Review comments for Constraining microphysics assumptions on the modeling of Atmospheric Rivers using GNSS Polarimetric Radio Occultations

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.

This paper investigates the potential of Polarimetric Radio Occultation (PRO) to provide insight into hydrometeor vertical structures, using WRF simulations and the ARTS scattering database to simulate differential phase (ΔΦ) for comparison with satellite observations. The use of PRO in this context is still emerging, and this study contributes to ongoing efforts to assess its sensitivity to different microphysical assumptions. Nonetheless, continued exploration of PRO for hydrometeor evaluation is valuable, and further development of physically consistent frameworks is encouraged.

That said, a fundamental concern arises from the physical inconsistency between the WRF microphysics (MP) schemes and the scattering properties derived from the ARTS habit database, which fundamentally undermines the interpretation and validity of the results.

1. Mismatch Between WRF MP Assumptions and ARTS Scattering Properties

The WRF MP schemes (e.g., Thompson, Morrison) are bulk microphysics parameterizations. They assume a fixed set of physical properties for each hydrometeor category: shape, density, and particle size distribution (PSD). For example, snow in the Thompson scheme may be represented by soft aggregates with a specific mass-size and PSD relationship, while graupel has a different assumed density and terminal velocity. These assumptions are not explicitly

output but are embedded in the diagnostic formulas that compute water content and number concentration.
In contrast, the ARTS habit database provides scattering properties (x-parameters) computed for discrete particle habits, each associated with its own shape, refractive index, and PSD assumptions. These may include bullet rosettes, dendrites, plates, spheres, or irregular shapes, and often with a PSD that differs from the one assumed in WRF MPs.
By using x-parameters from ARTS in combination with WC fields from WRF, the method effectively combines microphysical representations that were never meant to work together. This is a physical inconsistency — mixing the absorption/extinction/scattering characteristics of one assumed hydrometeor population with the mass distributions of another. Even if the x-parameters are bounded within ARTS values, this does not correct the mismatch in underlying particle physics.

2. Optimization Does Not Evaluate Microphysics Skill

The second issue concerns the interpretation of the optimization procedure. The authors optimize the x-parameters (within ARTS bounds) to minimize the difference between observed and simulated differential phase shift (Kdp), holding the WRF-derived WC fixed. This means that any biases or errors in the WC fields are effectively absorbed by tuning the x-parameters. Therefore, the optimization outcome does not evaluate whether the MP scheme is accurately predicting WC or phase shift — it only shows which x-parameters (within ARTS-defined bounds) can reconcile the WC fields with observations. This decouples the validation from the actual physical outputs of the MP scheme. Two different MP schemes could produce very different WC fields, but the optimization could find different x-parameters for each that yield similarly good fits to observations — misleadingly suggesting both are good, or that one is better based on fitted x alone.

3. Misuse of "Forward Operator" Terminology

The paper also uses the term "forward operator," which typically refers (in data assimilation) to a physically consistent transformation of model state variables (like WC) into observation space (like Kdp), based on known physics. In a proper forward operator, the mapping is fixed, and the state variables are adjusted (via a cost function) to minimize differences from observations.
However, in the present methodology, the forward operator is not fixed — it is being modified by optimizing x-parameters. This blurs the boundary between model physics and observation operator and makes it unclear what is actually being evaluated. Optimized x-parameters are not state variables and cannot be used to adjust the model state or improve forecasts, thus limiting the method's value even in a diagnostic context.

4. Suggested Alternative

A more physically meaningful approach would be to:
(i) Use x-parameters consistent with each MP scheme's assumed particle properties, either by matching ARTS habits or computing scattering from scratch;
(ii) Use those fixed x-parameters to compute simulated Kdp, and compare directly to observations, without optimization;

(iii) Evaluate the WC fields directly by assessing how well they reproduce observed Kdp under physically consistent scattering assumptions.

Summary
In short, while the idea of using observations of phase shift to evaluate model microphysics is important and timely, the current implementation is flawed due to the physically inconsistent blending of ARTS scattering properties with MP-derived WC, and an optimization procedure that does not test the microphysics predictions directly. The conclusions drawn about MP scheme performance are therefore not supported by the methodology, and a revision of the experimental design is recommended.

