is part of the group of developing the L2 radar retrieval product.

Please refer to the ATBD file for details:

https://pps.gsfc.nasa.gov/Documents/ATBD_DPR_2015_whole_a.pdf

Thanks for your comments on the large polarized branch in Fig. 10a. We were originally puzzled of this branch because we thought that the precipitation layer, when detectible by the Ku-band, can always effectively block the ocean surface polarization signal, but apparently it's not always the case. So we explained it by the light precipitation scenes. Unfortunately, right now we cannot tell snowfall scenes apart from the rainfall scenes, so we cannot further separate them out and interpret the results more clearly, as also noted here by the reviewer. We now include your comment in the text.

Some of the figures in the paper are difficult to read. I suggest the authors to revise the figures to better understand the results. Eg. Figure 1, Left Bottom panel: you'd better use the same colorbar for comparison. It seems to me that at 89 GHz PD is also up to 12-16 K and is comparable with the 166 GHZ PD values.

We now made the color scale the same for 89 and 166 GHz PDs.

Figure 2, the y axis range of the left and right panels are not the same and difficult to find the right value that described in the text.

The x-axis and y-axis are now made identical for easier comparison.

Figure 4, it is not easy to read it and please optimize the figure.

We enlarged the font size and bolded the colored lines now.

Figure 10, The values of the color and contours are not described either in the figure or the figure caption.

Values of the color/contour scale are not important, but the total areas they cover have been normalized to unity, and plotted in log-scale. This description has been added to the figure caption. Thanks.

Specific comment:

1.P1, L20. ”increase slightly with latitude”, How slight ?
Other than the [-70,-50] latitude band where the results may not be significant due to limited sample size of cloudy-sky cases, the increase of the peak amplitude with latitude falls in the range of 2-4K as visually estimated from Fig. 4.

2.P1, L25. the authors claimed that in deep convective cores, PD is reduced due to turbulence mixing. It is ambiguous, are there more ice or more liquid water ? As the authors discussed in the text, attenuation by liquid water and water vapor lead to a decrease of PD.
That’s not quite what we meant. Turbulent mixing within deep convective core inevitably promotes the random orientation of ice, liquid and mixed-phase particles, which ultimately reduces the PD to close to 0. Now the wording has been altered to: “On the other hand, turbulent mixing within deep convective cores inevitably promotes the random orientation of these particles, a mechanism works effectively on reducing the PD.”

3.P1, L34. references are missing here.
Examples of different measurement techniques have been added.

4.P1, L37. references are missing here. Please indicate which models you mentioned here. (better name one or two).
We prefer not to name one or two models explicitly in the main text due to complicated reason (mainly because of funding sources). What we can say at this point is that operational model developers in the United States have realized this long-standing issue quite a while ago, and have been working diligently on changing the precipitation hydrometers forecast variables instead of the current diagnostic variables. Colleagues in Europe, especially ECMWF, have realized such a function, and that is one of the major reasons why all-sky data assimilation (clear + cloudy + precipitating scenes) is ingested better by the ECMWF model. A reference is given in the revised manuscript for interested reader.

5.P2, L6. It is not appropriate to refer to Xie et al. 2015, better cite a general one.
It has been replaced by Comstock et al. [2007]. Orientation’s effect on the IWC retrieval uncertainties have rarely been mentioned before though.

6.P2, L9. I didn’t find this reference in the bibliography (Xie, 2012)
The reference has been added. Thanks.

7.P2, L15. inappropriate references (Miao et al 2003 and Xie and Miao 2011). It would be also good to mention the paper from Defer et al. 2014. Since they also investigated polarization signals at 157 GHz (Defer, E., V. Galligani, C. Prigent, and C. Jimenez, First observations of polarized scattering over ice clouds at close-to millimeter\ frequencies (157 GHz) with MADRAS on board the Megha-Tropiques mission, DOI : 10.1002/2014JD022353, J. Geophys. Res., 2014.)
Thank you very much. We completely overlooked this reference before the revision. This paper is very informative, and we’ve now included it in the reference list as well as
in the literature review paragraph in Section 1 and we spent a bit discussion to recognize it (paragraph 3, Section 5).

We feel like Miao et al. [2003] and Xie and Miao [2011] are appropriate to cite here in the literature review paragraph, as their obs. are based from ground and look upward, and hence the PD signal they found are more likely to be attributed to the snow layer.

8.P2, L23. Davis et al. 2005 did observe polarized signals, but it is not significant as it was mentioned in the paper.
Thanks. “Significant” has been replaced by “noticeable”.

9.P6, L9, didn’t find the reference in the bibliography Greenwald et al., 1997
The citation has been replaced by Wu and Jiang (2002), e.g., section 6.5.7 therein.

10.“habit” instead of “habitat” throughout the paper
Revised. Thanks.

11.P16, L22,”obsolete” or “oblate”?
Thanks. The typo has been corrected.