Author comments on the review from Anonymous Referee #2

We thank the reviewer for the offered criticism. We will address all comments sequentially, with our responses to the comments in blue font.

Reviewer comment: I don't think the results of this paper are worse publishing at this stage and I think it needs a serious rewriting and reinterpretation of the datasets with a rigorous treatment specially concerning the modelling of the 36Cl dataset.

Authors' reply: In our opinion, 71 new ^{36}Cl concentrations along the Sparta Fault scarp that attempt to replicate a pioneering study, and an extensive REY dataset that points to previously-unrecognized controls on their distribution along carbonate faults scarps constitute results worth publishing. We do agree, though, that the manuscript can be improved, including a consideration of the valuable points raised by this reviewer.

Reviewer comment: First the message convey by the manuscript is confusing and the scope of the paper is not clear. Goodfellow et al. have sampled for cosmogenic dating the exact same site as in Benedetti et al. 2002, they argue that their aim was to understand why the 464 BC was not found in the 36Cl profile made and analysed by Benedetti et al. in 2002, they also want to "redate the paleoseismicity" and finally complement by rare earth elements the paleoseismic history. However in their conclusion, while they use the 36Cl record to derive seismic events, they argue that the 36Cl concentration in the profile might vary with mineralogical variations and thus interpretation in terms of exposure duration might be difficult…this is contradictory, and I would suggest the authors to better explained what they mean and strengthen the scope of the paper to either a methodological paper or a paper on the paleoseismicity of the Sparta fault. As it is, none of their conclusions appear convincing to me (see details below).

Authors' reply: Thank you for highlighting this: we now also see this apparent contradiction. In modelling earthquakes from our results, we had trouble separating what might be an earthquake signal from noise in the ^{36}Cl transect attributable to mineralogical variations. Indeed, from other recent studies where ^{36}Cl dating is applied to reconstructions of paleoseismicity on carbonate faults, noisy data appear to be a common problem.

We will improve the clarity of our paper by better stating how we modelled our data. Our primary focus was to fit a model curve to our data, with the location of inflection points (which might be former soil surfaces) being a secondary consideration (because of mineralogical variations creating noise, which complicates the inference of former soil surfaces). Extensive testing using this methodology resulted in a present best fit of 5 earthquakes. To get the best statistical match between modeled and empirical ^{36}Cl data, we varied both the number and locations of earthquakes inferred from inflection points in the ^{36}Cl data. The final deduction of where former soils surfaces are located on the fault scarp, and how many earthquakes may have occurred, is based on the best statistical returns, rather than trying to infer an earthquake for every inflection point in the ^{36}Cl curve (because, at least partly, these inflection points appear to be influenced by fault scarp mineralogical impurities).

A greater methodological focus may indeed strengthen our paper. For example, it will strengthen our conclusions when we advance a clear recommendation that detailed petrological examinations (including optical light microscopy and SEM on thin sections) accompany fault scarp dating in studies using ^{36}Cl/REY analyses of bulk and trace element scarp chemistry. It would be surprising to us if the Sparta Fault is the only carbonate fault scarp where interpretations of apparent paleoseismicity are complicated by the presence of subordinate silicate minerals, which might be missed because petrological examinations are not routinely done. Such a recommendation could help motive efforts to improve sample preparation for elemental and ^{36}Cl analyses.

With regards to the comment that '*none of their conclusions appear convincing to me*', we highlight here that we present important REY data that clearly point to distributions in the fault scarp and hanging wall soil being primarily related to mineral compositions, rather than pH. The literature on applying REY analyses to the reconstruction of paleoseismicity on limestone fault scarps has focused on the role of pH (Carcaillet et al., 2008; Manighetti et al., 2010; Moraetis et al., 2015, 2023), and important controls on REY distributions exerted by mineralogical variations in hanging wall soils, and therefore fault scarps following postulated soil-scarp REY exchange, both remains little explored and has the potential to greatly complicate interpretations of paleo-earthquakes. This is an important general conclusion for the scientific community, rather than it being a conclusion specific to the Sparta Fault.

Reviewer comment: Also according to previous studies using ^{36}Cl as a paleoseismological tool, mineralogical variations in the fault scarp are taken into account in previous papers with the chemical composition of each sample (see details in Schlagenhauf et al. 2010). In the paper I could not understand why they use a mean composition and not the chemical composition of each sample to avoid this problem.

