Response to reviewer #1

Reply to the review by reviewer # 1 (Dr. Josep M. Aparicio) of **AMT-2023-132: "Forward operator for polarimetric radio occultation measurements"** by Daisuke Hotta, Katrin Lonitz, and Sean Healy

We would like to express our sincerest gratitude for spending your precious time on kindly reviewing our manuscript. We believe that revision of the manuscript following your thoughtful suggestions have certainly allowed us to significantly improve the manuscript, especially in terms of precision and clarity.

Please find below our point-by-point responses to you comments. Your comments are cited in red colour, followed by our replies in black. For references cited in the responses below, please see the list of references provided in the revised manuscript.

For convenience, we have attached a tracked-changes version of the revised manuscript (`diff.pdf`) generated with the `latexdiff` utility; in this file, newly added and deleted texts are highlighted, respectively, with blue and red colours. Line numbers and page numbers shown in the responses below refer to those in the `diff.pdf` file.

We hope that our revision succeeded in addressing all your concerns, and thank you again for your careful assessment.

Point-by-point responses

The manuscript presents an interesting exploration of the ability of an NWP system to estimate observed RO polarization observations, a precondition to assimilate them. The authors find qualitatively good forward estimates, with an ability to identify the effects of rain and snow. Although the agreement is still not accurate in the quantitative sense, this can be interpreted as both the uncertainty of the detailed hydrometeor field, and a margin associated with the oblateness, and probably the tilt, of hydrometeors, quantities with large uncertainty.

L23-24: "radio waves travel through oblate objects"

The principle is true both with oblate and prolate objects (hydrometeors may be both). Also, a large fraction of the effect happens because radio waves travel next to such objects (not just through). I suggest a minor adaptation of the sentence, for instance "radio waves travel through a medium containing ellipsoidal objects".

Response:

Thank you for pointing out our imprecision. We agree that K_{DP} can be negative if the wave passes through prolate objects. We corrected the description as suggested to make it more precise (L24-29).

L92: "to avoid negative values": Would it be inappropriate to have negative values? I understand that it may be desirable to maintain the monotonicity of the interpolated field, and not contaminate it with a spurious wavy interpolation, but please correct me if I am wrong (or better, specify in the paper): is the differential phase shift necessarily positive? I would guess that it could be positive or negative, as a function of the oblateness/prolateness of hydrometeors, and one could have both along a line of sight. At least, I understand that atmospheric snow/ice can appear prolate in polarimetric radar. Please elaborate/clarify.

Response:

We thank you again for pointing this out. It is true that K_{DP} in nature can be negative depending on the orientation of the particles through which the wave passes. Here in this context, however, we are discussing interpolation of the K_{DP} field that is simulated from model forecasts, and in our formulation described in Section 2.2, K_{DP} is always non-negative, so that monotonicity implies avoidance of negative values. We made this point clear in the revised text (L103-L106).

L95: "tangent point drift is not crucial for the regular RO observations". It looks oddly expressed. It was found to be important, albeit it is indeed a small fraction. I agree that the difference must be much larger for hydrometeors.

Response:

We agree with the reviewer that "not essential" was not the right word to describe the role of accounting for tangent point drift. Following your suggestion, we modified the text in the revised manuscript (L111-112) removing the phrase "the effect of tangent-point drift is not crucial" and wrote that *(assimilation of bending angle) can be beneficial even when the effect of tangent-point drift is neglected*.

Also, L95 "presumably due to the weak horizontal gradient". The word "presumably" also looks odd, as it is quite established that for this very fact one gets a perceivably less accurate, although still reasonable, result if the drift is ignored. Consider rephrasing.

Response:

Again we agree with the reviewer and removed the specified phrase (L111).

L122: "axis ratio of the ice particles" Since this is finally applied to water also, should it not "axis ratio of the particles", thus including water droplets? Also, it looks strange that the water shape vs rain rate relationship being relatively well established, it turns out to be less developed in the paper.

Response:

We agree and dropped "ice" preceding "particles" in the text below Eq. (3) that explains notation used there (L138).

It is true that our treatment of rain and liquid particles is too simplistic. Refining liquid water treatment is beyond the scope of our current paper but we do plan to explore a more rigorous approach, as we mentioned in the section on "future directions" (L381-L385). As we stated there, we plan to incorporate formulations adopted in RTTOV-SCATT to make our operator more consistent with the other components of the NWP system, and this includes more sophisticated simulation of K_{DP} for rain that leverages more advanced understanding of scattering by rain and liquid particles.

Given the importance of the ice vs liquid water phase that is being featured in the manuscript, could some approximate indicator of the height of the freezing point be added to some figures (notably the panels in Fig 1). To some extent, it is visible by the level where the rain signal becomes non-zero in each panel (in the range 2-6 km). If that curve happened indeed to be a reasonable indicator, you may mention it in the caption.

Response:

We thank the reviewer for this useful suggestion. We also had the same suggestion from Prof. Jennier Haase in her community comment (https://doi.org/10.5194/amt-2023-132-CC1). Following the suggestion, we replotted Figure 1 with the freezing levels indicated with black thin horizontal lines. Consistent with your expectation, these levels do roughly coincide with the levels at which rain/liquid begin to show non-zero contributions. We revised the text accordingly

(L223-225 and the caption of Figure 1).

Sec 4.2: Sensitivity to displacement. A 10 km displacement is introduced. It is however not mentioned whether this is a good/inaccurate estimation of the geographic accuracy of IFS. Presumably, it was selected because this is order of magnitude the position error of AR and TC features within IFS at short lead time (say, about 6h). Is that so? Please comment whether these figures are indeed representative of IFS's accuracy.

Response:

We thank the reviewer again for raising this point. From tropical cyclone forecast verification, we know that in IFS Cy47R3, the average tropical cyclone position error for 12 hour forecasts, verified against best track analysis, is about 30 km. Considering the error of best track analysis itself, and taking into account that the first-guess used in data assimilation is usually a shorter-range forecast (on average a 6-hour forecast for a 12-hour assimilation window), we consider that the ~ 10 km displacements that we examined in this study are commensurate with cases of successful TC forecasts. Unfortunately we do not have quantitative estimate of position errors for AR cases, but we speculate that the forecast accuracy for TC and AR cases is not too different. In the revised text, we provided some explanation about these (L245-248).