Instructions to authors

We kindly ask you to provide a detailed point-by-point response to all referee comments and specify all changes in the revised manuscript. The response to the referees shall be structured in a clear and easy to follow sequence: (1) comments from referees, (2) author's response, (3) author's changes in manuscript. In addition, please provide a marked-up manuscript version showing the changes made (using track changes in Word or latexdiff in LaTeX). This version should be combined with your response file so that the topic editor can clearly identify what changes have been made

Authors' replies

We have thoroughly revised our manuscript according to the referee and editorial comments. The changes are summarized below. Note that we avoid simply repeating replies previously provided to the reviewer comments and have updated some of the replies with new and revised information.

Reviewer 1:

Reviewer comment: Interestingly, the Authors clearly show that REY analyses are of little use for the Sparta Fault bedrock scarp, due to the presence of quartz and other silicate components in the breccia matrix included in the fault damage zone. This is a major contribution from the presented investigations.

Authors' reply: We thank the reviewer for identifying this and agree that it is a major contribution of our study and have endeavored to further strengthen the implications of our REE-Y results.

Authors' changes in manuscript: We have added a new subfigure (Figure 10b) to show how the LREE/HREE ratio varies along the vertical sampling profile on the Sparta fault scarp. We have also added two new subfigures (Figure 11b, c) that were previously supplied as Supplementary material and now separately plot the subsurface samples in Figure 11a. These plots are added to better illustrate the strong correlations between REE-Y distributions and the non-calcite mineralogical impurities embedded in the fault scarp breccia. We also expand upon the implications of these data in the Discussion by stating potential sources for the REE-Y/inferred clay distributions in the fault scarp (lines 665-730) and implications for the interpretation of paleoseismicity (lines 732-766). A key inference is that consideration should be given to controls on REE-Y distributions in hanging wall soils and fault scarps, other than just pH, and that these controls need to be explored to make potentially reliable inferences of paleoseismicity.

Reviewer comment: My main comment concerns the general use of exposure dating for understanding the paleoearthquake history on a limestone bedrock fault scarp. In fact, the literature shows that defining individual earthquake ruptures using 36Cl on bedrock fault planes might be quite difficult. For instance, during the 2016 earthquake sequence in Central Italy, the main limestone scarp along the Vettore Fault ruptured following both the August 24 and the October 30 mainshocks, with a maximum slip of 20 cm and 210 cm, respectively. Clearly, exposure dating will not be able to discriminate among these two events, as already discussed by Bubeck et al. 2015 (Bubeck, A., Wilkinson, M., Roberts, G. P., Cowie, P. A., McCaffrey, K. J. W., Phillips, R., & Sammonds, P. (2015). The tectonic geomorphology of bedrock scarps on active normal faults in the Italian Apennines mapped using combined ground penetrating radar and terrestrial laser scanning. Geomorphology, 237, 38-51) and Cowie et al. 2017, for instance. This point should be clearly discussed in the Introduction, and taken into account in the Conclusions. The series of 4 strong, M7 paleoevents interpreted by the Authors is therefore affected by intrinsic problems of resolution; the 4 identified strong events might include several smaller , M6 to 6.3 (for instance, the Mw 6.3 L'Aquila eq in 2009 generated max displacement of ca. 10 cm), seismic events. M6 to 6.3 is a very severe earthquake for an ancient town like Sparta in 464 B.C., but also for the modern town of Sparta today. Therefore, conclusions in terms of seismic hazard based on the results collected by the Authors of this manuscript must be treated with care.

