Dear Editor,

Thank you for reviewing our manuscript. We have carefully reviewed all the reviewers' suggestions. We have addressed all these suggestions in the revised version of our manuscript. You will find in this rebuttal letter all reviewers' comments with our incorporated answers. We also include in this resubmission a revised version of our manuscript, as well as a revised manuscript with tracked changes.
We hope that, after addressing all the comments, you will now find this revised version of our manuscript suitable for publication.

Reviewer 1 (R1) comments

R1: This work is of great interest, especially with regard to the improvement of our knowledge of the seismogenic behaviour of active faults in the Betic Cordillera, which is essential for the improvement of seismic hazard estimates in the region. It also provides valuable information useful in the field of archaeology to open a line of improvement in the understanding of the temporal evolution of the Argaric civilisation in the SE of the Iberian Peninsula. The fault studied provides little data for understanding its seismogenic behaviour, due to the geomorphological characteristics of the area and the difficulty of obtaining datable paleoseismic evidence in the materials affected by the fault. Furthermore, it is a fault whose Quaternary activity has been little studied. All this, in my opinion, makes the work presented quite meritorious and worthy of publication.

We really appreciate the words of R1. The study area presents several issues for palaeoseismological analyses, mainly related to the subtle geomorphological expression of the fault, as she/he points out.

R1: There are, however, some aspects that I believe could be improved. In the annotated pdf I attach a number of comments with several suggestions. I can summarize the most significant comments in the following points:

-I believe that all papers based on the interpretation of paleoseismic excavations should provide graphical information in the form of photographs of the main paleoseismic features used as evidence for the interpretation of earthquake history. Or at least some examples of them. It is a pity that the paper does not show

photographs of the trench record or of some paleoseismic features, which could be included either in the manuscript or as supporting material. I know the quality of the research team behind this work and I am confident in the quality of the interpretations shown in the log of the trenches. However, I believe that the inclusion of field photographs would enrich the manuscript.

We agree with R1. Therefore, if the manuscript is accepted for publication in Egusphere, we are adding as supplementary material high resolution images of the photomosaic logs. In these images, all the palaeoseismic features will be shown.

R1:-It would be necessary a description/interpretation of the sedimentary environment that explains the very different sedimentary records in both fault walls. I find it somewhat surprising that for such a small time interval (only a few hundred years between the ages of units A and F) there is such a different record on either side of the fault. I think this deserves some description and interpretation.

R1 emphasizes that, based on Figure 7, there appears to be distinct sedimentary records in both fault walls of the RUB trench. The overall sedimentary arrangement in the trench is predominantly homogeneous, comprising silt and fine sand beds with intercalated levels of fine conglomerates. That is, the sedimentary record on both sides of the fault is not very different. We think that the issue lies in the potentially misleading depiction of Figure 7.

In Figure 7, three common units (A, B, and C) are illustrated, cropping out on both sides of the fault. From that point to the top, we represent distinct units in the hanging wall and the footwall. Units A, B, and C shared the same name and colour on both sides of the fault because they are observable across the fault, with their bottoms and tops traceable through the fault zone. The same cannot be said for the other units. We think that this is because the fault zone developed in an area with lateral facies changes. For instance, conglomerates of unit X in the footwall laterally grade to silts, corresponding to unit D in the hanging wall (which is made up of silts with interfingered conglomerates). The lateral transition of unit Y is not as straightforward. It is likely that the lower silt level of unit Y corresponds to unit E and part of unit F. We hypothesize that the conglomerate level within unit Y laterally disappeared around the present fault zone, and it was the source for colluvial wedge CW3. Therefore, under this assumption, the upper silt level of unit Y could correspond to the rest of unit F. Finally, unit Z would correspond to unit G. Due to these uncertainties, we decided to assign different names and colours to these units in the hanging wall and in the footwall in the original version of our manuscript. However, we now understand that this can lead to confusion. In response to R1's suggestion, we have included a new paragraph in the manuscript (lines 312-339 of the new version of our manuscript) explaining the discussed lateral transitions and inherent uncertainties. Additionally, to prevent misunderstandings, we will modify Figure 7 and revise the labelling of units. It is important to emphasize that these uncertainties do not compromise our palaeoseismological interpretation of the trench.

