Precipitation in the mountains of Central Asia: isotopic composition and source regions

Response to the Referees' Comments

Dear Referees and Editors,

The authors are grateful to the anonymous referees for their helpful comments.

All comments have been addressed and implemented in the revised manuscript. In this document, we explain how the suggestions have been implemented and signpost the changes in the manuscript. The authors' responses are presented after each comment and highlighted in blue. We refer to the lines where changes have been made as in a clean copy of the revised text. All references, used in the Response to the Referees' Comments are listed at the end of the re-submitted manuscript.

We have submitted a revised copy of the manuscript with tracked changes.

On behalf of all authors,

Dr. Saidaliyeva and Prof. Shahgedanova

Anonymous Referee #1

General comment

This paper presents original data on the isotope composition of event-based precipitation in different areas of Central Asia. The manuscript covers a necessary topic and is of interest to be published in this journal. I would highlight the presentation of a very large isotopic dataset of single precipitation events, which is important to fully investigate the main factors of variability and to evaluate the relationship between the source areas of precipitation and their isotopic composition. The authors provided a good introduction of the context, and they clearly indicated their own original contribution. However, I feel the manuscript is too immature at this stage for publication because of a series of methodological imprecisions (see comments 8-9 below) and it needs to be revised significantly. Results and discussion should be presented in a clearer and well-structured way taking care to accurately separate the presentation of data from the interpretation provided by these authors. I am not a English native speaker, but the text also includes some typos and grammatical errors. Some sentences are too long (including titles of sections), and some parts are repeated and redundant, so the paper can be shortened. The manuscript would benefit from careful proofreading by a native speaker. Then, considering these limitations, but aware of the effort made by the authors in collecting such a large number of precipitation events and certain of the importance of the data presented here, I suggest reconsidering this manuscript after major revisions. I would be delighted to revise the manuscript again after a general reorganization, a clearer presentation of the methodology, and a more concise discussion of the results. Authors can follow some suggestions below and minor revisions are in the attached file.

We are most grateful to Referee 1 for the exceptionally thorough, knowledgeable, and helpful comments which helped us to improve the paper. We have separated Results and Discussion sections as Referee 1 suggested. The text has been revised overall and proof-read by a native speaker.

Specific comments

Please check the isotopic terminology through the manuscript and see USGS recommendations (https://wwwrcamnl.wr.usgs.gov/isoig/res/funda.html). Generally, the isotopic community use the expressions as follows:

- higher vs. lower values or more/less positive vs. more/less negative when isotopic measurements are compared.
- heavier vs. lighter or enriched vs. depleted when the isotopic composition of a material/substance is defined relative to a reference.

The isotopic terminology has been corrected through the manuscript.

Introduction

Comment 1: The authors reported the name of D-excess through the manuscript. I think the "d-excess" is the correct abbreviation for the deuterium excess. Dansgaard (1964) defined this parameter as d-index. Then Rozanski et al. (1993) called the parameter *d* cited in Dansgaard (1964) as d-excess. Many articles used d-excess or more easily *d*.

Corrections have been made throughout the manuscript.

Comment 2: I suggest changing lines 78-80 because they are not clear. It seems that a shorter distance between the cloud base and the ground is known to increase the d-excess, but this is not correct. The d-excess tends to decrease as raindrops go through the air column because of sub-cloud evaporation.

The paragraph, explaining variations in *d-excess*, has been re-phrased (**Lines 76-78**).

Comment 3: Researchers usually use past tenses when describing the research activities they performed. I suggest changing the verbs from the present to the past when describing the objectives (lines 105-106) and presenting the data (e.g., line 292 and forward).

We have revised the objectives and changed the verbs to the past tense when describing and discussing the results.

Data and Methods

Comment 4: At the beginning of subsection 2.1 authors should better describe the study area and provide further details about the sampling sites. How many catchments? How many sampling sites? In which basins are they located? What is the climate for each region/site during different seasons? It could be useful to describe the climate according to Köppen's classification system.

