We have carefully addressed all the comments made by the two anonymous reviewers on our manuscript (EGUSPHERE-2026-3213) entitled “Effects of freeze–thaw processes on the sources and pathways of shallow groundwater recharge in the Qinghai Lake Basin”. The comments have helped us greatly improve the overall quality of the manuscript. The following is the point-point response to all the comments.

Response to Anonymous Referee #1:

This study focuses on the Qinghai Lake basin in northeastern Tibet, employing comprehensive data from stable water isotopes (δ^2H, $\delta^{18}O$, lc-excess), soil moisture content (SWC), and the MixSIAR mixing model to systematically characterize the dynamic changes in recharge sources and pathways of shallow groundwater during different stages of the freeze-thaw cycle (melting phase, post-melting period, and freezing phase). The research aligns closely with current cutting-edge topics in hydrology and permafrost degradation under climate warming in high-altitude regions, featuring well-defined scientific questions, rigorous experimental design, ample data, and appropriate methodological applications. These findings hold significant scientific value for understanding groundwater formation mechanisms and water resource management in high-altitude regions under climate change. However, while fully acknowledging its scientific contributions, I identify certain shortcomings in the manuscript regarding the rigor of its argumentation logic, the depth of interpretation for certain data, and the completeness of methodological descriptions. To further enhance the paper's quality, I recommend a comprehensive revision. Below are the key issues and specific recommendations for improvement, for the author's consideration.

Reply: We sincerely thank the reviewer for the positive and encouraging comments. According to your suggestion, we will make significant revisions to the manuscript, focusing on improving the accuracy of the wording, refining the description of the methods, and adding a description of the associated limitations (see the replies to the following comments).

Major Comments:

1. Comment:

The conclusion that soil water serves as a "transitional reservoir" lacks sufficient support. The author repeatedly emphasizes in the abstract and conclusions that "soil water is a key transitional reservoir linking precipitation/snowmelt water with groundwater recharge" (Lines 38–40,511–513). However, the MixSIAR model only provides contribution ratios for each element and cannot directly demonstrate that soil water fulfills a transitional role of receiving, storing, and releasing water over time. To substantiate this argument, it is recommended to supplement the analysis with data on the lagged variation of soil water $\delta^{18}O$ over time (e.g., the lag time in soil water isotopic responses following precipitation events). If such evidence is unavailable, the author should downplay this aspect in the manuscript. The current discussion (Section 4.1) focuses more on changes in contribution rates than on direct dynamic evidence supporting its "transitional" nature.

Reply: We fully agree with your view that water isotopes can only quantify the relative contributions of end-members and cannot directly indicate the lagged variation in soil water on short timescales. We will revise the statements in the manuscript, particularly in the abstract and conclusion, regarding soil water as a key transitional reservoir.

2. Comment:

The accuracy of the MixSIAR model is highly dependent on the representativeness of element values. In this study, precipitation elements are derived from monthly mixing samples, whereas soil water and groundwater values are based on monthly averages. However, during the melting season (May–June), diurnal freeze-thaw cycles occur, causing precipitation to manifest as rain, snow, or wet snow with distinct isotopic signatures. Additionally, snow cover may be influenced by sublimation of pre-melt snow, meltwater runoff over frozen or partially frozen surfaces, and soil surface evaporation following snowmelt. It is recommended that authors explicitly describe how such mixed precipitation samples are processed in the Methods section (2.4.3) and include a discussion (Section 4.4: Limitations) acknowledging the uncertainties introduced by this simplification, along with an analysis of its potential impact on conclusions regarding the contribution of meltwater (7.9–22.0%).

Reply: We will clarify in Section 2.2 how precipitation samples containing rain, snow, or wet snow were collected, composited, and processed before isotopic analysis. In Section 4.4, we will discuss the uncertainties associated with temporal averaging and processes such as snow sublimation, meltwater runoff over frozen ground, and post-melt evaporation, and will explain how these uncertainties may affect the estimated meltwater contribution of 7.9–22.0%.

3. Comment:

The text interprets a "lc-excess value closer to the precipitation end-point" as indicative of enhanced preferential flow, while a "lower LC-excess value" suggests dominance of piston flow (a soil water evaporation indicator) (Lines 43–44,226–228). However, in Figure 3c, the variation in groundwater lc-excess across the three periods is minimal (approximately between –5‰ and –2‰). Moreover, the term "close to" is overly vague. It is recommended that the authors: a) include the difference between groundwater lc-excess and precipitation-end-point lc-excess (Δlc-excess), or provide monthly data on precipitation, soil water content at different depths, and groundwater levels; and use seasonal variations of this metric to support the claim of "gradual enhancement of preferential flow," thereby enhancing the objectivity of the argument.

