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Dear editorial board, dear reviewers

We are happy to provide a revised version of our manuscript "Aegean-style extensional deformation in the contractional southern Dinarides: Incipient normal fault scarps in Montenegro".

As you will see from the attached documents, we extended and redid large parts of our manuscript. According to the excellent comments of all reviewers, we rearranged existing text passages and added many new ones in order to achieve a clearer storyline and to more detailedly elaborate on controversial topics.

In particular, we further stressed the fact that two different normal fault scarp formation mechanisms are anticipated. Furthermore, we substantiated our argumentation on why we assume a co-seismic formation and growth of the structures. By doing so, we extended the discussion on possible non-seismogenic formation mechanisms (particularly gravitational sliding). Several features on and around the fault scarps are described more extensively. We more critically discuss and 'downgrade' our qualitative fault scarp profiling method as opposed to our absolute ^{36}Cl dating, which we now describe in more detail. This allowed us to also further extend our conclusions on the slip behaviour and history across the introduced faults. Having mentioned a few major adaptions, all 43 reviewer comments are detailedly answered point by point on the upcoming pages.

We again thank the editor and reviewers for their enormous effort and their constructive comments that greatly enhanced the quality of the present manuscript.

Best regards on behalf of all co-authors

Peter Biermanns

Detailed reply to the review of **G. Roberts**:

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

   → We implemented this by citing the work of Palumbo et al., 2004; Schlagenhauf et al., 2011; Benedetti et al., 2013; Roberts et al., 2014; Tesson et al., 2016; Cowie et al., 2017; Goodall et al., 2021; Iezzi et al., 2021 (lines 44 ff.). By slightly re-arranging the introduction and responding to your comment #2, we achieved a clearer setup giving examples of general/undifferentiated paleoseismological studies, as well as specifically 36Cl-related studies in Italy and Greece.

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.

   → To meet a few other criteria – largely implied by Prof. Benedetti – we generally re-arranged the full text body (not only the end) of the introduction and added some extra information, particularly on the aim and setup of our work (lines 37 ff). This should perfectly serve your request already. However, we also added a conclusive sentence providing an outlook on the upcoming manuscript (lines 63 ff).

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.

   → Done (now in line 111). We additionally added this perfect reference in chapters 1, 3.1.2 and 5.2.

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

   → Following the argumentation in our "final response", we kept the 18 ± 3 kyr date. However, we greatly extended our discussion in section 5.3 (lines 345 ff.) to best-possibly justify the chosen date and to more critically scrutinise the NFS profiling method itself. Even the three divergent reviewer comments exemplarily reflect that the used method and particularly the presumed timing are a matter of heated debate. We therefore made sure to keep our phrasing very defensive.

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 added the following two sentences (lines 130 ff.): "This sample spacing is rather large compared to previous studies of cosmogenic fault scarp dating and hence it is not possible to identify the stepwise ^{36}Cl concentration pattern (so-called cusps, e.g. Schlagenhauf et al., 2010). In our study, this is not a disadvantage, since the capable offsets of the small faults are lower than the ~2 m of coseismic offset required for the generation of a stepwise ^{36}Cl concentration pattern. We take the low sample density into account in the modelling approach. As published, such resulting modelled slip rates are comparable to slip rates derived from dense (continuous) sampling (Beck et al. 2018; Iezzi et al., 2021).**”**

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.

   → Done (now line 138). We additionally added this reference in chapters 1, 3.1.1 and 5.2.

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.

   → Concerning the 50 cm sample spacing: we added some sentences in chapter 3.1.2 (lines 130 ff.; see reply to comment 5). Furthermore, we added in section 3.2.2 (line 164 f.): “The code was adapted to the low sampling spacing and the mapped ribbon heights were added to the input parameters.“

   → (1) All input parameters, including the mentioned pre-exposure, are given in table S14.

   → (2) The missing file was uploaded immediately after your review already. Sorry again for the inconvenience!

