

Response to Referee's Comments for Niebaum et al.: Constraining Rain Evaporation from Shallow-Clouds in the Trades using an Observation Based Superdroplet Model

We thank the two referees for their constructive feedback. The referees raise a few important issues that we are happy to address in our revision. The specific comments of the referees are addressed below, with our responses inserted in green font.

Furthermore, during the revisions we discovered a bug in the breakup calculation. Correcting this bug led to a much stronger impact of collisions and drop breakup on rain evaporation in our study. Before addressing the individual referee comments, we describe these differences and how they are represented in our revised submission in more detail here:

Difference after Bug Fix in breakup formulation

We had to update the model version because there was a bug in the calculation of the probability of breakup, which was significantly underestimated in our previous submission.

While the EvapOnly setup is not affected by the bug fix, the values are slightly different due to the randomness of the model initialisation (primarily the superdroplet initialisation). However, they are all within error and there is nothing qualitatively different in our results. The changes in the values are listed here:

	Previous Value	Updated Value
Number of clouds analysed	102	91
Surface precipitation / mm h^{-1}	Mean: 1.00 ± 1.89 Median: 0.28	1.07 ± 1.91 0.33
Column integrated evaporative cooling / W m^{-2}	Mean: 149.1 ± 171.4 Median: 72.4	152.0 ± 174.5 78.7
Evaporation fraction / %	Mean: 30.3 ± 20.1 Median: 25.6	28.5 ± 18.4 24.1

Likewise the EvapCoal run showed no qualitative change, and our conclusion that the difference between the mean and median values of the evaporation rate are within 5% is unchanged. Quantitatively the changes are very small, lying well within error, as recorded as follows:

	Previous Value	Updated Value
Column integrated evaporative cooling / W m^{-2}	Mean: 137.07 ± 150.76	139.2 ± 153.1

Evaporation fraction / %	Mean: 29.1 ± 19.3	27.5 ± 17.7
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The bug fix increased the probability that breakup occurs and thus significantly affected our conclusions from comparing the EvapCoalBuRe-few and EvapCoalBuRe-many setups to EvapOnly. There are therefore noticeable changes to Figure 10, Figure 11 (new adaptation of what was previously Figure A2), and Figure A1.

For EvapCoalBuRe-few, the profile of the evaporation rate (Figure 10) changed considerably, however the mean statistics for evaporation remained similar to the EvapOnly setup and unchanged within error. These are listed as follows:

Column integrated evaporative cooling / W m^{-2}	Mean: 148.2 ± 171.9	167.4 ± 205.7
Evaporation fraction / %	Mean: 29.7 ± 19.3	29.0 ± 17.9

For EvapCoalBuRe-many, both the profile of evaporation and its mean statistics changed significantly, as follows:

Column integrated evaporative cooling / W m^{-2}	Mean: 222.03 ± 359.00	490.08 ± 936.44
Evaporation fraction / %	Mean: 32.05 ± 18.01	40.4 ± 21.1

We have therefore revised our conclusions regarding the importance of breakup. Previously we concluded that collisions have negligible impact on rain evaporation except in cases of extreme breakup beyond rain rates of $\approx 3 \text{ mm h}^{-1}$. Now we find that cases of extreme breakup can significantly increase evaporation beyond rain rates of $\approx 0.2\text{-}0.3 \text{ mm h}^{-1}$. Additionally collision-coalescence and less extreme breakup can have a significant impact on evaporation when rain rates exceed $\approx 1 \text{ mm h}^{-1}$.

We have therefore also revisited our previous take-home message at the end of Section 3.2, which said *“These results suggest that, although droplet collisions negligibly impact evaporation beneath the shallow trade-wind cumuli we have simulated, its role becomes increasingly important as clouds deepen and their DSDs widen. We thus expect droplet collisions to play a greater role in controlling evaporation rates for more heavily precipitating clouds.”*.

Instead we now conclude *“Overall, these results suggest that droplet collisions and especially drop breakup can have a substantial impact on rain evaporation beneath the shallow trade-wind cumuli we have simulated. As breakup becomes increasingly important as clouds deepen and*

their DSDs widen, we expect droplet collisions to play an even greater role in controlling evaporation rates for deeper and more heavily precipitating clouds”.

