Response to referee comments on "Sub-cloud rain evaporation in the North Atlantic winter trades derived by pairing isotopic data with a bin-resolved microphysical model" by M. Sarkar, A. Bailey, P. Blossey, S. d. Szoeke, D. Noone, E. Q. Melendez, M. Leandro and P. Chuang.

We thank both the referees for their recommendations and suggestions on the manuscript. These feedbacks have helped us refine our results and made the overall presentation better. The referee comments are shown in blue and author answers in black.

Reply to Referee #1 comments:

This study attempts to understand the sub-cloud rain evaporation via the simulated rain evaporation flux and water vapor isotope ratios using a one-dimensional model. I think overall the authors well address the comments brought by the reviewers in the first-round review. I recommend it for publication with some minor revisions, which are listed as below.

Title: The current generally looks good, but given the primary method and the significant portion of the study is related to a one-dimension model simulations, I feel that the title should be improved if the model was incorporated, which was also mentioned by anther reviewer in the first-round review.
We have changed the title to include the model and observation use:
"Sub-cloud rain evaporation in the north Atlantic winter trades derived by pairing isotopic data with a bin-resolved microphysical model".

Line 4: Since here the authors mentioned that the change in temperature was also simulated, I expected there would be some discussions regarding temperature-related results later but actually not.
The mean ambient temperature at cloud base remains fairly consistent between 292-293 K over all the cases studied here. Following this, we did not find any interesting contrasts in the rain evaporation characteristics due to temperature differences.

Line 11: 'between cloud base and the surface, as compared to a 'bottom-heavy' profile' -> 'between cloud base and the surface than a 'bottom-heavy' profile'.
Done.

Line 13-14: It's better to move model performance evaluation to earlier place in the abstract, e.g., before reporting essential results. How about the model performance of other parameters?
As you suggested, we have moved the model evaluation earlier in the abstract now.

Lines 148-166: it is not necessary to separate each parameter in each line, which can save space of the body text.
We tried writing the parameters within a paragraph, but that was hard to read. In this format, we feel the clarity was more.

Lines 355-356: From Fig. A2, it seems that the REF dramatically decreases with N0, and the decreasing slope is not small compared to the case in Dg-REF or sigma-REF relations. Be cautious to draw a conclusion as "The influence of N0 on REF is smaller compared to Dg and σ (Figure A2d). This is because when REF is expanded in terms of N0, Dg and σ, N0 appears in the numerator and denominator and almost cancels out".
To emphasize on the stronger relationship between Dg and sigma with REF compared to N0, the correlation coefficients (r) has been added to the plot. The r for N0-REF is -0.3 compared to -0.7 and -0.8 for Dg and sigma, respectively.

Line 360: add "and" before "N0 appears in the ....".
We have added 'the' before 'N0 appears in the …' which seemed more appropriate.

Line 370: "figure 9"-> "Figure 9".
Done.

Line 432: "equation 11"->"Equation 11".
Done.

Line 484: Replace "But" with "In contrast".
Done.

Code availability: Just specify where the code could be accessed. I agree with another reviewer in the first-round review that "it could be useful for providing Monte-Carlo estimates of surface rain isotopic composition in future studies."
The code is submitted to the ACP and should be available to readers upon the manuscript publication.

Fig. 4. The color bar label is missing.
Added.

Fig. 5: Panel number annotation is missing. Also delete "Only the average Dg and σ over five CSET cases were available and are shown by single dots in b-c)". Also add a legend to denote what the colors of circles stand for.

Panel numbers are added along with some statistical parameters. A legend is also added. We have retained the statement “Only the average Dg….”, since this will clarify why only one data point for Dg and sigma is provided as opposed to five data points for No and RH_surface.

Fig. 6: Some tick names overlaid each other.
The figure has been revised now.

Fig. 8: Add a legend to denote what the colors of circles stand for in the panel. In the caption, delete “The dashed black line is the one-to-one ratio line for reference”. Also, it is conventional to put the slope information (0.63) in the panel using a linear fitting equation, instead of the caption.
Legend is added. The line “The dashed …” is removed. The slope information is revised by adding the fit equation.

Reply to Referee #2 comments:

General comments:

I've reviewed the authors' revisions and response to both reviewers' comments. I find the revised manuscript much improved and nearly ready for publication.

Regarding value of the work to the field, I agree that the evaluation of the subcloud BL and microphysics is important for our understanding cloud-topped boundary layers. What remains in my view, is whether this study shows that rain isotope observations constrain the rain evaporation estimates independent of other estimation methods. If the authors have quantitative support of that question, adding it would increase the impact on the field. It is a very nice contribution to make this model available to the community.

