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## Comment on acp-2022-300

Anonymous Referee #2

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Referee comment on "On the global relationship between polarimetric radio occultation differential phase shift and ice water content" by Ramon Padullés et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-300-RC2>, 2022

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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

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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 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.

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Recommendation :Major revisions

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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.

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Major Comments:

I56: "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.

I60: 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).

I151: 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.

I159: 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.

I236: Why is data below the environmental OC level not masked?? This seems like a first-order error in your approach.

I258: These are NOT Cloudsat "observations", they are retrievals. This is a very important point to emphasize.

I320+: Do the authors account for the viewing angle of RO? Ie that it is not always parallel to the orientation of falling particles?

I347: There is no such proportionality. This is false.

I360-363: 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.

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Minor Comments:

I3: replace "since that time have also" with "has also"

I3: Replace "for" with "to"

I4: Replace "detection" with "detect"

I8: Should be "especially"

I8: Remove "the" before "..major precipitation..."

I9: Recommend that authors hyphenate "over-ocean" and "over-land"

I10: Recommend author add "possibly" or "likely" before "involving"

I11: Replace "validated" with "evaluated" or some other such word

I23 (and elsewhere): Strongly recommend that "GV" is not used for this acronym, as it is commonly used to refer to "ground validation" campaigns.

I25: Beginning of this sentence should be plural

I25: Replace "and to lower" with "and in lower"

I51: The reference to "it" is not clear.

I73: Add "us" between "enable" and "to"

I81: "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.

I96: Replace "in a tangential way" with "tangentially", remove parentheses

I109: Say "The first is that..." (remove "one")

I131: "used" is a strange word here

I134: Reword: "Therefore, analysis of the statistics..."

I142: replace "between" with "it cannot distinguish between the effects of..." or something like that

I144: "Thing" is too colloquial here, and the sentence should be reworded.

I155: Add the word "statistically" after "performed", remove "built" and "in statistical terms"

Fig. 5: Make Height the y-axis here

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

I221: Explain the significance of this brightness temperature.

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

I291: 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.