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Atmospheric Measurement Techniques Discussions

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Interactive comment on "Introducing hydrometeor orientation into all-sky microwave/sub-millimeter assimilation" *by* Vasileios Barlakas et al.

Anonymous Referee #2

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Review comments for "introducing hydrometeor orientation into all-sky microwave/sub-millimeter assimilation" by Barlakas et al.

This manuscript thoroughly studies the feasibility of using a simple and fast strategy to incorporate the frozen hydrometeor orientation induced radiance difference between V- and H- polarized (V-pol and H-pol) channels at high frequency microwave/sub-millimeter (MW/sub-mm) band into the current ECMWF data assimilation (DA) system. This simple strategy involves a modification of the optical depths calculated from model simulated hydrometers at V-pol and H-pol so to make their ratio satisfying a fixed value. This approach is proved in the manuscript to be able to mimic the observed arch-shape of PD-TBv relationship at 166 GHz, and greatly reduce the skewness of O-B distribution so to make the all-sky DA really possible. At 89 GHz, it's found more difficult to

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Discussion paper

achieve a best-fit because it's also impacted by surface and water emission from below the frozen hydrometer layer. Then, three sets of modifications are tested (modifying V-pol only, V-pol and H-pol together, and H-pol only) for the impact on model forecast. Although the impact is in general neutral, this method allows DA schemes to be more consistent among channels with V-pol and H-pol, as well as across different satellite instruments that have different polarizations at the same channel frequencies.

I enjoyed reading this manuscript. This work is novel and ground-breaking as it's the first research I've ever seen to put effort to DA the frozen hydrometer orientation information. Although currently no GCM microphysics schemes really simulate the orientation characteristics of hydrometers, polarization difference (PD) at high-frequency MW/sub-mm channels is a major hurdle for all-sky DA for these channels mainly because the skewness induced by the microphysics. This work proposes an easy and physically meaningful and consistent way to tackle this problem. The results are very encouraging and meaningful for the preparation of new missions such like ICI as well as opening up a new door to reprocess existing satellite observations. I fully support the publication of this work and looking forward to reading the companion paper (Geer, 2020, to be submitted to AMT).

Still, there are some minor issues that I think require further clarifications. (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.

(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?

(3) Other than the impact on forecast, what are the impact on other variables, for example, total column IWP (all frozen hydrometers), TOA radiation budget, etc.? I doubt whether a discernable 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.

Minor comments: 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.

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.

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.

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