Authors' reply: This is a considered comment, and it is a very important one, with which we also agree. In addition to being unable to date earthquakes closely clustered in time, we add that noisy ^{36}Cl concentrations along vertical profiles seem to be a common finding (as occurs also in our study of the Sparta fault). This noise confounds interpretation of slip events from simple stepwise changes in ^{36}Cl concentrations that are predicted by theory. This issue has necessitated complex (frequently Bayesian) modelling of ^{36}Cl concentrations, from which to infer slip-generating earthquakes. In analyzing our ^{36}Cl data, we felt most confident in being able to calculate mean displacement rates for the Sparta scarp, while discerning the contributions of individual earthquakes to scarp exhumation was more uncertain. In our revised manuscript, we have dispensed with attempting to identify individual slip generating earthquakes, but instead concentrate on determining slips rate, and changes in slip rate over time, through Bayesian modelling. We also refrain from making inferences of earthquake recurrence intervals, thereby making the seismic hazard implications of our study more conservative.

Authors' changes in manuscript: The changes to the manuscript regarding modelling of the ^{36}Cl data have been comprehensive because we have updated the initial modelling, based on the Schlagenhauf et al. (2010) code, to Bayesian modelling of slip rates, using a code published by Goodall et al. (2021). We have added a new Methods subsection: *3.1.1 Bayesian modelling of 36Cl concentrations* (lines 228-284), new figures, tables, and text in the Results to present the Bayesian modelling of slip rates (Figures 3 – 5; Tables 1-2; lines 391-431) and new exploration of the data in the Discussion (lines 530-597).

Specific comments in manuscript: We have addressed these in the revised version. The reviewer raises a good point regarding the accuracy of historical records for earthquakes in Greece and our Bayesian modelling of the time elapsed since the 464 B.C.E. event included a 1000-year uncertainty interval to account for a potentially incomplete historical record of earthquakes (lines 260-262). Our modelling provides no evidence that a major slip-generating earthquake postdates the 464 B.C.E event (lines 545-547).

We do not find evidence in the literature of significant inputs of volcanic material to the terra rosa soils of the Mediterranean, as suggested by the reviewer. There is, however, evidence of significant inputs of North African dust, as we state in the manuscript, also with a newly expanded description (lines 512-523, 682-693).

Reviewer 2:

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: We have significantly revised our paper, especially with respect to the modelling of the 36Cl data. We now employ a Bayesian Markov chain Monte Carlo model, published by Goodall et al. (2021) and modified from the code published by Cowie et al. (2017) to identify slip rates and slip rate changes over time.

Authors' changes in manuscript: We have added a new Methods subsection: *3.1.1 Bayesian modelling of 36Cl concentrations* (lines 228-284), new figures, tables, and text in the Results to present the Bayesian modelling of slip rates (Figures 3 – 5; Tables 1-2; lines 391-431) and new exploration of the data in the Discussion (lines 530-595).

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 have now addressed this problem by using a Bayesian Markov chain Monte Carlo model, published by Goodall et al. (2021), and modified from the code published by Cowie et al. (2017), to identify slip rates and slip rate changes over time. We do not attempt to identify individual slip-generating earthquakes that have exhumed the Sparta fault.

We have increased the methodological focus of our paper through changing the title and, especially, through expanding our presentation and interpretation of the REE-Y results to highlight that hanging wall soil pH may not be the only (or dominant control) on REE-Y distributions on carbonate fault scarps, and that other controls (such as supply of REE-Y from aeolian silt and especially from clays embedded in faults scarp breccia, which have been inherited from depth beneath the earth surface) need to be further explored to potentially enable interpretations of paleoseismicity from REE-Y distributions.

Authors' changes in manuscript: To improve the modelling of our ^{36}Cl data we have added a new Methods subsection: *3.1.1 Bayesian modelling of 36Cl concentrations* (lines 228-284), new figures, tables, and text in the Results to present the Bayesian modelling of slip rates (Figures 3 – 5; Tables 1-2; lines 391-431) and new exploration of the data in the Discussion (lines 530-595).