The rain evaporation estimation is calculated directly from the isotope-initialized microphysics-resolved model. The proportionality between the difference of d-excess between the cloud base and surface and the rain evaporated fraction supports that the isotope observations could be a reliable method to study rain evaporation. We have now added correlation coefficients in the necessary plots to emphasize on this.

This RSD-isotope combination method works independent to other methods such as radar-based rain evaporation. A future study could be formulated to compare the rain evaporation estimated from the two methods. We have now added a paragraph in the conclusion describing this:

"In general, our isotope-initialized microphysics-resolved model performs reliably well in characterizing the sub-cloud rain evaporation in the shallow rain regime sampled during the ATOMIC/EUREC^{4}A campaign. This model also only requires in-situ microphysical and rain isotope observations and is independent of any remotely-sensed rain observations. However, a comparison between rain evaporation evaluated from remote-sensing platforms (e.g., mm-wavelength radars) and our in-situ-based model could be useful for error analysis."

The new model results improve agreement with the observations on the Ron Brown ship. I agree that d-excess is most sensitive to evaporation and should be the metric of analysis. The previous presentation made it hard to see the full picture of the isotopic evolution though the BL. The isotopic presentation is improved. Figure 10: It is my understanding that the authors consider the modeled rain at 85% RHsrf (magenta line) is in good agreement with the observations from the Brown (red line). I don't see any of the model results in Fig 10 that get above 10 permil, but Fig 11 model results extend to 11.5 permil. Perhaps it is my ability to read the figure line colors, but if there is an inconsistency, please fix.

We realize that the extent of the P3 domain in the contour plot (now removed) not matching the P3 modeled values seemed confusing. The P3 domain box in the contour was not intended to represent the d_p. It was only included to provide the Dg and sigma ranges during the P3 and compare it with the CSET ranges. Since the 22 P3 cases have different RH_surface (66-87%), and the contours in figure 11 were run at constant RH of 70% and 85%, the d_p over the P3 box did not match the P3 modeled values.

We have now removed the contour and shown the effects of RH, Dg and sigma on d-excess in terms of histogram lines in the new Figure 11b. We have run two run at a) RH_surface=85% and Dg=0.5 mm, sigma (1-2.5) from P3 cases (yellow line) and, b) RH_surface=85%, Dg=0.5 mm, sigma=3 (purple line). The d_p for these two runs is higher than the default P3 cases where Dg, sigma and RH were generally lower. The higher modeled d_p ranges for the two new model runs are also closer to the Brown observations. Along with evaluating the model with the Brown observations, these experiments also show that both thermodynamics and microphysics contribute to the higher d_p.
Since this plot is crucial to show our model evaluation with the observations, we hope that the interpretation is more comprehensible now.

The linear relation between REF and dp features prominently in the key findings of this paper, yet the figure is in Supplemental with no statistical metrics given. Please give statistical analysis and uncertainty at minimum. There are some values that could use some discussion in the figure caption or the main text if warranted. Values of REF=1.0 indicate the rain evaporates completely therefore d_p,sf is NA. Likewise rain isotope observations where d-excess increases instead of decreases are curious given the conventional assumption that d-excess decreases with evaporation influence.

The aforementioned figure is now added to the main manuscript (figure 8) with a linear fitting equation, correlation coefficient and the p-value.

The cases with very high REF tend to but are always smaller than 1. The one remaining case (1 out of the 22) on 4th February where rain completely evaporated (REF=1) is not included in the plot since its d_sf is NA (just as you explain). A note has been made about this in the caption.

The cases with 1-d_sf/d_cb greater than 1 are due to d_sf being negative (see figure 7i for reference). This makes 1-d_sf/d_cb greater than 1. This note is also added now in the caption.

Specific comments:

Page 7, line 205: Can you be more specific about what an appropriate integration time would be?

The time series of the surface rain measurements at the Brown station last around 10 minutes. There are longer showers as well but the signal is very patchy. This gives us confidence to use an approximate length scale of 15 minutes to compute the accumulated rain evaporated vapor in section 3.5.

Section 3.2: When discussing correlations in Figure, provide quantitative correlation and statistical significance. The correlation described in Fig 5d may not be significant.
Both the correlation coefficient and p-value are now added to describe the plots. The correlation between RH_surface and rain rate are indeed weaker than the other three microphysical variables.

