Solid Earth Discussions

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Comment on se-2021-97

Gerald P. Roberts (Referee)

Referee comment on "Aegean-style extensional deformation in the contractional southern Dinarides: Incipient normal fault scarps in Montenegro" by Peter Biermanns et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-97-RC1, 2021

Overall, I enjoyed the paper and found the work on these intriguing scarps very interesting. The field documentation of the scarps seems thorough, and I think they leave a good legacy of data for others to consider. I applaud the authors for undertaking the 36Cl analyses as such work can be arduous and time consuming, and publication of quite sparse datasets like this one are nonetheless valuable in my opinion. I think the modelling of the 36Cl data is quite basic, but nonetheless they acceptable, and the results are interesting, and I hope they prompt more 36Cl sampling on these faults, with more detailed modelling, but that is for a future paper not this one. I look forward to this present manuscript being published as a paper.

Comments and suggestions for some modifications

1) In the introduction please also cite some of the papers that have used 36Cl to study fault scarps in the Italian Apennines.

2) At the end of the introduction please add a few sentences setting out the structure of the work conducted: mapping, sample collection, 36Cl sample prep, AMS, modelling of 36Cl; tectonic interpretation.

3) Line 86 I think you should cite Cowie et al. 2017 which gives the most complete account of what geomorphic requirements need to be satisfied for fault plane 36Cl sample sites.

4) Line 91 Typo? Do you mean 15 ±3 ka? That is what most people use.

5) Line 96 Please explain why you think 50 cm sample spacing is adequate. Some would argue you need to use denser sampling to identify so-called cusps (e.g. Schlagenhauf et al. 2010) whereas others (e.g. Beck et al. 2018) suggest less dense sampling is fine as long as the modelling approach takes this into account.

6) Line 99 Cowie et al. (2017) were the first to say that a trench at the bottom is needed, not Mechernich so cite that paper.

7) Section 3.2.2 on the 36Cl modelling needs more detail. I agree that the stripes on the fault plane mean spotting cusps is unlikely to work and hence I support sampling sparsely (and within available funding/logistics constraints), at least in this initial study. However, the Schlagenhauf et al. (2010) code is usually used to spot cusps, so I think you need to write some justification of why you think it is OK to use it on your 50 cm sample spacing. I think it is a good code to use as a first pass, perhaps prompting modelling with other codes in a later paper if you gain more 36Cl samples that may provide more insights (e.g. Bayesian modelling, evidence of convergence between Markov chains, iteration of

variables such as colluvial densities, attenuation lengths, production rates, slip per event, age of initial 36Cl production/scarp age, etc.). But to use the Schlagenhauf code one must show/state some things that are used in that code (e.g. pre-exposure). Please state/show the following in the text or in a supplement: (1) value for pre-exposure, with some justification for why that value was chosen; (2) provide a data table with all elemental compositions for each sample, or at least what you have, with Ca concentration vital; (3) how you use the Schlagenhauf code if you do not try to resolve cusps, that is how you choose and propose earthquake slip histories and their implied 36Cl concentrations for comparison with the measured concentrations; (4) how and why you model the "sliding event" in Fig. 7.

8) Section 4.2.2 provides useful information, and its contents should be published because they are interesting. However, please provide more detail. Tell us exactly why you think there is a robust relationship between the slip history you propose and the measured 36Cl concentrations. In other words, explain your results and how you derived them, rather than just stating what you think the results are. How do the model results relate to the data error bars for example.

9) Section 4.2.2 should also be longer. I would expect the results section to be significantly longer, with text explaining what exactly the reader should look at in each of the "results" diagrams", with a summary at the end explaining the overall result which would set the scene for the following discussion section.

10) Section 4.2.2 should also perhaps discuss other possibilities for the 36Cl modelling results, stating why the chosen one is thought most likely to be correct. For example, the "result" that there is a "sliding event" (see Fig. 7) needs more explanation. Why is the 7c the "most likely" (see the caption)?; please explain. Is there geomorphic evidence for a "sliding event"? Please describe it, or if not say so. How is this constrained with the modeling? Do you mean a landslide event? If so, please clarify. Another example is the claim that slip commenced at 6 kyr ago (see abstract). Can you clarify why you think this? Could it not also be that slip is clustered, with a cluster starting at 6 kyrs BP, with a period of no slip before that (an anticluster), perhaps with other clusters and anticlusters in the time period before that resolved by your 36Cl data? In other words, perhaps the slip and the new tectonic regime is not so "incipient" as you claim in the title of the paper. In other words, (a) in an interpretation that considers clustering, slip did not "commence" at 6 kyrs BP, but rather long-term slip was ongoing before then, but a cluster started at 6 kyrs BP, whilst (b) in an interpretation that does not consider clustering, slip "commenced" at 6 kyrs and so the deformation is "incipient". I think the paper would be improved if both of

these possibilities were considered (a clustered interpretation and one with no clustering). I think the paper would be cited more widely if you include both. However, this is up to the authors and I do not insist on this.

11) Line 201 Typo? 15 ±3 ka?

12) I found the discussion section interesting and thought provoking, which is good.

13) In the supplement, please re-organise and rotate the photographs and diagrams so that they can be viewed without having to rotate the page. Most people will read this as a pdf and having to rotate pages can be annoying.

14) In Fig. S7 use a linear rather than log scale for the y axis, as this is the standard approach for this type of plot.

15) Fig. S8 Please indicate the source of the topographic data in the caption.

16) Please add the rock geochemistry for the 36Cl samples to the supplement.

17) I have a slight concern that I may have missed some supplements (apologies if this is the case), but I found it slightly difficult to be sure I had accessed all available material on the review website.

Thanks to the journal for asking me to review the paper.

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