Reply: We will revise the description of the methods in Section 2.4.2 of the revised manuscript by comparing differences in Δlc-excess values during the freeze–thaw period to demonstrate that the isotopic evidence clearly indicates preferential flow during thawing. Meanwhile, Δlc-excess values for precipitation and soil water will be added to Section 4.3, providing more comprehensive evidence for the preferential flow observed during the freeze–thaw period.

Minor Comments:

1. Comment:

This manuscript uses hydrogen and oxygen isotope methods to investigate the effects of freeze-thaw processes on the sources and pathways of groundwater recharge. It is recommended that the method used be explicitly stated in the title to make the manuscript's theme clearer and more specific. For example: Freeze-thaw processes influence shallow groundwater recharge sources and pathways in the Qinghai Lake Basin: insights from water isotopes.

Reply: We will revise the title in the revised manuscript to "Freeze-thaw processes influence shallow groundwater recharge sources and pathways in the Qinghai Lake Basin: insights from water isotopes" to further highlight the main theme of the manuscript.

2. Comment:

Line 31: "exerts a strong influence" It is recommended to change to "exerts a strong influence on hydrological processes” make the expression more complete.

Reply: We will revise the manuscript to change “exerts a strong influence” to “exerts a strong influence on hydrological processes.”

3. Comment:

Line 34: “takes the Qinghai Lake basin (QLB) as a case example”. It is recommended to change to“takes the Qinghai Lake Basin (QLB) as a case study” more in line with academic conventions.

Reply: We will revise the manuscript to change “takes the Qinghai Lake basin (QLB) as a case example” to “takes the Qinghai Lake Basin (QLB) as a case study.”

4. Comment:

Line 37: The abbreviation "FT periods" appears for the first time; it is recommended to define it as "freeze-thaw (FT) periods" in the abstract when it first occurs (it has been defined in the main text but not in the abstract; consistency should be maintained).

Reply: We will define “freeze-thaw” as “freeze-thaw (FT) periods” when it first appears in the abstract of the revised draft and maintain consistency throughout the manuscript.

5. Comment:

Line 70: Are the references "Zhang et al., 2026" and "Biskaborn et al., 2025" unpublished or preprints? Please confirm whether they have been formally published; if in review, please indicate this.

Reply: We will verify the relevant references cited in the manuscript, clearly indicate their publication status in the revised version, and standardize the reference format in accordance with HESS requirements.

6. Comment:

Line 83: “midwestern united states”,“United States” first letter should be capitalized.

Reply: We will correct “united states” to “United States” in the revised manuscript and simultaneously check the full text for similar capitalization issues.

7. Comment:

Line 134: For data such as "2.97×10^4 km^2" and "1.23×10^4 km^2," standardize the index format (use superscripts) and ensure no spaces are added between numbers and units throughout the text (e.g., "2.97×10^4 km^2").

Reply: We will consistently use superscript formatting in the revised manuscript and check the entire text to ensure that spaces are inserted between numbers and units, maintaining consistent formatting throughout.

8. Comment:

Figure 1: The subgraphs (a)–(e) in the figure caption lack sufficient detail; it is recommended to specify the exact content displayed by each subgraph in the legend (e.g., the relationship between sampling point distribution and permafrost/seasonal frozen soil boundaries).

Reply: We will add specific descriptions for subfigures (a)–(e) of Figure 1 in the revised manuscript and clearly specify the content corresponding to each subfigure.

9. Comment:

Table 1: The numbering conventions ("P1, P2..." under the "Precipitation" category and "G1, G2..." under "Groundwater") should be specified in the table header or footnote (whether based on elevation or watershed location).

Reply: We will add a note to Table 1 explaining the numbering convention, specifying that sampling points are numbered sequentially from upstream to downstream within each watershed.

10. Comment:

Line 186: "Filtered through 0.45 μm membrane filters": Are all water samples (including meltwater) filtered? Is groundwater filtered? Please provide consistent clarification.

Reply: We will clarify in Section 2.2 that all water samples, including rainfall, snowwater, soil water, and groundwater, were filtered through 0.45 μm membrane filters before isotopic analysis.

11. Comment:

Line 196: In the analysis accuracy, the expressions "$\delta^2H \pm 0.5‰$" and "$\delta^{18}O \pm 0.1‰$" should have the symbol "‰" the numerical value; it is recommended to revise them to formats such as "δ^2H: ±0.5‰".

Reply: We will uniformly revise this statement in Section 2.3 of the revised manuscript to "δ^2H: ±0.5‰ and $\delta^{18}O$: ±0.1‰".

12. Comment:

Line 261: "In the 0–30 cm soil layer... lowest LC excess." Please supplement with statistical differences (and their significance) between the 30–60 cm and 60–90 cm layers; it is recommended to specify the significance test method used in the methods section to enhance the rigor of the analysis.

