भारतीय उष्णदेशीय मौसम विज्ञान संस्थान
(पृथ्वी विज्ञान मंत्रालय का एक स्वायत्त संस्थान, भारत सरकार के अधीन)
डा. होमी भाभा मार्ग, पुणे ४११ ००८

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Dr. Franziska Aemisegger
Associate Editor
Atmospheric Chemistry and Physics

Subject: Submission of the revised manuscript

Dear Dr. Franziska, Dated: 12th May, 2026

We are submitting the revised manuscript (**egusphere**-**2025-2390)** entitled "Assessing raindrop evolution over northern Western Ghat from stable isotope signature of rain and vapour". As per the suggestions of two reviewers, we have substantially reorganized the manuscript. Briefly, we have made the following changes:

(1) Reduced the size of the Conclusions (Section 5),
(2) Shifted majority of the text from Conclusions to two subsections-Limitations (Section 4.6) and Impact (Section 4.7),
(3) Transferred the Uncertainties in δD_r and d-excess$_r$ from main text to Supplementary Information (SI-10),
(4) Repeat discussion of matters given in the Supplementary Information is avoided in the main text.
(5) We have reduced the text, avoided repetition, corrected typos and grammatical errors.

Detailed point-by-point responses to the reviewers' comments and suggestions are attached to this letter.

We hope that you will find the revised manuscript suitable for publication in your esteemed journal.

Sincerely yours

Saikat Sengupta

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Editor's comments

Dear Authors

Many thanks for the revisions of this manuscript, which have improved the scientific quality as acknowledged by the reviewers. However, both reviewers in this second round raise several major issues and point out that the manuscript still needs major improvements of the text's presentation, organisation, precision of terminology, conciseness and clarity. I urge you to take these comments very seriously and rework the presentation of the results in the written text very thoroughly, which is a prerequisite for me to accept the paper after this second round of revisions. I will not send out the manuscript for a third round of review. If the revisions are incomplete, or if the text still shows major weaknesses, I will have no choice but to reject the paper. In the current form, the paper's scientific merit is overshadowed by presentational and organisational weaknesses. Thank you for carefully taking the very constructive feedback from the two reviewers into account.

Kind regards

Franziska Aemisegger

Reply to the Editor's comments and responses to the reviewer's suggestions

We sincerely thank the editor and the two anonymous reviewers for their critical review of our manuscript and for providing several useful comments and suggestions. Our responses to the reviewers' comments are provided in blue below.

Reviewer-1

This work evaluates rain and water vapor isotope ratios collected during the monsoon period near Pune, India using a Below Cloud Interaction Model (BCIM), which allows one to evaluate how rain evaporation affects the measured rain isotope ratios. The BCIM is run with different assumptions about the relative humidity profile, temperature profile, and isotopic lapse rate. These assumptions produce three runs, which I will refer to in my comments below. This is my second time reviewing this work, and I can tell that a great deal of new work has been added to the manuscript. While I find many of the new tables and figures to be quite a bit more effective in communicating the results, I find that the key message of this paper is lost in the rather long text, especially in the Discussion. I also find several inconsistencies in the narrative.

We thank the reviewers for appreciating our efforts in revising the manuscript and providing new comments and suggestions. The replies are appended next to each comment in blue letters.

Major concerns:

In Run-1, the isotopic inputs for the BCIM isotope lapse rates are taken from the measured surface δD and d-excess. But these measurements should already be influenced by rain evaporation, if rain evaporation is indeed occurring. I don't see why these values should be representative of the atmospheric column before droplets form, precipitation falls, and rain evaporation occurs.

R-1-1: We thank the reviewer for clarifying the issue. The reviewer is right about the effect of addition of rain evaporated moisture. This is the reason we altered the surface values in Run-3 to make a better fit. A detailed clarification is given below.

In Run-1, we use the surface measured parameters as the input to the BCIM and construct their profiles using the Rayleigh distillation formulae. Run-1 resulted in an unacceptable level of disparity between the observed and model rain δD values (of the order of ±10 ‰). This indicates that the isotope values of vapours (pre-rain) that gave rise to the raindrops differ from our measured values (post-rain) because they are modified by the addition of vapour from other sources with different isotope values. This conclusion is based on *a posteriori* reasoning and invoking downdrafted evaporation-generated vapour as a possible source is a speculation. We have added the following discussion now in Section 3.2.3 : **" In this context, we note that the measured surface vapour refers to post-rain ground-level vapour, which may suffer from downdrafted vapour with contributions from drop evaporation. This contribution may change the surface vapour when we measure it, making it different from the vapour that gave rise to the raindrops aloft. However, this suggestion is based on *a posteriori* reasoning and invoking downdrafted evaporation-generated vapour as a possible source is a speculation."**

The use of surface vapour as a first approximation in the Rayleigh model (Run-1) yields results which guide us to a better approximation in the BCIM (Run-3). Graf et al (2019) also used the Rayleigh model for a demonstrative purpose. For their actual rain isotope modelling, they used output from a limited area isotope model COSMO-iso to construct the vapour isotope profile. We do not have such a limited area model and Run-1 model allows us to improve the vapour isotope profile. This is the justification for using the measured surface vapour as the starting point to construct a vapour profile in this study.

