

Review of: Impact of seeder-feeder cloud interaction on precipitation formation: a case study based on extensive remote-sensing, in-situ and model data

Authors: K. Ohneiser, et. al.

General Comments:

In general this is an interesting paper. The observational set up is impressive and this may be among the best-observed seeder-feeder situations not involving aircraft. I applaud the authors for their work and fully recognize the amount of effort that went into collecting and interpreting the data.

Having said this, in the end I was a bit underwhelmed with what we actually learned as part of this study. What is new? What did we not know before? What was quantified? With all of these excellent observational platforms, what new perspectives led to new insights? In addition, the model evaluation portion of the work was very underwhelming and resulted in over-generalization of recommendations in the results section.

In the end there is nothing technically wrong with the study and could be published nearly “as is”. However, its impact seems likely to be limited without more detail on what the novel findings are as a result of the work put into the collection of this dataset.

Specific Comments:

Below I include some specific comments on what is discussed above in the general comments section. Hopefully the authors can consider these comments and assess whether they are able to improve the manuscript as requested based on these comments.

- Line 114: What quantities from the ceilometer are going into VOODOO? Some more detail would be helpful.
- Line 122: Cloudnet ““categorize”, or “category” files? Categorize doesn’t make sense in context, unless it is the specific name of the files.
- Line 130: Consider defining what is meant by “lowest troposphere”.
- Line 152: How do you know that the model is “good”? What does “good” mean? Can you quantify this?
- Section 3.4: Are there any particular considerations for use of HYSPLIT in areas of complex terrain? Are there publications that highlight the performance of this model in such regions? If so, it would be worth highlighting here.

- Section 3.6: It would be useful to know how the use of one algorithm versus another is prioritized in this study. Can you provide additional details?
- Lines 229-230: “Likely presence of only small particles”. To what extent has this been evaluated?
- Paragraph starting on line 352: Is the aerosol stuff relevant for the rest of the study? Other than a short mention of INPs, I didn’t see any real mention of aerosols. Recommend removing this, along with the associated figure, given that it’s not referred to later in the document and it’s based on model output.
- Figure 4: The purple and orange curves for the streak tracking don’t look as I would anticipate. Are they correct? They don’t seem to follow maximum reflectivity, as described earlier in the manuscript.
- Lines 410-411: I do see a slight enhancement of EDR...
- Paragraph starting at line 435: This is an important “check”, but I feel like it is over-described. Can this PAMTRA section be condensed to 1-3 sentences?
- Line 467: How are we sure that one of these peaks is not locally-generated ice crystals?
- Figure 7d: There are several “liquid droplets only” points identified around 3-4 km. Yet there is no real discussion of these. Seems noteworthy, given their colocation with some of the other features that are discussed?
- Line 585: “reduced LWP must be the result from being used for riming the ice crystals”. First, it’s not clear to me that this *has* to be the result of riming only. That’s certainly a plausible explanation, but “must be” is very strong language. Second, this sentence structure can be significantly improved (e.g., “The reduced LWP must result from riming of ice crystals.”).
- Line 587: What leads to this assumption? Is this based on some calculation? Or just a number pulled out of thin air?
- Section 6.2: I found this section to be severely lacking. Sure, it’s nice to *somehow* tie the observational study back to modeling. However, to do this right you would need to look at a variety of different things, including microphysical tendencies, model thermodynamic state, etc. Here this comparison feels like an afterthought that was more included because it was deemed necessary, rather than an actual insightful evaluation of the model and the processes that are (and are not) represented in it. I would recommend removing this altogether since the paper is already quite long.
- Line 626-627: This conclusion on the modeling is really only for this case, at this location. Is the model even equipped to handle the seeder-feeder process, in terms of parameterizations?

- Line 634-637: This language on model shortcomings is an over generalization. Only one model was evaluated, and only for one case.

Technical Corrections:

This section includes corrections and recommendations of a technical nature.

Line 15: “big” seems like an odd word here.... It is used twice. What is “big remote sensing”?

Line 112 and all of manuscript: At times, the word “the” is used excessively. I recommend the authors read through the manuscript again and read sections that include “the” without the word “the” and see if it still makes sense. If so, take it out!

Line 196: Recommend: “enables detection of liquid layers in a cloud.”

Line 236: Recommend: “Within updrafts, liquid droplet formation can be enhanced.”

Line 317: “Ice ICNC” – use of “ice” is redundant.

Line 426: “probability of riming are visible”. Should be either “probabilities” or “is visible”.

Line 444: This sentence structure needs some work and doesn’t translate correctly in English.

Line 600: “This even takes place...”