Reply: We will specify the statistical test used to compare lc-excess among the 0–30, 30–60, and 60–90 cm soil layers in Section 2.4.4. We will also report the corresponding test statistics and p-values, and clarify which differences are statistically significant.

13. Comment:

Line 263-264: "precipitation shifted overall from depleted to enriched", It is recommended to specify the exact change in the average $\delta^{18}O$ value (e.g., from-8‰ to-5‰) to enhance persuasiveness.14. Figure 3: The color differentiation in subplots (a), (b), and (c) of the figure is insufficient; it is recommended to use a color-blind-friendly palette.

Reply: We will include specific changes in the average $\delta^{18}O$ values of precipitation in the revised manuscript and optimize the color scheme in Figure 3 to make the subfigures clearer and easier to read.

14. Comment:

Line 295: "GWL was closest to the 60-90 cm SWL" , Provide R^2 or the slope difference to quantify the degree of "proximity."

Reply: We will include the difference in slopes in the revised draft to more clearly quantify how close they are.

15. Comment:

Line 361-362: The author mentions that "the contribution of meltwater is significantly lower than that of rainfall," yet Figure 6 shows that during the melting period, meltwater still accounts for 11.88–16.17%. It is recommended to emphasize in the discussion section that "although low, it cannot be overlooked" to avoid inconsistency in tone throughout the text.

Reply: We will explicitly state in Section 4.1 of the revised draft that, although the contribution from snowmelt is relatively small, it cannot be ignored.

16. Comment:

Line 459-461: The bidirectional freezing process causes soil water to transition from liquid to solid state. Please elaborate on the potential impact of this process on lc-excess (whether it may lead to isotopic fractionation), as this aspect has not been addressed in the current discussion.

Reply: We will further discuss the potential effects of soil-water phase transitions during bidirectional freezing on Lc-excess and possible isotopic fractionation in Section 4.4 of the revised draft.

17. Comment:

References: The literature formats are inconsistent: some entries include volume and issue numbers, while others lack issue numbers (e.g., Alley et al., 2002; Kooi, 2016). It is recommended to strictly adhere to the author guidelines of the journal Hydrology and Earth System Sciences and maintain uniform formatting. Additionally, there are several instances of inconsistent "et al." formatting (some in italics, others not); please review these cases.

Reply: We will uniformly review and adjust the formatting of references and the use of “et al.” in the text throughout the revised manuscript to ensure compliance with the HESS Author Guidelines.

Response to Anonymous Referee #2:

This manuscript investigates the effects of freeze–thaw (FT) processes on shallow groundwater recharge sources and transport pathways in the Qinghai Lake Basin, combining stable water isotopes, hydrometeorological data, and the MixSIAR model, given ongoing permafrost degradation across alpine regions. The study provides useful insights: soil water acts as both a dominant recharge source and a transitional reservoir linking precipitation and snowmelt to groundwater, and thawing progressively enhances vertical soil-profile connectivity. Overall, the experimental design is reasonable, the data are abundant, and the analytical methods are appropriate, making this a well-structured manuscript. However, further improvements are needed in the following aspects: (1) specialized terminology should be further standardized and used consistently throughout the text; (2) figures and tables need refinement; (3) the conclusions and innovative points need further refinement and sharpening.

Reply: We sincerely thank the reviewers for their constructive and encouraging feedback. In accordance with your suggestions, we will make substantial revisions to the manuscript, focusing on improving the accuracy of the wording, refining the figures and tables, and further clarifying the conclusions and points of innovation (see the replies to the following comments).

1. Comment:

Use consistent terminology throughout the manuscript, including "freeze–thaw processes," "freeze–thaw periods," "soil water," "shallow groundwater," "recharge source," and "recharge pathway."

Reply: We will carefully review the manuscript to ensure consistent use of "freeze–thaw processes," "freeze–thaw periods," "soil water," "soil water content (SWC)," "shallow groundwater," "recharge sources," and "recharge pathways."

2. Comment:

Refine the novelty and practical implications. The manuscript should more explicitly identify what is new relative to previous isotope-based studies of groundwater recharge in the Qinghai–Tibet Plateau. The practical implications for groundwater-resource management, ecohydrological protection, or river-baseflow maintenance in the Qinghai Lake Basin should also be stated more specifically.

Reply: We will further synthesize the deficiencies in previous research and clarify the innovations of this study in the Introduction of the revised manuscript. We will also introduce a new Section 4.4 to explicitly discuss its practical implications for groundwater-resource management, ecohydrological conservation, and sustaining river baseflow in alpine regions, including the Qinghai Lake Basin.

3. Comment:

Line 83: “united states” should be capitalized: “United States.”

Reply: We will correct “united states” to “United States” in the revised manuscript and carefully check capitalization throughout the manuscript.

4. Comment:

Lines 83-84: Grammatical error. “snowmelts” is not a verb form. Please revise to “when snow melts and then refreezes in mid-winter.”