   → (3) How we use the Schlagenhauf code: We included now “The code was used by iteratively modelling constant slip rates of the Bar fault, which fit the ^{36}Cl best (lines 192 f.). […] We applied the criteria that all scenarios covering the 68% uncertainties of at least 3 of the 5 ^{36}Cl samples are considered, so that a 1σ in ^{36}Cl-internal uncertainty of this constant slip rate is derived.” (lines 197 ff.)

   → (4) The “sliding event” is modelled to approach the lowest possible slip rates on the Bar fault. Here as well we used the Schlagenhauf-code and added a 7,5 m offset within a small amount of time (1 year).

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 added further details on the methodology in Section 3.2.2 and also some additional methodologic points in section 4.2.2. As requested, section 4.2.2 is restructured and rewritten, so that we first present the results without interpretation.

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

→ We significantly expanded this section in the frame of the restructuring mentioned in point 8. We also highlight the overall results of the ^{36}Cl modelling. The suggested summary is included in the last paragraph of the chapter.

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.

→ Concerning the "sliding event": We explain now that the purpose of modelling a "hypothetical sliding event" was to find out which is the lowest slip rate the ^{36}Cl data is fitting to (lines 273 ff.). As this is hypothetical, we repeatedly state that no indicators of such sliding events have been found (e.g., lines 200 f., 326, 391 ff.).

→ concerning the originally mentioned "slip commenced at 6 kyr": In the abstract, we now write "commencing, or resuming, ~6 kyr ago " (line 18). In chapters 3.1.1 (lines 119 f.), 5.3 (lines 368 ff.) and 6 (lines 412/418 f.) we explicitly name the possibility of clustering. We stress that ~6 kyr is only the age of the free-face. Already the full scarp age is probably

~14.8 kyr, which is fitting well to the findings in the Apennines (line 333). Additionally, we included in the discussion (Section 5.2), that according to the maximum offset, the fault did not start its activity just ~6 kyr ago, also in the case of non-clustering. For the calculated slip rate of ~1.5 mm/yr and the max. offset of 400 m, the fault would have initiated ~270 kyr ago. In terms of fault age, this is a very young and “incipient”. As in chapter 5.2, we incorporate ribbon widths, we moved the according chapter (now 5.1) to the front.

11) Line 201 Typo? 15 ±3 ka?

→ See our response to your comment # 4 on why we chose an 18 kyr time frame.

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

→ Thanks for this motivating comment. Following the other reviewers’ comments, we still worked on the discussion sections and hopefully further improved them.

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 changed this according to your suggestions.

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.

→ Owing to the reasons listed in “final response”, we decided to keep the log scale. We are dealing with 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. Thank you for your understanding!

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

→ We added the source in the image caption.

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

→ See your comment #17: The missing file has been uploaded in the meantime. Sorry again for the inconvenience.

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.

→ The missing file has been uploaded in the meantime. Sorry again for the inconvenience!

Detailed reply to the review of **L. Benedetti**:

18) The question of the surface expression of the faults affecting an area and how those are interpreted in terms of kinematics and seismotectonic of an area is crucial. However, the authors somehow avoid to thoroughly discuss this question, and very quickly interpret those faults as active and as the surface expression of an active extension, however normal faults have been observed also in compressional context (see my last comments)

→ We added several passages to the Abstract (lines 21 ff) and chapters 1 (Introduction), 2 (Geological setting), 5.4 (Discussion) and 6 (Conclusion) to make our argumentation clearer and to make sure that key aspects/statements are not overlooked.

