Response to Referee #2

To begin with, we would like to thank the anonymous referee #2 for her/his time and efforts in reviewing our submitted manuscript and providing feedback. We also would like to express our gratitude for her/his kind words about our manuscript. Finally, we would like to inform the anonymous referee #2 that we will revise the manuscript according to her/his comments and the comments of the anonymous referee #1. Below we respond to the main questions/comments raised by the referee, and outline how we will revise the manuscript. To that end:

- referee's comments are given in blue,
- our responses are outlined in normal format, and
- **any suggested textual changes are given in bold format**.

Responses to general comments (GC) from referee #2 (GC2)

(GC2.1) 89 GHz PD, as also discussed in the manuscript, is complicated by not only the surface PD signal contamination, but more importantly, but liquid emission. It is a damping effect if liquid emission from water cloud or rain layer beneath the frozen hydrometer layer is completely random oriented, however, rain droplet tends to be horizontally aligned as well. This adding an extra dimension of difficulty which was not mentioned in the paper, and not considered at least in full RTM simulations. I would use a lot of caution of applying a best-fit ratio to 89 GHz.

This is true. We thank the reviewer for pointing out both the damping effect due to liquid emission and the horizontal orientation of liquid spheres (e.g., Ekelund et al., 2020). We will include these suggestions in the revised manuscript.

In fact, due to the high complexity of this channel, in the forecast impact assessment and the final configuration of RTTOV-SCATT v13, the polarization ratio found at 166.5 GHz was adopted. In the revised manuscript, we will further clarify this point.

(GC2.2) The best-fit ratio is achieved globally on a statistical sense, and a fixed value is applied globally. In reality, I would imagine it should vary by weather systems and/or locations. For example, snow crystal shape, size and orientation would be different be hind the cold front versus ahead of it; analogously, snow characteristics in an Arctic low should be different from those in a tropical deep convective system. Can the value of rho be latitudinal varying or weather regime dependent (e.g., convective versus stratiform pixels in GCM grid). I'm not asking to perform these analysis, but I'd like to see authors' response on this question: in other words, would a varying rho be potentially more beneficial to the DA from the satellite retrieval perspective?

We totally agree that hydrometeor orientation and polarisation should be situation dependent. However, this is more applicable to satellite retrievals, if not to DA yet. In DA, a more intricate scheme would require significant extra tuning work and it would be even harder to validate its performance. But, studies that have looked at latitude and seasonal dependence of the polarisation arches have shown a surprising fair consistency (Gong and Wu, 2017; Galligani et al., 2021). This encouraged us to use a single global fit.

(GC2.3) Other than the impact on forecast, what are the impact on other variables, for example, total column IWP (all ice hydrometers), TOA radiation budget, etc.? I doubt whether a discernible impact but it would be nice if these "climate" impacts could be discussed or at least mentioned. In the future, if model physics start to include orientation impact on, e.g., radiation, or depositional growth of particles, I would imagine water cycle and the radiation budget would be impacted eventually.

The impact of the all-sky assimilation on the hydrometeor mixing ratios is short-lived; any changes in cloud properties in the forecast would come from the response of the model moist physics to any changes in the synoptic situation in response to changes in dynamical variables, and not directly from the assimilated information on hydrometeors. Hence, we are not expecting these variables (e.g., forecast IWP) to be affected given the relatively small impact measured on the main forecast variables. Once parameter estimation can be used to update the forecast model moist physics parameterizations to better fit the all-sky observations, then the benefits to cloud variables can be carried into the forecast.

Responses to minor comments (MC) from referee #2 (MC2)

(MC2.1) L154: "if they are large enough, they tend to be oriented". This is not quite correct. Only if the aspect ratios are large (i.e., flatter) and the ambient environment flow is relatively stable (e.g., stratiform regime), that large frozen hydrometers tend to be oriented in a predominant direction. In some cases DPR's DWR indicated big-sized particles but collocated GMI 166 GHz PD signals are small.

The referee is absolutely right; our description was incomplete. We propose the following elaboration:

However, ice hydrometeors are characterized by non-spherical shapes and thus non-unit aspect ratios**. This could potentially lead to preferential orientation driven by gravitational and aerodynamical forces (Khvorostyanov and Curry, 2014) or even by electrification processes (lightning activities at deep convective systems, Prigent et al., 2005). Under turbulence-free conditions, small non-spherical hydrometeors (diameters below $\approx 10\,\mu$m) are totally randomly oriented owing to Brownian motion (Klett, 1995); but, if they are large enough, they tend to be horizontally oriented as they fall depending on their shape: this holds true for thick plates with a diameter above $\approx 40\,\mu$m (Klett, 1995), while oblate spheroids and thin plates would adopt horizontal orientation at sizes larger than $\approx 100\,\mu$m (Prigent et al., 2005, and**

references therein) and $\approx 150\,\mu$m (Noel and Sassen, 2005), respectively. However, turbulent effects can easily disrupt any orientation especially for small hydrometeors or introduce a wobbling motion around the horizontal plane at larger sizes (10–30 μm) (Klett, 1995). In addition, tumbling motions in strong turbulent conditions, e.g., within deep convective cores, induce total random orientation (Spencer et al., 1989).

(**MC2.2**) Figure 3, top right panel: it looks divergence trend hasn't reached a minimum by rho=1.5 yet. Also, for these statistics, are surface-contaminated pixels removed? If yes, I'm a big confused why ocean and land skewnesses are so distinctly different at 89 GHz.

First, we should clarify that ocean (land) represents pixels solely over the ocean (land), excluding pixels across the coastline, while global represents the overall number of pixels (including the coastline). In all figures, results are presented after screening out any surface contamination. In case of 89 GHz, the rather strict two-fold screening method resulted in a rather low sample over the ocean. In addition, the IFS simulates this channel with less confidence over the land compared to the ocean. To highlight, Geer (2021) reported that the combination of the IFS and RTTOV-SCATT does not simulate deep enough brightness temperature depressions in tropical convection over land. These are the reasons why one sees such differences in the statistics between land and ocean at 89 GHz. Note here that a minor bug has been corrected at the same time, whereby points with PDs above 15 K were not fully included in Fig. 5 or in the divergence calculations; however this makes no difference to the other statistical metrics (mean and skewness). Accordingly, the divergence has now a clear miminum that occurs at a polarization ratio of 1.45. But, DA assumes that errors are Gaussian and unbiased; hence we prioritize minimising the measure of skewness.

In the revised manuscript, we are going to provide additional clarifications.

(**MC2.3**) L440: just a comment – I like your discussions here. Several possibilities are presented, and you leave some room for future exploration. Actually, we've tried to connect collocated lightning data we GMI negative PD signals but failed to establish a statistically robust relationship. Maybe it's simply because lightning happens at instantaneous time-scale that typical collocation criteria (10-15 mins time difference) doesn't work, but geographical distribution of negative PD also doesn't direct point to an association with lightning. I honestly doubt in real world, cloud ice could generate a cold 166 GHz TB as cold as 125 K (e.g., your Fig. 6c), which means tremendous number density and extremely large plate-type of cloud ice. As CALIPSO only sees 1-5% of chances of horizontally oriented ice globally, I believe cloud ice orientation doesn't happen as often as snow aggregates, and it's impact should be minimal at 166 GHz.

We agree with the reviewer's comments, and look forward to a future improved understanding of particle orientation, and its dependence on habit, cloud type, and cloud processes.

Best regards,
Vasileios Barlakas, Alan J. Geer, and Patrick Eriksson

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