We further adjusted the discussions of the effect of collisions and breakup on evaporation in the

- Abstract: “We find collisions and associated drop breakup processes to have a significant impact on rain evaporation for rain rates larger $\sim 0.2 - 0.3 \text{ mm h}^{-1}$. Depending on the breakup scenario, column-integrated evaporation increased by 10.1 % (breakup with few fragments) and 322 % (breakup with many fragments). Our results thus stress the importance of better understanding and modeling drop breakup processes, and more accurately measuring and modeling DSDs for rain evaporation in the winter trades.” and
- Conclusions:
 - Sec 4.1: *“We assessed the effect of droplet collisions on sub-cloud rain evaporation by comparing four different microphysical scenarios: excluding collisions (EvapOnly), including collision-coalescence (EvapCoal), and further including rebound and breakup with either very few, or very many fragments produced in the breakup event (EvapCoalBuRe-few and EvapCoalBuRe-many, respectively). While adding collision-coalescence decreased column-integrated evaporation by 8.5 % compared to EvapOnly, including also breakup increased column-integrated evaporation by 10.1 % (breakup with few fragments) and 322 % (breakup with many fragments). The collisions only become relevant for clouds with a cloud-base precipitation flux exceeding $0.2 - 0.3 \text{ mm h}^{-1}$, and suggest that collisions become even more relevant for deeper convection with higher rain rates.”*
 - Sec. 4.2: *“We found collisions and especially drop breakup to have a significant impact on rain evaporation for our set of shallow cumuli with rain rates $\sim 0.5 - 10 \text{ mm h}^{-1}$. The results further suggest that correctly representing drop breakup processes is even more important for robustly quantifying rain evaporation from more heavily precipitating deeper convection. Given that drop breakup represents the most uncertain liquid microphysical process to quantify (Morrison et al., 2020), its substantial impact on rain evaporation and the subsequent triggering of cold pools represents a critical knowledge gap. A clearer understanding of the fragment size distributions resulting from drop breakup would improve the representation of breakup processes in SDM and help constrain their effect on rain evaporation and cold pools.”*

Along with addressing the referee comments, we rewrote unclear or lengthy parts of the other sections of the manuscript, including abstract, introduction, methods and conclusions. The changes can easily be seen in the track-changes version of the manuscript.

Reviewer 1

1. Sum up: This study uses a newly developed microphysical scheme for warm rain processes, called Superdroplet Model (hereafter SDM), for estimating evaporation rates below precipitating shallow clouds in the trades. The SDM takes as an input the in-situ measurements of rainwater content taken from the instruments onboard the ATR aircraft during the EUREC4A observational campaign, which took place in the Barbados between January and February 2020. Taking account of several microphysical processes like evaporation, collisions, rebound and coalescence, the SDM computes a vertical profile of rain evaporation below cloud base, from which can be deduced a surface rain rate and an integrated evaporation fraction. The SDM takes its originality by assuming a Lagrangian particle to which the Kohler theory can be applied to treat evaporation/condensation processes. The dataset is composed of 102 precipitating shallow cumuli, from which the SDM parameterization provides vertical profiles of the evaporation rate. The multiplicity of clouds sampled allows for a rigorous sensitivity analysis of the SDM scheme to rain concentration at cloud base and relative humidity in the subcloud layer is analyzed, without taking account of more complex processes droplet collisions, coalescences and rebounds. The scheme seems particularly sensitive to these parameters: cloud base rainwater content mostly controls the integrated evaporative cooling, while relative humidity controls the vertical profile of evaporation. These results seem quite unaffected by the activation of droplet collisions, coalescences and rebounds in the scheme, except for very important cloud base rain rates (higher than 3 mm.hr⁻¹), where the inclusion of these processes tend to increase significantly the integrated evaporative cooling in the subcloud layer.

2. General comments: This study is definitively innovative and interesting, as it tackles an important but difficult task, which is to extract some observational constraints of the subcloud evaporation rate. This proxy is obviously key to better understand the cold pools dynamics, which have been proven to be ubiquitous in the trades thanks to the EUREC4A (2020) campaign and, more recently, to the ORCESTRA (2024) campaign. Beyond cold pools, evaporation rates are also an important control of the clouds and PBL energetics. The paper is also well written and comprehensive, and the ideas are, overall, well exposed. For these reasons, **this paper definitively deserves to be published.**

However, I am convinced that some significant improvements could render it even more convincing and would « ground » the overall message deeper. Therefore, I recommend **major revisions**, to allow enough time for the authors to implement the modifications I suggest, in case they agree with it.

I actually have three major concerns.

2.1 SDM scheme referencing: It is not obvious to me if the couple of papers Bayley et al, 2025 a,b are already accepted or not. I could access to the preprints but I never found any proof saying if these studies have been definitively accepted. Reading Bayley et al, 2025 b helped me a lot to understand the underlying assumptions and parameters incorporated into the SDM scheme. I maybe missed it, but in case these papers haven't yet been accepted, it seems a bit

ambiguous to refer to it so often without providing to the reader more specific informations about this scheme. As an example, the « multiplicity » is somewhere evoked at the beginning of the paper but never really explained. Hence, the reader must refer to Bayley et al, 2025 b to catch what this variable means. Occasionnally, it is of course justified to refer to another paper to avoid long and useless explanations. Nevertheless, in this specific case, since the SDM scheme is brand new, such a referring loses a bit of relevance. As this paper somehow assesses the relevance of the SDM scheme, one may expect the SDM scheme to be already « validated » itself through the peer review process.