- In the Introduction we now very clearly underline the fact that we consider at least two different formation mechanisms of the normal fault scarps (including a syn-contractional formation). In this context, we already go into detail on what formation mechanisms we consider exactly (lines 55 ff). In your further comments, you often refer to the 'El Asnam' earthquake, which is a good example to be discussed and compared. We therefore added a couple of references on this topic (lines 56 ff). Furthermore, we added a few sentences on the structure of our work and the techniques we used to prove the activity of the normal faults and to derive slip rates (lines 37 ff). In that sense, we pre-announce that the reader can expect a well-founded and detailed storyline and argumentation in the upcoming chapters, justifying our initial statements. In our opinion, this is enough detail for the introduction, since this chapter should not pre-empt the methods, results and discussion chapter too much.
- In chapter 2 – "Geological setting" we added a bit of extra information on the geodynamic setup the Dinarides-Hellenides (lines 67 ff, 81 ff). We hope that this underlines the close proximity of a hinterland extensional domain in the Hellenides which MIGHT play a role during fault scarp formation. Furthermore, we added the (important) relation between fault scarp orientation and bedding in this chapter. already (line 89). Apparently this information got lost in the last version of our manuscript due to an insufficiently prominent placement.
- In chapters 5.4 and 6 we still introduce our initial two explanation approaches concerning fault scarp formation. To avoid confusion and to make things even clearer in both chapters, we made sure to mention the 'syn-convergence' theory first and/or to say very clearly that we rate this as the likelier option (lines 379 f., 389 ff, 410 f.).
- Referring to your reproach that we 'very quickly interpret those faults as active': We are of the opinion that we already delivered substantial arguments for that, especially in chapters 4.1 and 5.3 of our first manuscript version. However, we still extended these aspects (e.g., lines 217 ff., 286 ff., 391 ff.). See also our detailed responses to your further comments.

19) The tone of the paper somehow provocative and assertive […]

→ We tried to avoid the impression of a provocative and assertive tone.

- On the one hand by making our storyline clearer and underlining that we consider different formation mechanisms (also see e.g., our response to your first comment, comment #18) and openly discuss any further ideas. In the introduction, we modestly underline that our paper is a first-time description of possibly seismogenic fault scarps (lines 37 f.). We also describe our initial 36Cl dating as “low spatial resolution” (line 40).

- On the other hand we looked for wording that may leave a ‘provocative’ impression. For example, we rephrased the sentence that you particularly critizised *‘We consider this view obsolete’* (old ms version: line 63; now in lines 84 ff).
- Furthermore, we did our best to incorporate very ‘defensive’ phrasing when facts are not 100% assured (e.g., lines 117 ff., 222 ff., 347 f., 350 f., 364 ff., etc.)

20) On Figure 4, the colour are difficult to distinguish but the normal faults appear to correspond to the contact betwen Mezosoic carbonates and Eocene or Paleocene. This is puzzling since if there is activity over the Quaternary there should be some Quaternary deposits on the hanging wall attesting for the hanging wall subsidence […]

→ We exchanged the original colours of the official geological map so that the single units are now way easier to differentiate. To highlight that the NFS (particularly KFS) do indeed NOT correspond with the close-by tectonic nappe contacts, we implemented a less generalised display of the nappe contacts. In the re-arranged map both features are well-distinguishable. Furthermore, we added the indeed present Quaternary hanging wall deposits. Thanks for this valuable comment!

21) Moreover I have not seen in the paper a mention about the bedding of the carbonates. It is important since hanging valleys could appear as such if the bedding is vertical and not be related to the recent fault activity. Is it possible that those are exhumed features due to active folding?

→ The bedding is now already mentioned in the abstract (line 12 f.) and geological introduction (line 89) with the information that the NFS cross-cut uniformly NE-dipping limestone beds at high angles. Also, the bedding symbols are now explained in the legend of Fig. 4.

