Review of manuscript egusphere-2023-170

Title: Observed Process-level Constraints of Cloud and Precipitation Properties over the Southern Ocean for Earth System Model Evaluation

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Summary:

In this manuscript, Stanford and coauthors examine the occurrence of cloud and (below-cloud) precipitation over the SO using cloud radar, two lidar ceilometers, and balloon-borne soundings measurements collected during MICRE. They compare the measurements and derived occurrences (for various low cloud types) with simulations from the GISS-Model3E using, radar and lidar instrument simulators.

Recommendation: Publish in with minor revisions.

I very much enjoyed reading this article. The manuscript is well written and well organized, and the data analysis has been done with care. Frankly, I think the manuscript is perfectly publishable in its present form. What comments I have, are frankly being rather picky.

This is Roger Marchand. I normally do not identify myself in the review processes, but I do so here for a couple of reasons:

First, I only realized when I got to the end of the manuscript that I am acknowledge for some past conversations I have had with the authors. Had I realized this, I probably would have declined to be a reviewer. I don't think my review is biased, but I think it is appropriate to note this fact. Regardless, thank you for the acknowledgment.

Second, as you will see below, I have some specific suggestions as regards ongoing work by my students.

Comments:

1) Title: Constraint on Physical Processes, "Properties", Representativeness of the site

I do not care for the title.

(i) While I agree that the observations have the potential to provide constraints on physical processes, this is not actually demonstrated or explored in this manuscript. I think doing such would make for a nice paper, and I hope that you will do such as part of future work. But it seems like overreach to simply claim the observables constrain processes, and problematic to provide no information on what processes or to what degree they might be constrained.

(ii) “Properties” is rather vague. The manuscript is focused on cloud and precipitation occurrence for warm, supercooled and partially supercooled clouds. Why not put these terms in the title? I think this will make the paper easier to find for people interest in how supercooled and warm clouds differ.

(iii) This bothers me the least, but with a high degree of confidence there is a significant latitudinal dependence in some of the occurrence statistics which are (or maybe) comparable to the differences you note between the observations and model output.

Might I suggest a title change to something such as:

“Earth System Model Evaluation using Cloud and Precipitation Occurrence for Supercooled and Warm Clouds over the Southern Ocean (based on observations during MICRE).”

Or

“A Comparison of GISS-Model3E and MICRE Observed Cloud and Precipitation Occurrence for Supercooled and Warm Clouds over the Southern Ocean using Instrument Simulators.”

2) Depolarization lidar phase retrievals (L. 165)

You note in the manuscript that processing of the depolarization lidar has been a problem. This is true. But my student, Emily Tansey has been working on this for the last year and I think we have the problems “worked out” as best we can. We are submitting a publication on this in two weeks. My point here is not that you should delay publication of this manuscript, only that I would prefer if you wrote something to the effect of “The depolarization lidar data have calibration stability and other problems which are being corrected and will be released soon [manuscript in preparation, Tansey and Marchand, University of Washington]

3) The Comstock, Z-R Relationship (Line 233)

You may be interested to know that I have another student (Litai Kang) looking at Z-R relationships based on SOCRATES aircraft data. In spite of the fact that the Comstock relationship was based on VOCALS data (subtropical StCu), it seems to hold surprisingly well for the SO. I list our equations below. We are working to get this published, but I suggest you add something to the effect of “An examination of in situ aircraft data from SOCRATES finds the Comstock relationship holds well for drizzle falling from SO stratocumulus [personal communication or manuscript in preparation, Kang and Marchand, University of Washington].”

Based on in situ data (~ 150m above the surface) ➔ $Z = (63.8 \pm 47.1)\, R^{(1.3 \pm 0.05)}$

Based on SOCRATES radar-lidar retrievals ➔ $Z = (31.6 \pm 1.4)\, R^{\ 1.4}$

(The R^1.4 is an interesting story for another day. And I think the "a" coefficient appears to be a bit higher because the number of drizzle droplets appears to be higher on average for the SOCRATES measurements as compared to VOCALS, and I suspect this is climatologically true but pretty small datasets, really.).