In the absence of a published work to stick with, I think this paper has to provide enough explanations to avoid the reader to refer to Bayley et al, 2025a,b. By « enough explanations », I don't mean to copy paste the whole description of the SDM scheme in the paper, but to give a succinct explanation of the parameters taken from it, which are of importance to understand the present study (such as « multiplicity » for instance).

Since our initial submission, Bayley et al, 2025a,b have completed the peer review process and been published as GMD papers (Bayley et al, 2026a,b), which therefore allays the main concerns raised here. Nevertheless, we now include a more comprehensive description of what SDM is in our introduction. L50-L59 now states:

An alternative and arguably more realistic way to model rain evaporation is through the Superdroplet Method (SDM; Shima et al., 2009; Sölch and Kärcher, 2010; Andrejczuk et al., 2010; Riechermann et al., 2012; Hoffmann et al., 2015; Arabas et al., 2015; Naumann and Seifert, 2015; Brdar and Seifert, 2018; Bartman et al., 2022; Bayley et al., 2026a), which is a Lagrangian particle-in-cell method for cloud microphysics. In SDM, many real droplets with similar attributes (e.g. wet radius, dissolved aerosol mass, and spatial coordinates) are represented by a single simulated particle called a “superdroplet”. Each superdroplet evolves freely and individually according to the same equations for motion and condensation/evaporation as real droplets would. Additionally however, a superdroplet has a “multiplicity”, which indicates how many real droplets each superdroplet represents. In this way, all microphysical processes are modeled according to fundamental theory, except for collisions between droplets which are enacted probabilistically, such that the average outcome over many superdroplets collisions is the same as that of real droplets for a given collision kernel (Shima et al., 2009; Bayley et al., 2026b).

2.2 Figures: I think some figures are not easy to read and do not really support the argumentation in the main text. I specifically think about Fig 6 and Fig 11.

- Fig 6 would certainly deserve a rescaling such as to enhance the discrepancies in term of evaporation efficiency. Indeed, although almost all the points lie in between 10% and 60% (Fig 4), the log-scale gives the impression that most are very close to 100%. One suggestion would be to add more isolines of evaporation, with a high color contrast between each one of them fraction between 10% and 100%, or to shade with different colors every bin of 10%, and/or to to enlarge the plot such as to better highlight the variance of evaporation fractions.

We have added a second panel to Figure 6 which includes the re-scaling and additional isolines recommended by the reviewer.

- Fig 11 is even harder to interpret because here the idea is precisely to compare the different evaporation rates regarding the inclusion or not of collisions, mergings and rebunds of droplets. Why not plotting instead the evaporation fraction anomaly (in %) with regard to the « evap-only » setting, as a function of the cloud base precipitation flux ? Instead of the column integrated evaporation ? Fig 6 already shows the log-log linear relationship between these two variables. Here the goal is to show the sensitivity of the evaporation fraction to the microphysical settings of the SDM scheme.

We have replaced Figure 11 with an adaptation of what was previously Figure A2. The new Figure 11 now shows the relative cloud integrated evaporation and evaporation anomaly as a function of cloud base precipitation flux, as suggested.

I am also quite perturbed by the absence of axis in all figures. Whereas I understand the idea to lighten the figures by displaying ticks devoided of X-Y axis, I find it difficult for the eye to figure out the distance of the plots with regard to a X-Y reference. This absence of reference gives the curious impression that the plos are somehow « floating ». Ultimately, 2 this absence of axis makes it a bit hard to access to the « quantitative » values taken by the SDM outputs plotted. This is not a critical concern but I would suggest the authors to add X-Y axis, at least appearing very thin would be enough I guess.

We have added x-y axes lines to all our figures.

2.3 Section 3:

I think section 3 has to be revisited. This specifically concerns section 3.1, which is really hard to follow.

- The figure referencing in the main text doesn't follow the figure numbering. Of course, this is not a strict requirement something to stick to, but here we jump from Fig 5 to Fig 8, then back to Fig 6. Later, Fig 9 comes just after Fig 7b. This makes the whole subsection difficult to read.

We completely rewrote Section 3 such that it is more concise and more logically structured, with discussion of the Figures in the correct order.