What happens if vapor from non-rainy days is used instead? I am not necessarily asking for more analysis to be done, but I would like to see some justification of this important assumption.

R-1-2: We could not use vapour isotope values of non-rainy days as the source because the isotope values of vapour vary from day to day, and our goal was to reproduce the rain values within a few per mil, which cannot be achieved with such large variation. The second issue concerns the selection of a non-rainy-day; we do not know which non-rainy-day vapour can be used for a particular rainy-day modelling. Considering the level of agreement we seek, the above-mentioned issues are moot.

Run-2 uses old TES (satellite) data from a decade before the study period to calculate characteristic isotopic lapse rates that can be scaled to measured surface isotopic values.

(Some of the same issues raised for Run-1 thus also apply here.) I appreciate the extra work that was put into exploring the use of AIRS, whose measurements overlap in time with the period of analysis. I am confused by the conclusions in the Supplemental, however. The Supplemental suggests that AIRS does not produce values that match observed rain isotope ratios and therefore cannot be used. This does not feel like a sufficient justification for throwing out or ignoring actual data. Is it possible that AIRS simply lacks the sensitivity to the lower altitudes? Or the sample size is too limited by cloudiness? Were five years used, same as TES? Do the sensors give totally different answers for the same period? Once again, I am not asking that each of these questions is tested exhaustively. But I am asking for some justification as to why it's okay to simply throw out the AIRS results because they don't give the desired answer.

R-1-3: We agree with the reviewer and add the following text in the supplementary information (SI-8). **"We investigated the origin of deficiency of AIRS data. The AIRS (Atmospheric Infrared Sounder) satellite data is less effective at lower altitudes, specifically within the planetary boundary layer and near the surface. This is primarily due to challenges with infrared penetration through clouds, high humidity, and surface emissivity in the lower troposphere (Diao et al., 2013). Satellite retrievals of water vapour isotopes from the Tropospheric Emission Spectrometer (TES) provide direct measurements of HDO and H_2O owing to its high spectral resolution (Herman et al., 2014; Schneider et al., 2012). In contrast, isotope retrievals from the Atmospheric Infrared Sounder (AIRS) are derived using adapted TES retrieval algorithms and exhibit higher uncertainties and reduced sensitivity, particularly in the lower troposphere (Worden et al., 2019). Although AIRS-based δD estimates are broadly consistent with TES, they are characterized by larger errors (~30‰ compared to ~15‰ for TES) and lower vertical sensitivity (Worden et al., 2019; Herman et al., 2020). In addition, we find that the near surface values obtained from AIRS deviate systematically from the measured surface values (too negative by about 50‰ in δD values). Thus, we see that AIRS calculates atmospheric profiles while accounting for the thermal radiation of the Earth's surface. Surface emissivity (how efficiently the surface emits infrared energy) varies greatly over land, making it difficult to accurately retrieve temperature and humidity in the first kilometer of the atmosphere. While AIRS is a powerful instrument for atmospheric sounding, these factors degrade its performance in the atmospheric boundary layer compared to the upper troposphere."**

Run-3: If the dD and dxs profiles are tuned to replicate the observed rain isotopic values, it produces an excellent fit by design. The question in my mind here is whether the vapor values selected in the tuning are reasonable and make sense physically. I believe that the answer is in Fig. S8b-2. What does it mean that the dxs profile that produces the closest rain isotope values to observations is about 10 permil rather than 20 permil in value (note that 20 is the input used for the other runs)? Is this because the 20 permil is the value obtained post-rain evaporation (rather than pre-rain evaporation, i.e. the initial condition)? Some discussion of this would be valuable in place of several paragraphs talking about the goodness of fit of a tuned simulation.

R-1-4: We thank the reviewer for raising an excellent point. We agree with the reviewer that our measured vapour isotope values show post-rain evaporation effect. The rain vapour isotope exchange is demonstrated in Fig. 6b and discussed in Section 4.2. Due to this alteration, when we normalise the vapour isotope profiles with our surface measurements, we could not reproduce the rain isotope values. As vapour d-excess values increase and rain d-excess values decrease due to raindrop evaporation, we need to lower the d-excess values of vapours (pre-rain) that generated the drops to obtain a better match. This is a viable explanation behind the tuning and we discussed this possibility in Section 3.2.3. See also the reply R-1-1.

We omit long discussion on goodness of fit as suggested.

