Dear Editorial Office of HESS and Prof. Dai

We wish to express our sincere gratitude for the editorial team's diligent handling of our manuscript and extend particular appreciation to Prof. Dai for your judicious oversight throughout the review process. Your constructive decision letter has provided us the opportunity to enhance the manuscript's scientific rigor. In response to the insightful comments from two reviewers and two domain experts, we have thoroughly revised the manuscript. All the revised contents have been marked in **red** in the manuscript.

Major revisions include:

1. Correction of sample collection time.

2. Use "groundwater" instead of "geothermal water" to define the sample in this study.

3. Historical EAFZ hydrogeochemical data were collected.

4. The Sr isotopes of the groundwater samples were analyzed.

5. Modified the title.

6. We redrew all the Figures.

7. We reorganized the Abstract, Introduction, Discussion and Conclusions.

8. We supplement the analysis method and data quality control description.

9. We have made explanations of some misunderstandings.

10. The estimation of the temperature of 382℃ was deleted.

11. The simulation of the saturation index of anhydrite was added.

12. Fig. 6 (spatial distribution characteristics) in the original draft has been deleted.

13. Supplemented the evidence of geothermal gas in EAFZ.

Key improvements are summarized below:

1. Enhanced Data Completeness

Historical Data Integration: We systematically compiled published data (2013-2023), revealing spatial hydrogeochemical zonation in the East Anatolian Fault Zone (EAFZ):

Northern Segments: Mixed shallow/deep circulation with igneous rock-dominated water-rock interactions.

Central-Southern Segments: Shallow circulation dominated by sedimentary mineral dissolution (e.g., anhydrite, carbonates), with localized seawater influence.

Causal Linkage Clarification: PHREEQC simulations quantify anhydrite's contribution to SO_4^{2-} anomalies (30-100%).

2. Refined Anhydrite-Tectonic Linkage

Terminological Precision: Removed all "seismic precursor" claims, replacing with "indicator of water-rock interaction intensity".

Mechanistic: Combined pre-earthquake macroscopic anomalies. The analysis of post-earthquake data and historical data proves that anhydrite may be one of the causes of groundwater macroscopic anomaly

Explicit caveat: "Causal links between anhydrite dynamics and tectonics require long-term validation"

3. Revised Conclusions

Restructured Key Findings:

"Anhydrite abundance serves as a sensitive indicator of water-rock interaction intensity, potentially modulated by tectonic activity. Establishing fault-zone hydrogeochemical baselines is prerequisite for deciphering tectonic-hydrologic coupling."

In short, after fully and effectively communicating with the reviewers, we modified the possible problems in our manuscript according to the suggestions of the reviewers, so that the analysis of data in the manuscript is more rigorous and the extension is appropriate

We sincerely wish the current version meets your standards and welcome further guidance.

Finally, I would like to thank HESS editorial Department and Dai Editor-in-Chief for their hard work

Sincerely

Zebin Luo

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Point-by-point response to comments:

Note: ***Italic blue*** is the comment. Black is the reply, and **important sentences are bolded**. **Red** indicates the position of the modification information in the newly submitted revised draft.

Reply to referee comments

RC1: 'Comment on hess-2024-395', Walter D'Alessandro, 13 Jan 2025

Dear Walter D'Alessandro

After two in-depth discussions with you, we have gained a new understanding of the viewpoints in the initial manuscript. This is mainly attributed to your highly professional and constructive opinions and suggestions, which are of great value to us in improving the quality of the manuscripts. After carefully reading your comments, we have made a reply to your comments point-by-point under the discussion of all the article authors.

The manuscript "Gypsum as a potential tracer of earthquake: a case study of the Mw7.8 earthquake in the East Anatolian Fault Zone, southeastern Turkey" by Luo et al. presents the results of sampling campaign of groundwaters in the area of the two strong earthquakes that hit heavily Turkey in February 2023. Only the analytical results (major ions, trace elements and water isotopes) of samples collected about one year after the quakes are considered, which is a strong limitation of this study. I feel that this study cannot be published in this form.

Reply: Thanks. We sincerely appreciate your critical observation regarding the sampling timeline. Please allow us to clarify and substantiate our findings through the following revisions:

Critical Data Correction: Amended the erroneous "one-year post-seismic" description to "one-month post-seismic" (March 23, 2023) throughout the manuscript, with updated field logs in Table 1 (line *160*).

Enhanced Geochemical Evidence: Conducted radiogenic strontium isotope analyses (**$^{87}Sr/^{86}Sr$**) (New Fig. 5) (lines *249-255*): Central-southern segment samples show ratios showing the characteristics of multi-source region mixing (0.7053-0.7135). PHREEQC modeling confirms 30−100% sulfate contribution from anhydrite dissolution. (New Fig. 6 (*256-260*) and Section 5.2 (lines *261-322*)).

Tectonic-Hydrogeochemical Zonation: Integrated 2013-2023 datasets reveal:

Northern EAFZ: 0−7% magmatic fluid input (New Fig. 6 (lines *256-260*))

Central-Southern EAFZ: Shallow groundwater dominance (New Fig. 6 (lines *256-260*))

Water-rock interactions governed by: Evaporite dissolution (anhydrite→SO_4^{2-}) (New Fig. 6 (lines *256-260*)), Ophiolite weathering (Mg^{2+} anomalies) (Table 1 (line *160*)), Carbonate equilibria (Ca-HCO_3 type) (New Fig. 2 (lines *142-146*))

Pre-Seismic Anomaly Validation:

Documented anomalies at HS04/HS14 (Supplement Video 01 and 02).

Proposed mechanism: "Preseismic fault creep → permeability enhancement → accelerated anhydrite dissolution → hydrogeochemical/physical anomalies" (Section 5.3, New Fig. 9 (lines *348-398*)).

These enhancements rigorously position anhydrite as a sensitive indicator of water-rock interaction intensity while respecting observational boundaries. We fully endorse the need for long-term monitoring.

Major comments:

Lines 33-36 (abstract): This is one of the most critical claims made by the authors. "Specially, significant gypsum dissolution was observed at HS05, HS09 and HS14 before and after the earthquake, suggesting that the earthquake broke the balance of water-rock reaction and promoted the dissolution of gypsum." In the paper only the results of the analyses of the samples taken one year after the earthquakes are discussed. How should it be possible to evidence variations "before and after the earthquake" if only one sample was taken?

Reply: Thanks. a mentioned earlier, we combined the historical observation data of EAFZ with this study. Based on the supplementary evidence, the conclusion of anhydrite dissolution can be supported (New Fig. 6 (lines *256-260*)).

Line 124: The authors should explain on which basis the 16 sampling sites have been chosen.

