## Replies to reviewer 1

**Line 34:** amended to 'fault scarp **surface**, as requested.

**Line 62: since your reference is over ten years old, it would be beneficial to include examples of several recent destructive earthquakes in this region, occurring after 2013, to better emphasize the significance of your work.**

We have now added two references, Meng et al. (2021) and Ozkula et al. (2023), which highlight recent large magnitude earthquakes in the region.

**Lines 72-74: The accumulation of 36Cl occurs not only after earthquakes but also in reduced amounts where the fault surface remains covered by colluvium near the surface. please revise accordingly, just for readers clarification.**

**Line 74: I suggest expanding the paragraph to further explain the method, especially for those who are not familiar with the concept of fault scarp dating.**

We have expanded the text to accommodate these two requests. It now reads as (lines 69-79): "The concentration of cosmogenic $^{36}Cl$ (Zreda and Noller, 1998; Mitchell et al., 2001; Benedetti et al., 2002; Palumbo et al., 2004; Schlagenhauf et al., 2010; Tesson et al., 2016; Cowie et al., 2017; Iezzi et al., 2021; Mozafari et al., 2022) has been used to infer paleoseismic activity in limestone normal faults. This nuclide is produced from spallogenic and muonic reactions occurring in $^{40}Ca$ in limestone exposed at the surface and in the uppermost few meters of the subsurface. Following an earthquake, the newly exposed scarp segment accumulates $^{36}Cl$, the concentration of which is dependent upon the duration of subaerial exposure plus the preceding duration of subsurface exposure, potentially allowing the earthquake to be dated. The higher the concentration of $^{36}Cl$, the older the earthquake which has exhumed the scarp segment. Because the highest part of a scarp was exhumed first, ages increase towards the top of a scarp."

**line 86: The authors should clarify in what aspects the code is more robust.**

We have amended the text to (lines 88-93): "These challenges have helped motivate the development of probabilistic models for determining exhumation histories from $^{36}Cl$ concentration profiles (Tikhomirov et al., 2019). For example, Bayesian modeling incorporates prior geologic information (Cowie et al., 2017; Beck et al., 2018; Tesson and Benedetti, 2019; Goodall et al., 2021, Iezzi et al., 2021) to identify the most probable exhumation history from complex $^{36}Cl$ data and make inferences on the seismogenic potential of a fault (Tesson et al., 2016)."

**Line 89: then lie dormant for similar, or even longer, periods**

The reviewer indicates that the second comma 'is extra', but we elect to keep it because 'or even longer' is parenthetical material, which needs that second comma to close.

**Line 106 'subareally'** has now been corrected to 'subaerially'

**Line 116: The sentence and the following goals of the study suggest that finding evidence for this specific earthquake might be an additional aim of the research?! If this is indeed the case, please adjust the objectives accordingly.**

We appreciate the good suggestion. We're being careful to not overinterpret the data, so prefer to leave the study objectives as they are.

**Line 120: I think this last part can be omitted from the objective section. It's up to the authors.**

We agree and have now deleted: using prior knowledge such as the 464 B.C.E. earthquake

**Line 154: If that's true, how can we be sure that the lower sampled part wasn't also covered by those same sediment wedges? From the figure, it appears that the two sediment wedges were once part of a single body, with the central part being removed later on. If this is the case, the lower part of the profile at Agonia A should be analyzed as subaerial samples. This poses a challenge, as it is difficult to determine when the sediment wedge has been formed and when missing parts have been removed.**

We have spent a lot of time thinking about those sediment wedges and appreciate the reviewer also thinking about those and their potential significance to dating. Our interpretation of that particular fault scarp segment is that the two sediment wedges are local features and were not remnants of a single larger feature. Evidence for that includes the scarp surface looking essentially the same between the sediment wedges and beyond the outer margins of those sediment wedges (so no differential weathering reflecting varying exposure times). Further evidence supporting our interpretation is that the hanging wall soil surface is uniform along the fault scarp strike. We think the text is OK so elect to leave it as it is.

**Line 242: I definitely think it is necessary to provide a summary clarifying the analysis aspects used in the MCMC code here. I see some statements in the next pages, but they are insufficient. I suggest that, at the very least, you add a brief explanation of why the code assumes all the ruptures are the similar size.**

We have addressed the reviewer comment with the insertion of the following text (lines 241-244): "This constant length stipulation is a requirement of the MCMC code and is acceptable because the MCMC code focusses on modelling slip rates rather than identifying individual earthquakes, which is an appropriate methodology for our $^{36}Cl$ data."