Discussion.
I had a hard time following the narrative through the Discussion. My comments below are organized in three groups:

1. Key points.

I feel that the discussion could play a critical synthesis role in this manuscript by clearly stating which parameter inputs critically influence the ability of the BCIM model to reproduce the observed rain isotope ratios and by linking these inputs to known processes. Instead, various inputs/processes are highlighted in different places, without a clear indication of their relative importance. For example, Line 549 states that downdrafts and other processes "override" surface meteorological parameters. However, Line 447 talks about surface water vapor isotope values being critical determinants of rain isotope values, and elsewhere it is suggested that surface relative humidity is important. Near Line 628, the size of raindrops and intensity of precipitation are invoked. Perhaps some places are talking about the effect on the isotope values while other places are talking about the effect on the bulk rain evaporation flux. The Discussion should make these points clearly and concisely while also synthesizing the take-home message from the 3 Runs.

R-1-5: Sorry for the confusion. We have now revised the discussion part thoroughly. It is now divided in two separate parts. The results from rain and vapour isotope observations are discussed first (Sections 4.1 and 4.2). This part does not include any results from the model. Next, in Section 4.3, we discuss the model; we estimate raindrop evaporation using the BCIM results, explore the factors controlling raindrop evaporation, quantify the uncertainty in drop evaporation estimation (Section 4.4), and discuss the relationship between rainfall amount and drop evaporation fraction in Section 4.5.

The summary of individual runs of BCIM and the take-home messages from each of them are discussed in the results (Section 3.2.1, 3.2.2 and 3.2.4). The factors controlling the model rain isotope values are discussed in Section 3.2.3 and the uncertainty of model rain isotope estimation is given in Section SI-10 and briefly discussed in Section 3.2.5. This restructuring of the text is guided by the comments of Reviewer-2 in the earlier revision. Reviewer-2 suggested that various BCIM runs and the uncertainty in model rain isotope estimation are the core results of this manuscript and thus should be included in the results section.

Lastly, the reviewer rightly pointed out that different parameters control model rain isotope values and raindrop evaporation, as shown by the two multiple regression equations (Eqn-1 and 2, respectively). The model rain isotope values are majorly affected by the vapour isotope values (Section 3.2.3), while drop diameter mostly controls drop evaporation fraction (Section 4.3). Vapour isotope has no effect on the evaporation. We have now clarified these two different effects following his advice.

2. Evaporation fraction estimate.

Equation 2 (Line 681) gets close to the kind of synthesis I'd like to see, but fails to tell us how the evaporation fractions are calculated (this is only explained in the next section).

R-1-6: Following the comment of the reviewer, the procedure of raindrop evaporation from the BCIM is now explained at the beginning of Section 4.3 before discussing the related results- **"The output of BCIM in Run-3 predicts that the mass of the drop reduces as it falls. The ratio of final mass to the initial mass (m/m_o) can then be used to estimate the fractional mass loss suffered by the drop on its way down. The difference ($1-m/m_o$) represents the effective rain evaporation."**

I had thought that estimating the evaporation fraction was a prime goal of this work, but the result (average 23%) is buried in a parenthetical 5 pages into the Discussion (Line 690).

R-1-7: As mentioned in R-1-5, the Discussion section is now divided in two parts: the first part deals with observations only and does not include results from the BCIM runs (Section 4.1 and 4.2) while the second part deals with the BCIM results (Section 4.3-4.5). There are two reasons for restructuring this section.

Firstly, we show that our observations independently establish variable raindrop evaporation from d-excess- $\delta^{18}O$ relationship in Fig. 6b and 6c. This provides a motivation for using the BCIM for quantifying drop evaporation via mass loss which is an output of the model. Therefore, the model part dealing with quantification of evaporation naturally comes later in the Discussion. It is true that estimating evaporation fraction is the prime motivation but the context for this estimate is given in the first part of the Discussion.

Secondly, we were advised doing so by the first review. Please see the following comments of the Reviewer #2 in the first review: **"A more impactful study might focus thoroughly on the observational dataset, strengthening the analysis in that area, and revising the modeling component. Observational measurements are less common and highly valuable, and thus the sampling of rain and vapor isotopes is compelling. The authors have produced an observational dataset of high value, and the measurements are likely to provide a valuable and novel contribution to this research field. The authors can strengthen their findings from the observational data."**

I'm also unsure why two evaporation fraction equations are needed (Eq 2 normalized, Eq 3 not).

R-1-8: The reason for two equations is as follows. Equation 2 is used to find which parameter among RH, T and Diameter is the dominant one in determining the evaporation. For this we need normalized equation which is unit independent (Section 4.3).

Equation 3 is used for finding the net uncertainty in evaporation fraction (Section 4.4) based on quadrature involving the combined uncertainty in the three parameters (RH, T and Diameter). We need unnormalized regression equation for that because the partial derivatives act as weights in the quadrature formula (see the reference on the Uncertainty analysis in the text).

In summary, simple (or unnormalized) multiple regression uses variables in their original units, while normalized multiple regression transforms all features onto a similar scale, allowing for direct comparison of feature importance

3. Downdrafts.

Much of the Discussion (or at least first four pages) talks about downdrafts, when this is not a process that is captured in the BCIM. What does it mean if the BCIM reproduces the observed rain isotope values but misses the process that the manuscript argues might be most critical for setting the near-surface water vapor isotope ratios (Line 587)?