22) The assumption that the fault scarps are 18 ± 3 kyr supposed that all faults started to resume activity at that time which is not correct since some faults could have started to resume a seismic activity later for example 10 or 12ka ago, with a long quiescence time between 15 and 20 ka ago. The 36Cl dating is an absolute dating of the scarp exhumation whatever the cause for this exhumation, seismic or others. So I don’t think the comparison brings anything to the paper and does not make the slip-rate calculation convincing to me. You can use the assumption that those scarps are post-glacial if you have no dating but if you have an absolute dating you can discuss this assumption by mentionning that the yielded ages for the fault scarp are in agreement with an hypothesis of post-glacial exhumation but note use an age based on an hypothesis to compare a result you yield with an absolute dating. Moreover the LGM in the Appenines is probably closer to 21 kyr ago and this could be different in the Dinarides […]

→ We generally agree with you that the absolute dating method is the way more ‘solid’ evidence and should therefore be moved clearly to the foreground. However, since we only dated 1/4 of our selected sites by 36Cl dating, we still believe that the alternative profiling method is a valuable tool, also due to the differing setup (orientation, bedrock, are they connected?, etc.) of the BFS and KFS sections that might yield differences in their slip behaviour. Additionally, the qualitatively derived rates set a frame for the absolute lower limit in terms of slip rates. We added these statements at the end of Section 3.1.1. (lines 117 ff.) to explain our intention. Furthermore, we do not see any reason to exclude gathered data and calculations from the manuscript. Now that (according to your good suggestion) we highlight the absolute dating, each reader can independently decide on how to rate/weight these data.

- In the abstract, we highlight the 36Cl dates as more reliable and with higher resolution (line 17)
- In 3.1.1 (lines 106 ff), we changed the order of both methods and say that the profile-based slip rate calculations were an auxiliary low resolution tool for comparison of four sites with the absolute 36Cl date from site BFSN. We list major weaknesses (as compared to an absolute dating method) in lines 117 ff.
- In 5.3 (lines 338 ff., lines 347 ff., lines 364 ff.) we thoroughly discuss the profiling method, its significant weaknesses, and the use of the 18 ± 3 kyr time frame. We highlight that the profiling method is a simplistic (lines 249 and 340) low resolution auxiliary tool that we particularly used because not all four sampling locations were dated by 36Cl and to derive an absolute lower limit. In lines 347 ff., we justify - but at the same time critically scrutinize - the 18 ± 3 kyr time frame. We made sure that the phrasing is very defensive, since we know that both the method and the timing are a matter of debate. Please also see our response to G. Robert’s comment #4 on why we decided to keep the 18 kyr.
- In chapters 4.2.1 (line 248) and 6 (lines 411 ff.) we added passages once more underlining that the profiling-derived slip rates were particularly calculated because not all four sampling locations were dated by 36Cl and to derive an absolute lower limit.

23) The identification of earthquake slip is based on qualitative observations that are very difficult to interpret in my opinion. The pictures presented do not allow the reader to actually reproduce those observations or appreciate their quality. The authors do not discuss their origin at all and interpret them as seismic exhumation. This is very discutable and should not be presented as straighforward. How are those ribbons oriented in comparison to the slope ? Could snow or others processes of erosion produced similar features ? how the 5 horizons were distinsguished ?

→ After a thorough re-reading of the old manuscript version, we are still surprised about the reproach of not thoroughly describing the ribbons or discussing their origin. Although we did not see a lot of room for maneuver, we hopefully managed to add and/or puff up a few aspects and details. We apologize for the quality of the images that you critizise. However, we cannot really change this, since the presented pictures are the best we have available. First of all, many of the photos were taken in heavily forested terrain (which also has an effect on the state of the ribbons) with poor lighting. Secondly, we are dealing with relatively narrow ribbons that are partly not perfectly preserved or defaced (probably different from what you know from Italy/Greece; By the way, we openly address this problem: lines 220 f., 301 f.). A perfectly easy distinction might therefore only be possible from close distance in the field. However, we are of the opinion that our images indeed DO show perceptible and distinguishable horizons.

In Section 4.1 we added a few descriptive aspects (lines 217 ff, 221 ff.) as well as a reference to our discussion chapter 5.3 (line 224). In the latter, we added a large text block where we discuss and exclude all kinds conceivable of non-seismic agents that could possibly create such ribbons (lines 287 ff), including your proposed snow theory.