Authors' reply: We state in the Methods section that we use chemical composition for each sample for one of our ^{36}Cl transects. Unfortunately, we did not assess this for both of our ^{36}Cl transects; hence, the need for a mean composition on that second transect and for remodelling the Benedetti et al. (2002) profile (from which the relevant sample specific chemical data are also missing, given that it was a pioneering paper). However, using the sample specific chemical data on one of our profiles provides little to simplify the interpretation of paleoseismicity from that profile.

Reviewer comment: My second concern relates to the dataset treatment and the associated modeling. Goodfellow et al. have sample for cosmogenic dating the exact same site, but they sample only half the profile that was previously sampled by Benedetti et al. 2002 and have mixed several types of samples with different thickness which affect the 36Cl concentration in each sample and could affect the comparison in between samples. This is not discussed.

Authors' reply: We used slabs cut to a depth of 3 cm, supplemented with 14 drill cores cut to a depth of 2.5 cm. The effect of sample thicknesses on age calculations are minor for samples up to 3 cm thick. Hence, the 0.5 cm difference in thickness between our sample sets is minor. Furthermore, sample thickness is explicitly accounted for in the modelling using Schlagenhauf et al. (2010). Although sample thicknesses are reported in Table S1, we will explicitly state that sample thickness is accounted for in age modelling in the revised manuscript.

Reviewer comment: Moreover, they made several unexplained adjustments that are not justified, such as increasing arbitrarily the concentration by 5% of the depth core profile.

Authors' reply: We respectfully disagree with the reviewer. We explained why we made this adjustment in the manuscript (Lines 330-336). However, we will endeavor to improve this explanation in the revised manuscript, including a statement that this adjustment has no impact on the inference of earthquakes from the Schlagenhauf et al. (2010) model.

Reviewer comment: More importantly, the modeling of the dataset, which is also based on the Schlagenhauf et al. 2010, is made without explaining several assumptions that are crucial to the results. The 36Cl modeling is not well explained and treated in the paper as straightforward. In the model used by Schlahgenhauf et al. the choice of the discontinuities in the 36Cl profile is crucial and allows determining the number of events. Here, the authors do not explained why they chose to model their datasets with 5 events, what results would yield others models with less events ? would the RMS or AIC be better ?

Authors' reply: We did model varying numbers of earthquakes which allows us to present the earthquake number that provides the best statistical fits between the modelled and empirical ^{36}Cl concentration profiles. We also refer to our response to a previous comment that in trying to get the best statistical match between modeled and empirical ^{36}Cl data, we varied both the number and locations of earthquakes inferred from inflection points in the ^{36}Cl data. As mentioned, we will improve our description of the methodology in the revised manuscript.

Reviewer comment: how do they define the position of their events? Others models to unravel the seismic history with 36Cl dataset on a fault plane have been published so far (e.g. Tesson and Benedetti 2019, Tikhomirov et al. 2019, Beck et al. 2018, see also Iezzi et al. 2021) but the authors do not cite them and does not explained why they chose the one published in 2010.

Authors' reply: The simple reason is that this study was started in 2013. We thank the reviewer for highlighting this weakness and an opportunity to improve the present manuscript as we are including an updated modelling procedure.

Reviewer comment: Besides, the production rate they chose for Ca spallation is arbitrary, on which publication is it based ? They cite Lifton et al. 2005 but this is not a publication related to the production rate of 36Cl from Ca. This production rate is almost 18% higher than the one used by Benedetti et al. 2002, this has obviously an effet on the age of the yielded earthquakes. The authors do not discuss this aspect, but argue that their results allow finding the 464 BC event, but would a different production rate yield the same result for the Benedetti et al. record ?

Authors' reply: We disagree with the assessment that the production rate is arbitrary; however, the citation that we give (Lifton et al., 2005) is incomplete. The production rate we use is written in the Schlagenhauf et al. (2010) model code as Lifton et al. (2005) but we should cite Schlagenhauf et al. (2010) modified from Lifton et al. (2005).

Regarding the choice of production rate; it is standard practice to choose an accepted production rate and work with that, as long as all data are available for anyone to recalculate using a different accepted production rate. Also, from Schlagenhauf et al. (2010): "…all models but that of Dunai (2001), produce similar [36Cl] profiles, and so we conclude that the time variability of the geomagnetic field has a limited impact on the ^{36}Cl production rate." In other words, the choice of production rate and associated geomagnetic field model does not have much impact on age calculations from ^{36}Cl, as long as Dunai (2001) is avoided.