The reasons why we have considered this code to be most appropriate are driven by our data and are explained in The Results section (end of paragraph 1 and then all of paragraph 2 in section 4.1). We avoid including that explanation here in Methods to avoid misplacing/pre-empting results in methods.

We have read the remainder of the methods and re-visited Goodall et al. (2021) and Cowie et al (2017). From that, we have not identified further information that might be missing from the description of the modelling. We have written carefully to try to be as clear as possible.

**Line 248: You should clarify why prior to 1500 yrs is selected. I believe this is the upper bound of the last occurred earthquake of 2500 Ma, considering 1000 years of uncertainty (mentioned in line 261). If this is the case please explain here too. Since, so far, the manuscript only mentions the year 464 B.C.E. and not age of 2,500 years, this might confuse the readers.**

The reviewer has been confused by our incorrect writing and we much appreciate having this issue pointed out. The 1500 yrs is the estimation for 1σ uncertainty around the selected scarp age of 8000 years. The text is now amended to (lines 249-250): We defined the scarp age as a 1σ normally distributed prior of 8000 ± 1500 years.

The paragraph explains in detail the selection of 8000 years, including that a wide Gaussian prior (5000–16 000 years), is assigned in our modeling to account for the uncertainty in scarp age.

**Line 260: It would be good to try a shorter time frame, just in case there are historical earthquakes missing from the existing archives. Or if there is any evidence or historical documentation confirming a lack of movement in the past 2,500 years, that information should be included.**

We have written (lines 262-264): The elapsed time is defined as 2500 years, based on the youngest known earthquake on the Sparta Fault of 464 B.C.E. We assign a 1σ uncertainty of 1000 years to reflect uncertainty in the historical record.

We argue for leaving the text as it is. As stated, the 464 B.C.E. event is the youngest known earthquake on the Sparta fault (according to the written historical record). There is no historical documentation of an earthquake having occurred after 464 B.C.E. (i.e., there is no historical information that we can cite for an earthquake having occurred since 464 B.C.E.). To account for the possibility that the historical record is incomplete, we are being conservative in assigning a 1σ uncertainty of 1000 years. From this parametrization, the MCMC model has freedom to explore the possibility of younger earthquakes. However, the model results do not indicate a scarp-exhuming earthquake that postdates 464 B.C.E. (see Figure 4a and revised Table S2).

**Line 361: Is there any potential source of difference to explain this? Various source of shielding, difference in dip angle of hillslope above profiles A and B, etc? Is the fault surface strike consistent along both of these profiles that are 50 meters apart?**

We elect not to interpret our results here in the Results section. However, we do discuss this result in paragraph two of Section 5.2 of the Discussion. There is no difference in hillslope dip angle that we could determine, but we also do not think that would cause the observed differences in Anogia A and B profiles, at the base of a 6 m high fault scarp. We do not know the reason, but speculate that it could relate to non-calcite impurities (most likely explanation according to the available evidence), which vary between the Anogia A and B profiles, or to the differential erosion of the scarp surface or former presence of sediment wedges (both of these explanations are less likely, in part because the available field evidence provides minimal support for differential erosion between the Anogia A and B profiles).

**Lines 368-369: The authors should explain the source of these exhumation values. I suppose they are derived from modeling, please reference the relevant table, figure, or other sources, as I hope I haven't missed this in the manuscript.**

We have clarified this by inserting the follow text at the end of the sentence (lines 371-372): "(calculated by dividing the scarp height by the number of model earthquakes)"

**Line 382: Perhaps you can include this to the objectives of your paper at the end of the introduction section.**

We have re-written this sentence, which has misled the reviewer. It now reads as (lines 384-385): "These changes, especially to scarp age, invalidate comparisons of slip rates between the two profiles."

**Line 405: I suggest clarifying in the figure caption what the dashed lines represent, as I suppose they indicate MAP estimation.**

Thanks for highlighting this. The notations of the vertical lines were incorrect and have now been amended. The dashed lines indicate 95% confidence intervals, colour-coded according to the number of modelled earthquakes.

