

Review #1

Dear reviewer,

Thanks again to the reviewer for spending time reviewing and helping us improving the paper. We really appreciate the effort.

Following the reviewers suggestions, the major changes that the paper has undergone are the following:

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The ratio between the mean climatologies is still included and discussed, as it was in the previous versions of the manuscript.

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Below there is the answer to each of the comments by the reviewers.

Thanks again for the efforts reviewing this paper.

Ramon Padullés, on behalf of the authors

Thanks for following my general suggestions from the last review. I am now happy with the manuscript on the overall level, but there are still details to consider. They are listed below. I raise a number of things, but they are all relatively small and I consider this a minor revision.

- Line 8: Not sure if I agree that there is a clear difference between land and ocean. More below.
- End of abstract: The conclusion of the study is expressed quite vaguely. As I see it, the derived ratios between Kdp and IWC are the main result, and the found range should be stated (0.03-0.09 mm/km?). But also clarifying that there is uncertainty due to IWC retrievals. Again more below.

We have modified the abstract and the conclusions to include the found range and to mention the uncertainty

- Line 20: Here (and elsewhere) you get the feeling that just "sinking" occultations are used. Is this correct? Are there not also "rising" occultations?

For the PAZ mission, only setting occultations are collected. The capability of collecting rising occultations was disabled for this particular experiment, due to the number of available ports in the receiver (in general, one port is used for setting and the other one for rising. In this case, one is used for the H antenna and the other one for the V antenna).

- Lines 21-23: This sentence can be removed. Just start next one with "The" instead of "This".

Corrected

- Line 27: No need to bring up mm and km here. And the general rule is to use SI units.

Corrected

- Line 31: Please clarify that "equivalent diameter" means the diameter of a sphere of solid ice having the same mass.

Clarified

- Equation 2: The factor $1e3$ is wrong. If the result should be mm/km, the factor should be $1e6$. That said, it is much clearer to stick to SI units in equations (i.e. remove $1e3$). This does not contradict to later still report Kdp in terms of mm/km.

Ok, understood

- Lines 37-38: It is correct that IWC is proportional to the third moment of $N(D)$ (with D defined as done here), but there is no general relationship between size, shape and type (and what is type?) for ice particles. Rewrite these sentences, for better clarity.

We have removed the sentence that contained "linked to shape and type". We have also rephrased the following sentences to account for the particularities of ice water content, e.g. that it is affected by the third moment of $N(D)$, but also other factors such as effective density, orientation, etc. must be taken into account for this work.

- Line 98: A practical question. My memory is that CloudSat orbits all start at the equator. If correct, will not your segments end up at specific latitudes, roughly 10 deg apart? For example, in Fig. 3 you report statistics for every 2 deg. How do you ensure an even sampling at 2 deg resolution?

The number of segments each orbit is split in is lower than what would correspond to an even splitting. Then, the first segment does not correspond to the "0-index" of the cloudsat orbit, but it is initialized at a random index within what would be the first segment. From there, the segments are placed one after the other in a sequential way.

- Figure 3: Figure title says 7 km, while the text below says 8 km. What is correct? Anyhow, no need for a figure title here.

This was an error that came from the first draft. Corrected.

- Line 157: The text says "some features are still recognizable". This indicates a very different pattern. This is not the case. For land, the black and red lines deviate a bit around the equator, but not in a dramatic way. My reaction is the opposite, that the results are surprisingly similar between land and ocean. That is, reconsider the conclusion that there is a clear difference between land and ocean.

The feeling that there were large differences between ocean and land came from the results of the correlation coefficients in the first draft – i.e. using the CWC-RO retrieval. It is true, though, that now the results look better in terms of the ocean/land comparison. Still, the correlation coefficients are higher over tropical ocean at higher altitudes (>9km) than they are for tropical land. So, we have rephrased the conclusions to state that there are some differences (not clear differences) between ocean and land, but overall both regions show good agreement.

- Line 168: Why 80th and 90th percentiles? Why not more distinct ones? Such as, 50 and 95. Further, it would be helpful if you explain what you mean by "climatology for the 80th percentile", and that you take the ratio between the 80th percentile of Kdp and the 80th of IWC. To help the readers further, please explain what you get out of looking at these percentiles, including pointing out that if all particles have the same Kdp-IWC ratio, the ratio for the mean, 80th and 90th percentiles should all be the same. Now it is first on lines 320-321 that you make some comments in this direction.