- Fig 8 is not an histogram, as said in the caption. Also the authors might switch X-Y axis. It is quite unusual to see these kind of plots under this form, usually the Y axis refers to the magnitudes and X axis refers to the variable. I also disagree with the comment on this figure between L 217-225. The link of causality here advanced is abusive to me: it is impossible to conclude from Fig 8 that « RWC responds rather than controls the rate of evaporation ». This figure only shows correlations.

We have replaced Figure 8 with hex-bin plots of the evaporation rate as we agree this plot was unusual and we also found it difficult to interpret. We have also removed our comment about causality and now, based on our new hex-bin plots, we instead discuss the trends between the

evaporation rate and the three variables of interest, namely relative humidity, RWC, and mean mass radius. L247-L258 now states:

Finally, Figure 9 addresses the controls on the vertical gradient or profile of evaporation by averaging the evaporation rate across altitude and (a) relative humidity, (b) RWC and (c) rM . Relative humidity decreases linearly with height from 100 % to around 75 – 80 % for all the clouds in our sample (Figure 9a). The coloring shows that the evaporation rate increases systematically with the decreasing relative humidity and altitude. Meanwhile, changing RWC only has a second-order impact on the evaporation profile (Figure 9b). At a given height, i.e. relative humidity, the evaporation rate increases with increasing RWC. This is as expected since there is more available rain to evaporate with increasing RWC. In contrast, for each cloud RWC decreases towards the surface and yet the evaporation rate increases. This shows that the decrease in relative humidity with height more strongly impacts the evaporation rate than the decrease in RWC. rM typically increases towards the surface as smaller droplets evaporate completely, but the evaporation rate as a function of rM is highly variable, showing a peak in evaporation around mm-sized radii (Figure 9c). This unclear relationship may be a result of two competing effects of increasing rM on evaporation. On the one hand, DSDs with larger rM have larger RWC and therefore greater evaporation rates, on the other hand, for very large rM the scarcity of small droplets which can evaporate efficiently lowers evaporation rates.

- Fig 10 panels a,b are not commented (except I missed it ?) while a whole paragraph in Page 12 is devoted to a Figure in the Appendix. Why not them switching these 2 figures ?

Due to our updated results (see bug fix discussion), we think it is important to include Figure 10 in the main manuscript rather than in an Appendix. We now explicitly reference Figure 10 as a whole, as well as all three of its panels individually in their relevant discussion paragraphs. We have not moved Figure A1 into the main text since this would significantly extend the length of the manuscript and is merely supporting evidence to the numbers which we state in the main text rather than additional results.

Overall, section 3 deserves to be more structured and the link between the comments and the figures must be reinforced in order to strengthen the results presented. Since this section is the core of the study, I suggest the authors to rewrite it to facilitate the reading. The paper would greatly benefit from that.

We have thoroughly rewritten Section 3 to improve the structure of the section and the link between text and figures. We also removed unnecessary and lengthy parts, and added a short discussion of the reasons for the bottom-heaviness of most evaporation profiles, together with a new Figure showing the evolution of the DSD with altitude in the appendix (Fig. A2).

3. Specific comments:

3.1 Introduction:

L22 I suggest « ... driving unsaturated downdrafts which form regions of dense air masses which collapse and spread radially outward ... »

Changed to: ...driving unsaturated downdrafts which form regions of dense air masses that spread radially outward...

L23 « Dynamic processes » : which ones ? Lifting ? Vorticity ?

Clarified to: [These cold pools] suppress surface-forced convection in their interior and trigger new convection at their gust front, which [influences the mesoscale organization and amount of clouds.]

L27 What is the difference between « during a day » and « averaged over all days » ? Are you sure it's 73% and not 7.3% instead ?

To clarify the sentence, we changed it to: Overall, cold pools occur about 8% of the time in the winter trades (Vogel et al. 2021, Touzé-Peiffer et al. 2022).

L31 « ... it's magnitude have not yet been constrained. » : Even during the RICO Campaign ?

While RICO campaign observations and associated large-eddy simulations have significantly improved our understanding of warm rain from shallow clouds, we are not aware of a study that particularly focused on rain evaporation using RICO observations. The work of Zuidema et al. 2012 on cold pools is of course cited in our study.

L38-39 « Disentangling ... elusive » : Didn't Tridon et al, 2017 use field observation already ? If so, this sentence is then a bit confusing, and would certainly deserved to be more accurate.