Reply: We will revise the sentence to “when snow melts and then refreezes in mid-winter” and carefully check the manuscript for similar grammatical errors.

5. Comment:

Line 106: This phrasing is informal. Consider replacing with “In the Three-Rivers Headwaters Region, for example,” for a more formal academic register.

Reply: We will revise “Taking the Three-Rivers Headwaters Region as an example” to “In the Three-Rivers Headwaters Region, for example,” to better suit a formal academic style.

6. Comment:

Line 107: “Younger” refers to mean residence time or isotopic age, but this is not explained. Please briefly clarify what “younger” means in this context (e.g., shorter mean transit time).

Reply: We will explicitly state in the revised manuscript that “younger” denotes a shorter mean residence time in this context, corresponding to a shorter water transit time, and add this clarification where appropriate.

7. Comment:

Lines 121-122: Please revise to past tense: "we established a comprehensive ecohydrological monitoring network within the QLB."

Reply: We will revise the sentence to "We established a comprehensive ecohydrological monitoring network within the QLB" and will check the manuscript to ensure that descriptions of completed work are consistently written in the past tense.

8. Comment:

Line 165: Missing comma after "2015" and after "2023". Please standardize to "Peng et al., 2015; Li et al., 2022; Peng et al., 2023."

Reply: We will revise all citations in the revised manuscript to conform to the format "Peng et al., 2015; Li et al., 2022; Peng et al., 2023" and carefully review the punctuation across the entire manuscript.

9. Comment:

Lines 187-190: This sentence appears at the end of the sampling section but describes a spatial classification that affects the entire study. It would be better placed earlier in Section 2.1 when the study area is introduced.

Reply: In the revised manuscript, we will move the sentence "Considering the influences of elevation, topography, and groundwater burial depth, the spatiotemporal variations in this study are classified into the downstream basin and middle and upper reaches of the basin" to Section 2.1, "Study area."

10. Comment:

Line 290: "its R^2 decreased" — decreased relative to what baseline or period? Please specify the comparison explicitly.

Reply: We will explicitly state in the revised manuscript that this R^2 value is compared with that of the thawing period and revise the sentence to read: "The R^2 value during the thawed period was lower than that during the thawing period"

11. Comment:

Line 296: Spaces are missing in the legend entries, e.g., "SW 0–30cm" should be written as "SW 0–30 cm".

Reply: We will revise "SW 0–30cm" in the figure legend to "SW 0–30 cm" and carefully examine the full manuscript for other instances of missing spaces.

12. Comment:

Lines 300-323: The SWC values are reported to two decimal places (e.g., 26.90%, 28.41%), implying a precision that may not be justified given the spatial heterogeneity of field measurements. Please report values to one decimal place.

Reply: We will ensure that all SWC values are reported to a consistent number of decimal places in the revised manuscript.

13. Comment:

Line 378: The unit "‰" is incorrect here; SWC is expressed as a percentage (%). Please correct to "26.90%–31.05%."

Reply: We will revise the units of all SWC measurements in the revised manuscript from "‰" to "%".

14. Comment:

Line 388: Throughout the manuscript, contribution values are reported to two decimal places (e.g., 13.01%, 9.33%). This value should be given as "10.60%" for consistency.

Reply: We will revise "10.6" to "10.60%" in the revised manuscript and review the number of decimal places reported for all contribution values throughout the manuscript to ensure a consistent format.

15. Comment:

Lines 423-426: "This is consistent with results from modeling analyses indicating that spring hydrological processes in the alpine regions of the QTP are mainly governed by a combination of active layer temperature, thaw depth, and SWC" — no citation is provided for this modeling result. Please add the appropriate reference

Reply: We will add the appropriate reference(s) supporting this modeling result and review the manuscript to ensure that all statements based on previous studies are adequately cited.

16. Comment:

Line 469: Decimal places should be kept consistent throughout Figure 8.

Reply: We will verify and harmonize the decimal places of all numerical values in

Figure 8 in the revised manuscript to ensure consistency.

<u>17. Comment:</u>

Line 474: “RWL” has not been previously defined in the manuscript. Please define this abbreviation on first use.

Reply: We will correct the abbreviation “RWL” to “SWL” and define “soil water level (SWL)” at its first occurrence.

<u>18. Comment:</u>

Lines 492-495: Consider restructuring as: “Observations in an alpine meadow further confirm that by regulating active layer thickness, FT processes significantly alter the proportions of piston flow and preferential flow (Musa et al., 2016; Li et al., 2026b).”

Reply: We will revise the sentence as follows: “Observations in an alpine meadow further confirm that FT processes significantly alter the proportions of piston flow and preferential flow by regulating active-layer thickness (Musa et al., 2016; Li et al., 2026b).”