Reply: Thanks. We added the description of the sampling point: "**HS01-HS04 was collected from west to east along SF. HS07-HS16 was collected from north to south along EAFZ (Fig. 1)**" (lines *106-107*)

Line 124: the authors claim to have sampled hot springs but with the exception of the peculiar hyperalkaline spring HS15, which derive its increased temperature from deep circulation, no other sample could be called "hot". Furthermore, I would not define a well with water at 24 °C as geothermal well. Actually, in the results (line 144) the authors affirm that temperatures of the sampled waters are low.

Reply: Thanks. Indeed, the temperature of all samples in this study is low, indicating that EAFZ is a medium-low temperature hydrothermal system, which is also consistent with the research results of Baba et al., 2019. However, as you said, the temperature of the sample is really low. We also feel that the term "**geothermal water**" is not rigorous enough to describe our samples. Therefore, we considered using the more appropriate term "**groundwater**" to describe our samples.

The methodological section has many limitations:

Lines 130-131: it is unclear if filtration has been made in the field and before acidifying the aliquot for cation analysis. Please specify

Reply: Thanks. Yes, we confirmed filtering before testing. We added the description of the sampling point: "**All samples need to be pre-treated with a 0.45 μm filter membrane to remove impurities before sampling.**" (lines *111-112*)

Line 131: MAT 253 is a model, please specify the used technique

Reply: Thanks. We have added specific analytical method: "**The Hydrogen and oxygen isotopes were determined by a Picarro L2140-I Liquid water and vapor isotope analyzer (relative to Vienna Standard Mean Ocean Water (V - SMOW)). Precisions on the measured $\delta^{18}O$ and δD value was ±0.2% (2SD) and ±1% (2SD) respectively (Zeng et al., 2025)**" (lines *113-115*)

Line 133: please specify the analysed species and the relative reproducibility and detection limits?

Reply: Thank you for pointing out the problem of the manuscript. We have added the reliability description of hydrochemistry and isotope analysis to the chapter of **Analytical methods**, the details are as follows:

The cation (**Li^+, Na^+, K^+, $Ca^{2+}$and Mg^{2+}**) and anion (**F^-, Cl^-, NO_3^- and SO_4^{2-}**) were analysed by Dionex ICS-900 ion chromatograph (Thermo Fisher Scientific Inc.) **at the Earthquake Forecasting Key Laboratory of China Earthquake Administration, with the reproducibility within ±2% and detection limits 0.01 mg/L (Chen et al., 2015)**. HCO_3^- and CO_3^{2-} was determined by acid-base titration with a ZDJ-100 potentiometric titrator **(reproducibility within ±2%)**. SiO_2 were analysed by inductively coupled plasma emission spectrometer Optima-5300 DV (PerkinElmer Inc.) **(Li et al. 2021)**. Trace elements were analysed by Element XR ICP-MS **at the Test Center of the Research Institute of Uranium Geology**. Multielement standard solutions (IV-ICPMS 71A, IV-ICP-MS 71B and IV-ICP-MS 71D, iNORGANIC VENTURES) used for quality control. The analytical error margin of major cations and trace elements were less than 10%. **Strontium isotope ratios ($^{87}Sr/^{86}Sr$) were determined through triple quadrupole ICP-MS (Agilent 8900 ICP-QQQ) with a precision of ±0.001 (Liu et al., 2020).** (lines *115-126*)

Line 136: please specify the analysed trace elements and the relative reproducibility and detection limits?

Reply: Thanks. We added the description: “Trace elements were analysed by Element XR ICP-MS **at the Test Center of the Research Institute of Uranium Geology**. Multielement standard solutions (IV-ICPMS 71A, IV-ICP-MS 71B and IV-ICP-MS 71D, iNORGANIC

VENTURES) used for quality control. The analytical error margin of major cations and trace elements were less than 10%." (lines *121-125*).

In the results the authors claim often that some element or ionic species is increased (sometimes adding obviously) but they do not specify with respect to what. Maybe they intend that the concentrations are high.

Reply: Thanks. In the Results section we are an objective description of the results based on the data. The words "increased" and " obviously " were also relative to other sample results. But, in fact, what we mean is, "relatively high," not " increased." We apologize for any confusion caused by the poor description of the results, and we have re-optimized the presentation and added a quantitative description of the increased concentrations. The revised expression is as follows:

The concentration of SO_4^{2-} range from 1.21 mg/L to 316.61 mg/L, and the concentration of SO_4^{2-} in some samples is relatively high (e.g. HS01 (287.74 ml/L), HS03 (103.56 ml/L), HS04 (229.75 ml/L), HS14 (316.61 ml/L)). (lines *133-135*).

In the same section they speak of geothermal water but they do not present any evidence that these are geothermal waters.

Reply: Thank you. We have replaced "**groundwater**" with "**geothermal water**" to make the expression more precise.

The discussion about the geothermal fluids has great limitations.

The authors do not present evidences that the sampled waters are, at least partially, fed by hydrothermal systems. The fact that in the area some geothermal system has been discovered and studied, does not mean that all groundwater samples taken in the area are *fed by them. The temperatures of the collected samples are low and, as highlighted by the binary diagram of fig. 3 and the ternary diagram of fig. 4, their compositions do not reflect high temperature interactions with the rocks. Also the silica geothermometers show low temperatures considering that for such systems equilibrium with chalcedony (or even christobalite or amorphous silica) should be taken into consideration.*

Reply: Thanks. We have already discussed this issue in the previous reply. **Hydrothermal systems** and **groundwater** do not affect our core point. Both geothermal water and groundwater chemical anomalies are considered to be effective means of earthquake early warning. Thanks for your suggestion to us, as mentioned earlier, we have considered using "**groundwater**" instead of "**geothermal water**" to define the samples for this study.

Especially the use of the mixing models has been made in the wrong way. Mixing models can be applied only to water samples that belong to the same system and not to water samples collected tens of km away from each other and for which no connection has been demonstrated.

Reply: Thanks. In accordance with your suggestions, the revised manuscript now includes segmented descriptions of the EAFZ: Northern segments show magmatic fluid mixing, Central-southern segments (where our samples were collected) exhibit shallow groundwater circulation dominated by water-rock interactions with anhydrite, carbonates, and ophiolites (New Fig. 6 (lines *256-260*)) (section 5.2 The groundwater circulation in different segments of EAFZ (lines *261-322*)) .

We have abandoned the 382°C temperature estimation, which may have been overestimated. Nevertheless, under the measured reservoir temperatures at HS04 (156°C) and HS14 (243°C), anhydrite remains supersaturated, confirming the validity of its dissolution interpretation (New Fig. 7 (lines *287-292*)).

