Response to referee comments on “Sub-cloud rain evaporation from shallow convection in the north Atlantic winter trades” 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 comments on the manuscript. We believe these feedbacks have made our manuscript more comprehensive and informative. The referee comments are shown in blue and author answers in black.

Reply to Referee #2 comments:

Major changes have been incorporated throughout the manuscript since the last submission in terms of paragraph organization and story-telling. The introduction and conclusions are rewritten to make the paragraphs tighter and clearer. The broader implications of this work are also discussed in the introduction. The comparison with previous campaigns has been removed from the introduction and discussed more elaborately in the results and conclusions sections. Some figures have been removed, others improved, to better represent the theme of our work.

In addition to some unit corrections in the code, the vertical resolution is also improved. Instead of a fixed 50 m vertical resolution, a nominal step size of 1 m is used. In addition, an adaptive step size is employed to make the model stable and adaptive for very small droplets. The MATLAB code function is made available to the ACP for publication. The improvements have led to an increase in the overall rate of evaporation. This has increased the total column rain evaporation fluxes and the surface d-excess of rain.

This paper uses observations from the 2020 EUREC^{4}A field campaign in combination with a one-dimensional model that simulates the change in drop size and isotope composition of the drops for given initial conditions that are constrained by aircraft observations (raindrop size distribution, isotope composition of the drops). I very much enjoyed reading this nicely written paper. Below cloud evaporation is a strongly under-researched topic. Since it is one of the two components of precipitation efficiency (together with conversion efficiency in clouds), the lack of constraints on below cloud evaporation from observations has important consequences for our ability to correctly predict precipitation at the weather event as well as the climate timescale.

I have four major comments, listed below, as well as a short list of minor/technical comments below:

Major comments:

A) It would be very valuable to point out the more general implications of this work already in the introduction (as well as at the end of the conclusion) e.g. trying to constrain precipitation efficiency with isotope observations would be one aspect to add to the now rather narrowly focused introduction. Of course, there are other aspects such as the impact of moisture recycling on mesoscale organization, which might be what the authors are more interested in. Right now the introduction reads like a nice summary of reference values of subcloud layer rain evaporation F_e in terms of energy input into the subcloud layer, but these values would be much more interesting to compare with the author's results later on in the discussion.

The introduction has now been revised to discuss more general implications of this work which includes the rain evaporation flux contribution to BL stability and large-scale circulations, on the link of rain evaporation efficiency to cloud albedo and surface rain estimates, and the overall understanding the rain lifecycle. The Fe values from previous campaigns have now been removed from the introduction. They appear later in the results and conclusions sections.

B) Given the motivation of the authors to investigate below cloud evaporation, because it represents a substantial energy and humidity input into the subcloud layer, I think that the results from Section 3.6 are very disappointing. Can this aspect be discussed in more details? It seems unlikely that such an important process would leave no distinguishable isotope signal in ambient vapour. Do these results imply that, even though below cloud evaporation strongly impacts the amount of precipitation that reaches the surface, for the subcloud layer moisture budget, it is a negligible process? Or is it only important, when integrated over larger spatio-temporal scales than a single precipitating cell?

Our results indicate that shallow rain evaporation with cloud base rain rates as low as the average P3 cases with ~1 mm/day could produce substantial evaporation fluxes and cooling rates for the sub-cloud layer. This could have BL stability implications depending on the vertical flux structure. This evaporation signature is easily measurable from the rain isotope ratios at the surface.

However, whether or not, these evaporation signals would be detectable by the vapor isotope analyzers depends on two conditions. Either the evaporated flux Fe needs to be high, or the evaporated vapor needs to accumulate in the sub-cloud layer for a sufficient time without advecting or diluting into the surrounding air. Sufficiency of either of the two cases, would ensure the detectability of the change in the vapor isotope ratios.

For example, considering the highest raining case during the P3 which has a maximum of 2 Wm-3 of Fe, if the evaporated vapor accumulates over 10 minutes, then the change in absolute humidity in the sub-cloud layer would be 0.5 g/m3. For an observed dDv of -71 permil and dDe of 5 permil, this will yield 2.4 permil of isotope change in dDv which is well-perceivable with the airborne isotope analyzers.

If Fe were smaller, then the accumulation time needed by the vapor to make a measurable change in dDv should be longer. It is also possible that in rain cells with more strong precipitation rates than the P3 cases, Fe would be higher. This might be the case for the Brown observations or the ATR observations made aboard the French ATR-42 (ATR) operated by SAFIRE. Both these platforms had rain rates more intense than during the P3. For such more intense rain cases, even over short time intervals, the Fe could be large enough to be detectable by the vapor isotope analyzers. However, ascertaining this is beyond the scope of our study.

How much does the authors' finding depend on the uncertainties of the aircraft and ship-based observations?

