

Solid Earth Discuss., author comment AC4
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Final response on RC1

Peter Biermanns et al.

Author 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-AC4>, 2021

Dear Professor Roberts,

we would again like to cordially thank you for your highly qualified and well-founded comments on our manuscript, as well as for your kind words and benevolent résumé. In our author comment 'AC2' from Nov.5, we responded to your individual comments already to encourage further discussion. Since we posted that author comment, our conception of the manuscript has not significantly changed so that the present "final response" corresponds 95% with the cited earlier author comment. We only specified a few of our statements. In the following, we list our thoughts and plans referring to individual comments:

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

Nothing speaks against mentioning these papers in the introduction. Furthermore, we will check if the list of cited papers could be expanded by further useful literature.

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.*

Certainly, these aspects should be part of an introduction so that we will readily accept this task!

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.*

We will certainly do so.

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

This seems to be a matter of debate, as can be seen by the fact that the second reviewer suggested 20 ka in this context. We chose 18 ka in all conscience, after (from

our point of view) synthesizing an ideal age from works in the surrounding areas. In section 5.1, we discuss the reliability of this age which is, e.g., also used by Papanikolaou et al. 2005, or Giraudi and Frezzotti, 1995. As our slip rate calculations based on the proposed LGM age are easily reproducible and “convertible” from the provided tables, we prefer to maintain our 18 ka age – also as a compromise between both reviewers.

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.*

We will clarify this in the text and refer to Beck et al. 2018.

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.*

No objections. We will certainly do so.

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.*

This will be added.

... 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;

This value is given in Table S13. We will add a justification.

... (2) provide a data table with all elemental compositions for each sample, or at least what you have, with Ca concentration vital;

This was already prepared in Tables, but unfortunately not uploaded, see author comment ‘AC1’.

... (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;

A quick explanation how we use the code will be added.

... (4) how and why you model the “sliding event” in Fig. 7.

An explanation will be added.

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.

We will extend this paragraph by adding the methodology including further points to the uncertainty calculation and the results in more detail.

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.

As mentioned in point 8, we will expand this chapter. Good idea to include a summary highlighting the overall results of the ^{36}Cl modelling.

10) Section 4.2.2 should also perhaps discuss other possibilities for the ^{36}Cl 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.

We say in line 234, that there is no evidence of a "sliding event"

... How is this constrained with the modeling? Do you mean a landslide event? If so, please clarify.

Thanks for highlighting that our ideas on the "sliding event" are not yet fully comprehensible. We will include further clarifications on the modelling of the "sliding event".

... 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 ^{36}Cl 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.

Indeed, the "commenced at ca. 6 kyr" in the abstract is misleading and we will clarify this. We will check if it is reasonable and fitting to this ms to include a discussion on a clustering of the earthquake events.

11) Line 201 Typo? $15 \pm 3 \text{ ka}$?

No typo, see # 4.

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

Thanks a lot for this motivating comment! With your further constructive but more critical comments, we are stoked to further improve and put the final touches to our manuscript.

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.*

We will do so.

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.*

We are fully aware and agree with you that this is the common way of illustrating such plots. However, we purposely decided for a log scale since we are looking at a large range of free face (eroded and non-eroded) heights. In a linear scale, especially the (particularly interesting) non-eroded scarp heights would hardly be distinguishable. We therefore prefer to keep the present display.

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

Thanks for this valuable hint - we totally missed this. Those are TanDEM-X data which we will certainly cite.

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

See # 17: This was part of an xls sheet that we simply forgot to upload.

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.*

This was our mistake, as we forgot to upload an xls sheet that was supposed to be part of the supplement. This happened, as all other supplementary material (except for the xls file) is combined in one PDF. We immediately uploaded the missing file to the discussion. We apologize for the inconvenience and thank you for disclosing this problem.