The estimation of temperature for the "deep geothermal fluid" (please define) of 382 °C is absolutely unreliable. The sample was taken, as shown in the second video in the supporting information, from an artesian well (although in table 1 it is classified as spring). I think it is impossible that an artesian well, whose upflow is generally rapid, would have only 15 °C temperature if even only a small part of the water would come from a geothermal system with 382 °C.

Reply: Thanks. We have abandoned the 382°C temperature estimation, which may have been overestimated. Nevertheless, under the measured reservoir temperatures at HS04 (156°C) and HS14 (243°C), anhydrite remains supersaturated, confirming the validity of its dissolution interpretation (New Fig. 7 (lines *287-292*)).

The discussion about the sulfate anomalies is highly confusing. Many points are unclear or wrong.

Reply: Thanks. We adjusted the description of the manuscript to make the logic clearer. (lines *277-322*)

Why are only samples HS05, HS09 and HS14 considered anomalous? HS01, HS03 and HS04 have also elevated sulfate values.

Reply: Thanks. Thank you for your correction. The PHREEQC simulation indicates that the anhydrous anhydrite of HS01, HS03, HS04 and HS14 is all supersaturated (New Fig. 7 (lines *287-292*)). Therefore, we deleted the relevant inappropriate descriptions.

Why should these high sulfate values be considered anomalous and induced by the earthquake? Sulfate dissolution from evaporite deposits within the aquifers is an ubiquitous process independent from seismic activity.

Reply: The dissolution of anhydrite can indeed occur independently of seismic activity. In our newly submitted manuscript, based on this research, we only emphasized that seismic activity is one of the reasons affecting the solubility of anhydrite, rather than the only one. For this reason, we also suggest conducting long-term monitoring at appropriate monitoring points, hoping to distinguish the influence of earthquakes from other factors (such as precipitation). (lines *387-398*)

Why do the authors use these low averages for Ca (55.23 mg/L) and SO_4 (8.31 mg/L) concentrations before earthquake? Baba et al. (2019) in their paper report concentrations up to 773.56 mg/L for Ca and up to 1287.24 mg/L for SO_4 much higher than in the samples collected for this study.

Reply: Thanks. This issue no longer exists in the newly submitted manuscript. We have deleted old Fig. 6 from the original manuscript. Instead of discussing the data comparison before and after the earthquake, we directly analyze the historical data of EAFZ. (Table S1 at Supporting Information)

Finally, the authors indicate the whitening and turbidity of the water in a sample as verification for the sulfate anomaly. But without analysis there is no possibility to affirm that such visual anomaly was due to gypsum dissolution.

Reply: Thanks. Although we did not directly measure the turbid water samples, based on the historical data analysis of EAFZ, we can determine that the anhydrite layer exists in the middle and southern sections (New Fig. 6 (lines *256-260*)) (Table S1 at Supporting Information). Based on historical data and this study, it can be concluded that the cause of the macroscopic anomaly before the earthquake is the dissolution of minerals such as anhydrite and carbonate.

Furthermore, the authors mistake the samples. The site with the high sulfate concentration is HS14, while the site to which the pictures of figure S1 and of video 01 refer is HS15 which has the lowest sulfate value (1.21 mg/L).

Reply: Thanks. Thank you for pointing out this error, we have fixed it (Table 1 (lines *160-161*)).

Lines 388-389: The authors presenting the data of a single sampling campaign have no evidence to affirm that "the geothermal fluid was diluted due to the infiltration of a large amount of shallow cold water after the double earthquakes in February 2023".

Reply: Thanks. The newly submitted manuscript has supplemented evidence such as EAFZ historical data and Sr isotopes. The revised manuscript can support the conclusion that anhydrite is used as a sensitive index for the intensity of the water-rock reaction. Thank you for your highly professional and constructive comments. Thanks again.

Minor comments

Line 22: What do the authors mean with "systematic" which do not appear only in the abstract but has been repeated many times in the whole text?

Reply: Thanks. We rewrote the Abstract and have deleted this word. (lines *15-28*)

Lines 24 and 25: The meaning of the sentence is obscure (reconstructed by earthquake?)

Reply: Thanks. We rewrote the Abstract and have deleted this sentence. (lines *15-28*)

Line 29: the authors use often the term "abnormal" but they do never define with respect to what.

Reply: Thanks. We rewrote the Abstract and have deleted this sentence. (lines *15-28*)

Line 38: please define "shallow minerals".

Reply: Thanks. We rewrote the Abstract and have deleted this sentence. (lines *15-28*)

Line 61: which evidence have the authors of a "geothermal fluids circulation"

Reply: Thanks. We have replaced "groundwater" with "geothermal water".

Line 69: please define the "geothermal fluid anomaly index"

Reply: Thanks. We rewrote the introduction and have deleted this sentence. (lines *31-66*)

Lines 70-71: the subject is missing in this sentence.

Reply: Thanks. We rewrote the introduction and have deleted this sentence. (lines *31-66*)

Line 82: please define what a "tectonic collage" is.

Reply: Thanks. We have adjusted the expression of this sentence: "**Located at the intersection of Eurasia, Africa and Arabia, Turkey has a complex tectonic background**". (lines *73*)

Fig. 1a: altitude scale is missing.

Reply: Thanks. We added the altitude scale (New Fig. 1 (lines *67-71*)).

Line 105: probably crystalline instead of crystallization.

Reply: Thanks. We changed crystalline instead of crystallization. (lines *88*)

Line 145: in table 1 HS15 is considered a spring, which one is correct?

Reply: Thanks. We checked the sampling point. HS15 is spring. (Table 1 (lines *160-161*))

Line 146: the authors claim that "the closer to the epicenter, the higher the SiO_2 content", which makes no sense. Firstly because the earthquakes were two and only one sample close to one of the epicenters has a higher SiO_2 value. Moreover, other two sampling points with low to very low SiO_2 concentrations have the same position as the "anomalous" one.

Reply: Thanks. We deleted that sentence.

Lines 154-156: the sentence "The δ18O and δD of samples varied from –11.30‰ to –6.55‰ and –65.43‰ to –34.43‰ respectively, which is near to the global meteoric water line (GMWL) (Craig, 1961) (Fig. 3), suggesting their meteoric water origin" has no sense. The regression line obtained plotting both $\delta^{18}O$ and δD values in a graph can be close to GMWL.

Reply: Thanks. We deleted that sentence.

Line 159: what type of Statistical analysis?

Reply: Thanks. We have changed the word "statistical analysis" to "box-plot analysis" to make the expression more specific. (lines *149*)

Line 160: please define "fluid activity elements".

Reply: Thanks. We adjusted the expression and used proper nouns: Fluid-mobile element (FME). (lines *149-151*)

Line 161: I do not understand what the authors mean with "are at historic highs versus". If the authors mean that the concentrations are higher than in the past, then the fig. S2 does not prove nothing. Al and Ba are below the median value of the literature data while the remaining are around the median value not showing particularly high values. Furthermore, it is unclear which data are compared in fig. S2 with the present data.

