

Review of: On the global relationship between polarimetric radio occultation observable delta_phi and ice water content by Ramon Padullés, Estel Cardellach, and F. Joseph Turk

Dear reviewer,

First of all, thank you very much for your time spent reviewing this manuscript. The comments and suggestions clearly contributed to improve the paper.

Following yours and reviewer #1 comments and suggestions, we have performed quite a lot of work which can be, in general, summarized as follows:

(1) We have repeated the analysis using the DARDAR product. This is a more up-to-date and well maintained product containing IWC retrievals from Cloudsat. Its algorithm performs different assumptions regarding the particle size distribution and shapes of the particles. This changed the results especially near the freezing level. The changes have not been dramatic, but the discussion part has been re-written accordingly.

(2) We have changed the way we presented the results in Section 4. Different analysis and different plots are shown now, and we believe that our points are made clearer.

Below there is a point-by-point response to all reviewer's comments.

Thanks again for reviewing this manuscript.

Ramon Padullés, on behalf of the authors.

Summary: This manuscript details research comparing the measurement of polarimetric radio occultation data to retrievals of ice water content from Cloudsat radar. The first portion compares climatology of observed RO delta_phi to Cloudsat ice water content (IWC) retrievals that have been mapped onto RO sampling geometry. The latter dataset is collated for a large sample of Cloudsat data, facilitating comparison with RO delta_phi. Following this section is an comparison between forward-simulated delta_phi and IWC based on size distributions of plausible particles related to Cloudsat IWC retrievals. Overall the research presented seems valuable. There are numerous minor wordsmithing, grammar, and typo corrections that should be made. Also, more discussion should be offered on the limitations of the Cloudsat retrievals that are treated here as a benchmark. These products involve numerous assumptions are only partly supported by the state of knowledge on the global distribution of ice properties and size distribution characteristics. The authors should try to frame the scope of the work more clearly in light of the uncertainties in Cloudsat retrievals, as well as other uncertainties related to ice phase clouds. For example, the word "verification" is excessively strong for the current work, which is closer to cross-comparison. There are a small number of major comments (see below) that involve statements or assertions that are questionable, misleading, or just plain wrong. These should be revised. I recommend major revisions.

Recommendation: Major revisions

General Comments:

The use of Cloudsat radar retrievals as a point of comparison is highly questionable and should be treated with skepticism. A single W-band measurement of cloud properties is insufficient to provide a reliable estimate of the likely degrees of freedom in ice particle distributions, in particular as those particles become larger and attenuation and resonance scattering effects dominate over the small-particle-assumption ("Rayleigh") limit. I would expect in many cases that Cloudsat retrieval errors

contribute as much, if not more, to the mismatch between Cloudsat and PAZ PRO. The reasons for Cloudsat retrieval errors should be obvious, but of course includes uncertainties related to size distribution and particle property assumptions. A more robust approach would be to include ground-based radar KDP, or ground-validation campaign data that includes a comprehensive suite of instruments (for example, radar, lidar, in situ cloud probes, etc). While the scope of the current work is sufficient, that scope and its limitations should be accurately conveyed to the reader.

We agree with the reviewer. And we also believe that a comparison with radar observations would be nice. In fact, we are currently working on this, but the amount of work and time make it no feasible to be included in this analysis. We believe the two studies are complementary, and the new analysis will use many of the results presented here. Another major challenge is the amount of coincident measurements between ground-based or space-based radars, but this is being overcome as PAZ satellite keeps collecting observations.

Major Comments:

l56: "water content" and "ice water content" need to be made clear, given the strong differences in scattering between liquid and ice particles. Perhaps avoid "WC" should be avoided altogether, unless total water content is being shown. Instead, replace with the unambiguous "liquid water content LWC" and "ice water content IWC". For example, Fig. 2 shows ice water content, but the plots are labeled "WC". This is confusing.

We agree. We have changed all figures to show IWC. Also, now there should be no confusion since we have also masked out the non-frozen part of the observations (see answer to comment regarding 1236 below).

l60: It should be mentioned here that this is performed using ground-based polarimetric radars at S-band (maybe some at C- or X-?). It is not made clear anywhere in the manuscript what frequency the PAZ operates at. This may be common knowledge to many, but should be mentioned here for completeness. The reader should not be forced, as I was, to look up that it's somewhere in the L-band (1-1.5 GHz).

We have noted that previous studies used radar observations at S – K bands, and we have also included the frequency at which GPS operates.

l151: The statement that KDP and IWC "depend" on the third moment is disingenuous. It's accurate to say they are both affected by M3, but neither is likely to be proportional to it for ice or mixed-phase particles (or even liquid). One can expect a correlation, but not a unique "relationship". It's not entirely clear what you mean to suggest by "relationship", but in any case, this discussion is highly misleading and must be revised.