We thank the reviewer for this important point. We fully agree that the WRF microphysics schemes make their own assumptions about PSDs, densities, and particle shapes, which do not directly match the ARTS particle habits. As the reviewer suggests, one way to reduce this inconsistency would indeed be to adapt the PSD assumptions used in ARTS so that they reflect those in WRF. This is a valuable point that we have applied in the modified manuscript.

However, in our study we chose to keep the water content fields directly from WRF as input and then test which particle habits from ARTS are most consistent with the resulting PRO signal. Our goal is not to reproduce the exact internal assumptions of WRF schemes, but rather to ask: given the WC that WRF predicts, which types of particles would explain the observed PRO signatures? If we were to be 100% consistent with WRF microphysics, we would be restricted to spherical particles. In that case, the predicted $\Delta\Phi$ would be essentially zero because spherical particles do not produce differential phase shift. Such a test would not be meaningful for PRO, which is specifically sensitive to nonspherical hydrometeors.

Therefore, while we acknowledge the physical inconsistency highlighted by the reviewer, we emphasize that the strength of our approach is precisely testing wether water content output from WRF, when combined with realistic nonspherical particle habits, can reproduce the observed $\Delta\Phi$. This allows us to evaluate which microphysics schemes produce WC distributions that are most compatible with the scattering properties observed by PRO. We have clarified this reasoning in the revised manuscript and also added a discussion about the physical consistency and observational sensitivity. Also we have added a new reference of an study where they also couple WRF + ARTS and they demonstrate that the performance of the coupling has a good agreement with observations, [1].

[1] Wang, D., Prigent, C., Aires, F., & Jimenez, C. (2016). A statistical retrieval of cloud parameters for the millimeter wave Ice Cloud Imager on board MetOp-SG. *IEEE Access*, *5*, 4057-4076.

About the comment on the forward operator, it is true that looking for the best x-parameter may not fit within the strict definition of a forward operator. In this case, what we are doing is to use a simple, general operator that allows us to map WC into Kdp, and look for the best parameters that could be used in a potential operational forward operator. We can see in Hotta et al. 2023 that using fixed values for all cases may not lead to the right results, but rather that specific “x-parameters” may be better for specific phenomena (e.g. what works for

AR may not work for Tropical Cyclones). In this study, we define a way to infer such parameters, and we apply it to the case of Atmospheric Rivers.

Besides, there are some other comments related as follows:

L43: Passive microwave radiometers have also been utilized to interpret precipitation vertical structures, (Turk…
Comma here should be removed

Comma has been removed

L47: These change depending…
These changes depend

Corrected

L49: simulations will be conducted…
were conducted

Corrected

L100: As the PRO rays traverse the derived from PRO.
This is a broken sentence

Phrase deleted

L101: provides valuable insightse from GPS to LEO
Typo "insightse"

Corrected

L101: This, in turn, provides valuable insightse from GPS to LEO, refractivity gradients cause bending, resulting in rays becoming tangent to the surface at their lowest point, termed the tangent point, ht.
This whole sentence is not grammarly correct. Please fix.

Rephrase done

L107: matching a regular grid between 0 and 20 km
What was the reason to pick top height at 20km? Is this limit of the WRF model top?

The reason to choose 20km as the superior limit is because we do not expect the presence of clouds at those heights. The files include values until heights of 60km however, the 0.1km resolution is only for the first 20km.

L127: For some of the simulations, instead of using the RRTMG schemes for shortwave and longwave radiation, the New Goddard Shortwave and Longwave Schemes were used due to certain errors that occurred in specific simulations
Could these mis-match lead to any difference among MP results used in the following comparison?

This is a good point to remark. We are aware that by using different radiation schemes in some of the simulations we are not evaluating the microphysics over the same conditions. However, for each observation, the four different simulations varying the microphysics used the same radiation scheme so the comparison between them is not affected because the four simulations are done over the same conditions. The conditions then vary between observations, but since we analyze 37 cases and only a few of them are simulated with a different radiation scheme, we do not think that this mis-match could affect the statistics in a significant way.