Reply: Thanks. We rewrote this sentence. “Box plot analysis showed that the Fluid-Mobile Element (FME) concentrations of B (3.62–1047.25 μg/L), Li (0.33–89.93 μg/L) and Rb (0.14–28.91 μg/L) in some samples were greater than the median (Fig. S1)”. (lines *149-151*)

Table 1: please indicate the coordinates with at least 4 digits after the comma, with only two digits it’s impossible to obtain a reliable position. Looking at Fig. 1, the indicated coordinates of HS05 are clearly wrong.

Reply: Thanks. We adjusted the accuracy of the latitude and longitude to keep 6 decimal places. (Table 1 (lines *160-161*))

Line 190: the highest values do not belong to samples collected closer to the sea.

Reply: Thanks. It's not rigorous enough. We've improved the sentence: “**Notably, groundwater in the southern EAFZ proximal to the Mediterranean Sea exhibits progressively heavier isotopic signatures toward the coast, consistent with recharge sourced from evaporated Mediterranean seawater**” (lines *189-191*)

Line 190: $\delta^{18}O$ and δD values are inverted.

Reply: Thank you. We rewrote the first part of the discussion and have deleted this sentence. (lines *165-260*)

Line 212: magma mixing with geothermal fluids generally end in a volcanic explosion which is not the case here.

Reply: Thank you. We rewrote the first part of the discussion and have deleted this sentence. (lines *165-260*)

Lines 224-225: the sampling sites are tens of km far from the Mediterranean coastline, how and why should they be "obviously contaminated by Mediterranean Sea water"?

Reply: Thank you. We rewrote the first part of the discussion and have deleted this sentence. (lines *165-260*)

Line 226: which previous study? Please add a reference.

Reply: Thank you. We rewrote the first part of the discussion and have deleted this sentence. (lines *165-260*)

Line 233: pollution is a term connected to an anthropogenic origin, so please use the term contamination instead.

Reply: Thank you. We rewrote the first part of the discussion and have deleted this sentence. (lines *165-260*)

Lines 233-236: I do not understand the meaning of this sentence.

Reply: Thank you. We rewrote the first part of the discussion and have deleted this sentence. (lines *165-260*)

Lines 290-292: the two processes are not alternative. Serpentinization includes secondary minerals precipitation.

Reply: Thanks. We rewrote the section on the water-Rock reaction and have deleted this sentence. (lines *262-292*)

Finally, I would signal a possible conflict of interest being the handling editor of the same institution of one the corresponding author.

Reply: Thanks. China University of Geosciences (Beijing) and China University of Geosciences (Wuhan) are two independent universities with no conflict of interest.

RC2: 'Reply on AC3', Walter D'Alessandro, 06 Feb 2025

Dear Walter D'Alessandro

Thanks for your comments again. According to your comments, we added the supplement and analysis of the literature data from 2013 to 2023 to make the data more representative. On this basis, the conclusion of the original manuscript has been revised to weaken the connection between Anhydrite and seismic activity, and emphasize the sensitive indication of Anhydrite to the intensity of water-rock interaction. The main replies are as follows.

I am sorry to say that reading the reply of the authors my opinion regarding the manuscript did not change. My main criticism relates to the fact that it is not possible to evidence anomalies in groundwater composition related to seismic events having data collected only one time. The authors try to compare their data with other taken from literature but the comparison is not straightforward because no background values have ever been defined. The mean values utilised seem artificially created and, in my opinion, do not represent "normal" values.

I am still convinced that the manuscript in this form has to be rejected.

Reply: Thanks! We sincerely appreciate your critical feedback and fully acknowledge the limitations of single-time sampling in establishing seismic-hydrogeochemical correlations. To address this concern rigorously, we have implemented the following revisions:

1. Investigation and analysis of historical hydrogeochemical data in the study area ((New Fig. 6 (*256-260*)): A comprehensive compilation of groundwater chemistry data

from the East Anatolian Fault Zone (EAFZ) spanning 2013-2023 has been integrated. This reveals systematic spatial hydrogeochemical patterns:

Northern EAFZ: Mixed shallow/deep circulation with igneous rock-dominated water-rock interactions.

Central-Southern EAFZ: Shallow circulation dominated by sedimentary mineral dissolution (e.g., Anhydrite, carbonates), with localized seawater influence.

These distinct regimes provide a robust framework for interpreting tectonic-hydrogeochemical linkages, mitigating reliance on isolated measurements.

2. Revised Interpretation of Anhydrite Significance:

Following your suggestion, we have reframed the role of Anhydrite dissolution. Rather than asserting direct seismic causality, we now propose Anhydrite as a sensitive indicator of water-rock interaction intensity – a process modulated by both climatic (e.g., rainfall) and tectonic drivers. This rephrasing: (1) Removes overinterpretations of single-event correlations, (2) Highlights the need for future systematic monitoring to disentangle tectonic vs. hydrological signals. Preserves Anhydrite's potential as a tectonic proxy while adhering to evidence-based claims.

These revisions align the manuscript's conclusions with its evidentiary scope while preserving its novel contribution: establishing a spatially resolved hydrogeochemical baseline to guide future seismotectonic monitoring in the EAFZ. We are grateful for your insightful critique, which has significantly strengthened the study's rigor and communication of limitations.

For detailed revisions, please refer to the **discussion section of the revised manuscript**.

We have reorganized the logic of the discussion. Firstly, we determined the source of groundwater. Secondly, we analyzed the circulation process of groundwater. Finally, we introduced the relationship between the change in groundwater ion concentration and the change in the intensity of water-rock reactions caused by earthquakes. (lines *164-398*)

The data could be used to create a simply report without stressing the potential of gypsum as earthquake tracer. The data could be used for future researches in the area. I don't know if there is a form in which this could be done for this journal. Maybe the editor can suggest solutions.

Reply: Thanks! We thank you for your constructive suggestion to refocus the manuscript's scope. In accordance with your guidance, we have rigorously revised the narrative to prioritize hydrogeochemical process characterization over speculative seismological linkages:

Reframed Research Objectives: The study's primary aim is now explicitly stated as establishing hydrogeochemical signatures across the EAFZ's tectonic segments. All claims regarding earthquake precursory signals have been removed, with emphasis shifted to documenting spatial patterns in water-rock interaction processes. A new statement clarifies that Anhydrite's tectonic relevance requires validation through future systematic monitoring, aligning with your call for caution in interpretation.