24) Moreover, the way they are presented in the abstract is misleading because it suggests that the slip amount and age is deduced from the 36Cl profile. While it is not possible to retrieve event on a 8 m-high scarp with such low resolution (5 samples). The way the age of the event is retrieved is not clear. Did the authors introduce the slip yielded from the ribbons observations and injected those values in the model as a direct model to yield the ages ? This has to be much better explained in the text.

→ In the abstract, we clearly distinguish which dates were derived from 36 Cl dating, and which ones were obtained from the qualitative method (lines 15 ff.). By saying that the absolute dating is more reliable and higher resolution, we clearly differentiate between both methods. Also in the remaining manuscript, we implemented a lot of changes to differentiate between absolute and qualitative methods and clearly rate their quality (for details, also see or response to your comment #22). In terms of the 36 Cl method, we still admit that we chose a low sampling density (e.g., line 40, lines 130ff., 135 ff., 164 ff.) and justify why we expect robust results nevertheless. In Chapter 4.2.2 we added details on the calculation method of the earthquake ages at site BFS_N.

25) In the introduction the relations between the normal faults identified and the present day kinematic of the area is problematic to me. The presence of normal faults in the Apennines and in Albania is in agreement with the seismotectonic of the area while geodesy and focal mechanisms support no active extension in the Dinarides. So mixing those aspects in the introduction is misleading. Even more that the authors have not yet shown their observations and discuss the origin and the mechanisms underlying their observations. So I would present all this with much more caution, saying that while in the Dinarites active tectonics is driven by compression, the presence of those two normal faults is puzzling and the purpose of your pape is to understand how those features can be interpreted. First by answering the question, are those faults active or not ? The fault potential activity should be thoroughly discussed, after reading the paper as it is I am not convinced that those are active normal faults. Second, if we assume those faults have been active over the Quaternary, they could be the surface expression of flat and ramp fold as it has been described during the El Asnam earthquake in 1980. The mechanical processes is explicitated in this paper Avouac, J. P., Meyer, B., & Tapponnier, P. (1992). On the growth of normal faults and the existence of flats and ramps along the El Asnam active fold and thrust system. Tectonics, 11(1), 1-11. Such possible explanation should be added in the discussion and the bibliography concerning surface expression of folding should be thoroughly studied and discussed in that paper. It could make the paper much more appealing. If those faults are actually the surface expression of the fold an thrust affecting the Dinarides they could indeed be use to retrieve the seismic history of compressional events.

→ In our response to your comment # 18, we already react to the majority of your points of criticism. To achieve a clearer structure of the manuscript and to synchronise our statements more clearly, we added a few sentences about the aim and the setup of our work and methods (lines 37 ff; including the question if these are active faults). Furthermore, we clearly highlight that we do not consider active extensional regime the only explanation for the existence of our NFS. We explicitly (and repeatedly – not only in the introduction) mention that a syn-convergence formation of the NFS is conceivable (e.g., lines 55 ff.), now underpinned by some of the literature you suggested (lines 56 ff). According to our “final response”, we prefer to leave detailed descriptions and discussions in the according chapters (4 and 5), as the introduction is designed to give an overview of the topic as well as an outlook on the upcoming chapters without pre-empting all of their content.

26) line 23-26: all those aspects are purely speculative and should not be in the abstract, you have not proven or provide strong evidence for a kinematic change and no evidence of geophysical observations showing the upper plate of the slab is affected.

→ See our response to your first comment. We clearly distinguish and name two possible NFS formation mechanisms. In the abstract we say that we '*consider*' … '*explanation approaches*' which is a quite a reluctant statement (lines 21 ff). The existence of a close-by extensional domain in the Hellenides is not speculative but well-acknowledged and justified in the main body of the manuscript (particularly in chapter 2).

27) line 41: you probably mean instrumental earthquakes and not historical ?