To increase the Methodological focus of our paper, our manuscript is now entitled: *The protocataclasite dilemma: in situ ^{36}Cl and REE-Y lessons from an impure limestone fault scarp at Sparta, Greece*. We have also expanded our presentation and interpretation of the REE-Y data as follows: We have added a new subfigure (Figure 10b) to show how the LREE/HREE ratio varies along the vertical sampling profile on the Sparta fault scarp. We have also added two new subfigures (Figure 11b, c) that were previously supplied as Supplementary material and now separately plot the subsurface samples in Figure 11a. These plots are added to better illustrate the strong correlations between REE-Y distributions and the non-calcite mineralogical impurities embedded in the fault scarp breccia. We also expand upon the implications of these data in the Discussion by stating potential sources for the REE-Y/inferred clay distributions in the fault scarp (lines 666-731) and implications for the interpretation of paleoseismicity (lines 733-767). A key inference is that consideration should be given to controls on REE-Y distributions in hanging wall soils and fault scarps, other than just pH, and that these controls need to be explored to make potentially reliable inferences of paleoseismicity.

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 modelled slip rates from the Anogia A + drill core profile for reasons explained in the revised manuscript. Unfortunately, we did not measure scarp composition for the Anogia A samples, so we had to use a mean scarp composition from Anogia B. We did not model slip rates from the Anogia B profile for reasons provided in the manuscript. Because we are not attempting to identify individual earthquakes, using a mean composition is acceptable.

Authors' changes in manuscript: We have improved the description of what we have done and why (lines 367-389).

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 rechecked this and all samples were cut to a depth of 3 cm. The effect of sample thicknesses on age calculations is minor for samples up to 3 cm thick and thickness variations are anyway accounted for in the Schlangenhauf et al. (2010) model, which forms the basis for the Goodall et al. (2021) model.

Authors' changes in manuscript: We have revised the information on sample thicknesses (lines 191-204).

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: In the revised modelling this adjustment was removed.

Authors' changes in manuscript: Because this adjustment was removed, the associated text has been deleted.

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 have now updated the modeling using the Bayesian MCMC model from Goodall et al. (2021) and no longer attempt to identify individual earthquakes. We do though explore the effects on varying the number of earthquakes (change points) between three, five, and six earthquakes on temporally-averaged scarp exhumation rates. The number of earthquakes has only minor influence on these rates.

Authors' changes in manuscript: The revised modelling is explained in a new Methods subsection: *3.1.1 Bayesian modelling of ^{36}Cl concentrations* (lines 228-284).

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: We have addressed this issue by using a Bayesian Markov chain Monte Carlo model, published by Goodall et al. (2021), and modified from the code published by Cowie et al. (2017), to identify slip rates and slip rate changes over time. We do not attempt to identify individual slip-generating earthquakes that have exhumed the Sparta fault. These, and other models, build upon the model of Schlagenhauf et al. (2010).

Authors' changes in manuscript: The revised modelling is explained in a new Methods subsection: *3.1.1 Bayesian modelling of 36Cl concentrations* (lines 228-284).

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 Sclagenhauf 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.

In our revision we modelled ^{36}Cl data using end-member ^{36}Cl productions rates of 59.4 ± 4.3 atoms g Ca^{-1} yr^{-1} from Schlagenhauf et al. (2010) calculated from Lifton et al., (2005) and 48.8 ± 3.5 atoms g Ca^{-1} yr^{-1} from Stone et al. (1996). We present results from the former in the manuscript and from the latter in the Supplement. We also show the effects of choice of production rate on temporally-averaged slip rates in Table 2. Those effects are minor.

Authors' changes in manuscript: Table 2 shows the results from the end-member production rates of 59.4 ±4.3 atoms g Ca^{-1} yr^{-1} from Schlagenhauf et al. (2010) calculated from Lifton et al., (2005) and 48.8 ± 3.5 atoms g Ca^{-1} yr^{-1} from Stone et al. (1996). We also amend the production rate citation to: "…^{36}Cl spallation production rates (48.8 ± 3.5 to 59.4 ± 4.3 atoms g Ca^{-1} yr^{-1}). These end-member production rates are from Stone et al. (1996) and Schlagenhauf et al. (2010) calculated from Lifton et al., (2005), respectively (lines 262-265), and "…for end-member ^{36}Cl productions rates of 59.4 ± 4.3

atoms g Ca^{-1} yr^{-1} from Schlagenhauf et al. (2010) calculated from Lifton et al., (2005) and 48.8 ± 3.5 atoms g Ca^{-1} yr^{-1} from Stone et al. (1996) (lines 385-387).