R1: It is not clear to me what the objective of the seismic elastic deformation scenario carried out in Chapter 4.3.2 is. Moreover, the results of this scenario are not used in any of the discussions and conclusions of the paper. On the other hand, the complex geometry of the fault would require an in-depth discussion of the rake used in the model. A single rake for sections with such different orientations (almost 45º in some sectors) can lead to results that are difficult to interpret. I think it would have been more interesting to calculate the PGA associated with the ground shaking that would affect the archaeological sites described, taking into account the proposed maximum earthquake scenario.

The objective of the seismic elastic scenario in Figure 11 is to present a glimpse of the potential use of our data in terms of seismic hazard assessment. We completely agree with R1 that it would be interesting to delve deeper into further hazard assessment, for instance, in the PGA derived from our data. However, we maintain that this is beyond the scope of our work. Actually, such an assessment could be the focus of a future paper, as we mention in our Conclusions section. Despite this, we have followed R1's suggestion and have added a further discussion about the implications of this elastic scenario in terms of the distribution of archaeological sites related to the deformation derived from our elastic model (lines 599-601 in the new version of our manuscript).

Regarding the complex geometry of the fault, we agree with R1 that it needs to be better explained. Our model actually addresses this complex geometry and kinematics, as it accounts for the different orientation and kinematics of different faults sections. However, this was not clearly indicated in the original version of our manuscript. Moreover, we presented in Figure 11 the results of only the horizontal displacement, which likely added more confusion. Therefore, we extended the paragraph related to this model to better explain the geometric and kinematic complexities (lines 547-549 in the new version of our manuscript). Furthermore, we added to Figure 11 the results that we obtained of the along-dip and maximum displacements.

R1: The rest of the comments are mainly suggestions to improve the understanding of the text and to correct some typing errors. All of them can be found in the attached PDF.

We will address all the comments indicated by R1 in the pdf, and we will correct all the errors.

Other comments from R1 annotated in the manuscript (line numbers at the beginning of each comment refer to the original version of our manuscript):

Line 35: R1 indicates a discrepancy between the convergence values reported in the text and in Figure 1. There is an error in figure 1. We corrected the figure, so both values are now in agreement.

Line 46: R1 indicates that the intensity of the Galera Earthquake was VII and not VIII. We corrected the value (line 46 of the new version of our manuscript).

Line 50: R1 suggests using probabilistic seismic hazard assessment. We followed the reviewer suggestions in the new version of our manuscript.

Line 114: The reviewer indicates that the way we cited the work of Medina-Cascales et al. (2021) created confusion, as it was preceded by the mention of Figure 1. Therefore, it was understood that we referred to Figure 1 in the work of Medina-Cascales. That is not the case, however; we referred to our Figure 1 in this study. Therefore, we changed the position of the cited reference to avoid confusion (line 115 of the new version of our manuscript).

Line 120: indicates that we cited the Spanish seismic catalog in a sentence where we describe both the seismic parameters and the seismogenic adscription of the Galera earthquake. As R1 points out, the reference that is included is related only to the seismic parameters. We added the extra reference suggested by the reviewer to clearly indicate the seismogenic adscription (line 123 of the new version of our manuscript).

Line 128: R1 asks for a reference for the seismogenic adscription of the cited earthquakes. We added the reference suggested by the reviewer (line 126 of the new version of our manuscript).

Figure 4: R1 indicates that the label of the samples in grey in the palaeoseismic interpretation panel cannot be seen. We improved the labels in this panel.

Figure 4: R1 underlines that the trenches in Figure 4 are named A, B, and C, while in the text, they are referred to as 1, 2, and 3. We corrected Figure 4 to match the text.

Figure 5: R1 suggests that it would be sufficient to include only part C of the figure. We understand R1's suggestion, but we think that, despite that the information that is shown in 5C is the combination of that shown in 5a and 5b, the concept of this figure is to show our interpretation of the additive character of the palaeoseismic history of these trenches. Therefore, we consider it important to show both the original data (PDFs for each trench, Figs. 5a and 5b) and, separately, the interpretation of the complex palaeoseismic history (Fig 5c). Thus, we think that the figure should remain in its original form.

Figure 5: R1 indicates that, in the figure caption, we mentioned trench PIL2, but we referred to trench PIL3. We corrected this error.