→ We changed this (now in line 53).

28) line 47-49: please look carefully in the litterature about normal faults in active fold and thrust belt, they can also be the surface expression of contraction (see my comments below but there are probably more examples now since El Asnam).

→ We repeatedly introduce the syn-convergence explanation approach (e.g., see responses to your comments #18 and 25). In the Introduction, we now particularly stress this in lines 55 ff and added some of the suggested literature.

29) line 56: Extension in the Apennines is also attributed to Adria microplate rotation (see papers by D'Agostino et al. 2008, Nocquet 2012), please also cite those papers.

→ We think that the text says very clearly that the extension in Italy is also due to the Adria microplate rotation and more or less a mirror-image of the Dinarides-Hellenides setting. We added the two suggested papers as references in this context (line 74).

30) line 63: it is not a view, this is based on evidences and before considering them obselete you should at least present your evidence and discuss the previous published ones. The tone is problematic to me, it is not an opinion paper, it is a scientific paper.

→ We added a bit of extra information on the geodynamic setup of the Dinarides-Hellenides and tried to underline very very clearly that there IS a close-by extensional domain in the (internal) Hellenides (lines 81 ff, see our response to comment 18). We cite several high-quality papers (lines 74 ff, 82 f.) on this well-acknowledged extensional region that can also be easily distinguished in Fig. 1. Additionally, we rephrased the sentence that you particularly critizised '*We consider this view obsolete*' (now in lines 84 f.).

31) line 87: what to you mean ? 36Cl dating is not affected by vegetation. Maybe you mean for 36Cl sampling ?

→ We removed the information in the brackets. Indeed, we wanted to convey that we removed the vegetation for sampling (as you say). However, this is not really a crucial information.

32) line 91: where does the date 18 ± 3 kyr come from ? please cite papers or discuss this date.

→ In this particular passage, we now cite Papanikolaou et al., 2005 (line 115). We additionally refer to the discussion chapter 5.1 where we justify and scrutinize the use of our 18 kyr age. Please also see our response to your comment #22 and G. Robert's comment #4.

33) line 131-133: what do you mean ? not clear to me.

→ We rephrased this sentence to the best of our knowledge and beliefs (lines 169 ff).

34) line 176-178: the radial pattern suggest landslide feature, why not discussing it ? could it be realated to bedding slip ?

→ We significantly extended our discussion on landsliding and why we do not see this as an agent creating the NFS (lines 391 ff.).

35) line 184: five horizons are very speculative, please discuss what could be their origin besides seismic slip.

→ We significantly extended our discussion on possible formation mechanisms (lines 286 ff). See also our response to your comment #23.

36) line 257: really not convincing, how is the bedding ? if it is perpendicular to the fault plane it is more convincing, please discuss that.

→ As described in previous responses: We highlighted the (indeed perpendicular) relation NFS/bedding by mentioning it earlier in more prominent sections (Abstract, Geological introduction) of the manuscript. See your comment # 21

37) line 265-268: what do you mean ? it is not clear whether you suggest those faults are an effect of the contractionnal regime and it appears in contradiction with what you said in the introduction.

→ We hope that we now managed to VERY clearly show that we are – and always have been – considering two possible formation mechanisms. We added a sentence in the Abstract, listing both options (lines 21 ff.); we did basically the same in the introduction (lines 55 ff.), in chapter 5.4 (Discussion, lines 379 ff.) and in the Conclusion (lines 408 ff.)