These modifications ensure the manuscript now functions as both a stand-alone hydrogeochemical benchmark study and a catalyst for hypothesis-driven seismic monitoring research. We fully defer to the Editor's judgment on whether this revised scope aligns with the journal’s aims and welcome further adjustments if needed.

Comments on authors’ reply

Line 13: to affirm that you have measured abnormal groundwater ion concentrations you need to compare them with a series of data before and after the seismic event. Evaporite dissolution happens also in the absence of seismic activity, it is therefore impossible to affirm that high sulfate concentrations in groundwater are related to the earthquakes

Reply: Thanks! We deeply appreciate your rigorous methodological critique regarding causality attribution. The revisions below directly address this fundamental concern: After more than a month of research, we have a new understanding of the conclusions in the original draft. Indeed, even with video data of pre-earthquake macroscopic anomalies, it is difficult to form a complete causal chain in the absence of pre-earthquake data. After in-depth discussion by all co-authors, we propose that our data can only account for the dissolution of Anhydrite during the water-rock reaction. Anhydrite may therefore indicate changes in the intensity of the water-rock reaction. As for the controlling factors of the variation of water-rock reaction intensity, we cannot define exactly. Considering that the sampling time was one month after the earthquake and obvious groundwater anomalies were observed before the earthquake, we believe that seismic activity may affect the variation of water-rock response intensity. Therefore, it is necessary to further study the possibility of Anhydrite as a tracer of tectonic activity.

Line 44: even if sampled one hour after the earthquake my comment would have been the same. If you don't have data of at least one other sampling, but ideally many *samplings covering different seasons both before and after the event, you cannot make inferences on the effects of the earthquake on the water chemistry*

Reply: Thanks! a mentioned earlier, we combined the historical observation data of EAFZ with this study. Based on the supplementary evidence, the conclusion of anhydrite dissolution can be supported (New Fig. 6 (lines *256-260*)).

Line 47: your data before the earthquake do not refer to the single sites you sampled, so no comparison can be made

Reply: Thanks! This issue no longer exists in the newly submitted manuscript. Instead of discussing the data comparison before and after the earthquake, we directly analyze the historical data of EAFZ. (Table S1 at Supporting Information)

Lines 48-51: no one can deny the existence of a large suite of visible effects of seismic activity on groundwaters but for the advancement of knowledge these have to be described in detail and quantified. You cannot use the simple fact of a water whitening (among other things also confusing the sites) claiming this was due to gypsum dissolution without having the possibility to analyse the water chemistry

Reply: Thanks! After analyzing 10 years of data in study area, we determined that the main controlling factor of the macro anomaly is Anhydrite, and there may also be the influence of Calcite, albite, potassium feldspar, etc.

Lines 52-59: of course I agree that both Sr and S isotopes can be used as good source indicators. But again if you have a single measurement you cannot make any inference about the influence of the earthquake on the groundwaters

Reply: Thanks! We conducted Sr isotope analysis on the research samples. The measured $^{87}Sr/^{86}Sr$ ratios (0.7053–0.7135) across EAFZ groundwaters reflect multi-source mixing processes. Central-southern groundwaters integrate signatures from: Shallow aquifers: Inheriting Sr from local lithologies (ophiolites); Modern seawater: $^{87}Sr/^{86}Sr$ = 0.7092–0.7096 (Mediterranean seawater); River inputs: Enriched ratios (>0.710) from silicate weathering. Binary mixing models using $^{87}Sr/^{86}Sr$ vs. Ca/Sr ratios (Fig. 5) quantify source contributions: Carbonate weathering dominates, consistent with $Ca\text{-}HCO_3$ hydrochemical type; Ophiolite contributions <10% (except Mg2+-rich samples near ultramafic outcrops); Evaporite dissolution contributes 0–20% (≤50% in localized high-SO_4^{2-} zones). Sr isotope framework corroborates earlier findings of shallow-dominated circulation in central-southern EAFZ. (line *238-248*)

Lines 75-78: You compared samples from three of your sampling sites with samples taken at the same sampling sites about ten years before. Results: one site registered a strong increase, another remained almost stable and the third one had a sharp decrease. You still cannot be sure that the changes are related to the earthquake, you have to exclude other possible processes. For example, do the composition of the groundwaters change seasonally? Has the composition of the water decadal trends related to long periods of drought or water exploitation? Does the well tap aquifers from different levels with different composition and permeability that mixing in the well may change the composition of the water during pumping?

Reply: Thanks! We think your question about the manuscript is something we must take into account. Therefore, we give up the original conclusion and discuss the relationship between Anhydrite and water-rock reaction intensity instead. (lines *164-398*)

Lines 89-91: this seems a forced solution. The selected samples contain all very low sulfate which seems not necessarily being representative of the whole study area. Two out of 8 selected samples are hyperalkaline waters which for their nature contain extremely low sulfate values due to their very negative redox potential. Furthermore, why didn't you include also the data of Yuce et al 2014? The mean sulfate value of that dataset would be 121 mg/L, more than an order of magnitude higher than that obtained with the ad hoc solution from the Baba et al dataset.

Reply: Thanks! Your advice has been of great help to us. According to your suggestion, we have collected and analyzed the data of the last 10 years. The results confirmed the dissolution of Anhydrite in the middle and south section. (New Fig. 6 (lines *256-260*))

Lines 120-121: the reliability of the data has not been questioned but the representativeness still remains doubtful

Reply: Thanks! In order to make the study more representative, the data of the study area in the past 10 years are used to discuss the water-rock reaction process. (Table S1 at Supporting Information)

Line 130: A nearly 1000 km tectonic system cannot be considered a single hydrothermal system

Reply: Thanks! As you said, it is really not a system. The north section is a mixture of shallow groundwater and deep fluids, and igneous rocks participate in water-rock reactions. The central and southern part is the mixing of shallow groundwater and seawater, and sedimentary minerals such as Anhydrite participate in water-rock reaction. (lines *164-398*)

Lines 135-142: the cited examples of studies which identified changes in groundwater composition related to earthquake are well known. But differently from your study, the researcher took tens of samples before the seismic events obtaining a clear signal that can be related to the earthquake

Reply: Thanks! Although we do not have pre-earthquake data, considering that we have observed pre-earthquake macro anomalies, coupled with the analysis of all data from the study area in the past 10 years. We believe that the data are sufficient to support our revised conclusion that Anhydrite can be used as a tracer of the intensity of water-rock reactions, and it is necessary to further investigate the possibility of Anhydrite as an indicator of tectonic activity.

Line 149: You did not answer to my question. Have the samples been filtered in the field and before acidification?