We have clarified the number of catchments and sites and their locations including river basins. We have added a brief description of temperature and rainfall regimes to the text and made a clear reference to Figure 1(b, c) showing annual cycles of temperature and precipitation at the sampling sites. We have added the following description according to the Köppen classification: "In the UA, AA and CKS catchments in the north of the region, the Köppen climate classification is subarctic and tundra in the high mountains changing to humid-continental climate in the middle mountains and to the semi-arid grassland steppe as elevation decreases. In the CHK and KF catchments in the south, the Köppen climate classification changes with elevation from subarctic in the high mountains to humid-continental, then to a Mediterranean climate and to semi-arid grasslands and desert on the plains."

Comment 5: The reader may get a little confused with the acronyms. Authors could code each sampling site with the acronym of catchment followed by a progressive number (e.g., UA1, AA1, CKS1, CKS2, CKS3, etc.) in order to immediately understand in which area a site is placed. Then, authors could label the sites in Fig. 1a, and report the same codes in Fig. 1c.

We have introduced the site codes and used them throughout the text. All figures have been corrected to include the site codes.

Comment 6: At line 136 the authors cited the PALMEX collector model RS1 to collect monthly precipitation, but they did not provide any details about the collectors used for event-based precipitation. Which collector did you use? You should mention that any anti-evaporative system was not used because samples were collected immediately after the events. Are you confident with the reliability of collecting precipitation with no anti-evaporative system?

Samples were collected using traditional Tratyakov rain gauges without any anti-evaporative system because the meteorological observers were available at the station 24 hours a day throughout the year and collected samples immediately after the precipitation events. This is a common practice for minimising evaporation effect and this gives us confidence that the collected samples were not affected by evaporation to a significant extent. An explanation has been added to the Methods section (**Lines 148-150**).

Comment 7: At line 148 authors should also report the error propagation associated with the d-excess. According to the formula proposed by Natali et al. (2022), the d-excess error was about ±2.5‰, by using the total errors of ±0.2‰ for $\delta^{18}O$ and ±0.6‰ for $\delta^{2}H$. Hence, some differences between sampling sites or seasons may not be significant. Please, check it through the manuscript. Pay also attention to significant digits for d-excess.

The error propagation associated with the *d*-excess has been calculated and added to the text in the section 2.2 (**Lines 157-158**) with a reference to Natali et al. (2022).

Comment 8: At lines 163-169 authors were confusing when describing regression techniques. The sentence "This approach potentially increases uncertainty in the interpretation of results…" is misleading because different regression methods are useful for different applications. Precipitation-weighted regressions are useful to minimize the impact of possible evaporation processes when dealing with small precipitation amounts (Hughes and Crawford, 2012). So, these lines are more appropriate to represent hydrologically significant precipitation, which is important for local hydrological applications, especially when investigating stable isotopes of groundwater in relation to precipitation recharge. Conversely, unweighted regressions are advisable when researchers aim to evaluate the atmospheric and hydrometeorological processes that govern the isotopic composition of precipitation. Moreover, theoretically, the RMA and MA regressions are more suitable than OLSR because they consider errors in both correlated variables ($\delta^{18}O$ and $\delta^{2}H$). I suggest computing only the OLSR and RMA regressions, and the corresponding weighted models, and comparing these lines in relation to different aims.

We are grateful to Referee 1 for the explanation and advice. We have added a comment on different applications of LMWL, calculated using different methods, to the text. We have decided to retain the comparison of all six methods of regression because it is expected to aid future studies in CA especially where precipitation depth may not be available and assessment of uncertainty, associated with the use of non-weighted precipitation, may be required. A similar comparison has been provided by Wang et al. (2018) for the Chinese Tien Shan and the adjacent regions. Publishing detailed comparisons for both regions will complete the assessment for the mountains of CA and north-western China.

Comment 9: The HYSPLIT analysis described in subsection 2.4 has some methodological issues. Firstly, the authors ran the HYSPLIT model for four sites, but CKS and CHK catchments include three sites each, so we cannot know at which site trajectories were calculated.

A single site was used in each catchment and this has been clarified in the text. We have explained that the sites with the longer and uninterrupted sample series were used. We used site codes throughout the text on the trajectory analysis for clarity as suggested in Comment 5.