The uncertainty in the vapor dDv and d18Ov measurements from the aircraft is quite small (2 permil and 0.8 permil, respectively) for altitudes with higher water vapor concentration such as in the sub-cloud layer that we are interested in (figure 8 Bailey et al. 2023). The rain isotope ratio observed from the Brown have even smaller uncertainties of 0.8 permil and 0.2 permil for dDp and d18Op, respectively (table 1 in Bailey et al. 2023). These estimates are now also mentioned in the data sections of the manuscript (L104-118).

Also, can this aspect really be assessed with the model at hand given the assumption that vapour contributed by rain evaporation is neglected (L. 125).

The assumption of negligible contribution of vapor by rain evaporation in the model was confusing, and it has now been removed. Because the rain evaporation model is in steady-state, it is implied that the effect of vapor from evaporation on any future rain is ignored. The ambient vapor is obtained from the aircraft measurement close to the surface. This vapor is assumed to include the vapor from rain evaporation that has taken place already. Therefore, any rain evaporated vapor computed in the model is not further added to the background vapor.

C) There is very limited literature about below cloud evaporation effects, I agree, but I think there are a few studies from different settings with which the results in this paper can be compared to. For example, Aemisegger et al. 2015 GRL used a combination of numerical experiments and isotope observations to assess the importance of below cloud evaporation for a cold front passage. They found that over the whole frontal precipitation event neglecting below cloud evaporation leads to depletion biases of 20–40‰ in d2Hp and 5–10‰ in d18Op as well as to an increase of 74% in rainfall amount. This impact on total rainfall amount is very close to what the authors find in their study over the tropical North Atlantic. Also, in this paper, a substantial impact of below cloud evaporation on ambient vapour was found. How comes that in the winter trades the impact is so small? (Different region, different dynamics).

The major difference between the Aemisegger et al. 2015 case and the P3 case studies is the intensity of rain rates. The surface rain rates in the Aemisegger et al. study is 1-7.5 mm/hr which is substantially larger compared to the P3 cases. Higher rain rates could be due to higher Dg and sigma at cloud base that eventually reached the surface, and hence to higher Fe. Higher Fe cases can produce higher concentration of evaporated vapor over a given time, which could be detectable in the measured vapor isotope ratios as shown in the Aemisegger et al. study.

We suspect that because the rain rates during the P3 were sufficiently lower compared to the Aemisegger et al. study, the P3 Fe might also be smaller. Therefore, for the P3 cases the evaporated signal would be detectable if integrated over a longer time. Perhaps, the Aemisegger et al. study would be better compared with the ATR cases where rain rates are significantly higher than the P3.

D) The discussion of the impact of F_e on stability is interesting but confusing. The statements at L. 274ff and in the conclusions (L. 432) are contradicting. Please clarify. I don’t understand, based on which of their findings they draw these conclusions on stability. If F_e has limited to no impact on subcloud layer temperature and specific humidity (see also major comment B) then how can stability be impacted?

The impact of Fe on stability has now been elaborated in the revised manuscript. Using the model results we define the top- or bottom- heaviness of the Fe profiles, and then see how they relate to the microphysical parameters. This is shown in figure 8b-d and discussed in section 3.3.3. The model results show that the top-heavier profiles are linked with smaller Dg and sigma (but not N0), and vice-versa. We also find that a low RH_{sf} is also linked to more top-heavy profiles.

The possibility of the Fe vertical structure influencing the BL stability is based on previous studies done by like Srivastava 1985, Paluch and Lenschow 1991 and others, where rain evaporation closer to cloud base or surface has been linked with BL stability.

Additionally, it is important to note here that even if the evaporated vapor concentration is small compared to the ambient vapor concentration, it could still be energetically significant. This is because even for small vapor perturbations, FeT is still 10-350 Wm-2, with either top- or bottom- heavy vertical structure. Even if the vapor does not accumulate over a long period of time, the energy produced in that time instant could still potentially influence the local vertical circulation or contribute its moisture and energy to other rain systems in its vicinity. However, LES studies would be necessary to ascertain these effects.

Technical comments:

1) I think ACP titles are usually not capitalized and I would strongly encourage the authors to mention more specifically the type of precipitation events they are looking at: “Sub-cloud rain evaporation from shallow convection in the North Atlantic winter trades”

The title is now changed to “Sub-cloud rain evaporation from shallow convection in the North Atlantic winter trades”.

2) The variables should be in italics except for abbreviations such as RH.

The necessary variables are now italicized.

3) L. 5 not sure I immediately understand, in which phase dD and d18O were measured and used in the model

The phase is now mentioned. (L5)

4) L. 9: 65% of what? mass, volume, event duration, number of events?

65% of mass. This is clarified now. (L7)

5) L. 17: is precipitation in shallow convection regimes really “ubiquitous”? I would have said it is rather sparse with low precipitation efficiencies compared to other cloud systems?

Thanks for pointing that out. Ubiquitous has been substituted by sporadic. (L18)

6) L. 48: “facilitate or hinder boundary layer stability” sounds a bit strange, how about “Does Fe reinforce or weaken the subcloud layer stability?”