R-1-9: We thank the reviewer for this insightful comment. We have discussed in response to the earlier comments R-1-1 and R-1-4 that the surface vapour isotopes measured in this study possibly show a post-rain evaporation effect, which is evident in the d-excess-$\delta^{18}O$ plot. We note here that the raindrop evaporation is not limited to surface level. The drop evaporation can occur at any altitude up to the cloud base where RH is <100% and generate evaporated moisture. It is important to explore how one can bring down such vapour. We suggested downdraft could be one of the processes that can bring such high d-excess vapour (post-rain) from above to the surface during measurement. Therefore, the pre-rain vapour that got uplifted and generated the drops must have lower d-excess. This realisation helps to find the direction of tuning required to generate the isotope profiles in Run-3 (See Section R.2.3).

However, we agree with the reviewer that the BCIM does not incorporate downdraft mechanism and therefore we cannot demonstrate the role of downdraft on tuning the vapour d-excess values except as a speculation (see reply R-1-1 and Section R-2-3). Therefore, we have trimmed the discussion related to downdraft in the revised manuscript.

There is a lot of great information in the Discussion, and I think it will be conveyed much more effectively with some re-ordering, organizing, and trimming.

R-1-10: We have thoroughly restructured the discussion part. Please see replies R-1-5 and R-1-7.

Minor comments:

Line 177, missing Δ

R-1-11: Changed as per suggestion.

Line 185, phrasing is a bit awkward (Marshall-Palmer)

R-1-12: The phrase 'M-P distribution' is deleted.

Line 194, consider "option" for "imperative"

R-1-13: Changed as per suggestion (line 192).

Line 226, I do not follow what "input are taken accordingly" means.

R-1-14: The sentence is changed as **"Therefore, the BCIM inputs need to b considered only up to the rain drop introduction altitude (see Table 1)"** in the line 230 to 231.

Section 2.4.3 should explain where the isotopic input values come from, even if it is to say that they come from one of several possible sources, detailed later in the text.

R-1-15: The sentence is changed as **"The inputs for various simulations are obtained from several possible sources given in Table 1 and discussed in Section 3.2"** in the line 232 to 233.

I'm not sure that CLWC is ever defined in the text.

R-1-16: It is defined in Section 2.4.2 (Line 198 to 199): **"The CLWC is defined as the total mass of liquid water droplets suspended in a unit volume of air within a cloud, typically expressed in grams per cubic meter or per kilogram of dry air."**

Line 374, I would simply say "outputs from the GCM LMDZ". "Namely" implies that other models are also being used.

R-1-17: Changed as per suggestion (line 367-368).

Line 412, What is a "digital value" for TES?

R-1-18: The phrase is deleted.

Line 464, referring to Equation 1, the coefficients suggest a 1 permil change in dD vapor results in a 1 permil change in dD_rain. It is not a percent change.

R-1-19: Corrected as per suggestion (Line 443-444).

Line 498, Sodemann et al. 2017 is from the Mediterranean, not necessarily appropriate for the deep convective monsoon region.

R-1-20: We agree and the part is deleted.

Line 677, If the regression is normalized, does this mean we are using standard deviations of RH and T and drop diameter to predict the standard deviation of evaporation fraction? A one-line explanation/confirmation would be appreciated.

R-1-21: Simple (or unnormalized) multiple regression uses variables in their original units, while normalized multiple regression transforms all features onto a similar scale, allowing for direct comparison of feature importance. We used normalized equation for understanding the factors controlling raindrop evaporation (Section 4.3) and unnormalized equation for uncertainty estimation (Section 4.4).

Line 720, all uncertainties are given in percent except for temperature. Why?

R-1-22: The values are absolute for all (not percent). Sorry we did not explain well. The RH is itself given in % unit. We revised the part as follows (Supplementary Information, SI-10, Line 337 to 352):

"The absolute uncertainties associated with RH and T in radiosonde observations are 8 % and 0.3 °C (Sapucci et al., 2005), but they vary from day to day. The absolute difference is taken as the daily scale uncertainty. The error for each day is then taken as the absolute difference divided by √2 (to get the standard error of the mean of two observations).

To obtain the drop diameter error, we note that the mean and standard deviation of the diameter of drops (as calculated from the Marshall-Palmer relation using the rain rates), considering 29 samples are 1.00 mm and 0.3 mm i.e., the standard deviation is about 30 % of the mean diameter. We could have taken 30 % of the mean diameter as the common average uncertainty. However, Tokay et al (2001) quote an uncertainty estimate for the drop diameter, derived based on Marshall-Palmer distribution, which is less, about 20 % and which we adopt. Pune drop diameter varies from 0.606 mm to 1.796 mm over the 29 days. Taking the error in each day's diameter as 20 % of that day's value, the individual error varies from 0.12 mm to 0.36 mm. Adding these three errors to the above quadratic formula, we obtain the error in the evaporation fraction for 29 days, which varies from 7.4 % to 13.8 % (for EF values from about 4 % to 61%). The average for 29 days is ±8.9 %, which is taken as the overall error (common for all days) in the evaporation estimate. With these inputs, the mean uncertainty in the rain isotope value δD_r is 3.5 ‰. Using a similar exercise for the d_r we obtain the mean uncertainty for the d_r as 1.7 ‰."