Site elevations were used as starting altitudes for the model, but it is wrong because the starting altitude should ideally correspond to cloud elevation during precipitation. Did the authors have some radiosonde ascent measurements? If not, they may select an altitude that can be representative of the air column where most of the moisture is contained (please see Bershaw et al. 2012, Wallance and Hobbs, 2006, Krklec et al. 2018).

We agree that in general, the staring altitude for back trajectories is usually taken as 500-700 hPa when, importantly, sampling is conducted on the plains. In our case, sampling was conducted between 1255 and 2571 m a.s.l. Most moisture in the air column in the region is contained between 1500 and

3000 m where lifting condensation level is positioned and precipitation forms (Chen et al., 2024; Zongxing et al., 2016). When designing the experiment, we started HYSPLIT from the site elevation and 850 and/or 700 hPa for comparison and there were no appreciable differences in the outcome of the comparison. This explanation has been added to the text (**Lines 214-216**).

Authors should also consider the duration of each precipitation event. They ran a single trajectory at the beginning of each event, but one single trajectory could not be representative of the mean direction of air masses for longer events. Are these authors confident with the reliability of a single trajectory?

We agree that the difference introduced by calculating trajectories for the beginning and end of the precipitation events should be investigated. The median duration of precipitation events was 240 minutes. We have run trajectory analysis for the beginning and the end of the events whose duration exceeded the 90th percentile of precipitation duration. The differences between the trajectory coordinates after 120 hour iterations were within the spatial resolution of HYSPLIT. We note that in contrast to the maritime locations, several hours do not make a substantial difference in the continental interiors where precipitation originates either locally or where air masses travel very large distances from the ocean. We also note that single trajectories were calculated in the previous studies (Jorba et al., 2004; Wu et al., 2015; Pérez et al., 2015; Bagheri et al., 2019; Kostova et al., 2020).

The following explanation has been added to the text: "In line with previous studies (Jorba et al., 2004; Wu et al., 2015; Pérez et al., 2015; Bagheri et al., 2019; Kostrova et al., 2020) and to comply with the HYSPLIT cluster analysis function requirements, single trajectories were calculated instead of trajectory ensembles, potentially introducing uncertainty. The starting time of each back trajectory was defined as the hour closest to the start of precipitation event. The median duration of precipitation events was 240 minutes. A comparison between trajectories calculated for the start and the end of precipitation events exceeding 660 minutes (90th percentile) was performed. The differences between the coordinates of their point of origin was within the HYSPLIT resolution." (**Lines 216-225**).

Did authors establish the number of clusters based on the TSV criterion (e.g., see Kostrova et al. 2020)? The authors should provide more details on this.

Yes, the TSV criterion was used to determine the optimal number of clusters. The following explanation and references have been added to the text: "Total spatial variance (TSV), defined as sum of the spatial variances of all clusters, was used to determine the optimal number of clusters. The percentage of change in TSV was plotted against the number of clusters and the first large increase in the change of TSV was taken as an indicator of the final number of clusters (Wilks, 1995; Kostova et al., 2020)." (**Lines 229-231**).

The expressions "source regions of moisture" or "moisture transport" are frequent in the manuscript, but the results here presented are merely based on trajectory directions and distances. This analysis is correct to determine the origin and mean provenance of air masses producing precipitation, but any evidence of moisture uptake and transport may be found. I suggest using a specific humidity-based model (e.g., Oza et al. 2022, Natali et al. 2023), to account for the history of moisture dynamics along the trajectories, or merely to discuss the mean provenance of precipitation.

We agree with the Referee's comment. We have replaced the expressions 'source regions of moisture' and 'moisture transport' by 'trajectory source' or similar term throughout the text. We agree that using moisture tracking and/or humidity-based models would be beneficial, but it is not practical to include it in this paper because it already contains a large volume of data. We have, therefore, limited the discussion to the provenance of precipitation as the Referee suggested. References to Oza et al. (2022) and Natalie et al. (2023) have been added to the text. We plan to the use of suggested models in a subsequent publication. We have added this point to Discussion as one of the limitations of the study (**Lines 616-617**) and stated in Conclusions (**Lines 675-676**) that in the future, the method will be improved by accounting for the history of moisture dynamics along the trajectories.