The following is introduced in the manuscript to justify this mis-match:
"In a limited number of cases, the New Goddard Shortwave and Longwave radiation schemes were used in place of RRTMG due to technical issues. Both schemes are widely tested in WRF and provide consistent radiative forcing; since for each of the four simulations done for each observation the same radiation scheme is employed, and also since the amount of observations with this mis-match is low compared to the total amount of cases, we do not consider that this could affect in significance the statistics."

L139 whether hail or graupel is the third class of ice however,
A comma is needed before "however"
Section 2.2.1: The numbering of this section seems weird since there is no other subsection in section 2.2.

Comma corrected
Section 2.2.1 is now section 2.3

L215: where the x_b comes from? Is it from ARTS?
If so, please refer to my general comment

Equation (8) was written following the generic form of a variational cost function, but in our implementation we do not use a single firs-guess values x_b. Instead, the optimization explores values of x within predefined boundaries, as it can be seen in Figure 11. To avoid confusion, we have removed the first term from the equation in the revised manuscript.

L266: figure captions (a) and (b) are opposite to with what are defined in Figure 6. Caption

Corrected

L282: "Even though the minimization of J is performed jointly for both the microphysics scheme and x-parameter"
According to L210, x-parameters are obtained by optimizing the cost function as in (8) by fixing WC. Therefore, I don't understand what you meant as jointly.

By this we mean that both the optimization process is done at two levels, within a given microphysics scheme and overall (microphysics with lowest cost function evaluated at the optimal x values).

L293: The results indicate that the Goddard scheme consistently outperforms the others, followed by WSM6.
Since the simulation has used optimized x-parameters, which is case dependent (I think if there are other 32 points, you would get a different set of x-parameters), it is hard to say which original WRF simulation, and its corresponding scheme are the best. We can only say the Godard scheme was compensated by the specific “optimized” x_parameter in this particular case and such a combination performs the best.

We agree that, because of the x-parameters are optimized on a case by case basis, the evaluation cannot be interpreted as an absolute ranking of the original WRF microphysics schemes. Instead, the analysis should be understood as assessing how consistently each scheme, when combined with the scattering properties from ARTS, can reproduce the observed differential phase shift. In this sense, our conclusion that the Goddard scheme tends to align better with the observations reflects its relative performance within the optimization framework, rather than an absolute superiority of its microphysical assumptions.

Line 333. The first relevant thing to note is that the optimized values for ice and graupel often lay over the lower limit (the allowed range of x-parameters values is shown with a gray shaded area and related discussions).
This suggests the optimization problem is under-determined and constrained in a way that prevents a proper minimization of the cost function. As a result, I would question how robust the resulting conclusions are regarding the relative contributions of different hydrometeor species— especially if the optimization is not genuinely being achieved for some of them. The physical reason for not being able to minimize some of the MP results is that these MP schemes use different assumptions of the hydrometeor habits and therefore, you might not be able to get a convergence at all by using ARTS defined habits.
In its current form, the method raises a fundamental question about physical inconsistency between ARTS habits and MP assumptions. It might be helpful to examine whether the achieved x-parameters through Eq (8) match the assumption of the specific MP. If so, it might be meaningful to give some insights on habit distribution, matching the bulk parametrization but not described explicitly in MP. But it seems not the scope of current manuscript.

We indeed have made the optimization without boundaries, but the retrieved x-parameters for ice and graupel frequently became negative or reached unphysical values. This confirms that optimization problem is under-determined in some cases, and that unconstrained solutions do not provide physically meaningful results.

For this reason we choose to constrain the optimization by setting boundaries of the x-parameter to the minimum and maximum values found in the ARTS particle database. This ensures that the retrieved parameters remain within the physically plausible range of scattering properties. We are aware that these constraints may in some cases limit the cost function minimization but we consider them necessary to guarantee physically realistic outcomes.

We again agree that this limitation also can reflect the mismatch between WRF and ARTS, but as we have said this should be interpreted not as providing a unique particle characterization, but rather as an indication of the relative tendency of each microphysics scheme to align with certain scattering behaviors.