Line 220: R1 underlines that he "find(s) it intriguing that in two trenches separated only a few meters, in which the age record of sedimentary units is similar, 4 events are recorded, two in each fault and none in both.". There is an easy explanation for this feature. This occurs because of the relative position of the fault zone and the sedimentary units. In our trenches, we observe a common stratigraphy, including an old terrace and a younger terrace. But, in trench 1, the fault zone offsets the old terrace, while in trench 3, the fault zone offsets the young terrace. We added a sentence in our manuscript emphasizing this feature to avoid confusion (lines 227-229 of the new version of our manuscript).

Line 234: R1 indicates that it is debatable to obtain a slip rate with a single event. He suggests that it would have to be argued that the result obtained would be a maximum value of the slip rate since there is no evidence that even a complete seismic cycle has elapsed. We followed R1's suggestion, and we indicated that this value represents a maximum slip rate (lines 248-249 of the new version of our manuscript).

Line 236: R1 suggests that we should explain if the reported orientation of slickenlines represent average values and if this is homogeneous along the fault. These reported orientations are the average of several slickenlines measured in different fault strands with the same strike as the one where we dug our trench. Following R1's suggestion we added a sentence explaining these values (lines 251-254 of the new version of our manuscript).

Line 246: R1 indicates that perhaps it would be convenient to comment on the recurrence interval that we obtained from our Montecarlo analysis in Figure 5. We followed R1 suggestion, and we added to Figure 5 the obtained probability density function yielded by the Montecarlo model. We did the same in Figure 9 to homogenize the information related to both trenching sites.

Line 264: R1 suggests that "Since during the description of the units the tectosedimentary interpretations are already made (the fault is cited during that descriptions), I suggest to start this chapter with a brief description of the general structure observed in the trench; at least the existence of two fault blocks with different sequences in each of them and the existence of a band of material along the faults that separates both blocks (pink material in figure 7 is not described in the caption of the figure). This, although the detailed structure will be described later, I think it would help the reader to better understand the described tectosedimentary dynamics." We followed R1 suggestion, and we added an initial description of the general arrangement of the trench (lines 281-282 of the new version of our manuscript).

Lines 271-276: R1 marks these lines in yellow, but no comments are included.

Lines 281-282: R1 marks these lines in yellow, but no comments are included.

Lines 284-286: R1 marks these lines in yellow, but no comments are included.

Line 291: R1 asks if there are evidence of paleosoils, or traces of them, on the top of any of the clastic wedges. He considers that "It would be interesting to comment on this. Since they keep a very clear wedge shape all of them, is evidence of erosion of the top of the wedges observed. It is described in the text that all three are covered by conformable beds ofs units D, E and F. This would suggest that paleosoils were not formed? possible reasons for this? As I commented above, it would be interesting to provide field photos of the wall to support these interpretations" As R1 points out, we found no evidence of palaeosoils over the clastic wedges. We think that this is the consequence of relatively high sedimentation rates. Following R1 suggestion we added a sentence to our manuscript underlining this feature and our interpretation (lines 308-309 of the new version of our manuscript).

Line 295: R1 marks part of this line in yellow, but no comments are included.

Line 296: R1 marks part of this line in yellow, but no comments are included. We think this relates to comment in line 291 (already addressed)

Lines 300-301: R1 marks part of this line in yellow, but no comments are included. We think this relates to comment in line 291 (already addressed)

Line 307: we replaced the label TS with "top soil" so that the text of our manuscript matches Figure 7 (line 329 of the new version of our manuscript).

Line 320: R1 indicates that "A brief description of this fault zone would be very interesting. Is that pink material in figure 2 a fault rock? is it injected material? is it simply disorganized material from adjacent units? I think this is important to understand the magnitude of the accumulated displacement along the fault.".

The amount of displacement accommodated by this fault is small, as we mention in our manuscript (0.79 +-0.06 m). However, we understand now that the way we represented the fault zone in Figure 7 can lead to a misunderstanding, as we did not mention the internal structure of the fault zone. We followed R1's suggestion, and we added to our manuscript a brief description of the fault zone at the beginning of the paragraph (line 352-353 of the new version of our manuscript). This description focuses on deformational features to emphasize that the magnitude of accumulated displacement along the fault is small, as R1 requested. Please see also the comment of line 383.