Subsections 2.4 and 2.5 can be merged.

Done.

Results

Comment 10: "Results" and "Discussion" should be correctly separated into two sections that enable a clearer distinction to be made between the obtained results and the data interpretation provided by these authors. They included part of data interpretation in the "Results" section (see lines 229-231, 252-258, 295-297, 300, 303, 321-323, 419-421, etc.), but these parts should be moved to the "Discussion". The regression analysis between isotopes and geographical and meteorological variables (lines 259-290) could be moved to the "Discussion" and merged with considerations already done. In the presentation of results, authors could follow this scheme:

- Total isotopic means for all events
- Isotopic means for solid, liquid and mixed precipitation
- Spatial isotope variability
- Seasonal isotope variability
- Results of the Hysplit analysis and air masses origin

Then, the interpretation of these results by authors can be presented in the section "Discussion" along with the evaluation of relationships between the isotopic composition of precipitation and air masses trajectories.

We fully agree with this comment. We have re-organised the text moving the interpretations from Results to Discussion. We have aligned the order in which we present and discuss the results throughout the text (Abstract-Objectives-Results-Discussion-Conclusions) according to the Referee's suggestions (although we discussed seasonal variability before the spatial variability as this is appears to be a more logical order in this specific text). We have added the information on the total means: "The total means values of δ18O and δD for all events between 2019 and 2021 were -8.6±6.5‰ and -56.1±50.1‰, respectively." (**Lines 260-261-XX**).

Comment 11: In Fig. 2 the authors reported annual mean values for $\delta^{18}O$, δD and d-excess (line 237), but they collected precipitation events in the period between 2019 and 2021 (line 135). To which year did the mean values refer? I think it is better to calculate the mean values over the entire period.

The mean values and standard deviations have been calculated for the entire period. We added clarification to the text (**Line 260, 281-282**) and in Figure 2 caption.

Comment 12: The authors collected event-based precipitation samples at 8 sites, but then they discussed data for 4 catchments (e.g., line 241). Were the same events collected for different sites of each basin? If not, I don't understand how the authors can discuss the data by catchment and not by site. If yes, did the authors average the isotopic values of the same events between different sites of each basin? It's really difficult to follow what these authors did. I suggest presenting data for 8 sites first, in order to evaluate the spatial and temporal variability, and then focusing on trajectory analysis performed at selected sites.

The samples were collected from eight different sites. There were multiple sampling sites in the CKS and CHK catchments. The main sites, functioning for the duration of the programme without interruptions, were CHK1 and CKS2 while at CKS 1/3 and CHK2/3, the sampling period was shorter (see Table 1 in the text) and the number of samples was insufficient to show the boxplots by site. For this reason, we have included data from all sites in the boxplots in Figure 2. The data are shown by site for CKS and CHK in Figure 3. These points have been clarified in the text. If the Referee does not accept this explanation and insists that Fig. 2 should show the data by site, we will revise the figure.

Comment 13: The "stepwise regression" method (line 259) and the method of Dansgaard (lines 275-278) should be cited and/or briefly described in the "Data and methods" section. However, the authors had enough data to perform Spearman's correlation analysis between isotopes and climatic variables. The stepwise regression was used to quantify the relationships between isotopic composition of precipitation and geographical locations. Simple linear regression was used to clarify the relationship with temperature. We have calculated Spearman coefficients which are almost identical to SQRT from R^2. In our view, linear regression is preferrable because it allows one to calculate $\delta^{18}O$ and δD from temperature data. We have noted Comment 23 with which we agree ("More caution should be observed by the authors when they proposed using temperature data as a proxy of isotopic values for isoscapes reconstruction. The R^2 is around 0.5, so the uncertainties would be too large.) Therefore, our preference will be to retain the regression equations but if the Referee insists, we will replace it with the Spearman correlation coefficients. Section 2.2 was added to the 'Data and methods' section with brief description of "stepwise regression" method (**lines 174-176**) and the method of Dansgaard (**lines 176-183**)

Comment 14: Coefficients at line 282 are different from those in the equation 8-9. Please check.