Reviewer-2

Review of Egusphere-2025-2390 Revision

"Assessing raindrop evolution over northern Western Ghat from stable isotope signature of rain and vapour"

Thank you to the authors for your revised submission. The revised study is overall much improved. I commend the authors for the additional steps they have taken to strengthen their analysis. The authors have now provided complete context and information about their methods to allow the reader to assess the results. Overall, I feel the study is scientifically acceptable, with the exception of a few statements/conclusions that are made without sufficient support.

We thank the reviewer for appreciating our effort in revising the manuscript and providing new comments and suggestions. The replies are appended next to each comment in blue letters.

I caution the authors against making strong scientific claims that do not come with sufficient evidence. Or, to do so with the disclaimer that it is a hypothesis/speculative. Some of these cases are mentioned below.

R-2-1: We agree and generally follow the suggestion.

From an organization and writing quality standpoint, there is still much revision required to meet the journal standards.

R-2-2: We thank the reviewer for the suggestions. .

Scientific Merit

● Ln 485-486: I think you need to explain and justify the 6 data points that are discarded.

R-2-3: We are sorry for the misunderstanding. We just wanted to say that for 23 days out of 29 days, changes were small- we did not discard anything. We have revised the phrasing as (line 469-470): **"Most of the changes were small; in the δD_v, 23 out of 29 changes were within ±4‰ and similarly in the d_v 23 out of 29 changes were within ±3.4 ‰."**

● Ln 512 - 518: I recommend removing this paragraph. I'm not following this line of logic.

You're saying that Run-3 agreed well with observations, so this implies the microphysics in the BCIM model must be valid? Specifically, you claim that the good agreement means that all of your BCIM inputs were valid, and that your estimate of raindrop evaporation is therefore accurate? You seem to be reaching for a claim that has insufficient support. You do not need to assert that your results are 100% irrefutable: You have provided your methods to the reader, and they can discern for themselves how convincing they find your results. What matters is that you are transparent with the reader on what you have done to obtain your results.

R-2-4: As per suggestion the paragraph is removed.

● With the range of evaporation fractions being so large from day-to-day, the authors could consider reporting the range in their abstract and conclusions, in addition to the mean value. This may be more informative to the reader as they can get a sense for the variability throughout the ISM.

R-2-5: We agree with the reviewer and the range (4 % -61 %) is mentioned in abstract (Line 29) and conclusion now (Line 811).

○ On this topic: Your abstract says 23%, then the final paragraph changes to ~25%. It is recommended to remain consistent in how you report the results.

R-2-6: It is 23%. We have corrected accordingly (Line 811).

● Your radiosonde RH and T uncertainties are not quite correct: You've used uncertainties for a single sonde measurement, but because you have averaged two radiosonde measurements, this introduces a larger uncertainty that needs to be accounted for. As a rough estimate, your supplementary information indicate that the uncertainty on a daily RH value is +/- 10% and the uncertainty on temperature is ~2°C. But if you wanted to be more precise, you could take the uncertainty of the mean (the standard deviation of the two data points divided by the square root of the number of samples, in this case two).

R-2-7: We agree with the reviewer and revised the discussion related to uncertainty (Section 4.4; see reply R-1-22 also).

The average of absolute differences in RH and T are 8% and 1°C. Instead of taking a fixed value we now take the absolute difference for each day and calculate for that day. The revised discussion in Supplementary Information is (SI-10, line 337-340) " **The absolute uncertainties associated with RH and T in radiosonde observations are 8 % and 0.3 °C (Sapucci et al., 2005), but they vary from day to day. The absolute difference is taken as the daily scale uncertainty. The error for each day is then taken as the absolute difference divided by √2 (to get the standard error of the mean of two observations).** **"**

● 777-779: "The intense convective events, indicated by these anomalies, lift the air parcels to higher altitudes where the ambient vapour isotope ratios are highly depleted. Droplets formed from these vapours are correspondingly depleted" - I'm not sure that this is conceptually accurate: Is the isotopic depletion from intense convection due to the air being lofted up to higher altitudes, or is it because of Rayleigh distillation associated with condensation and precipitation? I think it is the latter, because convection, by definition, brings moisture up from the surface, and droplets form within parcels of rising air. Cloud droplets do not grow from the pre-existing ambient vapor at altitude, they grow from the moisture that was brought up from the cloud base. If I am wrong here, then please provide references to justify your claim.