We have seen in the literature that some authors relate IWC and Kdp using linear relationships (e.g. Bringi and Chandrasekhar, 2001, Eq. 7.101; Nguyen et al. 2019). It is true, however, that different types of hydrometeors may have different relationships, and we believe that using the more conservative statement "affected" is more accurate.

l159: Some effort should be made to convey how you focus exclusively on glaciated regions, and avoid precipitating liquid or mixed-phase regions. Uncertainties and limitations associated approach should be discussed. An explanation of your investigation of different tangent heights can be then related to this. Why are 7km and 9km chosen, for example? Why does fig 4b not include 7km? Why are values reported in Fig 5. below 5km (where significant liquid precipitation is expected,

especially for the tropics). The authors need to do more work to support this part of their research presentation.

We have re-analyzed all data and we have truncated the profiles at the freezing level to avoid major contributions from liquid phase precipitation. It is true, though, for this study we do not account for the effect of mixed phase precipitation. This is clearly stated.

1236: Why is data below the environmental 0C level not masked?? This seems like a first-order error in your approach.

We have done this now. See previous answer.

1258: These are NOT Cloudsat "observations", they are retrievals. This is a very important point to emphasize.

We agree. Thanks. We have emphasized it in the text.

1320+: Do the authors account for the viewing angle of RO? Ie that it is not always parallel to the orientation of falling particles?

Yes, we do account for this. However, the effect is almost negligible because the angle between the incidence of the rays and the plane parallel to the Earth surface is very small even at distances far away from the tangent point (but still below 20 km, which we assume as the upper limit where we can account for any hydrometeor-related effect).

1347: There is no such proportionality. This is false.

We have used reviewers suggestion of adding "affected by the 3rd moment" instead of saying "proportional" or "dependence".

1360-363: This discussion needs revision. The authors do not consider the possibility of, for example, compensating errors. These conclusions are a severe stretch, and must be hedged or qualified carefully.

Since we have removed the main contribution of liquid particles from the analysis, this discussion has been reformulated entirely.

Minor Comments:

Thanks a lot for the grammatical corrections and suggestions.

13: replace "since that time have also" with "has also"
Done.

13: Replace "for" with "to"
Done

14: Replace "detection" with "detect"
Done

18: Should be "especially"
Thanks.

l8: Remove "the" before "..major precipitation..."

Thanks.

l9: Recommend that authors hyphenate "over-ocean" and "over-land"

l10: Recommend author add "possibly" or "likely" before "involving"

l11: Replace "validated" with "evaluated" or some other such word

Done

l23 (and elsewhere): Strongly recommend that "GV" is not used for this acronym, as it is commonly used to refer to "ground validation" campaigns.

We have changed the first two paragraphs of the introduction following comments from Reviewer #1.

l25: Beginning of this sentence should be plural

Same as above.

l25: Replace "and to lower" with "and in lower"

Same as above.

l51: The reference to "it" is not clear.

Corrected.

l73: Add "us" between "enable" and "to"

Done

l81: "and has been operating until" isn't the best grammatical choice here. "has been operating" implies that it is still operating, "until" implies the opposite.

Corrected

l96: Replace "in a tangential way" with "tangentially", remove parentheses

Done

l109: Say "The first is that..." (remove "one")

Done

l131: "used" is a strange word here

We have removed it. Thanks.

l134: Reword: "Therefore, analysis of the statistics..."

Done.

l142: replace "between" with "it cannot distinguish between the effects of..." or something like that

Done

l144: "Thing" is too colloquial here, and the sentence should be reworded.

We have reworded the sentence. Thanks.

l155: Add the word "statistically" after "performed", remove "built" and "in statistical terms"

Done.

Fig. 5: Make Height the y-axis here

Done. Also, old Fig.5 has been split in two (now Fig.5 and Fig. 6).

Table 1: Is this any different data than what is in Fig. 5? Why is this a separate table???

We decided to show the results using figures and tables because we believe that the comparison with results in Section 4 are easier this way.

l221: Explain the significance of this brightness temperature.

We have removed this subsection following the suggestions and comments from Reviewer #1.

Fig. 8: It is hard to distinguish the different DDA estimates on this figure.

We agree. We have changed the plots in Section 4 and we believe that now the conclusions are more clear.

l291: It is confusing why this is referred to as a pristine ice particle, since it is unlikely that any realistic particles would form in this habit, beyond, say, frozen drops. Pristine ice particles (ie. those grown solely by vapor deposition) can have any number of densities. This statement is confusing and misleading.

We have changed the way we present and state the results in Section 4. However, in this statement we refer to the ability to simulate the forward scattering effects of all kinds of particles (from more idealized habits to aggregates and different densities / axis ratios resembling more fairly the reality). And to constrain which particles are able to reproduce reality, or not. We believe that our conclusions are now clearer.