There is no mistake. Equations 8-9 show the coefficients of determination for all samples from all sites. In the text, we discuss variation from site to site and refer to Table S4 which shows the coefficients of determination for individual sites. This has been clarified the text (**Lines 304**).

Comment 15: In the LMWL equations, the authors reported the standard errors associated with slope and intercept. I would suggest providing confidence intervals of the regression models, which is important for comparison purposes.

Confidence intervals have been added to the text: "The 95% confidence intervals in Equation 10 were 7.6 − 9.7 for the intercept and for 7.5 − 7.7 for slope, respectively. the respective confidence intervals in the Equation 11 were 7.2 − 11.9 and 7.3 − 7.8." (**Line 352-353**).

Comment 16: Lines 403-411 and 437 should be moved to the "Data and Methods" section. As indicated in comment 9, it is not clear how the authors defined the number of clusters (lines 403-404).

This text has been moved to the 'Data and Methods' section. The method of determination of clusters was added to the Data and Methods section as suggested in Comment 9 (**Line 249-255**).

Comment 17: I suggest moving the parts dealing with the evaluation of relationships between isotope values and trajectory groups (lines 435-470 + subsection 3.5) to the "Discussion" section.

This section has been moved to Discussion and is now Section 4.3.

Comment 18: I am not sure that the high d-excess values (line 467) indicated the contribution of the re-evaporated moisture. It could be due to the atmospheric conditions at the moisture source or to both factors, especially in winter and autumn.

This statement has been removed to shorten the text.

Comment 19: I have doubts about merging groups 1, 2 and 3 (lines 481-484). Trajectories of Group 1 came from a different area compared to the other groups, so precipitation associated with air masses coming from the north could have a different isotopic signature.

We agree with the Referee that this is one of the main limitations of the study. However, this limitation does not affect the CKS and CHK catchments where Group 'Westerly' is represented by a single cluster. In AA, clusters were merged in two seasons and in both, two out of three clusters contained a very small number of trajectories. All five clusters were present in UA but there was a clear seasonality in their frequency, e.g. Cluster 1 was registered once in DJF but was a predominant cluster (61 events) in JJA. The difference between clusters merged to form Group 1 was not always statistically significant in contrast to the difference between groups. For these reasons, the impact of merging clusters on results is either non-existent (CKS, CHK) or limited (AA and UA). We have added a detailed explanation to Discussion (**Lines 618-632**) and pointed out in Conclusions (**Lines 669-672**) that we plan to resolve this problem by using a larger number of tracers in the future.

Comment 20: Lines 487-489. Once the authors have established a good correlation between $\delta^{18}O$ and δD, what is the point of repeating the calculations for both variables? I suggest using only $\delta^{18}O$.

We state mean values of $\delta^{18}O$ and δD in **Lines 455-456** but do not refer to δD thereafter.

Discussion

Comment 21: lines 517-527 are not very useful in this section. Some sentences are repeated in the "Introduction", whereas others should be moved to the "Conclusions" as main outcomes from this article. Please, move lines 527-529 to the "Data and Methods".

Lines 517-527 have been moved to 'Conclusions' and rephrased. Lines 527-529 have been deleted (a repetition from the 'Data and Method' section).

Comment 22: lines 530-534. It could be due to the altitude effect. I note that UA is placed at a higher altitude than sites in the CHK catchment, so this trend could be determined by altitude and not by latitude. Please check the elevation of collecting sites in articles for the Chinese Tien Shan.

This is a very good point and we thank the Referee for bringing it up. We have checked the elevations of the stations in the Chinese Tien Shan and limited our comparison to the mountain stations only. Using stations with similar elevations has altered the outcome of the comparison. We have changed the text accordingly (**Lines 506-509**).

Comment 23: At lines 551-552 the authors indicated "a strong positive correlation", but they did not perform any correlation analysis. Please, pay attention to the difference between a correlation analysis (correlation coefficients, r) and linear regression models (coefficients of determination, r^2).

More caution should be observed by the authors when they proposed using temperature data as a proxy of isotopic values for isoscapes reconstruction. The r^2 is around 0.5, so the uncertainties would be too large.