R-2-8: _We thank the reviewer for bringing clarity to this issue and we agree that the word ambient causes confusion. Cloud droplets grow by the condensation of water vapor onto suspended particles (**Cloud Condensation Nuclei**), a process triggered when lifted air parcels cool to their dew point and become saturated. Cloud droplets grow by saturation of vapours being uplifted by cooling of the air parcels. We revise the part as below (line 797-798)**: "These events likely reflect uplift of moist air parcels to higher altitudes (~5.5 km), and condensation to droplets during ascent following Rayleigh distillation."**

● 782-783 and 785-787: You cannot draw this conclusion, because the higher CLWC peak could be a decoupled cloud layer, or even cirrus. Intense convection can create conditions for higher-level cloud formation (through anvils and detrainment of moisture into the upper atmosphere). I think it's an interesting observation that the higher CLWC is present during these cases, but it is mere speculation to say that the convective column extended all the way up to 5.5 km, unless you can provide additional evidence.

R-2-9: We agree. The sentences are omitted in the revised manuscript.

● 901: This is too strong of a conclusion to make without evidence. You have already shown that drop size is a dominant factor in the isotopic exchange, so why would the impact be small? You could instead explain that since the BCIM follows a single droplet there is no opportunity for collision coalescence, therefore you have to rely on your input droplet diameter being representative of droplet sizes that would occur through collision-coalescence processes. In theory this is all built into the M-P relationship.

(a) R-2-10: As per the suggestion, the text has been revised as (line 764-768)**: " Although some studies pointed out that collision-coalescence is an important warm rain process that occurs in the WG regions of India (Konwar et al., 2014), we did not include it in the model. Since the BCIM follows the evolution of a single droplet, there is no opportunity for collision coalescence, and we have to rely on the input droplet diameter being representative of droplet sizes that would occur through**

collision-coalescence processes. In theory, this is all built into the Marshall-Palmer relationship.”

631-632: What do you mean when saying they “are not independent”?

R-2-11: We have revised the discussion as (line 594-596)” **It is to be noted that the drop diameter in this study was not measured by disdrometer directly. They were estimated from rain rate using Marshall-Palmer relationship.”**

632-636: What is this section getting at? Are you saying that higher precipitation rates are associated with higher droplet diameters in your data, and this is a result of collision-coalescence?

R-2-12: The discussion is removed from the revised manuscript.

● This was a bit confusing, but if I understand correctly, I think all of this (631-636) can come together more concisely, for example: “We note that rain rate and droplet diameter are directly related through the M-P relationship we have used, so larger rate rates will always translate to larger droplet size. This physical relationship is thought to be a result of increased collision-coalescence during higher rainfall intensity (Law et al., 2021).”

R-2-13: We thank the reviewer. We have revised and reduced the discussion as follows (line 594-598): **“It is to be noted that the drop diameter in this study was not measured by a disdrometer directly. They were estimated from the rain rate using the Marshall-Palmer relationship. Therefore, larger rain rates will always translate to larger droplet sizes. This physical relationship is thought to be a result of increased collision-coalescence during higher rainfall intensity (Law et al., 2021).”**

Ln 641, 646: Did you mean upper left quadrant?

● It’s confusing that the figure does not have quadrants, yet you refer to the lower left quadrant. I now understand what is happening, but for me, it took me a while to catch on because my first instinct was to divide the figure visually into quadrants, and it would look like your data points lie in the upper right quadrant. Hence my confusion about the “lower left quadrant”. This can either be made much clearer in the text, or you can show all four quadrants in your figure.

R-2-14: The figure-8 is revised and all four quadrants are marked. We are sorry for the confusion and thank the reviewer for suggesting the correction.

Organization

Organization and readability of the beginning of the manuscript is much improved. However, the writing and organization steadily declines after around section 2.4.3. It is clear that much technical editing is still needed.

Here I offer some points of feedback on organization. The manuscript is quite long with a lot of redundant information, so I’ve also suggested areas where the manuscript can be shortened.

Thanks for the suggestions for improving the MS. All of which have been incorporated.

● Abstract: "3rd and 4th quadrants" has no context for the reader, so I would avoid using it in your abstract. Suggest something like the following: "Using the $\Delta\delta$-Δd diagram method of assessing sub-cloud rain and vapor exchange, our data suggest an equal share of equilibrium exchange and drop evaporation."

R-2-15: The abstract has been revised accordingly (Line 18-20).

● Section 2.4.3 Isotopic composition of the ambient vapour and hydrometeor
○ Here is where I would have logically expected to see your methods for obtaining the isotopic vapor profiles from LMDZ.

R-2-16: A brief explanation is required here. The vapour isotope profile is not known a priori and is changed as we perform BCIM runs in three situations as Run-1, Run-2 and Run-3. These profiles are different from each other. To clarify we write as below in **Section 2.4.3** (line 215-220):**" The isotopic composition of the ambient vapour at various heights is not known a priori. They are estimated from one of several possible sources and vary depending on the inherent assumptions. Three types of profiles were considered in this work, one after another, with the idea of improving the BCIM predictions to match the observed rain isotope results. To clearly present how this was done, we discuss the vapour isotope profiles along with the results for each choice in the Results section (Section 3.2.1 to 3.2.4; Table 1; See also SI-6 and SI-7). "**

● The "Key Findings" section seems to just be an abbreviated repeat of the results and methods section. There is quite a lot of redundancy with the previous discussion, as well as some new concepts that appear for the first time. I would suggest removing this section and incorporating anything that is not redundant into the results/discussion.