What we meant with this sentence is not whether field observations were used or not, but that it is difficult to disentangle the effects of underlying processes (like drop collisions) from field observations. As it was unclear, we adjusted and shortened the paragraph as follows:

Estimating rain evaporation rates from field observations directly is a challenging task. Tridon et al. (2017) developed a method to obtain droplet size distributions (DSDs) from Doppler velocity spectra measured with Ka-band and W-band Doppler radars. They retrieved evaporative cooling rates from a continental deep convection system in the sub-cloud layer between 0.75 – 3 km above sea level, with a large range up to 60 K d⁻¹ (or equivalently roughly 422 W m⁻²). Their retrieved vertical profiles indicate most evaporation in the middle of the sub-cloud layer. However there are large uncertainties in the retrieval method of Tridon et al. (2017), and, because deep convection over land differs from shallow convection over subtropical oceans, direct analogies are limited.

L52 « ... Bayley et al, 2025a ... » : As I already said, I couldn't find any proof this paper was already accepted

As mentioned above, the Cleo model description papers have been accepted in the mean time and the reference updated to Bayley et al. 2026b throughout the paper.

L54 « The multiplicity ... » : this would deserve more explanations because it's an important parameter of the SDM scheme. This concept is explained in Bayley et al, 2025b

Adjusted, see comment 2.1 above for details.

3.2 Data and Methods:

L140-142 Do you initialize the multiplicity parameter with the observed DSD ? This is not clear to me

We initialize the multiplicity with the fitted DSD, not the observed one. We rephrased the sentence to make this more clear:

To initialize the model with the fitted DSD, [we randomly draw a superdroplet from each of 2048 uniformly-spaced logarithmic bins between 50 μm to 3 mm and assign its multiplicity according to the fitted DSD.]

L145 I would replace « gridbox » by « vertical layer »

Changed to: vertical layer (or grid box)

L163 Same thing here

Changed to: vertical layer

L163 The notation employed for the evaporation fraction is the same as the one used for the multiplicity in Bayley et al, 2025b. This is not a big issue, but I would suggest to change it.

We changed the notation to E_{frac} , thanks for pointing this out.

3.3 Results and Discussion:

Following your comment 2.3 and also the suggestion of the other reviewer, we strongly adjusted Section 3 during the revision. Mostly in terms of rewriting, but also the results changed slightly due to a bug in the break-up implementation.

L193-196 I suggest to switch the 2 first sentences in order to start the paragraph with « Figure 5 shows ... »

We now start the paragraph with “Figure 5 shows...”. The first sentence was removed (see next comment).

L193-196 « ... remarkably similar for almost all the clouds ... » : I think I disagree with this statement because Fig 5 shows there some remarkable differences among cases. The more rain, the more evap peaks at low levels, why not comment about that ?

We removed this statement in the revised version. We further discuss the bottom- vs. top-heaviness of the profiles in a bit more detail and added Fig. A2 in the appendix (showing the DSD evolution of some example clouds with bottom- vs. top-heavy evaporation profiles.

L200 « Accordingly ... » : why accordingly since these 2 quantities are independent from each other ?

This was wrong and “Accordingly” was removed in the revised manuscript.

L211-212 « Figure 5 shows that below all clouds, RWC decreases towards the surface due to evaporation » : Fig 5 only show this indirectly. I would moderate this statement here.

We removed this statement in the revised version.

L217-218 Hard to follow the sequence, from Fig 5 we jump to Fig 8

The discussion is organized more logically in the revised manuscript.

Fig 6 I suggest: 1. to add the Pearson coef in the legend. 2. To add iso % between 10% and 100% 3. To enlarge the upper right region

We followed your suggestion and updated Fig. 6 accordingly.

L217-218 Not clear to me. Where this result appears from Fig 8 ?

We removed both this statement and the former Fig. 8 (see also response to General comment 2.3 above.)

L220-225 Not convincing to me. As both quantities vary together, it seems abusive to state that « ... RWC responds to rather than controls the rate of evaporation ... ». A correlation just shows a co-evolution, but nothing else.

We removed this paragraph in the revised manuscript.

L226-237 All this refers to SI. Why not then putting these figures into the main text ? Again, this makes the whole section very hard to follow.

As the relationship with RH is very weak, we decided to leave this Figure panel in the appendix. As we strongly restructured Section 3.1, the discussion should be much more easy to follow now.

Fig 7 I suggest to remove from the caption the comments like « is predominantly ... RWC » and « is predominantly ... DSD »

We removed these comments.

L239 « ... for the same RWC... » Ok but Fig 7b doesn't rank clouds by RWC ! Then how to conclude that from Fig 7b ?

We removed this sentence in the revised manuscript.

L242 « Figure 9 ... » Again, we jump from Fig 7 to Fig 9, which makes the section hard to read. Changed in the revised manuscript.

Fig 8 I suggest: 1. To flip the X-Y axis 2. To remove « Histogram » in the caption, since it's not an histogram 3. « Along altitude »: this is very confusing. Is it a weighted average ? What is it ? Former Fig. 8 is removed and replaced by a new figure (see also response to General comment 2.3 above.)

