Summary

This paper studies the Polarimetric Radio Occultation (PRO) technique to asses its sensitivity to vertical profiles of hydrometeors under varying microphysical assumptions in the context of atmospheric river cases. This sensitivity is theoretically explored using WRF model output from which differential phase shift $\Delta\Phi$ is simulated. The simulated $\Delta\Phi$ is compared against the observed with the aim to evaluate the applied microphysical schemes.

The study is well structured, clearly written and has informative figures. While I do think the study has the potential to be well-received, I have one major concern about the conclusions drawn from the optimization with the x-parameter method that is presented, in addition to some minor comments for clarity and quality improvements.

We sincerely thank the reviewer for their constructive and insightful comments, which have helped us improve both the clarity and robustness of our work. In this revised version, we have made several important changes to enhance the physical consistency and transparency of the methodology.

(1) We calrify that there is no direct coupling between ARTS and WRF for the main part of the analysis. Instead, ARTS is used diagnostically to evaluate which paritcle habits are most compatible with the WRF-derived water content and the best x-parameter obtained when comparing with actual observations of differential phase shift.

(2) Second, the optimization process has been refined and is now carried out at two levels: (i) within each microphysics scheme, to obtain the optimal set of x-parameters; and (ii) across all schemes, to identify the combination of microphysics and x-parameters that minimizes the cost function.

(3) In the comparison between the best x-parameter and those derived from ARTS, we now generate two distinct look-up tables that relate Kdp and WC, each corresponding to a different assumed particle size distribution (PSD). This modification ensures a closer alignment between the scattering properties used in ARTS and the microphysical assumptions in WRF.

Main comments

1. The method is based on x-parameters that relate water content (WC) to specific differental phase (KDP). Given the simulated WC, the 'optimal' x-parameter is then found by comparing simulated KDP against the observations. The authors conclude from this x-parameter that a specific particle habit is dominating the signal based on particle habits from the ARTS database. My concerns with this approach are 1): There will usually be a mixture of particles present, especially since measurements are done over a profile, and typically different particle habits dominate at different altitudes (temperatures). One example: Wouldn't a 50-50 mixture of particles with x-parameters of 0.1 and 0.3 yield an 'optimized' x-parameter of 0.2? In your current draft, you would then conclude that particles that relate to x-parameters of 0.2 are dominating. 2) The 'optimization' might lead to the correct results for the wrong reasons. E.g., a simulated water content that is much lower than in reality could be compensated by a higher x-parameter to achieve the correct KDP. 3) Keep in mind, that you 'overwrite' some of the particle properties that are used by the WRF microphysics schemes, by taking ARTS particle habits instead. For example, there are specific mass-size relations used, specific PSD shapes, and density assumptions. While I think this last point is

not a major problem, you should at least discuss it, since you goal is to 'evaluate' microphysics schemes.

(1) We agree that in reality the hydrometeor population within a profile is a mixture of particle types, and that different morphologies can dominate at different levels depending on temperature and growth regime. In the analysis, we are optimizing a cost function to get optimal x-parameters. Then, we obtain a range of values that generally represent best the observations, and are associated with specific particle types, but this is not a one to one link. The interpretation of the “dominant” particle should be understood as the effective habit or combination of habits that best explains the observations in a bulk sense, rather than as proof for a unique unique morphology.

Furthermore, because Atmospheric Rivers are large, spatially homogeneous events, and our study comprises 37 AR, much of the local variability of the particles is minimized. This helps ensure that the conclusions reflect robust large-scale behavior rather than being dominated by localized variability.

Finally, we view this work as a first step. For future work, we plan to refine the methodology by introducing a temperature-dependent operator, following the approach on [1]. We believe that event in its current form, the analysis is relevant enough to provide meaningful insights into the evaluation of cloud microphysics schemes.

[1] Kim, J., Shin, D. B., & Kim, D. (2024). Effects of inhomogeneous ice particle habit distribution on passive microwave radiative transfer simulations. *IEEE Transactions on Geoscience and Remote Sensing*, *62*, 1-20.

(2) We are aware that a bias in the simulated WC could in principle cause an offset to the optimized x that yields Kdp leading to the same results, but for the wrong reason. Our analysis is framed in terms of relative comparisons among microphysics, all of which are run with the same WRF dynamical core and physical forcings. By doing the same for a significant number of events, we try to minimize the effect of biases in the WC fields.

(3) We are aware of the importance of this points. For this reason we have changed the methodology and calculated the x-parameters from ARTS employing the PSDs used in the different microphysics in order to be more coherent with the assumptions from WRF. In the way that this is done (i.e. finding the best x-parameter, and then assigning each parameter to a potential particle habit) the miss-match of the assumptions becomes less relevant. Also, being fully consistent with WRF assumptions is not possible, since the assumption of particles being shperes would invalidate the rest of the study (that is, perfectly spherical particles lead to 0 differential phase shift).

