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Geoscientific Model Development Discussions

# *Interactive comment on* "(GO)$^2$-SIM: A GCM-Oriented Ground-Observation Forward-Simulator Framework for Objective Evaluation of Cloud and Precipitation Phase" *by* Katia Lamer et al.

**Anonymous Referee #1**

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This manuscript describes a forward-simulator designed to take general circulation model (GCM) hydrometeor fields and provide lidar and Ka-band radar measurements. Instead of basing the forward calculations on fundamental radiative transfer theory, the calculations largely rely on 18 empirical relationships to convert the hydrometeor fields into lidar and radar measurements. The authors interpret the ensemble of different empirical relationships as a measure of uncertainty. Phase classification is based on the forward-model calculations.

The article is well written and the descriptions of forward-model calculations like this



are a useful contribution to the peer-reviewed literature. My criticisms are mostly philosophical in nature.

1. The advantage of basing forward calculations on empirical relationships, as opposed to fundamental radiative transfer and scattering theory, is not well established in the manuscript. One justification for the approach in the paper is that using empirical relationships means that one does not need to make assumptions about scatterers (e.g., a spherical assumption), but this simply exchanges a known assumption with an assumption (or set of assumptions) hidden in the empirical relationships. And most of these empirical relationships are actually retrievals, just inverted! If we're going to do forward calculations based on retrievals, we might as well just use the retrievals on the observations and cast all the quantities in terms of geophysical variables, which are easier to interpret. This approach seems like a big step back compared to performing fundamental radiative transfer/scattering calculations on the model fields, which yields an independent forward calculation of the observational fields. Furthermore, the assumptions in the empirical relationships may not be consistent with the assumptions in the model cloud microphysical parameterization (e.g., the assumed distributions). Consistent forward calculations of model variables should use assumptions consistent with the cloud-physics scheme in the model.

2. The manuscript advocates a phase determination that is solely in forward-calculation space and fairly well articulates the reason for this. However, this approach does not take advantage of knowing the actual hydrometeor fields, and therefore this discards a great deal of potentially useful information. Is there any way the approach in the manuscript can take some advantage of the fields in hydrometeor (model) space?

3. Constructing an ensemble of forward calculations based on different empirical relationships is a good idea, but it is a stretch to portray it as quantifying uncertainty. The authors have no way to know to what extent the results from these calculations actually map to the PDF of possible outcomes. It is useful but is not statistically defensible to call it UQ. The authors should much more carefully word this claim.

4. The calculations are based on 30-minute instantaneous model hydrometeor fields. The article is focused on the actual forward calculations of the microphysical fields, but comparison of forward-model calculations and observations necessarily includes assumptions of spatial and temporal scale. Would the authors please discuss with a bit more detail on how the forward calculations (30-minute instantaneous calculations of lidar and radar fields) would be compared to observations? If nothing else, this would provide some guidance for readers using their forward simulator.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2018-99/gmd-2018-99-RC1-supplement.pdf

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