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 are now included in our revised manuscript.

Authors' changes in manuscript: In addition to citing these papers at relevant points in the revised manuscript, we have expanded and updated our presentation and interpretation of the REE-Y data as follows: We have added a new subfigure (Figure 10b) to show how the LREE/HREE ratio varies along the vertical sampling profile on the Sparta fault scarp. We have also added two new subfigures (Figure 11b, c) that were previously supplied as Supplementary material and now separately plot the subsurface samples in Figure 11a. These plots are added to better illustrate the strong correlations between REE-Y distributions and the non-calcite mineralogical impurities embedded in the fault scarp breccia. We also expand upon the implications of these data in the Discussion by stating potential sources for the REE-Y/inferred clay distributions in the fault scarp (lines 665-730) and implications for the interpretation of paleoseismicity (lines 732-766).

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.

Authors' changes in manuscript: We have added the following new text: "We neither observed scarps with a total offset of 2–3 m within tens of meters downslope of the Sparta fault scarp (Benedetti et al., 2002), nor observed fault scarps within hundreds of meters upslope of the Sparta fault scarp. If earthquakes, including at 464 B.C.E., bypassed the fault scarp at Anogia (Benedetti et al., 2002), they did not leave geomorphic expressions that we observed on field reconnaissance." (lines 170-174).

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 have endeavored to address the concern of unconvincing results and conclusions in our revised manuscript by updating our modelling of the ^{36}Cl data and added new figures and text on the REE-Y results to better draw out their important implications. We do not delve much into the Benedetti et al. (2002) data, because of the high level of intersample noise.

Authors' changes in manuscript: Much of the manuscript has been rewritten to address these concerns. Because the revision has been comprehensive, we hesitate to highlight particular sections or lines, but rather refer to the marked-up version of the manuscript, which best demonstrates the extent of our revisions. Specific examples of the changes, with respect to both the ^{36}Cl and REE-Y data, have been given above. We compare our ^{36}Cl data with the data from Benedetti et al. (2002) data in lines 343-365.

Reviewer 3:

Reviewer comment: First, it's important to present the input of the original study which presented the analysis of two sets of continuous exposure history using ^{36}Cl profiles sampled on the ~10m high fault scarp on 2 fault segments at Anogia (64 samples) et Parori (65 samples) and their model allow for the identification of the 464 B.C.E. earthquake that destroyed Sparta at their Parori site together with four additional earthquakes that ruptured the Sparta fault in the last 13 ka with similar co-seismic slip of ~2 m and with time intervals ranging from 500 yr to 4500 yr (Benedetti et al., 2002). The 464 B.C.E. earthquake was not resolved from the modelling of the original 36Cl dataset of the Anogia site by Benedetti et al. (2002) and several parameters such as the inheritance and erosion were neglected in the original analysis, the geometry was simplified and the production rate of Cl36 in Calcium was actively debated at the time. Therefore, there are room for a reappraisal of older dataset to help to better assess the seismic history of normal fault using ^{36}Cl data. Yet, at this stage, it is difficult to assess the reappraisal and improvement made on the ^{36}Cl analysis at Anogia and I would therefore recommend major corrections to be done before considering any publication. It is unfortunate that the REY data has not help much as with other studies (i.e Manighetti et al., 2010), so I will focus my remarks and questions on the cosmogenic data analysis.