Minor comments

1. Line 100-104: I found it hard to understand this paragraph. Partly, because some of the sentences are incorrect. I also think a small sketch visualizing the ray-path and the position of h_t on that path would help.

   This paragraph was rephased in order to make it more clear.

2. Line 105: I understand from this that the ray path is resolved with a given resolution. What is this resolution?

   Theryas are resolved with a resolution of 5km along the ray direction.

3. Line 115: Two-way or one-way nesting?

   We have employed two-way nesting in order to have more consistency across scales, to avoid mis-matches at the nest boundaries or to be more realistic regarding the large-scale evolution of the phenomena.

4. Line 120: What is the horizontal extend of the domain?

   The WRF simulations were configured with two nested domains. The outer domain uses a horizontal resolution of 15km with 130x146 grid points, giving an extent of approximately 1950x2190km. The inner domain has an horizontal resolution 3km with 466x526 grid points, corresponding to an extent of about 1398x1578km.

5. Line 127-128: Did you change the radiation scheme then for specific microphysics schemes only? Or for all microphysics schemes of that AR event?

   I have changed it for all the four schemes for one observation in order to be consistent between them when doing the comparison.

6. Line 134: Water vapor is typically not considered a 'hydrometeor'

   Corrected

7. Line 135: I would add here that the differences in assumed properties, such as particle density, are also important.

   Added

8. Line 153: Morrison is two-moment only for graupel, rain, snow and ice (not for cloud water). Also, Thompson predicts number concentrations (and thus, is two-moment) for cloud ice and rain. Here it sounds as if Thompson was completely one-moment.

   The paragraph was rephrased in order to be more clear.

9. Line 158: What were your conditions to define an AR event?

   We did not define the AR cases, we instead took them from a database [2] to see the coincident phenomena with our observations.

   [2] Guan, B., & Waliser, D. E. (2024). A regionally refined quarter-degree global atmospheric rivers database based on ERA5. *Scientific Data*, *11*(1), 440.

10.Section 2.2: Was there any nudging applied?

No nudging (neither spectral nor grid nudging) was applied in our simulations. . All cases were initialized with ERA5 reanalysis and the run freely with the selected microphysics schemes.

11.Line 243: I don't fully understand the reasoning here. Isn't water content directly output by WRF? Why do you argue based on $\Delta\Phi$ that snow is contributing the most?

Yes, the water content for each hydrometeor category is directly available from the WRF output. However, the key point here is that not all hydrometeors contribute equally to the differential phase shift. The contribution of each category depends not only on its water content but also on its scattering properties (x-parameter) and the geometry of the observation. In general the percentage of snow water content is greater in comparison to the rest of the hydrometeors, and in combination with the x-parameters the contribution is often the biggest.

12.Line 246: Could you see a height dependence? Or is that true for the full profile?

In some of the observations you can appreciate certain height dependence, however, we did not think that can be consider as a trend among the cases. For some of them in the first kilometers, the rain water content can achieve the same contributions as snow, whereas ice usually has the greater contribution above +-8km.

13.Line 256: How are you sure that Thompson is overestimating, and the other schemes are not underestimating snow water content?

Our interpretation that the Thompson scheme is “overestimating” snow water content is based on the fact that the resulting differential phase shift values in Thompson are consistenly higher than both the observations and the simulations from the other schemes, even when accounting for differences in particle scattering assumptions. However, we agree with the reviewer that this could also be interpreted as an underestimation of snow water content by the other schemes. We have added a new reference of an study where larger values of snow mixing ratio where found for the Thompson scheme in comparison with other schemes, like the WSM6.

14.Line 263-264: This sentence is confusing to me. Is there perhaps a word missing?

The sentence was rephrased for a better understanding.

15.Line 315: In my understanding that just means that the 'average' x-parameter that fits best is that of Rosette/Aggregate. See major comment 1).

See response to major comment (1).

16.Line 330: Is snow the largest contributor due to the largest x-parameter or largest WC, as indicated by Fig 7?

Snow appears as the largest contributor to the differential phase shift due to a combination of both factors: snow generally has comparatively large water contents in the simulated observations, and the x-parameters associated with snow habits are among the highest, reflecting their scattering efficiency.

17. Line 335: How is the error determined?

    The error of the x-parameters is calculated in equation (11).

18. Line 364: Greater contribution to what. $\Delta\Phi$?

    Yes, to the differential phase shift.

Technical corrections

1. Line 149: Brackes around the year only.

   Corrected

2. Figure 6: Panel (b): Colorbar label and Figure caption do not match. I think the figure caption for panel (b) is wrong.

   Corrected

3. Line 272: I think there is a word missing. Perhaps: ... no universal relationship ... **exists**, particularly...

   Sentence was rephrased