We have re-phrased this statement saying that "Air temperature serves as predictor for the isotopic ratios of the event-based precipitation". We accept that R^2 around 0.5 is moderate correlation and we removed its description as 'strong'. We have removed the statement on using air temperature as proxy for isotopic ratios and added the following sentence instead: "Air temperature was a statistically significant predictor of $\delta^{18}O$ and δD but coefficients of determination of 0.46 − 0.66 implied that using air temperature as proxy for isotopic signatures may lead to high uncertainty in the reconstructions of isotopolgues." (**Lines 480-484**).

Comment 24: The subsection 4.2 should be revised after modifications suggested in the comment 8.

We have added an explanation on the different use of the methods as suggested in Comment 8, however, as explained in the answer to Comment 8, it is important to produce a comparison to enable an informed choice of method in the future studies.

Comment 25: lines 628-632 may be omitted or moved to the "Introduction". Lines 634-637 and 644-648 are redundant and should not be repeated in this section.

Lines 628-632 and 644-648 have been deleted.

Figures

The figure numbers are wrong. At line 227 the authors cited Fig. 4, but Fig. 3 had not yet been presented. Please check through the manuscript.

Done.

Fig. 1: please use the same codes in Fig. 1a and 1c (see comment 5).

Figure has been corrected according to Comment 5. Sampling sites codes are now used in all figures.

Fig. 7: move the labels to avoid overlapping with boxplots.

Done.

Anonymous Referee #2

General comment

This paper discusses the use of rain water isotopologues for the investigation of moisture sources. This is an important topic of atmospheric and climate science and very well fits into the scope of Atmospheric Chemistry and Physics. The study is made for central Asia. The paper presents a new rain isotopologue data set for this region, performs a very detailed analyses on how the observed isotopologue ratios depend on location (latitude, longitude, altitude), season, and airmass flow patterns (HYSPLIT back trajectories). The main scientific result is, that the isotopologue data together with the back trajectories are used to determine the contribution of different moisture sources to the rain in central Asia.

Main comments: In general, I find the paper very detailed. Maybe the authors can try to shorten some discussions/descriptions that are not mandatory for understanding Section 4 and 5. Besides the presentation of the new rain isotopologue data set, the main outcome of the paper is to fit the moisture sources from the measured isotopologue data with the help of back trajectory calculations. To my understanding, the objective is to invert the two Equations (2x Eq. 4 for δD and δ18, and Eq. 5), i.e. to determine the moisture sources (fA, fB, and fC) from the measured δP (δD and δ18). However, the moisture sources are already known from the backward calculations, right? In fact, Table 2 and 3 show very similar results, e.g. the importance of all sources for UA, limited importance of the "Westerly" source for CKS, dominating importance of the "Local" source for AA and CHK. What do we gain from using in addition to the backward trajectory the isotopologue data? Or is the long-term/final objective to use the isotopologue data instead of the backward trajectory calculations (after having determined the fitting parameters by the combined availability of backward trajectories and isotopologue data)? I think a better explanation of the objective would be useful.

We are grateful to Referee 2 for the comments. We have re-arranged the text and removed repetitions which helped to shorten the text.

We agree that there are different methods of source contributions can be used including trajectory data together with precipitation depth measurements, climate models, and isotopes. As Referee suggested, we use both methods in this paper so that in in the future, the isotopologue data can be used instead of the backward trajectory calculations. We added a sentence to the Objectives to clarify this. Also, combinations of different methods can be used to reduce uncertainty, e.g. back trajectory and isotopologue data or (in the future) isotope-enabled climate models verified using isotopologue data. The results presented here may be used to assess the performance of the isotope-enabled climate models in the future.

Minor comments:

I recommend revising all the Figure captions for completeness, e.g. Fig. 2: what is shown in the right panels? The mean values, i.e. the same as in the other panels or is this something else?

We have clarified the figure caption.

I am not a native speaker, but there seem to be several mistakes in grammar/spelling.

The manuscript has been revised and proof-read by a native speaker. The comments below have been implemented and/or the text has been changed.

Maybe revise together with the Copernicus copy editing service. A few examples are:

line 20: WAS derived

line 556: rations -> ratios

line 694: we applied mixing model -> we applied a mixing model

line 701: is the inner -> in the inner