**Line 415: Refer to my comment on line 392.**

We address this comment in our reply to the comment on line 392.

**Lines 454-455: In the context provided, it seems that you are distinguishing between the general presence of $SiO_2$ (silicon dioxide) and the specific detection of quartz., the crystalline form of $SiO_2$, and $SiO_2$ refers to the overall silicon dioxide content, which can be present in different forms, including quartz.**

**If this is the case please clarify.**

We have clarified the text regarding the 6.2 m to 6.6 m level by inserting the following text (lines 457-460):

"The point counting and geochemical measurements were done on different aliquots of the sample slab extracted from each level, thereby causing variation in the determination of quartz concentration between the two methods, which is prominent at 6.6 m."

**Lines 545-547: Recalling from my comment on line 260, I learned that 2500 years was the youngest age of earthquake given to the code as input. However, from your statement here it appears that the code itself could search for the other possibilities of younger ages. Please clarify this contradiction.**

The model uses as prior information an elapsed time of 2500 ± 1000 years since the last earthquake. Given the assigned 1000 year 1σ uncertainty, 200 000 iterations, and the removal of a burn-in of 40 000 iterations, the model has the scope to explore a range of elapsed times rather than being confined to the one initially assigned (as prior information). However, our description starts getting circular, so we have amended the text to the following to be more conservative and to remove any circularity (lines 554-557): "Neither the historical record nor the $^{36}Cl$ concentrations measured on the Anogia A and B profiles (Figs. 3 and 4a), supported by measurements of geometry and hanging wall colluvium, and in Benedetti et al. (2002) provide evidence for large, scarp-exhuming, earthquakes after the 464 B.C.E. event."

We also tested modelling the $^{36}Cl$ data with an elapsed time of 1500 years ± 1500 years. To get a good model fit to the measured $^{36}Cl$ data with this shorter elapsed time requires changing input parameters (e.g., geometry) from those that have been measured to permit faster accumulation of $^{36}Cl$ in the scarp. An example is lowering the scarp dip angle to 58 degrees (the dip angle on the hanging wall colluvium could also be artificially increased and the colluvium density artificially lowered to attain a similar effect of increasing the rate of $^{36}Cl$ accumulation in the scarp). However, even here the MCMC code indicates that the elapsed time since the last earthquake is longer than 1500 years (i.e., a median elapsed time of 1902 years and a MAP of 2165 years are indicated). This provides further evidence against scarp exhuming earthquakes occurring more recently than the 464 B.C.E event.

**Lines 566: This is interesting, and I'm personally curious about how variations in the sample preparation methodology might lead to such, I wouldn't say huge, but notable differences among samples from the samples of the same level along the fault scarp height. It would be helpful if the authors could elaborate on this further.**

The problem is that we do not know for sure. We have privately speculated as to possible reasons but it is not at a level of insight worthy of publication. We do not suspect that there is any major issue in any of the data sets, but 20 years of advances in laboratory preparation can reasonably be expected to lead to improvements in the quality of $^{36}Cl$ measurements.

**Line 575: If there is greater erosion in profile B, it should be addressed and specifically accounted for in the modeling process, as the result would suggest an unrealistic younger age of earthquake.**

We focus our modeling on the Anogia A profile and exclude modeling of the Anogia B profile from our paper for reasons outlined in lines 376–385. These reasons include that we could not get modeled concentrations to converge with measured ones for the full Anogia B profile (i.e., the intensively sampled lower 2.1 m plus the widely dispersed drill-core samples from higher on the scarp). Modelling only the lower 2.1 m necessitated changes to scarp age and pre-exposure from the values used for Anogia A (because only the lowest, and youngest part of the scarp can be modelled at Anogia B). This then invalidates comparisons of slip rates between the two profiles. We therefore prefer to not introduce any new modelling of Anogia B to the paper, including greater erosion at the Anogia B site, which is also only speculative. See also our reply to the reviewer comment at lines 584-586.

**The sediment wedge could be one of the explanations (see my comment of line 154-155). In the provided image of Anogia B (Fig. 1 E), there's no visible wedge around the sampling profile. A zoomed-out image or field observation could probably clarify this.**

As stated in the reply to the line 154 comment, we have put a lot of thought into these sediment wedges and we lack field evidence for a sediment wedge having formerly covered the site of the Anogia B profile (as detailed in the responses to comments at lines 154 and lines 584-586). We have selected the best image that we possess to illustrate the Anogia B site.