The choice of 80th and 90th percentiles is rather arbitrary, but takes into account that lower percentiles, such as 50th, could fall below the mean since the data is heavily grouped towards 0. We have included a few sentences clarifying the implications of looking at these percentiles, but the ratios between the 80th and 90th percentile climatologies are not shown anymore (we found a bug in their computation, and we also think that their interpretation could be misleading). Still, same conclusions apply, and minor modifications had to be done in the text.

- Sec 3: Some statements on the nature of noise are needed. How big is it for individual observations (in mm)? Normally distributed? If not, on average zero?

We have included some statements about the noise in the third paragraph of Section 3. The noise essentially depends on the signal to noise ratio, and it is normally distributed around 0, ranging from ~2mm in the lowest layers to 0.5mm above 10km. The assessment of the noise is performed in Padulles et. al. 2020.

- Figure 6: What standard deviation is included? For me, the distinction here between standard deviation and standard error is not clear. The text speaks about a red line. There is no such line.

What we include now is the RMSE of the linear fit we use to extract the relationship between both quantities.

- Table 1: I don't find this table very useful, are not the existing figures enough?

Yes. This has been also pointed out by the other reviewer, so we have removed the Table.

- On the other hand, I miss a figure showing the distributions of Kdp and IWC inside a region and one altitude. That would be helpful to understand how constant Kdp/IWC is with IWC, and then also throw light on why the mean ratio is higher than the 80th and 90th percentiles ones. That is, I suggest replacing Table 1 with such a figure.

Such a figure has been included. Old figure 4 has been splitted between the climatology maps, and the scatter-plot. The scatter plot has become Figure 5, and there are two extra panels showing the distribution of integrated IWC and PAZ Kdp for a two specific regions and heights.

- Lines 225-228: It is understandable that you don't want to go into all details here, but please be a bit more specific. The mapping of the shapes assumed for DDA to spheroids, did that follow what is described below? What do you mean by "good agreement"? Deviations of 0.1%, 1%, 10% ...

We have decided to remove this paragraph because (1) the results were not shown, and (2) the methodology applied to the comparison was actually more complex than the simplifications followed in the rest of the Section. For each shape and size the specific parameters of the equivalent spheroid were computed, something that we do not do for the general simulations – as you point out in the next comment.

- Sec 4: What you have done here is fine, but it should be clarified that the standard assumption is that the effective density decreases with particle size (i.e. not is constant as you assume). Normally expressed as $m = a \cdot D_m^b$, where D_m is the maximum diameter.

We have clarified this in the last paragraph of Section 4, and mentioned it in the conclusions.

- Line 375 and Appendix A: It's great that you compare DARDAR and 2B-CWC-RO to stress that the IWC retrievals have uncertainty. But you stop a bit early. What is the impact of the mean Kdp/IWC ratio? Is it a few %, or 50%? I suggest replacing one of the panels in Figure A1, with the ratio between the mean IWC of 2B-CWC-RO and DARDAR as a function of height. That would give values that could be used to scale the Kdp/IWC ratios found by DARDAR.

We have followed your suggestion and we have included a panel (panel b) that shows the fractional difference between the mean DARDAR and mean 2B-CWC-RO integrated IWC as a function of height, for different latitudes. And we have included a mention to this figure at the end of Section 5, emphasizing the fact that larger discrepancies are near the freezing level while in this study we focus on the layers above.

Review #2

Review of: On the global relationship between polarimetric radio occultation observable delta_phi and ice water content

Ramon Padulles, Estel Cardellach, and F. Joseph Turk

Revised version

Dear reviewer,

Thanks again to the reviewer for spending time reviewing and helping us improving the paper. We really appreciate the effort.

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Ramon Padullés, on behalf of the authors

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Summary: This version of the manuscript is much improved from the initial submission, and better frames its results, and better details caveats and uncertainties associated with its analysis. There remain some ambiguities, awkward sections, and grammatical mistakes that need correcting. These should be straightforward to rectify in a timely fashion. I recommend minor revisions.

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Recommendation : Minor Revisions

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General Comments:

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Major Comments:

- I38: WC is proportional to the third moment of a liquid DSD. However, IWC is not necessarily proportional to the third moment, as this depends on particle density. As mentioned for the first draft, IWC and WC need to be made abundantly distinct throughout the paper. More work needs to be put into that.