4) Seeder feeder?

Having worked with these data extensively, I know there is not a tremendous amount of seeder feeder generated precipitation coming from (through) low clouds. Nonetheless, I wonder if you have done anything to filter such out? Have you looked at your statistics when limiting to single-layer clouds (if yes, does it matter)? I am bit concerned about this because you state on Line 140, that you looked at only the lowest cloud (and are not leaving much room for ambiguity in the LCB – see also comment line 356).

Specific comments:

Line 27. What about CMIP6? The material here seems a bit dated and there are several papers out now looking at radiative biases in CMIP6. I am somewhat amused by the fact that there are 99 or so papers by coauthor Cesana cited here, but not his recent paper looking at the SO radiative bias in CMIP6 models. You might also consider Lauer et al. 2023.

Line 140. As you note a few lines below, CBH here means the peak in the backscatter. The algorithm finds the peak well, but I think one might debate whether the peak is the best way to define cloud base. In fact, you discuss this in more detail later in the manuscript (or reference Silber's 2018 paper were he does). Perhaps move material form line 147 up and note the peak is generally found with a small RMS uncertainty (+/- 5 m) for liquid clouds?

Line 154. Merged in what way? Via a nearest-neighbor interpolation? Averaging the ceilometer in some time window? I did the later in my work with these data, in part because it provides an easy way to combine the ARM and UC data.

Line 219. Using a threshold (-55 dBZ) that is below the radar minimum detectable signal is not a problem per se, but you should not expect the results to be the same as what you would get from a radar that had a minimum detectable signal of -55 dBZ over the entire below-cloud region. That is there would be more precipitation at the lower thresholds with a more sensitive radar. I don't think this would qualitatively change your points, but I think you could be clearer on this. Perhaps in the appendix (see also Line 671).

Line 324. "… Rcb was also found to increase for decreasing CTT while controlling for cloud thickness (not shown) …". Why not add a panel to show this? I think this is a valuable point.

*Line 356. I think this should be 7% to 27%. If 27% have a LCB without a radar detection (at the location of cloud base) but 20% DO have a radar detection within 100 m, might it be the case that the radar is simply not able to see the bottom portion of the cloud and therefore only 7% of the time there is a "lidar only" detected cloud? (In my own work, I assume LCBs within 100m of radar detection are the same cloud and thus report a value closer to 10% for lidar-only detected clouds).

Line 394. The Comstock relationship was not developed using fog, which I believe is known to be composed of droplets that are smaller on average than that found for most clouds. I am not sure this is well justified.

Line 503. Too much stratocumulus? I would say this is debatable, since the study in question is based on ISCCP obs and ISCCP-clustering. (1) While there is too much of the "stratocumulus" cluster, I am not sure there is too much actual stratocumulus cloud (in total) and (2) one needs to be careful with ISCCP measures for low cloud occurrence (such is lower in ISCCP as compared with say MISR or CALIPSO/CloudSat). Perhaps change to "… and MAY now simulate …".

Line 505. "… 57% in GISS-ModelE3 compared to 76% in MICRE". So is it fair to conclude that cloud occurrence (and presumably stratocumulus) remains a bit low in GISS-ModelE3 (in spite of the previous statement)? Perhaps add a direct comment rather than leave such for the reader to piece together.

Line 531. "if satellite observations underestimate precipitation occurrence frequency relative to collocated ground-based observations … " Don't the next two sentences demonstrate that this "middle point" IS true. Perhaps you should break this sentence apart and comment on the degree to which each of these might be true? As-is, you give three reasons, and say nothing about two of them, leaving the reader to infer what they will.

Line 671. "...and 1 km"? I'm confused by this comment. Only near h_min is the sensitivity this good. Near 1 km you are near -36 dBZ, so a radar volume with -45 dBZ worth of precipitation at 1 km wont be detected.

Line 673. "underestimate", meaning the vendor LCB is too low in altitude (units are given in m not Pa)? I think you mean the CEIL LCB tends to be high !?! Perhaps rephrase to make clear.