Reply letter to Reviewer 1.

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.

We thank reviewer #1 very much for providing the review and for the thoughtful comments to the manuscript. (Side note: The line numbers that are being referred to in the responses are according to the revised version without tracked-changes.) The feedback provided us a guideline to improve our conclusions accordingly. We checked our concluding section and found that the text might have been a bit too general, so we went through that part again and added some concrete examples from the manuscript. Now, in our concluding section we have the following paragraph: "The study has several implications. Based on our observations we found three main insights: The application of multiple advanced remote-sensing methods such as fall streak tracking, Doppler peak separation, and ice shape retrieval shows a consistent view on the case study which highlights the robustness of the methods. This sets an important basis for future studies on cloud processes using similar approaches. Based on the observations, the interactions of the seeder and feeder cloud layers was obtained in unprecedented detail. It was found that the seeder-feeder interaction significantly enhances precipitation which has an impact on the water cycle. From the anti-correlation between surface precipitation and liquid water path we estimated that 20-40% of the precipitation stems from the feeder cloud. However, we have to note that the value of 20-40% is strongly dependent on the assumed reproduction rate of liquid water in the feeder cloud. Future studies should focus more on the quantification of the impact of feeder clouds. In the study, also the scientific understanding of microphysical processes like riming and ice crystal shape evolution are deepened. It was found that the ice crystals increased their velocity from around -0.8m s-1 to around -1.6m s-1 once they interacted with the feeder cloud due to the riming process. In the end, the fraction of rimed mass of the particles was around 90%.

The evaluation of output from the operational ICON-D2 model suggests that key microphysical and dynamical processes are misrepresented in current weather forecast models. Precipitation was significantly underestimated during seeder-feeder phases and

overestimated when no interaction occurred. It shows that a better representation of supercooled liquid water and mixed-phase processes is necessary in order to improve the weather forecast, particularly in regions affected by persistent low-level super-cooled stratus clouds."

Below, we'd like to address the specific comments of reviewer #1:

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.

We read through line 114 and the neighboring lines and found that it is not about VOODOO. We are thus not sure which paragraph/section/lines reviewer #1 addresses. The section 3.7 on VOODOO already contains the following paragraph, which to our opinion serves the request of Reviewer #1: "VOODOO utilizes cloud radar Doppler spectra from a vertically pointing radar, attenuated backscatter coefficient at 1064nm from a ceilometer, liquid water path (LWP) retrieved from a microwave radiometer, and temperature, relative humidity, and pressure from numerical weather forecast data from the European Centre for Medium-Range Weather Forecasts (ECMWF)."

- Line 122: Cloudnet ""categorize", or "category" files? Categorize doesn't make sense in context, unless it is the specific name of the files.

Yes, categorize is the specific name of the files in Cloudnet. It is concretized in the manuscript from line 129.

- Line 130: Consider defining what is meant by "lowest troposphere".

In this case, the windsond measurements were available in the lowest 500m above ground. It is now rewritten in the manuscript from line 138.

- Line 152: How do you know that the model is "good"? What does "good" mean? Can you quantify this?

The wording is changed. CAMS can be understood as a leading model in terms of aerosol distribution worldwide. Of course, regional biases can still occur in the model. We included the study of Amarillo et al., 2024 which deals with bias problems in CAMS.

- 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.

We use the standard HYSPLIT model. There are studies that show the influence of complex terrain on the accuracy of the trajectories. Hernandez-Ceballos et al., 2014 show that the change from GDAS to WRF-ARW enhances the accuracy of the trajectories. Problems in

complex terrain arise when the meteorological forecast data is spatially averaged. We added the information to line 162 in the manuscript.

- 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?

We added to the manuscript from line 186: “The wind-based fall streak approach is prioritized if precise wind data with a high resolution are available or the reflectivity field is weak or noisy. In contrast, the reflectivity-based approach is used if reflectivity features are directly associated with hydrometeor descent, or when there is no high-quality wind data available. Presence of directional wind shear remains the largest source of uncertainties.

- Lines 229-230: “Likely presence of only small particles”. To what extent has this been evaluated?