Fig 9 I suggest to add the slopes $r^{-7/4}$ and $r^{-5/2}$ in the legend

We followed your suggestion and updated Fig. 9 (now Figure 8) accordingly.

Fig 10 Panels a,b are never commented in the main text, unless I missed it. This is quite strange knowing that figures in the SI are sometimes commented earlier in the text body.

We slightly adjusted the Figure and now refer to it in the main text.

Fig 11 I suggest to change the Y axis into a percentage difference with regard to the 'evap-only' setting in order to make it readable

Following your suggestion, we adjusted the Figure such that relative differences compared to EvapOnly are shown now. We further split the information into evaporation rate (top panels) and evaporation fraction (bottom panels), and added a zoom to highlight the most relevant information.

L290-297 This paragraph must be « believed » by the reader, which cannot see any convincing proof in Fig 11. It is impossible to see potential differences (if any) between the different settings for $R < 10^3$ W/m². The large differences at $R > 10^3$ W/m² are quite visible but only for the « evapcoalture-many » setting. The sensitivity to the other settings are here impossible to read.

We redesigned Figure 11 such that the information is now much more readily extractable. We also considerably rewrote the section such that it should be more understandable now.

3.4 Summary and Outlook:

L331 « ... the reduction ... is included »: this is not visible in Fig 11 // **L332-333** Same here

As mentioned above, we redesigned Figure 11 and the information should now be visible. We also rewrote this paragraph in the revised manuscript, such that it should now be clear.

Reviewer 2

Niebaum, Bayley, Poydenot, et al. present a convincing and well-written investigation of rain evaporation profiles simulated with SDM, based on trade cumuli observed during EUREC4A and following the simulation setup described in Sarkar 2023. The study is wellwritten, and they pose an interesting secondary research question (whether collisional processes impact rain evaporation) and do a good job discussing the albeit underwhelming finding. Beyond this collisional impacts finding, the current manuscript does not oPer substantially novel techniques or findings from the original Sarkar 2023 study, but I believe there are several easy opportunities to capitalize on use of the SDM in this work to build a more substantial study. With some clarification and additional probing of cloud microphysical responses under rain evaporation, I believe this study will be suitable for publication.

Major points & suggestions:

- **Suggestion #1: show and investigate DSDs or surface raindrop spectra.** You compare and contrast the findings of this study with those of Sarkar 2023, who used bin microphysics to investigate a different field campaign. However, no attempt has been made to investigate specific below-cloud DSDs, and whether the improved representation offered by SDM has a notable benefit in representing DSD shapes compared to prior results. It would be interesting to see a few instances of the DSD and the competing rate of change / balance between evaporation, breakup, and coalescence as it impacts various particle-size regimes. This would strengthen the findings of the study as well as utilize the novelty of the SDM. It would also help bolster the argument in L295-297 regarding deeper clouds and wider DSDs.

Following your suggestion, we added and discussed two additional figures showing the DSD evolution with height – Figures A2 and A4.

Figure A2 focuses on the EvapOnly setup and shows the DSD evolution of three clouds with bottom-heavy, top-heavy, little evaporation, extreme evaporation, respectively. We added the following paragraph discussing Fig. A2 in Section 3 (L208-L213):

Looking closer at the DSD evolution with height for selected cases of bottom- versus top-heavy profiles (Figure A2), we find that the top-heavy profiles are characterized by an abundance of droplets with radii between 0.1 - 1mm at cloud base and comparatively few larger droplets. These 0.1 - 1mm droplets readily evaporate but are not replenished at lower levels, which leads to a maximum in the evaporation rate, typically at a normalized height between 0.25 - 0.5, and subsequently a lower evaporation rate nearer the surface. Similarly, Sarkar et al. 2023 showed that rain evaporation becomes increasingly top-heavy the smaller the mean drop diameter becomes.

Figure A4 is similar to Figure A2, but shows the three clouds from the EvapCoal, EvapCoalBuRe-few and EvapCoalBuRe-many setup which have the largest difference in evaporation compared to EvapOnly. The figure is described as follows in Section 3.2 (L294-305):