Line 337: R1 marked in yellow the word "identified". We replaced this word with "deduced" (line 370 of the new version of our manuscript)

Line 383: R1 mentiuoned that "Turning to the material that appears along the fault zone (pink colour in Figure 7), I think it would be worth a little more description. Whether it is mixed sheared rock or fault rock, I find it a bit intriguing that in such a short period of time as that spanned by the sedimentary units appearing on either side of the fault zone a fault rock of that thickness can be generated. Is it a rheological issue about the type of material?"
This was already addressed as a response to comment in line 320 (see above).

Line 392: R1 states that "If it is a oblique (strike slip fault with normal component), the strike slip compomen, together with the morphological irregularity of the base of the units, could explain complexities in the observed values of along-dip separations."

We agree with R1 that irregularities at the base of conglomerate levels could explain complexities of along-dip separation in a strike-slip fault. However, in the case discussed in this part of our manuscript, these are fine-grained deposits. Usually, fine grained levels present flat bottoms, so we think that is very unlikely that the horizontal component of the fault could be the reason for the already mentioned complexities.

Line 393: we followed R1 suggestion and replaced "apparent displcamente" by "separation" (line 434 of the new version of our manuscript).

Line 394: This was already addressed as response to comment in line 236 (see above).

Line 417: This would require some explanation or discussion. Looking at the fault trace map in Figure 2, several structural segments could be clearly interpreted. I am not sure if the sentence is a statement about the fault structure, or an assumption that is made to continue with the analysis. In this case it would have to be stated differently. I mean, to say directly that a non-segmented fault is assumed.
We think that the misunderstanding in this part of our manuscript is produced by the lack of a reference at the end of the sentence. The potential segmentation of the Galera Fault was addressed by Medina-Cascales et al. (2021). In this work, our research group applied the standard segmentation criteria (both geometric and kinematic) to the Galera Fault. As a result of this analysis we concluded that the different sectors observed in the Galera Fault are not seismogenic fault segments, as they do not comply with the required geometric (Boncio et al., 2004; Field et al., 2015) and kinematic (Chartier et al., 2019) segmentation criteria. To clarify this aspect, we added the missing reference in line 458 of the new version of our manuscript.

Line 421: R1 indicates that "Returning to the question of segmentation and looking at these differences, could it be possible that PIL Site is located at the edge of a NE segment and RUB Site in the central part of a different segment? The data are scarce and insufficient, but it could be an explanation?"

As we already mentioned, the quantitative analysis of the Galera Fault indicates that it is not segmented. Furthermore, as R1 points out, we think that palaeoseismological data are too scarce to obtain a conclusion about fault segmentation. The palaeoseismological record is intrinsically incomplete, so, in our opinion, only when much data from several trench sites are available can a discussion about propagation of earthquakes be achieved.

Line 461: I think this interpretation is very plausible, notwithstanding that the data are scarce. In addition there are recent works like the one of Yadzi et al. (2023) (10.1029/2023TC007917) in which they linked genetically, through interactions of CFS changes, the historical earthquakes of the south of Almería with the Baza earthquake. This is an area with very close strike slip faults and this type of coseismic interactions can modify the seismogenic rates in the medium and long term.

We find very interesting the comment of R1. Therefore, we added a sentence to our text pointing to this potential relation (lines 504-505 of the new version of our manuscript).

Line 465: R1 says "I find this chapter very interesting, both with regard to the estimation of the seismic hazard parameters, as well as the very interesting archaeological scenario that could relate the seismic activity of the fault with the evolution of the settlements of the Argaric culture in the area. However, I do not see the objective pursued with the scenario of seismic elastic deformation that is carried out. Moreover, the results of this scenario are not used in any of the discussions and conclusions of the paper. On the other hand, the complex geometry presented by the fault would require a deep discussion about the rake employed in the model. A single rake for sections with such different orientation (almost 45º in some sectors) can lead to results that are difficult to interpret. I would advise to eliminate this geodetic scenario and use the space saved to include more graphical information of the trenches, in particular photographs of the analyzed trench walls and/or details of structures and features key in the argumentation of the interpreted events."

Following R1´s suggestion, we add to our discussion section a paragraph examining the implications of this elastic scenario in terms of the distribution of archaeological sites related to the deformation derived from our elastic model (lines 599-601 of the new version of our manuscript). We also better explained how the model addresses the geometric and kinematic complexities of the Galera Fault (lines 547-549 of the new version of our manuscript). Instead of eliminating Fig. 11, we added to this figure the complete information of both horizontal and vertical displacement, together with the maximum observed displacement. High resolution orthophotomosaics of the trenches are included in the new version as supplementary material.