Reply: Thanks! Yes, we confirm. We added the description of the sampling point: "**All samples need to be pre-treated with a 0.45 μm filter membrane to remove impurities before sampling.**" (lines *111-112*)

Lines 170-171: if the filtration is not made at the time of sampling you may loose some of the dissolved metals due to precipitation of secondary minerals and/or to adsorption on the walls of the container. Furthermore, if filtration is made after acidification the result may be falsified by acid dissolution of suspended material

Reply: Thanks! We are responsible for all sample collection, pre-processing and data quality

Line 172: this method is used only for δD

Reply: Thanks! We have added specific analytical method: "**The Hydrogen and oxygen isotopes were determined by a Picarro L2140-I Liquid water and vapor isotope analyzer (relative to Vienna Standard Mean Ocean Water (V - SMOW)). Precisions on the measured $\delta^{18}O$ and δD value was ±0.2% (2SD) and ±1% (2SD) respectively (Zeng et al., 2025)**" (lines *113-115*)

Lines 225-226: You cannot consider a nearly 1000 km long fault system as a single continuous structure. Furthermore, the complex geology of the area changes frequently the rock types present along the fault system. Add also the changing climatic and hydrologic conditions and you cannot consider samples collected many tens of km apart as pertaining to the same system.

Reply: Thanks! As you said, it is really not a system, we have answered earlier.

Lines 235-237: to have a chain you need all rings to be connected. You don't have evidence that the water-rock reaction balance has been disrupted by the earthquake. Gypsum or other evaporite rocks are naturally present in many of the lithostratigraphic sequences of the area and when they are part of aquifers, their dissolution contributes naturally to the saline content of the circulating groundwater without the influence of seismic activity. If you consider the data of Yuce et al 2014, you see that in the area many of the collected waters have high sulfate concentrations with values even exceeding your highest value. So there is no evidence of gypsum dissolution as a consequence of the seismic events.

Reply: Thanks! We have abandoned the conclusion that the Anhydrite can be inferred from the seismic effects of the data collected. We now propose that Anhydrite can reflect the intensity of water-rock reaction. Considering that the sample collection time was about one month after the earthquake, it is necessary to further study the possibility of Anhydrite as an indicator of seismic activity. (Section 5.4 lines *354-398*)

Lines 301-301: I repeat again, even if you analysed a sample taken one hour after the earthquake, this could not confirm that the whitening and turbidity of the water before the seismic event was due to an increased sulfate content

Reply: Thanks! Although we did not directly measure the turbid water samples, based on the historical data analysis of EAFZ, we can determine that the anhydrite layer exists in the middle and southern sections (New Fig. 6 (lines *256-260*)) (Table S1 at Supporting Information). Based on historical data and this study, it can be concluded that the cause of the macroscopic anomaly before the earthquake is the dissolution of minerals such as anhydrite and carbonate.

Line 307: I don't understand how you have fixed it. The video refers to the sampling site HS15 which, as shown in your table, has the lowest sulfate concentration. This video is not a proof of a sulfate anomaly for two reasons: 1) you don't have the concentration of sulfate at the time of the whithening and 2) the concentration you measured one month after was only 1.21 mg/L

Reply: Thanks! There should be a misunderstanding here. We have stated in the first response that the macroscopic anomaly originates from HS14, which has a SO_4^{2-} concentration of 316.61mg/L. (Table 1 (lines *160-161*))

Lines 311-312: You are missing the main point: you have no evidence of variations that can be related to the earthquake

Reply: Thanks! We've revised our conclusions to be more precise. (lines *399-409*)

Line 327: The problem is that normal values have not been defined. In terms of time you don't have enough samples that you can surely correlate with yours. But the same holds true in terms of space, only 16 samples along a structure many hundred km long is not enough

Reply: Thanks! We have weakened the focus on time and only discussed the water-rock reaction process of Anhydrite. 10 years of data is sufficient to support spatial representativeness.

RC3: 'Comment on hess-2024-395', Anonymous Referee #2, 18 Feb 2025

Dear reviewer

Thank you for your comments and suggestions, which are of great value to us in improving the quality of our manuscript. The main replies are as follows.

The present work performs a systematic hydrogeochemistry and isotopic analysis of the geothermal fluids in the East Anatolian Fault Zone (EAFZ) to understand any clear relationship between geothermal fluid anomalies and earthquakes existing. I have found the language of the manuscript is fine but must have a proof-editing. I have some of my major comments regarding the work on the other hand.

Main motivation behind the work is to elucidate the role of gypsum dissolution as a tracer for earthquake activity in the East Anatolian Fault Zone (EAFZ). The research aims at establishing a link between geothermal fluid anomalies and seismic events, with the claim of using an innovative approach to earthquake forecasting. In this respect, it *examines shallow sedimentary minerals, particularly gypsum, as indicators of seismic activity. This concept, while explored in previous research, is further substantiated with empirical data in this study.*

At this stage my biggest concern stems from the fact that it relies on the data collected post-earthquake but it fails to provide a long-term pre-earthquake dataset for comparative analysis. This appears to undermine claims about gypsum dissolution as a predictive tool rather than a post-seismic indicator. Furthermore we understand that the manuscript never make an in-depth discussion or address other factors such as climatic conditions and seasonal variations robustly and only focus is given on the correlation between seismic events and SO42- anomalies is discussed.

The authors' uncertainty about the relevance of the results to earthquakes is evident in the final statement of the abstract. As readers, we expect the abstract of this study, which claims to bring innovation to earthquake prediction under normal conditions, to convey a clear take-home message.

In this respect I understand that authors are suggesting gypsum dissolution as a universal precursor. But I should remind that a comprehensive considering of regional geological differences or alternative explanations for observed anomalies is of great importance for earthquake hazard studies. Although potential limitations of using gypsum dissolution due to external environmental factors is acknowledge in the manuscript clear strategies for coping with these difficulties in practice.

Given its limitations in predictive validation substantial revisions are required for the present work. These revisions should include i) further evidences distinguishing *seismic-induced gypsum dissolution from other environmental factors ii) a decent discussion on possible long-term monitoring strategies to make gypsum dissolution as a reliable precursor, iii) quantitative examples that prove the statistical significance of the findings that are critical to improve the robustness of the conclusions.*

I also suggest adding a discussion that explore practical applications focusing on an integration of their findings into an effective earthquake early warning system.

In conclusion I do not think the manuscript is suitable for the publication in its current form and requires a substantial work to address the aforementioned fundamental concerns that would significantly advance the understanding of geochemical indicators in seismic studies and warrant publication.

Reply: Thanks! We sincerely thank you for recognizing the systematic approach of our hydrogeochemical investigation. Please find below our point-by-point responses:

Data base extension:

A meta-analysis of published datasets (2013-2023) reveals fundamental differences in water-rock interactions across the EAFZ (Fig. 1):

Northern EAFZ: Mixed shallow/deep circulation with igneous rock-dominated water-rock interactions.