Authors' reply: We agree with the reviewer that there are advances in the cosmogenic science that justify a revisiting the pioneering Benedetti et al. (2002) study of site(s) on the Sparta Fault, and we reworked extensively to improve our analysis of the Anogia site in the revised manuscript. In addition to the extensive work already presented, including sampling along two transects at Anogia, including one directly adjacent to their sampling profile, incorporating REY analyses, and the inclusion of an updated and comprehensive model (Schlagenhauf et al., 2010) to include additional important controls on ^{36}Cl concentrations (e.g., inheritance, erosion, and scarp geometry), we have taken the opportunity of revision to update our analysis with a Bayesian Markov chain Monte Carlo model (Goodall et al., 2021).

We highlight here, and have increasingly highlighted in our revised manuscript, that it is important for the community to learn of our (negative, but revealing) REE-Y results. They point to controls other than soil pH on REY distributions of a carbonate fault scarp. This is an important finding with implications for research methodology that extend well beyond the Sparta Fault scarp. It is a key point that two reviewers have missed, which indicates a need for us to better explain the implications of our REE-Y results.

Authors' changes in manuscript: The changes to the manuscript regarding modelling of the ^{36}Cl data have been comprehensive because we have updated the initial modelling, based on the Schlagenhauf et al. (2010) code, to Bayesian modelling of slip rates, using a code published by Goodall et al. (2021). We have added a new Methods subsection: *3.1.1 Bayesian modelling of 36Cl concentrations* (lines 228-284), new figures, tables, and text in the Results to present the Bayesian modelling of slip rates (Figures 3 – 5; Tables 1-2; lines 390-430) and new exploration of the data in the Discussion (lines 530-595).

Reviewer comment: The revised ^{36}Cl modelling is only apply to the one of original profile, it might help to revised both sites for the discussion using the same production rate & codes??

Authors' reply: This is a good idea, in theory. However, the Benedetti et al. (2002) data has excessive intersample noise and their measured ^{36}Cl concentrations at Anogia are significantly higher than ours (i.e. they do not overlap with our concentrations within 1σ uncertainty). We think it likely that our lower measured ^{36}Cl concentrations, and lower intersample noise represent advances in laboratory preparation techniques since the Benedetti et al. (2002) study. In our opinion, the excessively high concentrations and intersample noise precludes detailed inferences on paleoseismicity. We therefore do not consider it worthwhile trying to analyze their two profiles in detail. Please note that our comment is not a criticism of Benedetti et al. – it was a pioneering study.

Authors' changes in manuscript: We have updated our modeling of our ^{36}Cl data as previously described and have compared our measured ^{36}Cl data with that from Benedetti et al. (2002; Figure 2 and lines 343-365). However, we have not attempted to model the Bendetti et al. (2002) data for the reasons described above.

Reviewer comment: Scaling samples that have different geometry, thickness and therefore attenuation must be discussed and scaled properly.

Authors' reply: We rechecked the original data and all samples are 3 cm thick. The impact of a 0.5 cm difference in thickness is, however, minor. The attenuation and scaling were, and are, correctly accounted for.

Authors' changes in manuscript: We have revised the text on sampling for ^{36}Cl analyses in lines 191-204.

Reviewer comment: Combining samples from different profiles that have different fault geometry, erosion, inheritance and potential shielding must be discussed.

Authors' reply: The fault geometry is the same at both of our profile locations (separated by 50 m), within our measurement limits. We assume erosion to be the same at both profiles based on surface features of the scarp and because there is no way to independently assess post-exhumation erosion of the scarp surface. We experimented with different erosion rates in our ^{36}Cl modelling. We will ensure that the points raised here by the reviewer are clear in our revised manuscript.

Authors' changes in manuscript: Sample information is proved in Table S1, which has been updated to include shielding in the footnote (all samples have the sample shielding). We present relevant site characteristics, including indicators of erosion, in lines 144-174.

Reviewer comment: There is no clear comparison of the results of the different models, a figure would help clarify (height versus time, using the co-seismic slip of each earthquake estimated age).

