Review of "On the global relationship between polarimetric radio occultation observable ..." by Padullés, Cardellach and Turk

This manuscript deals with a novel use of the signal transmitted by GNSS satellites, denoted as ROHP. These signals are recorded in a limb sounding geometry, but, in contrast to standard radio occultation, polarised information is recorded and the differential phase shift between V and H is derived. As demonstrated in the manuscript, this phase shift is related to the mass, shape and orientation of ice hydrometeors in the atmosphere. Other satellite observations in combination with in situ data give us a fair knowledge of the distribution of mass, shape and sizes of ice hydrometeors, while the orientation of larger ice particles is still largely an open question. The most interesting aspect of the manuscript is then to what extent this new technology can constrain ice particle orientation. Such information is critically needed, for example, to make full use of both existing (e.g. GMI) and future (e.g. ICI) passive microwave data in numerical weather prediction (Barlakas et al., 2021).

Accordingly, the basic objective of the study is highly relevant. I said yes to review the manuscript just because we need information on orientation as input to our simulations to set up operational retrievals for ICI. Also the methodology is good. A statistical approach where CloudSat data are used to generate synthetic phase difference data is applied. A statistical comparison is needed as it is difficult to obtain collocations with CloudSat, and largely the same approach has been used to study other ice hydrometeor properties by Kulie et al. (2010) and Ekelund et al. (2020).

On the other hand, I find several weaknesses in both the analysis and the presentation. Interesting figures are presented but it is hard to judge the robustness of the results. As a consequence, I find little new solid information in the manuscript, compared to what we know from studies based on GMI. Despite this, there is hardly no review or comparison to older work. My overall judgement is that a major revision is needed to meet the standards of ACP.

Some words before going into the detailed comments. A main quantity of the manuscript is the ratio between IWC and phase difference. For simplicity, let me define this ratio as R:

$$R = \frac{\mathrm{IWC}}{\Delta\Phi}.$$

Compared to GMI, the main advantage of ROHP is that vertical information can be obtained and, in my opinion, the profiles of derived R-values (Fig. 5) are the most interesting results. However, the accuracy of derived R-values can be questioned (as 2B-CWC-RO used). In any case, it would be good to have an estimation of the uncertainty of R.

Further, I encourage the authors to see the retrieval of R as the main strength of ROHP, and not just as a step towards estimating IWC. As described below, the retrieval IWC precision could be poorer than the results seem to indicate. In addition, the poor horizontal resolution of limb sounding observations is a severe drawback if not the information is truly unique (said based on personal experience of limb IWC retrievals).

General comments:

- The CloudSat retrievals are largely taken as truth. Issues around attenuation and multiple scattering are mentioned, but there are other, more important, limitations. The ice water content (IWC) retrieved from CloudSat has significant uncertainties due to assumptions on particle size distribution (PSD) and shape.

- Having the point above in mind, the choice of using the 2B-CWC-RO product is unlucky. This is an old product. As far as I know it is still based on Austin et al. (2009). A product more actively maintained is DARDAR, with latest version described in Cazenave et al.

(2019). The advantages of DARDAR are (assuming no update of 2B-CWC-RO that I have missed):

- It is based on newer in situ data and its PSD assumptions should be more realistic.
- It incorporates Calipso and thus has a higher sensitivity to ice at high altitudes.
- It operates with a "soft spheroid" particle model. Similar models are used in Sec. 4 of the manuscript. In comparison, 2B-CWC-RO assumes spherical particles (consisting of solid ice, if my memory is correct). As this is inconsistent with the basic results of the manuscript it is a bit of contradiction to use 2B-CWC-RO.

Another difference between DARDAR and 2B-CWC-RO is the interpretation of reflectivites at temperature between -20°C and 0°C. DARDAR assumes that all back-scattering comes from ice hydrometeors, while 2B-CWC-RO assumes a gradual change from ice to liquid. This difference and the incorporation of Calipso in DARDAR should have a significant impact on R-values obtained.

That is, including DARDAR would give higher confidence in the results. To be clear, to reflect the uncertainty in the reference data, both 2B-CWC-RO and DARDAR should be included (at least when it comes to mean values).

- The manuscript shows mean profiles of R. Some differences between land and ocean are noted and discussed. In line with studies based on GMI, the mean polarisation signal appears to be relatively stable and varies little with e.g. latitude (Gong and Wu, 2017). However, it must be remembered that these are mean values, and they do not imply that the same R is valid for individual observations. There could be large local variations in R but the mean value could still be relatively constant. In fact, in Kaur et al. (2022) we show that GMI observations only can be understood by a distribution of shapes and orientations, resulting in cases giving different degree of polarisation. This matches a distribution of R. It would be interesting if the authors could find a way to estimate the variation of R, around its mean.