Collisions are more likely in broader distributions with larger r_m and their effect is seen most clearly by looking closer at the DSD evolution with height for clouds which are most strongly affected by collisions (FigureA4). In the EvapCoal setup, coalescence leads to fewer small droplets (with radii less than $\sim 0.1\text{mm}$) than in the EvapOnly setup. This lack of small droplets leads to lower evaporation rates throughout the sub-cloud layer. In contrast, in the EvapCoalBuRe-few and EvapCoalBuRe-many setups, the largest droplets undergo breakup rather than coalescence. This more than compensates for the effect of any coalescence, leading to more small droplets than in the EvapOnly case and thus an increase in evaporation at all levels. In the EvapCoalBuRe-few case, the increase in evaporation is largest at the surface, because this is where the over-abundance of $\sim 0.1\text{-}0.2\text{mm}$ droplets for this setup compared to EvapOnly is the greatest, and these droplets readily evaporate. In the EvapCoalBuRe-many case, the increase in evaporation is generally greater than in EvapCoalBuRe-few because of the greater abundance of $\sim 0.1\text{mm}$ droplets. The increase in evaporation compared to EvapOnly is also top-heavy, with the greatest increase in mid-to-upper levels (normalized heights between $0.25\text{-}0.75$), because at these levels the over-abundance of $\sim 0.1\text{mm}$ droplets is greatest of all.

• **Suggestion #2: explore sensitivity of evaporation rate and profile to sedimentation parameterizations.** I enjoyed Figure 9 and the associated discussion of the importance of ventilation to the evaporation fraction, particularly because the original Shima 2009 study, and many related SDM implementations, do not include ventilation in their representation. The parameterization of sedimentation velocity for raindrops similarly has variation in the literature, and might be expected to impact the resulting evaporation profiles – I suggest that you take advantage of the SDM capabilities to perform this sensitivity study, and further comment on the role of sedimentation parameterization uncertainty in evaporation and cold pool formation.

Whilst in bulk-moment microphysics schemes the sedimentation parameterisation is typically highly uncertain and often used as a tuning parameter to affect evaporation, in SDM, sedimentation parameterisations are well-constrained by observations. The difference is because the uncertainty in the sedimentation parameterisation for bulk-moment schemes arises from the uncertain assumptions made for the analytical function for the raindroplet cloud distribution, whereas in SDM the sedimentation is a function of individual droplet sizes (and the droplet size distributions are emergent) and this function is well constrained. This is shown clearly in Figure 6 of Bayley et al., 2026b. Since all (reasonable) sedimentation parameterisations for SDM are essentially the same, we do not think this sensitivity study is necessary for our study.

• **Suggestion #3: remove some redundancy in the section 3 discussion.** Many of

the results described in section 3.1, such as the impacts of RWC, humidity, and r_m on evaporation, or the impacts of coalescence and breakup on the DSD and r_m , follow directly from theory and are therefore more of a sanity check than a novel finding. (The bottom-heavy evaporation profile, however, is interesting, and could be an opportunity to investigate DSD variation in altitude.) In order to enable inclusion of additional content suggestions, these discussions could be made more concise.

We completely rewrote Section 3 such that it is more concise and more logically structured, with unnecessary and lengthy parts cropped and discussion of the figures in the correct order. We further removed the previous Fig. 8 and replaced it with what is now Fig. 9.

To discuss the reasons for the bottom-heaviness of most evaporation profiles, and the impact of collisions on the DSDs, we added two figures showing the evolution of the DSD with altitude in the appendix (Fig. A2 and A4) and discuss them in Section 3 (see also our response to your first comment).

• **Concern: Repository and archive.** The repo/archived data are in github, not a FAIR repository. Consider archiving the specific code versions in zenodo. Furthermore, consider pointing to the specific setup used by this study within the CLEO code base (CLEO/examples/eurec4a1d) to be very specific. As it stands, the documentation in the sdm-eurec4a codebase is not sufficient to replicate the results of this study.

We updated the code availability statement such that the study can be more easily replicated:

The current version of Cleo is available from its GitHub page:

<https://github.com/yoctoyotta1024/CLEO> (last access: 11 Sept. 2026) under BSD 3-Clause license, alongside its documentation: <https://yoctoyotta1024.github.io/CLEO> (last access: 11 Sept. 2026); v0.65.2 is used in this study; its microphysics was described and tested in \cite{bayley_cleo_2026-1}. All the code used to produce and plot the results in this paper is archived in the dataset on Edmond under DOI:10.17617/3.HY0OQP \cite{this_paper_dataset}, along with comprehensive instructions how to use it.

Minor points:

• “Observation-based Superdroplet Model” is a misleading concept in the title. While the steady-state thermodynamic profiles and cloud-base DSDs are prescribed based on observation, the SDM itself is based purely on the representation of Shima et al. 2009 and uses empirical process rates explained in Bayley 2025, but does not derive any of its included process rates or representations from the EUREC4A campaign. Furthermore, these results do not strictly “Constrain” rain evaporation, since only a single field campaign with relatively shallow trade cumulus is considered. I recommend being more precise in the title, something like “Investigating Rain Evaporation...using Observation-Informed Simulations with Superdroplet Microphysics”

Thanks for the suggestion. We changed the title to “Investigating rain evaporation from shallow cumulus in the trades using an observationally-constrained superdroplet model”.