R-2-17: In the first revised manuscript, we wrote this section in response to the suggestion in the first review. Following the present suggestion, we have condensed the material removing repeats and **six** points give a summary of the key findings in short so that the reader can have a quick idea about the take home results. Additionally, following the advice of the reviewer, we renamed this section as **Conclusions.**

○ 813-821: You do not need to repeat this discussion; you already did this.

R-2-18: Thanks. The discussion is removed now.

○ 822-831: Shouldn't this be with the discussion of the $\Delta\delta$-Δd plot, instead of being introduced for the first time?

R-2-19: The text is reduced and merged with the discussion of the $\Delta\delta$-Δd plot in conclusion section, as suggested (line 804-807): **$\Delta\delta$–Δd analysis further confirms the importance of sub-cloud evaporation, with ~50% of data points falling in the evaporation-dominated quadrant. The derived slope (−0.45) indicates stronger evaporation compared to mid-latitude systems, consistent with higher temperatures (~25°C) in Pune, enhancing kinetic fractionation effects."**

● There is no clear and concise “Conclusions” section. A conclusion section should be added, and it should be more or less equivalent to your Abstract, but with a little bit more detail, now that the reader has complete context. For example, you could mention quadrants in your conclusion, since the reader now has that context.

R-2-19: As suggested, a ‘Conclusion’ section is added (Section-5). Please see above R-2-17.

● Reorganization within the results section: I find that there are too many nested subsections, and I would offer the following changes:

Thanks a lot for the kind suggestions.

○ 3.2 “results of BCIM simulations”
■ The table should be moved up into this general area (not in section 3.2.1).

R-2-20: The table is shifted as per the suggestion.
○ 3.2.1 and 3.2.1.1 can be combined into one subsection under “3.2.1 Run-1 Results”

R-2-21: The two sections are combined under Section 3.2.1 “**Run-1: Rayleigh ascent results”**.

○ 3.2.2.1 RF and T from Radiosonde - This information may fit better under the methods section.

R-2-22: As per suggestion, this information is now shifted to Section 2.3 **Ground-based meteorological, Radiosonde and Satellite data**

○ 3.2.2.2, Vertical profiles of vapour isotopes - I feel this belongs in the vapour isotope methods.

R-2-23: Please see the previous reply, R-2-16. Three types of profiles were used one after another to improve the agreement with measurements and these attempts are better described in sequence along with the step-by-step results of the simulations. So we keep this discussion in Section 3.2.2 (Run-2 Results). However, we have reduced the length of the section. This forms the reorganization as suggested by the reviewers.

○ 3.2.2.3 and 3.2.2.4 can be combined into one section under “3.2.2 Run-2 Results”

R-2-24: We have made a section 3.2.2 Run-2 Results after combining sections 3.2.2.2 to 3.2.2.4, as per the suggestion. However, as the combined section is too large, we have made two subsections for better readability: (a) Mean vertical profiles of vapour isotopes from TES and LMDZ data and (b) Daily scale profile by adjustment technique.

○ Change 3.2.3.1 to “3.2.3 Possible sources of discrepancy between Run-1 and Run-2”
○ 3.2.3.2 becomes “3.2.4 Run-3 results”

R-2-24: We agree and combined the sections accordingly to 3.2.3. The title is modified to: Possible sources of failure in predictions of Run-1 and Run-2

The next section is: 3.2.4 Run-3 results

○ Remove section 3.2.3.3:

If it's in the supplementary, then it does not also need to be included in the main manuscript. The manuscript should be self-contained, not having in-depth discussion of figures that are in supplementary information. For example, this section can be condensed to a few sentences:

"We find that the vapor isotope value is the most important factor controlling rain isotope ratios in the BCIM. The uncertainty of the model predicted δD and d-excess are 3.5% and 2% respectively. BCIM sensitivity analysis and uncertainty calculation are available in SI-13,14."

R-2-24: We agree and condensed the Section (new name 3.2.5) (formerly, Section 3.2.3.3) as (line 497-502)

" We conducted an uncertainty analysis (SI-10) and a sensitivity analysis (SI-11) of the model rain composition using Run-3 results to study the effects of variation in vapour isotopes (δD_v), relative humidity (RH), temperature (T), and drop size (D). We find that the vapor isotope value is the most important factor controlling rain isotope ratios in the BCIM. The uncertainty of the model predicted δD_r and d_r are 3.0‰ and 1.7‰ respectively. In case of sensitivity, for a +10% change over the reference values of the parameters, δD_v, RH, T and D, the changes in the δD_r values (in ‰) are: +7.6, -4.1, +2.6 and -0.4 respectively (Fig. S11-1)."