  As far as I noted, this aspect is not considered in the manuscript, but has important consequences. Most importantly, this means that the relationship between a single $\Delta\Phi$ and IWC could in fact be weak. That the robust relationship is just valid for averages. The authors suggest the ROHP as a way to measure IWC, but there is no discussion of the impact of this issue on the IWC retrieval precision. To be clear, hail and cases where the particles exhibit totally random orientation should give very small $\Delta\Phi$, despite substantial IWC.

  I still see a value in more ROHP measurements, but not as a way to measure IWC. The selling point for ROHP, I consider to be the unique information on shape/orientation.

- In line with the last point, there could exist situations with $\Delta\Phi = 0$, but IWC> 0. This combination leads to $R = \infty$. That is, it would be better to define R as $\Delta\Phi$/IWC. It also feels more natural that spherical particles result in a factor that is 0 (such as ρ introduced in Barlakas et al. (2021).

- There should be some noise in the measurement of $\Delta\Phi$. To what extent is derived R affected by this noise? How are negative R values treated? Any measurements giving a negative R well above the noise level?

- Older studies using passive microwave data exploring the polarisation signatures of oriented particles are poorly reflected in the manuscript. Gong and Wu (2017), Gong et al. (2018) and Zeng et al. (2019) are mentioned, but there is no real discussion if the findings are consistent or not with these older papers. Other older studies to consider include Defer et al. (2014) and Kaur et al. (2022).

- The reader could get the impression that these are the first limb sounding measurements of ice hydrometeors, and the work on passive microwave limb sounding should be acknowledged. In particular the work on Aura MLS by Dong Wu, e.g. Wu et al. (2009). There is in fact even a study based on Aura MLS looking into shape/orientation using polarisation (Davis et al., 2005), and the results of that study should be considered.

- I don't doubt that there could be differences in microphysics between ocean and land areas, but I don't find the analysis performed in Sec. 3.3 sufficient to rule out diurnal variations as the cause to the deviating results obtained for land. (That a cold 11 μm radiance is found somewhere in the neighbourhood does not guarantee an apple-to-apple comparison. For example, the convective systems can still be either in an early or late stage.) Limb sounding results could again be used reference. In fact, CloudSat and observations at 6:00/18:00 are combined in Eriksson et al. (2010), exactly as here for ROHP, and large differences in IWC over land due to the local time sampling are shown. Further data on diurnal variations of IWC are found in Eriksson et al. (2014).

- The data presented in Sec. 4 are good and interesting, but need a better presentation. Most importantly, there are too many colours and symbols in Fig. 8 to safely discern the lines. The Abstract says ".. horizontally oriented aggregated ice particles and tilted pristine ice plates agree well with the observations". And I don't see how this claim is backed up. Anyhow, I don't think it makes sense to pick out a single particle model as best, considering the variations in mean R and the uncertainties discussed above. Accordingly, the most important part of Sec. 4 is to derive the general tendencies, such as how R varies with effective density, axis ratio and wetness. But I don't find any clear statements (or a figure) clarifying this.

- There are several comments around the impact of liquid particles that need to be clearer. For example, the text on line 236 indicates that the authors thinks that liquid particles contribute significantly to $\Delta\Phi$, while line 363 indicates the opposite. Some quantitative values would be good. Have the authors in any way estimated the possible $\Delta\Phi$ induced by liquid drops?

- There should be a proper Conclusion section. The present Sec. 5 makes it hard to extract the main outcomes.

Some minor comments:

- Without an explanation term $\Delta\Phi$ is understood by very few persons, and the title should be changed (with $\Delta\Phi$ explained in words).

- The first paragraphs of the Introduction is hard to follow. Anyhow, I don't find this and the second paragraph relevant for the main results of the study (nor to match the manuscript's title).

- Equations should be expressed in terms of SI units, to avoid confusion and the need to state units. (Another unit can still be used in figures. For example, no problem to plot IWC in g/m^3.)

- Line 122: No need to inform the reader what units that are used internally.

- Line 139: Huang et al (2015) is cited as reference for typical IWP values. I had a looked in the reference for curiosity and I must say that the values look to be far too low. According to Fig. 5 in Huang et al (2015) mean IWP in the tropics is about $1\,\mathrm{g/m^2}$. This about two orders of magnitude lower than what DARDAR reports, see e.g. Duncan and Eriksson (2018); Kaur et al. (2022).

- Figure 3: Any explanation for the "spike" over land around -60 degrees?
- Figure 5: Place altitude on y-axis, as done in Fig. 6.
- Line 404: I agree that the results appear compatible with Brath et al (2020), but I assume the general reader would need an explanation.

Kind regards, Patrick Eriksson

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