Line 506: R1 asks about the rake used in our elastic deformation model. This was already addressed in the previous comment.

Reviewer 2 (R2) comments

The manuscript represents an valid contribution to the seismic hazard assessment of the Central Betic Cordillera, an area dominated by "slow faults" with long recurrence intervals. Through the excavation and analysis of some paleoseismological trenches the Authors provide original data for the active oblique Galera Fault, within a highly populated area where the record of prehistoric earthquakes is very scarce, addressing relevant scientific questions that are appropriate for Solid Earth and of broad interest among the Earth science community.

The integration of various analysis methods is certainly very appreciable and cutting-edge. Starting from the paleoseismological analysis of the terrain, in fact, a study path is developed in a balanced way which includes: i) Bayesian analysis to model the surface rupturing history for the Galera Fault; ii) recurrence intervals evaluation; iii) temporal fault behavior vs. EQs clustering; iv) fault parameters calculation and geodetic rupture scenario; up to the archaeological application of the results, v) Impact of palaeoearthquake clustering on Bronze Age human societies.

Again, we would like to thank R2 for his constructive comments, that will improve our work if published.

GENERAL COMMENTS

R2: However, I think there are two main comments that the Authors should address to make the manuscript suitable for publication. The first criticism is aimed at the completeness in the presentation of geological data (in geological maps, cross sections, photos..), while the second concerns the method used in paleoseismological study.

1a) The first criticism is that in the manuscript (especially in sections 2 and 3) there is a strong deficiency and/or inaccuracy in the presentation of the basic geological-structural elements, both at the regional and mesoscale scales, which are fundamental for the reader to understand the geological structure and the seismotectonic context in which the study was carried out. I am referring in particular to the presentation of the geological data in the figures, which should summarize and clarify what is expressed in the text. However, many of the figures showing the geological aspects (e.g. figs 2,3, 4, 6) are incomplete and/or imprecise, as explained in detail in the "specific comments" section relating to the figures.

We thank R2 for such a detailed analysis of the figures in our manuscript. We agree with him, and we incorporated into the figures all his suggestions that improved them significantly.

R2: 1b) I don't think it's just my convinced opinion that studies of paleoseismological trenches must always show, in addition to trench logs and interpretative schemes, orthophotos of the trench walls, at least for the sectors of greatest interest (e.g. fault zones). This does not mean lack of confidence in the interpretation work done, but has the fundamental meaning of sharing geological data with other researchers. This aspect is even more true for paleoseismological trenches where the studied outcrops (from tens to hundreds of cubic meters of geological record) will be lost to future researchers.

We followed the suggestion by R2 (and R1), and we added as supplementary material high resolution images of the logs orthophotomosaics. In these images all the palaeoseismic features are clearly displayed.

R2: 2) I believe the Authors must clarify why in a structural context characterized by an oblique kinematic fault with a prevalent strike-slip component (based on the geological and geodetic studies as reported in the manuscript by the authors themselves), trenches have not also been dug parallel to the strike of the fault. As the Authors know, the orientation of the trench is dictated by the inferred sense of fault displacement. For oblique and strike-slip faults, therefore, it is common practice to perform parallel trenching in order to define the "real displacement" by the offset of piercing points, while "*fault perpendicular trenches are often used to locate an define the width of strike-slip slip fault zones*" (McCalpin, 1992). I believe that carrying out statistical analyzes on slip-rates evaluated mainly on "dip-separation" data in trenches perpendicular to the fault may offers some problems, also due to the very small extent of the displacements that are measured, for the majority of cases in even very narrow Holocene time intervals.

As R2 indicates, fault-parallel trenches in strike-slip contexts are the best way to determine the fault displacement. But, in these contexts, fault-parallel trenches rarely allow the recognition of the number of palaeoearthquakes recorded. This history is more likely to be reconstructed in across-fault trenches. So, ideally, in order to have a complete characterization of the seismological parameters (offset and number of events), both fault-parallel and across-fault trenches should be dug. However, we could not afford this analysis, both for time and economic reasons. Therefore, we prioritized the reconstruction of the palaeoseismological history of the fault, and we have tried to obtain an approximation of offsets using the observed vertical displacements and the rakes of the fault slickensides.