We have looked carefully through the manuscript and made sure that Ice Water Content is used all times we refer to Water Content, specially after Section 1, where in the beginning we refer to water content in a more general way. In the first revision we already changed the word “proportional” to affected, and in this new version we emphasize the fact that for Ice there are more factors such as orientation, shape, effective density and composition that play a role in the relationship between Kdp and IWC.

- I186: I'm not clear what it means to say "the profiles have been truncated below the freezing level" --- does that mean that only RO with tangent heights above the current 0C altitude are used? Or something else? This needs to be explicitly stated somewhere. This relates importantly to figures 5 and 6, which show data in liquid and mixed-phase regions. Is the data unmasked/untruncated? Or not? It is difficult to interpret these results with these remaining methodological ambiguities.

To truncate the profiles means that the portion of each observation whose tangent point is below freezing level is not taken into account for this study. This is why the results in the tropics below ~4 km are either non-existent or with high dispersion due to the lack of data. We have clarified this, and we also acknowledge in the end of the first subsection of Sect. 2 that for this study the effect of mixed-phase is not taken into account.

- I301: See I38 comment above

With the clarifications made in Sect 1. (see reply to comment I38) we believe that the word “affected by” is fine.

- I357: I find this statement highly dubious. I would strongly recommend rephrasing this in terms of showing that these ratios can be reproduced for different plausible distributions of ice crystals.

We have rephrased the statement saying that the observations could potentially help constraining the plausible distributions of ice crystals that can reproduce the ratios.

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Minor Comments:

- I17: Joint with what?

We have rephrased the sentence.

- 149: Change to "Furthermore, knowledge of XXX is crucial for..." and replace XXX with specifically what you're referring to (because its not clear right now)

Corrected

- 153: Change "its" to "their" and "their" to "MLS" to improve clarity.

Corrected

- 173: Does it still orbit? If not, change to "orbitted"

Cloudsat is actually still orbiting, but an anomaly happened in August 2020. Since August 2020 there are no available data, although as far as I know, there are plans to process some of the remaining data and make them available soon.

- 195: Change to "moving" and "rotating"

Changed

- 1103: I wouldn't say "actual amount" since its really the retrieved amount

We have removed "actual"

- 1140: I would remove "a lot of", which is vague and colloquial

Removed

- 1146: Sentence beginning with "Being the PRO..." is a bit of a grammatical mess. Please rephrase.

We have rephrased the sentence.

- 1161: Do you mean IWC or WC?

We mean IWC. We have changed it to IWC.

- 1164: What does "integrated" mean here? Along the RO path? Is this explicitly spelled out anywhere?

It has been explicitly spelled in Section 2.

- 1167: "integrated", "WC"

Integrated ice water content. We have changed it.

- 1170: "integrated" (this time its IWC... is that the same as WC??)

We have changed all the WC to IWC.

- 1209: Change to "single-particle"

Changed.

- I220: It is worth stating what ice assumptions are used for the Cloudsat IWC product, since it is used heavily throughout this work.

We have mentioned at the end of Sect. 5 that the DARDAR V3 algorithm uses non-spherical particles but makes no assumptions about orientation, at the same time that we acknowledged that the results depend on the IWC algorithm chosen for the study.

- I269: Change to "consists of"

Corrected

- I270: Change to "described, and proceeding.."

Corrected

- I279-282: This is a grammatical mess and should be rephrased.

The sentence has been rephrased.

- I301: Add "The" to beginning of sentence

Added. Thanks for the suggestion.

- I305: Last sentence: Change to "The correlation coefficients maximize for Tropical oceans" or something

Changed

- I309: Do you mean higher altitudes when you say "higher ends"? Please be clear.

It refers to the higher percentiles (80th, 90th), e.g. the tail of the distribution. This has been clarified in the text. Also, their ratios have been removed since there was a mistake in its calculation and also the way these were expressed could be misleading.

- I311: I think you should make clear that when you say this is a "longstanding issue for observations" you are referring to errors in the Cloudsat retrievals. (are you?)

It does not refer to Cloudsat specifically, but to observations in general. In the provided reference it is stated that for these specific type of clouds present in the Southern Oceans, there is a disagreement among the observations on the amount of snow present in such clouds. We clarify that one possible explanation could relate to the retrievals withing these type of clouds.

- I315: Change to "quantifying the empirical relationship between both" or something

Changed. Thank you for the suggestion.

- I318: I would change "accounts for non-unique relationships" to "may be explained in part bby the non-unique..."

Changed

- I320: Change to "The mean climatology ratio is higher for almost all..."

Changed

- I321: Change to "relates"

Changed