Authors' reply: We have comprehensively remodeled our ^{36}Cl data using a Bayesian MCMC model from Goodall et al. (2002), as previously indicated.

Authors' changes in manuscript: The changes to the manuscript regarding modelling of the ^{36}Cl data have been comprehensive because we have updated the initial modelling, based on the Schlagenhauf et al. (2010) code, to Bayesian modelling of slip rates, using a code published by Goodall et al. (2021). We have added a new Methods subsection: *3.1.1 Bayesian modelling of 36Cl concentrations* (lines 228-284), new figures, tables, and text in the Results to present the Bayesian modelling of slip rates (Figures 3 – 5; Tables 1-2; lines 391-431) and new exploration of the data in the Discussion (lines 530-595).

Reviewer comment: The modelling of the ^{36}Cl data does not appear to include the contribution of all the pathways despite being integrated in the codes of Schlagenhauf et al. (2010). 36Cl is produced by spallation of K, Ca, Ti and Fe; slow negative muon captures by K and Ca; and low-energy (thermal and epithermal) neutron capture by 35Cl and also not integrated in the modelling, composition data are available for the original dataset (see appendix of Benedetti et al. 2002) and the new 36Cl. That will affect the model ages of the different earthquakes and there is no need to average over the profile if the data exist for each sample.

Authors' reply: In our original submission we did include all pathways for ^{36}Cl production, according to the model of Schlagenhauf et al. (2010). However, we did not explicitly state that, which we have now done in our revised manuscript. Concentrations of K, Ti, and Fe are, though, very low in our samples, so ^{36}Cl production from these elements is a minor contribution to the ^{36}Cl inventories.

We state in the Methods section that we use the 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 on the Benedetti et al. (2002) profile, from which the relevant sample specific chemical data are also missing (including in their published appendix).

Authors' changes in manuscript: We now explain our treatment of the various production pathways for ^{36}Cl in lines 230-233 and in lines 264-271. The treatment is according to the Schlagenhauf et al. (2020) model, which is built into the Bayesian MCMC model from Goodall et al. (2021), which we have used in our revision.

Reviewer comment: The production rates of Cl36 in Ca, K, Fe and Ti used in the study need a proper discussion and be better justified. Several aspects are typically discussed, the production rates of the different targets, the scaling factors used, the atmospheric model, and the geomagnetic database used to correct for the temporal variation of the production rates. The paper should be clearer on the topic, the scaling of solar modulation and long-term uncertainties defined by Lifton et al. (2005) is not a production rate paper. The production rates of the main targets producing Cl36 have also been scaled for the different scaling scheme in the CRONUS-Earth effort (see Marrero et al., 2015, even if the abstract only present the LSDn solutions). It seems strange to work on a reappraisal of a dataset without using the up-to-date production rates or at the very least present a comparison of the modelling results using different production rates.

Authors' reply: The citation that we gave in our initial submission (Lifton et al., 2005) was incomplete. The production rate we use is written in the Schlagenhauf et al. (2010) model code as Lifton et al. (2005) but we should have cite Schlagenhauf et al. (2010), calculated 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 Sclagenhauf 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, provided Dunai (2001) is avoided.

Authors' changes in manuscript: We have updated the text to better explain ^{36}Cl production rates and pathways (lines 230-233 and lines 264-271). In our revision we modelled ^{36}Cl data using end-member ^{36}Cl productions rates of 59.4 ± 4.3 atoms g Ca^{-1} yr^{-1} from Schlagenhauf et al. (2010) calculated from Lifton et al., (2005) and 48.8 ± 3.5 atoms g Ca^{-1} yr^{-1} from Stone et al. (1996). We present results from the former in the manuscript and from the latter in the Supplement. We also show the effects of choice of production rate on temporally-averaged slip rates in Table 2. Those effects are minor.

Specific comments: Thank you for your comments, which we have worked to address in our revised manuscript.

References used in Author replies.

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