The following sentences have been added to the manuscript from line 239: “The size of particles at cloud top has not been evaluated. However, cloud tops typically consist of smaller ice crystals because the cloud top is prone to new particle formation. Therefore, we assume the particles are small at this range. When ice crystals are small enough they are closer to the Rayleigh scattering regime and the difference in scattering between 35 GHz and 94 GHz is minimal, resulting in a near-zero DWR and thus preferable conditions for calibration of the DWR.“

- 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.

We understand the concerns of the reviewer #1. Therefore, we removed Fig. 3d. However, we think it is relevant to mention, at least in a short paragraph, that there is dust available in the seeder cloud but not in the feeder cloud. We shortened the paragraph accordingly. The paragraph starts in line 365.

- 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.

Sometimes, the visual expectation of fall streaks in scenes and the algorithm do not seem to be equivalent, especially if there is strong wind shear like in this case around the border between the PBL and the free troposphere. However, we put a lot of effort into the fall streak tracking algorithm. The code was independently programmed by two different co-authors following the description in the methods section. This yielded the same results. Due to 3-dimensional effects, limitations remain in fall streak tracking, however, it is still a more suitable approach than just vertical profiles. This is already stated in the manuscript from line 195. The difference in visually expected and calculated fall streak may also be a result of the logarithmic color scale.

- Lines 410-411: I do see a slight enhancement of EDR…

Based on the comment of reviewer #1 we evaluated Fig 5f again and can confirm that there is indeed a slight enhancement. We thus removed the part of the sentence “however, here, Fig. 5f indicates that these were not enhanced.”

- Paragraph starting at line 435: This is an important “check”, but I feel like it is overdescribed. Can this PAMTRA section be condensed to 1-3 sentences?

Based on the reviewer’s suggestion, we shortened the paragraph starting in line 442 significantly.

- Line 467: How are we sure that one of these peaks is not locally-generated ice crystals?

We cannot exclude this, although we think it is not the most likely scenario. We included the statement that it is also a possibility that it could be the result of 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?

Yes, these points correspond to the same features that were discussed in Fig. 5. We referred now to the discussion of that figure.

- 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.”).

We thank the reviewer #1 for the recommendation and improved the sentence by replacing “must” by “might” to relax the statement.

- Line 587: What leads to this assumption? Is this based on some calculation? Or just a number pulled out of thin air?

We used peakTree to separate the small cloud droplets from larger cloud particles like drizzle, rain, or ice crystals. We then calculated the average vertical velocity of the small stratus cloud droplets (between 400-1200m, and 0:00-6:30UTC) because we assume they move with the vertical wind. The result was 0.059m/s. We then calculated for an adiabatic cloud how much liquid water content this would produce per time. The result was 4.2g/m^3/min (for calculation of the water vapour saturation pressure we needed the cloud bottom and cloud base temperature which we got from radiosonde profiles). Dividing the stable background LWP between 10 and 16 UTC of 304.5g/m^2 through the liquid water content that can be produced per minute, we get a regeneration rate of 73 minutes. We think the calculation is not very robust and worth to show in the manuscript, therefore we left it out. However, the regeneration rate of approximately an hour is not arbitrarily chosen

and we think that a quantification of the enhanced precipitation through the feeder cloud is a relevant topic for the manuscript. Therefore, we put it into the manuscript and would like to keep it as a motivating feature for future studies.

- 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 alltogether 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.

We thank the reviewer #1 for proposing to shorten the manuscript which is already quite long. We nevertheless think that the model evaluation part is quite a central part of the study. Having presented this at conferences, we also found that the model evaluation part triggered discussion and is of high interest for the community. We agree that some statements appeared too generalized. We thus modified the paragraph 6.2 to focus our discussion stronger on the specific case.

Similarly as above, we included statements that this conclusion is only valid for this specific case study (see line 648).

In the entire document, we wrote now that the over-/underestimation can be found for this specific case study. The statement is no longer generalized.

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"?

We removed the word "big" once.

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!

We went through the manuscript and deleted many "the"s if the context was not changed.

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

Okay, it is adjusted.

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

Okay, it is changed.

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

Yes, we removed “ice”.

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

Changed to “is visible”.

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

Okay, the sentence is reformulated to: The observed increase in DWR could even not be reproduced with PAMTRA when assuming 0.5g m^-3 (corresponding to 400g m^-2).

Line 600: “This even takes place…”

Okay, it is corrected.