● Lines 854-860: Once again, if it's in the supplementary information it means you do not need to discuss it at length in the main text. Condense this to one sentence and refer the reader to supplement for details. For example: "sondes are launched at 00 and 12Z and are generally not carried out when there is rain. We determined that two soundings taken on the same day are similar to within 10% RH and 2C on most days. Analysis of sonde daily variability can be
found in supplementary information".

R-2-25: As per suggestion, the discussion is now condensed and placed in Section 4.6 Limitation and Uncertainty of the derived parameters (Section 4.6).

● Section 5.2 is a good addition and much appreciated, but it should probably go ahead of your key findings. Perhaps at the end of the results section.

R-2-25: We made a separate section 4.6 Limitation and Uncertainty of the derived parameters and placed it before conclusion (section 5).

● 378 - 382: This can be condensed to just one sentence.

R-2-26: We have revised the sentence as (line 371-373)
"Unfortunately, when the vapour isotope values from LMDZ-iso over Pune are used as inputs of BCIM, the model values did not yield good agreement with observations (results not shown)."

● 537 - 540: Do not describe the supplementary information. Just state for example: "In our data, the vapor d-excess is not significantly correlated with temperature or relative humidity (Details are provided in S10-1)."

R-2-26: We have revised the sentence as suggested.

Typing Errors

The manuscript contains typing errors throughout that need to be given attention. I have listed some below, but it is not comprehensive:

We are extremely grateful to the reviewer for his careful review and detailed suggestions regarding the large number of corrections. We have corrected all of them and some more as indicated.

Notation and subscripting is inconsistent throughout the manuscript: vapor subscript is sometimes "v", sometimes "vap". Vapor d-excess is sometimes "dv", sometimes "dex", sometimes d-excessvap , and sometimes d-excvap-

Ln 97: "a mountain" or "the mountains". Mountain should not be capitalized. Done
Figure 1: (1901-2017) in ln 111, but (1901-2020) in ln 112 - Why? Corrected
Figure 1: Label for the rainfall contours is needed. Done
Ln 158: Graf reference has no year. Corrected
Ln 167: suggest "(3) tracking the microphysical evolution of a falling hydrometeor" Done
Ln 168: define (T) here, as it is used later in Ln 172. Defined in Line 132.
Ln174: needs paragraph indent Done (Line 173)
Ln 177: **Δ** symbol is missing Corrected (Line 177)
Ln 177: reference Graf for this concept. Done (Line 175)
207: "consider the cloud base height to be the Lifting Condensation Level" Done (Line 206-207)
217: remove "subsequently, these drops grow of diminish as they fall" (unnecessary sentence) Done
222: "using equations governing the microphysics." Done (Line 227)
226: "Therefore, the BCIM inputs are taken accordingly in the present study" - Odd sentence. Maybe say "therefore we have not included them in our BCIM inputs" Revised in Line 230-233.
Figure 2 caption: Ln 251 - put the (a) before d18O and move (b) before d-excess. Done
254: "The label, A, indicates…" Done
256: remove hyphen from southwest. Done
Figure 3 caption: mention that the data source is from ERA. Done
270: "fall below the overall mean (μ) minus half the standard deviation (σ)" Done (line 275)
284: need space after "Fig." Done (line 288)
291: delD "v" needs to be subscripted Done (line 294).
294: "rain" instead of "rains" Done (line 297)
318, 319: your subscripts are not subscripted. Done (line 320)
319,321,322: I'm confused by these (δv,0), (dv,0) notations… where are they used? The sentence is deleted.
Could remove 326-328, it is not needed. Removed
398: needs period after profile, before "Our". Revised in Line 373-374.
400: "The only justification" should be changed to something like "To achieve closer

representation of our sampling location, we use a boundary constraint…" The line is deleted.
406: "possibly added by" → "with potential additions from evaporation components…" Done (line 393)
407: areal → spatial Done (line 395)
413: minimum, mean, maximum do not need to be capitalized. Done (line 401)
412, 417: The use of "digital values" is a bit odd… perhaps the authors mean to say "discrete values". Done (line 404)
425-426: grammar and notation issues. Revised L412-413.
428: "for the three runs: Run-1, Run-2 …" Done (L414).
432: It's not clear what the authors mean by "adopted"? Sentence deleted.
435: "strictly correct, but it…" Revised (L425-428)
435: "yields" Sentence deleted.

454: Remove "based on previous discussions". Done
466: "size" -> "droplet size" Done (Line 445)
542: "drop in evaporation" → "evaporating droplet"? Or did they mean "decrease in evaporation"? Sentence corrected.
565: What is "mean~2%"? Removed.

574: Rain and Vapor do not need capitalization. Corrected (Line 54-542).
Figure 6 caption: subscripts. Corrected.
624,626: You cannot start sentence with a number. Spell out "fifteen", "fourteen" or revise sentence. Corrected (line 590)
Equation 2: What is the (norm) in parentheses mean? Normalised
684: There is an incomplete sentence lingering. The sentence is deleted.

806: Can remove the phrase in parentheses: you already explained this in methods, so don't need to repeat it here. Agree Done (Line 803)

828: uncompleted parenthesis. The line is deleted.