Central-Southern EAFZ: Shallow circulation dominated by sedimentary mineral dissolution (e.g., anhydrite, carbonates), with localized seawater influence.

These distinct regimes provide a robust framework for interpreting tectonic-hydrogeochemical linkages, mitigating reliance on isolated measurements.

Anhydrite as Process Indicator:

While avoiding direct seismic causality claims, three lines of evidence suggest anhydrite's tectonic relevance:

The abnormal plasma of SO_4^{2-} and Ca^{2+} was observed one month after the earthquake. Combined with the analysis of 10 years of data in the study area, it was found that anhydrite dissolution may be the cause of the abnormal ion concentration.

One month before the earthquake, the macro anomaly of white and cloudy well water was photographed (Video 01)

After analyzing pre-earthquake macro anomaly, post-earthquake data and literature data in the past 10 years, we propose that our data can only account for the dissolution of anhydrite during the water-rock reaction. Anhydrite may therefore indicate changes in the intensity of the water-rock reaction. As for the controlling factors of the variation of water-rock reaction intensity, we cannot define exactly. Considering that the sampling time was one month after the earthquake and obvious groundwater anomalies were observed before the earthquake, we believe that seismic activity may affect the variation of water-rock response intensity. Therefore, it is necessary to further study the possibility of anhydrite as a tracer of tectonic activity.

Clear research orientation:

Delete all references to "earthquake prediction". This study focuses on the analysis of EAFZ groundwater circulation process and attempts to establish the relationship between water-rock reaction intensity and tectonic activity. This study will provide a new research idea for the subsequent exploration of anhydrite as a tracer of tectonic activity.

Reply to community comments

CC1: Comment on Hess-2024-395, Giovanni Martinelli, 03 Jan 2025

I found useful and interesting the manuscript https://doi.org/10.5194/hess-2024-395 submitted by Luo et al. Significant geochemical anomalies in geothermal fluids were detected before to and during the Mw 7.8 earthquake in Turkey. To investigate the correlation between geothermal fluid abnormalities and seismic events, the authors conducted a comprehensive analysis of hydrogeochemical and isotopic study of geothermal fluids in the East Anatolian Fault Zone. The findings indicate that these geothermal fluids were affected by seismic activity. According to the chlorine-enthalpy model, the temperature of the deep geothermal fluid significantly rose. However, the data regarding the deep geothermal fluid was eventually affected by the influx of significant amounts of superficial cold water following the earthquake. The anomalous levels of Ca, Mg, SO4, Sr, and Ba in geothermal water indicate that the water has experienced complex water-rock interaction processes, including gypsum, calcite, dolomite, anorthite, and possible serpentinization. Substantial gypsum dissolution was noted at locations HS05, HS09, and HS14 both before to and during the earthquake, indicating that the earthquake favoured the dissolving of gypsum. The authors suggest that superficial sedimentary minerals, including gypsum, may serve as markers for earthquake warnings. During earthquakes, alterations in geochemical conditions result in variations in gypsum solubility, subsequently causing anomalous amounts of SO4, Ca, Sr, and Ba in geothermal water. The solubility of gypsum is influenced by several environmental variables, including meteorological conditions and seasonal variations, hence reducing its practical use for earthquake early warning systems. I think the paper is well organized but I found the possible lack of some sentences devoted to the mechanism of the observed upsetting. Redox conditions have been affected? Deep originated CO2 could be suspected as an eventual carrier of H2S? The addition of some comments about the listed topics could possibly help readers to better understand during the tectonically active period. I hope the paper will be soon accepted and published after some minor revisions.

Reply:

Dear Giovanni Martinelli

Thank you for your recognition of our work and valuable suggestions, which are very helpful for us to improve the quality of our manuscripts. Your two comments are exactly where we are lacking. At your suggestion, we plan to add a subsection to the discussion section for assessing the contribution of mantle degassing to EAFZ geothermal fluids. **See the revised manuscript for details 293-322.**

CC3: Comment on Hess-2024-395, Hafidha Khebizi, 17 Jan 2025

Dear Hafidha Khebizi

Thank you for your recognition of our work and constructive suggestions. This is very helpful for us to improve the quality of the manuscript, and also brings confidence for us to continue to explore. Thank you for sharing the very rewarding work you do. We get a lot of inspiration from your work. We would like to express my heartfelt thanks.

We've responded to each of your comments, as detailed below:

Dear authors and colleagues of the scientific community,

I congratulate the authors for their interesting work entitled Gypsum as a potential tracer of Earthquakes: a case study of the Mw7.8 2 earthquake in the East Anatolian Fault Zone, southeastern Turkey, and I hope it will be published soon. To find out the relationship between geothermal fluid anomalies and earthquakes, the authors performed a systematic hydrogeochemistry and isotopic analysis of the geothermal fluids in the East Anatolian Fault Zone (EAFZ). The results show that earthquakes reconstructed these geothermal fluids.

Reply: Thank you for your recognition of our work. Thank you.

Considering gypsum as an earthquake tracer is excellent reasoning for analysing the impact of anomalies after the earthquake, and the work could be a great reference for future studies related to the earthquake.

Reply: Yes, through the analysis of groundwater after the earthquake, we discovered the potential value of anhydrite as an earthquake warning. It is hoped that this work will attract the attention of more researchers and colleagues, and incubate more meaningful achievement.

To enrich this excellent analysis, I have some remarks concerning the implication of macroscopic and microscopic aspects of geothermal fluids before and after the earthquake, notably the relation with the structural geology of the region. For this, some questions seem important to be asked.

First, from a macroscopic point of view, it is necessary to understand, in the normal case (before the earthquake), from a geological point of view, if the existing deformations (faults) already have effective structures for the infiltration of meteorological waters and the implication of the disposition of the thermal springers according to the faults. After the earthquake, is there any sampling from Miocene groundwater and soil? Is there recent salt precipitation in the Miocene and upper Eocene-Oligocene soil and/or in the soil of the surrounding springer sources? Is there a rise in the ground level due to fault action, and are there marine intrusions that occurred after the strike-slip? Is there significant contamination of the water table (increased electrical conductivity)?

Reply: Hot springs and fault zones are often associated. Hot springs are considered as one of the potential means of earthquake warning. A large number of research results have been published in Japan, the United States, Iceland, Spain, China, Turkey... ... In EAFZ, many hot springs have been systematically studied, and the results show that these hot springs contain material supply from deep crust and even mantle. Therefore, it is highly possible to obtain valuable information by conducting post-earthquake hydrochemical and isotopic analyses of these hot springs.

Unfortunately, we only collected water samples after the earthquake and did not analyze soil samples. Your comment is a very good suggestion, reminding us that detailed analysis of surrounding rock may be needed in future work. Thank you.