Page 12, line 362: “Most of the P3 cases with higher Dg and s also have higher N0.” Is this expected or unusual? It seems important for the generality of the findings.

A general relationship between Dg, sigma and N0 is unclear especially considering the variability in rain rates and the microphysical parameters. Geoffroy et al. (2014) showed that rain at a mature stage is characterized by broader RSDs centered at larger drop sizes, and with slightly decreased but still high N0. This is closer to our finding. They further find that near the surface sigma gets narrower, and N0 is high if rain water content is high, and vice-versa. However, they note that the study is limited by a low number of raindrops and instrumental biases.
In another previous study, Feingold and Levin (1986) found that for low rain rates (<5 mm/hr), Dg, sigma and N0 vary considerably. But as rain rates get higher, sigma is remarkably constant while N0 and Dg are increasing. Despite these observed trends, other variations in the Dg, sigma, N0 correlation were also observed. Overall, the correlation of Dg, sigma and N0 seems to be dependent on the life stage of the rain, rain water content and the microphysical processes involved.

Page 14, line 402: “These ranges correspond well to the value show across other platforms…” What other values? The 3 ship platforms are the only rain measurements mentioned so far.
Other than the 3 ship platforms mentioned in the manuscript, the precipitation samples were also collected by the German research vessel *Merian* and French research vessel *Atlante*. Their ranges are provided in Bailey et al. 2023 Figure 11.

Page 14, line 406: “…with a high RHsfc of 86% is around 10 permil that matches the brown…” awkward wording.
The paragraphs in the section have been revised to make the interpretation more straightforward.

Conclusions:
Conceptual question: To what extent does the thermodynamic state of the BL control rain evaporation versus rain evaporation control the thermodynamic state of the BL?

The BL thermodynamic state and the rain evaporation are interdependent. The thermodynamic state at which a rain shower occurs determines the amount and distribution of the rain evaporation. Conversely, depending on the distribution, intensity and duration of the rain evaporation, the thermodynamic properties around the rain shaft will also change.

Page 16, line 480: The linear relation between REF and dp features prominently in the key findings of this paper, yet the figure is in Supplemental with no statistical metrics given. Please give statistical analysis and uncertainty at minimum.
Done.

Fig 1: Does 'rain flux' is the same a 'rain rate' right? It's not clear to me what the profile in the BL represents. It seems labeled as the instantaneous rain rate, but that would have to highest at cloud base in both small droplet and large droplet cases right? Does blue indicate larger rain rates and red smaller? Does the x-axis also indicate the rain rate? If so, the partial evap case should end at different relative location than the complete evaporation rain rate, which would end at zero. Do the rain evaporation arrows indicate moisture recycling within those altitudes? Maybe the profile is the rain evaporation rate? It might be helpful to label this figure with the top/bottom heavy classification used later in the manuscript.

Thanks for pointing these out. The schematic has been retouched to address these issues. A bulk rain evaporation flux of 28 W/m2 that is produced from the evaporation of 1 mm/day of rain is shown in the schematic. The confusion was due to the '=' sign and has now been replaced by '→' sign. The x-axis of the vertical lines is depictive of rain evaporation fluxes and are now modified for the complete and partially evaporating cases. The shades were removed as it was unnecessary. The top- and bottom- heaviness of the profiles are now labeled. The lines are not intended to be scaled. Isotope information has also now been added.

Fig 2: Label color bar as altitude (m).
Done.

Fig 4: Give units of color bar. Are they really plotted as contour lines? Or rather shaded?
Done. The contours are shaded and the shade is represented by the color bar.

Fig 5: Give statistical metrics of relationships/no relationships.
Done.

Fig A1: Give correlation statistics.
Done.

Fig 7: The cases where dDp decreases with altitude are curious. I think d is used throughout the manuscript instead of dxs. Please be consistent with the label in this figure.
The decreasing dDp with altitudes are for cases with a combination of high RH and relatively more enriched rain at cloud base. Additionally, this issue only happens for smaller drops in the RSD. We suspect that the higher RH subjected to these raindrops leads to stronger equilibration

signals as compared to evaporation signals. Since the rain in these cases are also more enriched, it could be losing the heavier isotopologues due to the exchange process and getting depleted.

Fig 9: Breakpoints suggest this figure may have been generated using model with 50 m altitude steps (previous version).

The breakpoints in figure 9 are due to the complete evaporation of smaller drops present in the RSD at a vertical level, producing the faint spikes in vertical Fe profiles. Similar breakpoints are also shown in figure 7f.

Figs 10-11: See previous comment.

Addressed.