Reviewer comment: Concerning the rare earth elements and Yttrium treatment, the authors fail to cite the most recent papers, and in particular Moroaetis et al. 2023 that have discussed specifically the mechanism of REE-Y impregnation on active carbonate normal fault scarps, moreover the data treatment is not well explained and the discussion thus poor and not up to date. See also Bello et al. 2023.

Authors' reply: We appreciate the reviewers' mention of papers that were published after we submitted our manuscript, especially as they too serve to further underline the importance of our REY data, which point to another control on REY distributions on carbonate fault scarps. These references will be included in our revised manuscript.

Reviewer comment: Finally, the presented fault geometry is oversimplified compared to the previous publication of Armijo et al. 1991. It is a pity that considering the means we have now to map the fault trace the authors did not take the opportunity to refine the initial map made by Armijo et al. also because in Benedetti et al. paper the authors claim that the 464 BC could not be seen at Anogia because it might have bypassed the main scarp. It would have been good that the authors explore this explanation, especially since they state it is one of their aim.

Authors' reply: We did look for evidence of small scarps, to some tens of meters below the main Anogia scarp (at the site of the Benedetti et al. (2002) sampling profile) but did not see anything that convinced us. The slope was regolith-covered. We also checked upslope of the fault scarp and did not find evidence of any secondary fault scarps there either. We will state this in our revised manuscript.

Reviewer comment: Thus the yielded results and conclusions are not convincing, I don't see what the paper brings in terms of new results or approach, since the paleoseismicity results appears similar to the conclusions of Benedetti et al. 2002. At this stage, one interesting aspect is that they allow a unique comparison of 36Cl dataset acquired at the same site with almost 20 years of difference. The comparison is outstanding since the difference in the 36Cl concentrations is of at most 19%. This appears exceptional considering that the chemistry extraction and the measurements were made in different labs, with different methods and measured in two different AMS (see Merchel et al. 2011 for an interlaboratory comparison of 36Cl). I am not sure many Quaternary geochronological dating techniques would yield such result. For the cosmogenic nuclides community this might be an interesting result.

Authors' reply: We will address the concern of unconvincing results and conclusions in our revised manuscript by clearly stating our conclusions.

"I don't see what the paper brings in terms of new results or approach, since the paleoseismicity results appears similar to the conclusions of Benedetti et al. 2002." Having worked with their data, it is difficult, at best, to infer individual earthquakes from the Benedetti et al. (2002) profile. This is because the data is noisy. We can only speculate why they were unable to delineate a 464 BC event in Anogia, whereas we can. We suspect that they might have overspiked their samples with Cl-carrier. We also suspect that they may have calculated their ages from grams of Ca, rather than from grams of rock (although grams of rock is cited in their ^{36}Cl results figure). If true, their ages for the exhumation of the fault scarp are too old and, therefore, unfit for identifying an earthquake that happened 2500 years ago, even if their data were less noisy. It is therefore of value to attempt to reinterpret earthquakes, using ^{36}Cl dating of the Sparta Fault.

"I don't see what the paper brings in terms of new results or approach, since the paleoseismicity results appears similar to the conclusions of Benedetti et al. 2002.". We highlight that 71 new ^{36}Cl data points from two transects located close to the Bendetti et al. (2002) profile provide valuable new results. We use these new data in our study to test for reproducibility (of earthquake inferences in addition to

reproducibility of the ^{36}Cl dating method), and they allow for an updated appraisal of paleoseismicity using a much more comprehensive model for determining the exposure history of the Sparta Fault, using more modern derivations of the ^{36}Cl production rate.

As helpfully highlighted by the reviewer, we can do more to highlight the reproducibility of ^{36}Cl dating. Indeed, our own data from the slab samples were processed in the same lab and measured for AMS in the same lab, but in two phases, 3 years apart. The reproducibility in this case was excellent, which points to outstanding sample preparation procedures and AMS at PRIME lab.

Regarding the significance of our results, one of the things that most reviewers missed was the significance of our REY findings. As stated in a previous reply, our data point to another mechanism of REY enrichment and depletion on carbonate faults scarps, other than pH. It is important that the scientific community becomes aware of this because it will demand a different approach in studying limestone fault scarps. Hence, we will better highlight this in our revision.

References used in the response:

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