Salt precipitation and electrical conductivity (EC). Before we can answer your question, we need to explain an error in the manuscript. Our sample was taken in March 2023 (within one month after the earthquake). In the video 1 we provided, the macro abnormal changes of HS14 were diluted by the adjacent stream, coupled with the fact that the samples were taken within one month after the earthquake and no soil samples were collected, we could not accurately determine whether salt precipitation existed. By comparing the EC of the same hot spring during the seismically quiet period and the seismically active period, we found that the EC of HS14 increased slightly (varying from 990 to 1305). Data of EC pre-earthquake from Yuce, G., Italiano, F., D'Alessandro, W., Yalcin, T. H., Yasin, D. U., Gulbay, A. H., Ozyurt, N. N., Rojay, B., Karabacak, V., Bellomo, S., Brusca, L., Yang, T., Fu, C. C., Lai, C. W., Ozacar, A., and Walia, V.: Origin and interactions of fluids circulating over the Amik Basin (Hatay, Turkey) and relationships with the hydrologic, geologic and tectonic settings, Chemical Geology, 388, 23-39, 2014.

Seawater intrusion was evident after the earthquake. Na+ and Cl− of HS14, HS15 and HS16 increased significantly, indicating the possible existence of seawater intrusion.

Rise in the ground level due to fault action is common. We have made a detailed study on the post-earthquake surface rupture and post-earthquake risk analysis. Article link：Liang, P., Xu, Y., Zhou, X., Li, Y., Tian, Q., Zhang, H., Ren, Z., Yu, J., Li, C., Gong, Z., Wang, S., Dou, A., Ma, Z., and Li, J.: Coseismic surface ruptures of MW7.8 and MW7.5 earthquakes occurred on February 6, 2023, and seismic hazard assessment of the East Anatolian Fault Zone, Southeastern Turkiye, Science China Earth Sciences, doi: 10.1007/s11430-024-1457-7, 2024.

776

777 Screenshot from Liang et al., 2024 doi: 10.1007/s11430-024-1457-7 (If the picture

778 cannot be displayed, please check it in the attachment, thank you).

From a microscopic point of view, gypsum is easily and quickly influenced by contact with water, thanks to its physicochemical characteristics, in particular its very high dissolution rate and its solubility in water that make it an excellent tracer of hydrochemical anomaly but also a tracer of lithological instability (Khebizi et al., 2022; Khebizi et al., 2023). For this, I am pleased to invite you to read the part concerning the gypsum implication on the lithological instability in my article published in Larhyss Journal and my oral communications, which expose, for the first time in Algeria, a new concept of the lithological vulnerability of the subsurface. Although the study areas differ, the analysis presented in my work shows the indication of gypsum dissolution at the regional scale as an excellent major risk indicator. The lithological vulnerability of the subsurface concept can be applied to different situations around the world, notably the case of earthquakes. It highlights the hydrodynamic anomalies' relation with the structural and geological context of the area to be studied.

Reply: Thank you very much for your sharing. It's a fantastic set of work. From my personal point of view, I can't agree with you more. Anhydrite's very high dissolution rate and solubility in water can be used for risk warning of earthquakes and geological disasters. Thank you again for your information. Your work gives us great encouragement and confidence.

Second, if there is a remarkable increase in calcium concentration in water after the earthquake, how do you explain the reaction of carbonate dissolution and the origin of CO2? Is it linked to magmatic activity? In this case, is there a signature of other gases on other cations? Or is it only related to carbonate since the calcite dissolution *is linked to the mineral's surface to be in direct contact with water?*

Reply: In my opinion, Ca may come from carbonate or igneous rocks. In order to accurately restrict the source area of Ca, we are also considering introducing Ca isotopes to distinguish its sources. Ca isotopes in carbonate rocks are lighter than those in igneous rocks and mantle. Ca isotope has a good potential in the source region that restricts Ca.

The index of CO_2 source region is very mature. Geothermal gases are well studied at EAFZ. The C isotope study of CO_2 shows that CO_2 is controlled by deep carbon and inorganic carbonate (−5.6 to −0.2‰) (Italiano, F., Sasmaz, A., Yuce, G., and Okan, O. O.: Thermal fluids along the East Anatolian Fault Zone (EAFZ): Geochemical features and relationships with the tectonic setting, Chemical Geology, 339, 103-114, 2013.). He isotope analysis also shows a large proportion of the mantle.

Explanation of the specific process: anhydrite dissolution and carbonate dissolution are together. In the manuscript, PHREEQC was used to simulate the water-rock reaction process. The results show that anhydrite dissolution alone is not enough to explain the Ca content in the samples, indicating that calcite and other minerals are involved in the water-rock reaction. Combined with previous studies, we believe that CO_2 from deep water is first dissolved in water, and then reacts with anhydrite or calcite. CO_2 is associated with magma, but does not form volcanic eruptions and may only exist in deep areas of partial melting.

Allow me to add that the underground water circulation, which is controlled by faults and hydraulic parameters (permeability), determines water-rock equilibrium. In *this case, water-rock equilibrium depends on the host rock spatial disposition of rock that guides water mineralization and the different processes. Consequentially, the water-rock equilibrium changes from one area to another due to changes in water mineralization according to the host rock lithology. For this, the information that can be taken from the geological map is that springer's water is related to ophiolite rocks. So, I think water geochemistry indicates similar water-rock interactions for all sources. However, a mineral's enrichment zoning can occur due to (i) the meteorological conditions, (ii) the proximity of the springer water from seawater, and/or (iii) the distance from the upstream. The earthquake reconstructed these geothermal fluids depending on the energy released which controls hydrothermal circulation and amplifies interactions with the surrounding environment whether at depth or on the surface. For this, vulnerability zoning in a horizontal and vertical direction can be done according to chemical variation, notably gypsum and probably halite enrichment. It can be indicated as shown in Fig. 8.*

Reply: I can't agree with you more. Water-rock reaction is affected by meteorology, rock properties, permeability, porosity, temperature, pressure... Multiple factors control. At present, our work is limited to the analysis of water chemistry and isotopes, and there is a lot of work to be done in the future. These works involve not only geochemistry, but also rock mechanics, numerical simulation and other interdisciplinary fields, and we hope to have more like-minded colleagues to explore together.

Earthquake warning is the most difficult problem faced by mankind. Groundwater is considered as one of the means to explore earthquake early warning. However, groundwater in its natural environment is very complex. There is still a long way to go to explore the relationship between groundwater and earthquakes.

Finally, the discussion on this topic is very significant, and the structural and lithological vulnerability and their tracers after the earthquake using vulnerability mapping of the Turkey earthquake seems very interesting for future work.

Reply: Thank you for your recognition of our work, your recognition is our driving force forward. Sincere thanks and best wishes.