The line is now rewritten as “Could Fe reinforce or weaken….”. (L46)

7) L. 58-60: “This is because as rain evaporates…”. I am not sure I can follow the implication that is formulated in this sentence. Vapour isotopes can be used independently to assess rain evaporation because rain evaporation leads to an enrichment of rain? This also joins my major point B above.

The sentences have been rewritten as: “In-situ measurements also provide stable isotope ratios of hydrogen and oxygen in water vapor, which can be used to independently assess rain evaporation. This is because as rain evaporates into the unsaturated sub-cloud layer, the isotopically light water transitions to the vapor phase more efficiently, causing the drops to become increasingly heavy (Salamalikis et al., 2016; Graf et al., 2019)”. (L56-59)

8) L. 114: degree W formatting

Done.

9) L. 124: what is the implication of ignoring collision-coalescence for your results?

We assume that any collision-coalescence process between the altitude of aircraft leg and the cloud base is negligible. This essentially ignores any change in RSD from the altitude of sampling and cloud base. The RSD at the sampling altitude is then assumed to represent the RSD at cloud base. This assumption is drawn from the proximity of the microphysical parameters for the similar rain rates but measured at different altitudes (figure 5). The assumption also ties with a stratocumulus study in Wood (2005a) where rain rates remain constant in the lower 60% of the cloud.

10) L. 125: This last sentence leads to confusion about how you can assess the impact of below cloud rain-vapour interaction with the chosen 1D modelling approach.

The line was misleading and is now removed.

11) L. 134: could the variables be described one after the author, instead of the long list of variables and then a long list of descriptions? (would be easier for a reader like me to grasp).

Done.

12) L. 150: "." Formatting, should go to L. 149.

Done.

13) L. 161: all parameters from Graf 2017: this is a bit vague. Which exactly and is this a good choice given the large contrasts between a cold front in the midlatitudes and shallow convection in the tropics? Maybe a summarizing table in the Appendix would help.

In the modified code, the parameters are re-checked and modified wherever necessary. The details and description of all the parameters are given in section 2.2. The code is also now made available to ACP.

14) P. 5: what is the imp act of RWC estimates from P3 observations on the modelling? How do they compare to the ATR observations from which large statistics are available at the cloud base level. See Bony et al. 2022 ESSD.

The ATR estimates in figure 10 in Bony et al. 2022 is based on cloud drops (5-80 microns) and the median LWC is 0.05 g/m3 at cloud base. The P3 estimates in our work is for raindrops with diameters of 0.125-6 mm. Therefore, the comparison might not be appropriate. However, a quick check with ATR remote sensing files shows that ATR RWC from the BASTALIAS RWC product has 0.8 g/m3 maxima. This is higher than the highest RWC of 0.1 g/m3 from the P3 cases. The higher ATR RWC could have different influence on the rain evaporation than the P3 cases. But this is beyond the scope of this study.

15) L. 184: this seems to contradict the statement at L. 125. Wouldn't an integration over longer time intervals (precipitation events of 10-30 min) be necessary to assess this aspect?

We agree. In the revised manuscript, we have considered the time interval over which the change in delta_v could be measurable by the isotope analyzers.

16) L. 274ff: is this speculative, or based on some specific results? And what dominates for changing stability: the evaporative cooling or the moistening effect (which are counteracting each other)?

This paragraph has been clarified under its own subsection (section 3.3.3). We have used our model to quantify the relationship of the microphysical parameters on the vertical structure of Fe. Then we have used the results of previous studies like Paluch and Lenschow (1991), Srivastava (1985), Sandu et al. (2011) to discuss the implication of Fe profile on BL stability.

17) L. 285-294: I like this finding a lot. Very clearly explained!

Thanks. We have now replaced the figure with a scatter plot to make our estimates more quantitative. Figure 8b-d, section 3.3.3.

18) L. 357: Also this finding is very interesting! Maybe: "Consequently, the amount effect may not be appropriate for describing the impact of rain evaporation on the isotope composition of rain"? Did the authors' consider using the Graf et al. 2019 ACP deltadeltaD vs. delta-dexcess phase space for assessing the impact of different N0, cloud base RWC, RHsf etc.?

We have not used the deltaD vs delta-dexcess space in Graf et al. 2019 for this study, but this could be definitely useful in a more detailed study to investigate the impact of microphysical and thermodynamic parameters on the isotopic composition of rain.

19) L. 415: this suggests as a consequence that the impact of sub-cloud evaporation on stability is negligible too (see again my major point B).

This paragraph has been rewritten considering that the rain evaporated vapor concentration accumulated over time could have measurable impact on the observed isotope measurements.

20) L. 418: North Atlantic

Done.

21) Conclusions: could the authors point towards bigger implications and open up on further research that could be done e.g. for better constraining below cloud evaporation in models?

Done.