- L27: Please clarify, do cold pools occur 7.8% or 73% of the time? Or are these metrics taken over different spatiotemporal scales? The current statement is Confusing.

Simplified to: “Overall, cold pools occur about 8% of the time in the winter trades”.

The 73% was referring to the fact that on 73 % (i.e. on three out of four days), at least one cold pool is observed at BCO.

- Figure 1 / L123-125: Please be more specific about the relative weighting of humidity profiles in order to make this study replicable. Additionally consider coloring the thin lines in Figure 1b according to their weights.

We now clarify how the weighting is done explicitly:

For the relative humidity profile, we also fit to the dropsonde measurements between 200-500m but give dropsondes with moister profiles between 100-700m more weight (with the weight somewhat arguably given as $(\text{mean}(\text{RHcloud}) - \text{min}(\text{RHcloud})) / \text{max}(\text{RHcloud}) + 0.3$)/1.3).

- Section 2: Please organize and clarify this section to more clearly explain the simulation setup at the start. As it stands, cloud-base DSDs and fitting procedures are introduced first (section 2.1) before the reader has a chance to understand what the actual parameters and setup of the 1D case are. It wasn't until deeper into section 2.2 that I understood that the 1D setup simulates below-cloud base rain droplets only, as I kept waiting for a discussion of the in-cloud humidity and aerosol concentrations before understanding. A schematic could be one helpful option to illustrate the test case.

To better prepare the readers for the Data and Methods section, we added a short paragraph at the beginning of the Section, before Subsection 2.1. Observational data. It reads as follows:

To constrain the amount and controls of rain evaporation in the sub-cloud layer of shallow cumuli, we combine observations from the EUREC4A field campaign with a 1D rainshaft model using the SDM Cleo. We first explain how we get the cloud-base DSD and sub-cloud layer thermodynamics of our sample of 102 raining clouds during EUREC4A; then describe how Cleo is setup to simulate below-cloud base rain in a 1-D rainshaft mode; and lastly show some validation of the setup.

We further added subheadings **DSDs** and **Thermodynamic profiles** to structure Sec 2.1 more.

- Section 2 / Table 1 / fv study: Please describe in section 2 how the no-ventilation study is performed and include it in Table 1.

Thanks for pointing this out. We added the EvapOnly-novent setup to Table 1 and describe it in the following paragraph (after the description of the four other setups):

To understand the influence of including ventilation effects on rain evaporation, we run an additional simulation with the EvapOnly setup that neglects ventilation effects (called EvapOnly-novent in Table 1). The EvapOnly-novent setup uses a fixed ventilation factor $f_v = 1$, in contrast to the other setups that parameterize f_v according to equation (3) of Bayley et al. (2026b).

- L114 “fitted cloud RWC” – is the RWC included as a quantity to establish cloud base DSDs? Or is it only use ex post facto to eliminate outlier simulations? If the former please clarify; if the latter, please don’t refer to it as “fitted” (appears elsewhere too).

*The latter is correct, we compute the RWC of the fitted DSD and check if it’s inline with the RWC of the observed DSD. We clarified this in L121 (“*The RWC integrated over the fitted DSD also aligns closely with the observed temporal mean RWC for each cloud, [...]*”) and in other places.*

- L183: could you add the MS Merian RR to the distribution plotted in Figure 3?
Checking the Acquistapace et al. (2022) reference carefully again, we found that the numbers we mention (taken from their Table 3) actually refer to daily-mean values and are thus not comparable to our cloud-averages. Acquistapace et al. (2025, QJRMS) present cloud-averages for shallow and congestus mode clouds from the same MS Merian dataset, but there were some issues with these values that we couldn’t clarify (median rain rates higher than mean rain rates, shallow cumulus rain rates larger than congestus rain rates), such that we decided not to refer to this 2025 paper.

We therefore removed the reference to the Merian papers in the revised version, and only compare our rain rates with the cold-pool climatology at the Barbados Cloud Observatory from Vogel et al. (2021).

- Please add a legend to Figures 1 and 10.
Done.

- Consider creating an idealized deeper cloud case (higher cloud-base height, larger cloud-base RWC) to validate the argument in L295-297 regarding the importance of collisions in deeper precipitating clouds.

After the bug-fix in the breakup implementation, we now find collisions and especially the scenario with strong breakup to substantially affect rain evaporation. We therefore decided not to create an idealized deeper cloud case to investigate the issue in more detail.