Nevertheless, we now realize, following R2's comment, that we need to provide a more thorough explanation of our approach to the calculation of offsets and subsequent slip rates. We added sentences to clarify the significance of our slip rates, making it explicit that these values should be approached with caution, as they are not direct measurements, but rather are derived from the vertical offsets (lines 251-254 of the new version of our manuscript).

R2 SPECIFIC COMMENTS

Figures:

R2: Fig. 1 - A clearer kinematic picture should be provided in this figure, indicating (with dashes, arrows...) at least the faults with normal kinematics and those with prevalent strike-slip kinematics.

- Indicate the Betic Cordillera with an acronym or other symbol. The Padul Fault and the Ventas de Zafarraya Fault referred to in the text in Fig. 1, are missing in Fig. 1.

We followed all of R2's suggestions, and we corrected the errors in Figure 1.

R2: Fig. 2 – Fig.2 is indicated in the caption as a Geological map. I find the legend in which only the ages are reported to be at least incomplete; in a geological map before the age the geological unit should be defined by distinguishing at least what type of deposits they are (fluvial, alluvial, lacustrine... and where possible lithofacies and the range of thickness).

-Insert kinematic indicators for the GF

-Furthermore, the symbol in the legend for "towns" is not the one shown in the figure.

We followed all of R2's suggestions, and we corrected the errors in this figure.

R2: Does glacis surface mean a glacis of erosion or accumulation of deposits?

A glacis is a geomorphic surface that can be both erosive or the result of sediment accumulation, depending on the considered area. This is the case of the Guadix-Baza Basin glacis. This surface is erosive near the borders of the basin and depositional in the central part.

R2: Fig. 3 – 2(a) same as fig. 2 (fix the legend: e.g. what do black point -dashed line and dashed black line-double point indicate? Distinguish geomorphological from geological features.... In Fig 3(c) the fault or fault zone crossed by trace I-I' (and from the trenches) is not represented in the geological cross section. In the latter it is clear that the geomorphological feature "Surface 1" is associated with the sedimentary body colored in green which is not indicated either in the map of Fig. 3a or in the caption.

We followed all of R2's suggestions, and we corrected the errors in Figure 3.

R2: Fig. 4 – The trenches in this figure are indicated as trench A and trench B, while in the text and in Fig. 2(b) they are referred to as trench 1, trench 2 and trench 3. Uniform and perhaps put the same acronym also along the cross section of fig. 3(c).

- the acronyms of the samples next to the logs of the geological units are difficult to read.

- Bedrock instead of Bed Rock

We followed all of R2's suggestions, and we corrected the errors in Figure 3.

R2: - In the wall of Las Acacias Gully in Fig. 3(b) it is clearly shown how the GF has lowered the fluvial terrace in its southern block (as also indicated by the red arrow),

while in Fig. 4(a) it seems that the block lowered is the one towards the north. Clarify these kinematic aspects of the fault well.

Figures 3 and 4 offer a different scale of observation. The photograph in Figure 3 shows the general kinematics of the fault strand cropping out in this area. Figure 4 shows the trench-scale detail of the fault zone. It is a common feature of strike-slip faults that the vertical displacement polarity changes according to the strike of the fault branches. That is the case of our study area. Despite the general kinematics of the fault strand lowers the southern block, the small branches observed in the trench wall produced small displacements lowering the north block. In any case, we added a sentence clarifying these kinematics aspects to our manuscript (lines 188-191 of the new version of our manuscript).

R2: Fig. 6 – For Fig. 6(a) and fig. 6(c) I suggest the same recommendations as in fig. 3(a) and 3(c).

- While I would suggest changing the name of the profile trace which for Fig. 3 and Fig. 6 is always I-I'.

We followed all of R2's suggestions, and we corrected the errors in this figure.

R2: Text (L=line)

R2: L76 – Guadix-Basa Basin: what kind of basin is it? Extensional basin? Pull apart basin?

The Guadix-Baza Basin is an intramontane basin with a complex tectonic evolution. Since the late Miocene, it has been an extensional basin. We clarified this aspect in our manuscript (line 76 of the new version of our manuscript).

R2: L96 – Specify from which type of studies "more than 2000 m of sediments accumulation" were estimated, geological, geophysical…?

It was estimated from geophysical studies (gravity and seismic). We clarified this aspect in our manuscript (lines 97-99 of the new version of our manuscript).