**Line 576: I suppose you should have the measurements in mineralogy section, if yes please explain.**

Unfortunately, we only have measurements for Anogia B, so cannot compare the scarp chemistry at Anogia A. We have inserted '(Section 5-3)' to direct the reader towards the section that discusses mineralogical impurities.

**Lines 584-586: Yes! Just as mentioned in my previous comment and comment on lines 154-155, this potential higher value of shielding should be adjusted in modeling (at least give a try). Not sure how it works with the MCMC code.**

We prefer not to do this for the following reasons. We propose possible reasons for the lower $^{36}Cl$ concentrations in a portion of the Anogia B profile, including that a colluvial wedge may have formerly abutted the scarp at this location. However, we argue in the next sentence that the available field evidence does not indicate that there was a colluvial wedge welded to the scarp surface at our sample site. We would therefore prefer to not model something that we did not think happened. In addition, if we were to attempt such modelling, we have no constraint on what the thickness of shielding material would be. Finally, the MCMC code is also not written to account for variable shielding caused by transient overlying sediment wedges on a portion of a vertical $^{36}Cl$ sampling profile.

**Line 588: What about if the colluvial lump is a bit younger than the 2500 years earthquake? Or its removal time is very close to its formation? If so, finding any evidence in the hanging wall could be very challenging or impossible.**

We agree, but the colluvium wedge would then have little impact on reducing the $^{36}Cl$ concentration in the scarp through shielding during the short time it was welded to the scarp surface following Earthquake-induced exhumation. Associated errors with dating the scarp exhuming earthquake would be minor and fall within the range of existing measurement uncertainty.

**Line 768: I would suggest you start the conclusion part by briefly explain that the MCMC code and this study do not deal with defining the age of earthquake, but rather calculation of slip rate.**

We have now amended the first paragraph of the Conclusion. It now reads as (where bold indicates new text; (lines 784-789): "Modelling of **slip rates from** $^{36}Cl$ data from the Sparta fault at Anogia, Greece, indicates an increase in average slip rate during exhumation of the scarp from 0.8–0.9 mm $yr^{-1}$ between 7.7 and 6.5 kyr ago to 1.0 mm $yr^{-1}$ between 6.5 and 2.5 kyr ago (the timing of the historic 464 B.C.E. earthquake). Average exhumation of the entire scarp is 0.7– 0.8 mm $yr^{-1}$. **Earthquake ages were not modelled from our data, but there is no indication from our analyses that earthquakes may have contributed to exhumation of the Sparta fault since 464 B.C.E.".**

The last inserted sentence replaces: Modelling does not indicate that earthquakes may have contributed to exhumation of the Sparta fault since 464 B.C.E.

We do not directly refer to the MCMC code, because that would require a reference to Goodall et al. (2021), which we wish to avoid in the Conclusion section.

**Line 1040: Including Anogia A would make it easier for readers to follow along.**

Good point and we have now amended the text to (line 1074) "the Anogia A profile."

**Line 1042: include Anogia B**

We have now amended the text to (line 1076) "the Anogia B profile."

**Line 1133 extra dot here.**

Extra full stop removed

**References:**

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## Replies to reviewer 2

We thank the reviewer for providing the reference for the new paper on the Sparta Fault (Çal et al., 2014) and for tephra inputs into Mediterranean soils (Lowe et al., 2012; Koutrouli et al., 2018). All are good tips, which we appreciate.

We now refer to Çal et al. (2024) in the Geological Setting (lines 129 and 140) and Discussion (line 541) sections.

We also take up the possibility that tephras from Quaternary volcanic centers in Italy and the South Aegean Volcanic Arc might be contributing REE-Y to Mediterranean soils. See Discussion lines 760-767. We focus on the potential for Holocene inputs (older Quaternary inputs are less relevant) and so refer to papers by Bourne et al. (2010), Smith et al., (2011), Koutrouli et al. (2018), and Vougioukalakis et al. (2019). We refer to Smith et al. (2011) because they show high concentrations of REE-Y in post-15 kyr Campi Flegrei (Italy) tephras.

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