R2: L110 – Transfer to what? It is not clear from the figures and the text on which structure(s) the GF transfers the deformation towards the east.

R2 is correct in that the term "transfer" is not correct. We meant that the GF presents a kinematic coherence with the Baza Fault. We corrected this error in the new version of our manuscript (line 112-113 of the new version of our manuscript).

R2: L141 – Where is the sedimentary record associated with tectonic subsidence? Explain why under 5 m of fluvial deposits the lacustrine deposit of approximately 2.5 Ma is found directly at both the roof and the fault bed; this places constraints on tectonic subsidence associated with fault activity and its relationship with the extent of pre-Holocene erosion.

This is because our trench was excavated across a secondary strand within the fault zone, as stated in our manuscript (line 140). That is, the fault strand traversed by the trenches is not the main slip plane. Therefore, our palaeoseismological site is located on the upthrown side of the fault. Moreover, as we mention in our manuscript (lines 100-105), since the middle Pleistocene, the Guadix-Baza Basin has been dominated by erosion, and present deposition is restricted to the basin margins (alluvial fans and piedmont deposits) and the modern drainage system (fluvial terraces and valley-bottom deposits). So, the Holocene alluvial deposits unconformably overlie sediments of different ages in different parts of the basin. In the case of the alluvial terrace that we excavated, it was deposited over lower Pleistocene lacustrine sediments. We found the same sediments on both sides of the fault because the thickness of this Pleistocene unit is much greater than the vertical displacement of the fault.

Nevertheless, the comment of R2 indicates that this aspect is not clear enough in our manuscript. So, we will better explain this feature in the new version of our work (lines 148-150 of the new version of our manuscript).

R2: L187 – On what basis is it established that F2 is the fault that "presents the largest offset"?

It is because it produces the larger offset of the bedding observed in the bedrock units. We clarified this aspect in our text (line 197 of the new version of our manuscript).

R2: L392 – In this way, however, do you exclude the hypothesis that the source could have been different?

L457-459 – It should be remembered that the liquefaction may be due to events on other nearby sources... (perhaps a check should be made with paleoseismological data on other nearby sources).

These two comments are related to the seismogenic source of the seismites observed in the RUB trench. As we stated in our manuscript (lines 328-332), we know that seismites can occur far from the seismogenic source of the causative earthquake. We agree with R2 that a comparison with palaeoseismological data on nearby sources will be of interest. Following his suggestion, we have compared the seismite-related events deduced from the RUB trench with the available palaeoseismological data of the nearby Baza Fault (Castro et al., 2018). Only one of the reported palaeoearthquakes of the Baza Fault overlaps in time with the events that we recognized in the RUB trench. The age of this Baza Fault event is poorly constrained (8485-785 BC), so it could correspond to any of the seismite-related events in the GF. Therefore, we cannot completely rule out that the Baza Fault was responsible for these liquefaction structures. However, as we also stated in our manuscript (line 332), because of the vicinity of the liquefaction features to the GF, we assume this latter as the seismogenic source.

Nevertheless, as we already indicated, we think that including the comparison between the RUB trench and the Baza Fault earthquake chronology would improve the manuscript. So, we added this discussion to the future version of our work (lines 405-412 of the new version of our manuscript).

R2 DETAILED COMMENTS

R2: L114 – Where is the pull-apart in fig. 1? Indicate in fig.1… or perhaps better in fig.2

We added an indication showing the position of the pull-apart area to Figure 2.

R2: L120 – mbLg (specify the type of magnitude, at least the name… considering the multidisciplinary nature of the journal… it must be aimed at a broad audience)

We added the word "magnitude" before mbLg (line 123 of the new version of our manuscript) because this magnitude scale does not have a proper name, and it is usually referred to using the acronym.

R2: L123 – insert a reference about who proposed the GF as the seismogenic source of the 1973 Huéscar EQ..

This comment has been addressed (line 126 of the new version of our manuscript).

R2: L128 – It should be Section "3", not "2"

This comment has been addressed (line 132 of the new version of our manuscript).

R2: L586-857 - References - Several references cited in the text do not appear in the bibliography (e.g., line 97 Vera et al., 1994; and other inaccuracies which must be resolved as well.

We addressed all these detailed comments indicated by R2, and we corrected all the errors.