Detailed reply to the review of **Reviewer 3**:

38) as observed in many cases, active thrusting can give origin to secondary features associated to it. Above all, bending moment faulting at the hanging wall of thrust faults is a typical feature secondarily connected to compressive deformation. In this term, even if the authors do not deal specifically with current activity of the thrust fault onto which the normal faults are supposed to grow, the authors themselves state that the supposed active and seismogenic normal faults under investigation occur along the coastal area, where active compressive deformation occur, and not in the hinterland, where extensional tectonics is ongoing (lines 63-67). In many cases in the Alpine chain, active thrusting give origin to dip-slip fault scarps, even some km long, on top and at the front of growing anticlines that resemble normal faulting, but which are secondary, passive, non-seismogenic features being extrados structures and large-scale gravitational sliding owing to forelimb collapse. Examples of this have been observed at different places in the central and eastern Alps, such as those investigated by Galadini et al. (2001) and Zanferrari et al. (2008) along the Mt. Baldo and the Mt. Jouf active thrust faults, respectively. On this topic see also Lettis et al. (1999)

→ We extended chapter 2 by a couple of aspects (lines 67 ff., 77 ff.). A broad description of the Dinaric thrusts in the particular region (lines 88 ff.) is accompanied by references to relevant papers from our own working group (line 95) and others. By answering particular comments of reviewer Benedetti (e.g., her comments 18, 21, 25, 26 and 28) and implementing many of her improvement suggestions in our revised manuscript, we were already able to more clearly (a) underline that we certainly consider syn-contractional formation mechanisms of the fault scarps and (b) clearly exclude non-seismic formation mechanisms (e.g., chapter 4.1, lines 208 ff; chapter 5.1, lines 286 ff.). Your comment particularly motivated us to further focus on arguments against gravitational sliding which we present in Section 5.4 (lines 391 ff.) We again refer to our 'final response' in which we justify our belief that the DSGS theory or the thoughts of Kastelic (2016) are not applicable to our sites. Studies that are better comparable with our setting are sufficiently cited throughout our manuscript.

39) The fact that the fault plane exposure is only due to tectonic movements and not to other non-tectonic phenomena is a critical aspect. The authors claim that fault exposure is not associated to landsliding because no indication of it is found in the sampling sites. Nonetheless, they do not provide any evidence of this assumption, such as detailed geomorphological maps of each sampling sites or pictures demonstrating long term (tens of thousands of years) slope stability. Moreover, at least sampling sites b, c and d in Figure 3 seam to coincide to visible stream incisions, testified by the white stripes (likely scree) evident in the provided picture. This appears even more evident in Figure S1, where sampling sites coincide or with stream incisions (and fault plane exposure can be simply the product of erosional exhumation) or with sectors of the slopes characterised by high topographic gradient, where gravitational component of the fault plane exposure cannot be ruled out and thus quantified. In this perspective, triangular facets and wine-glass-shaped valley are not tout court evidence of normal fault activity (lines 160-161), as stated by the authors. Indeed, formation of these supposed morphotectonic features can be due to differential erosion across the fault scarp. The authors do not demonstrate the lack of this process before claiming tectonic-related exposure.

→ We particularly extended our arguments against a landsliding hypothesis (Section 5.4, lines 391 ff.) and other possible non-tectonic mechanisms forming the ribbons, which we regard as an important indicator showing seismogenic formation of the fault scarps (Section 5.1., lines 286 ff.). We added the reference of Dramis and Blumetti (2005) to our existing argumentation in favour of seismogenic fault scarp exhumation (Section 4.1, lines 208 ff.) We again refer to our 'final response' where we already pointed out that (i) Figures 3 b,c and d do NOT show our sampling sites and (ii) Figure S1 shows that our sampling sites do certainly NOT coincide with stream incisions.. In the text, we clearly admit that there are large washed-out domains with stream incisions (and therefore differential non-tectonic erosional processes across the fault scarps, e.g., lines 215 f. or 243). We also clearly state that our sampling sites were selected after criteria strictly avoiding such domains (lines 109 ff.). To us, an incorporation of geomorphological maps for each sampling site seems somewhat excessive: As detailed descriptions and coordinates are provided, the reader can easily locate the sites on any desired additional map or e.g. Google Earth, if interested.

40) the assumption that supposed active fault scarp exposition has a post-LGM age, since supposedly during the LGM any slope would have been uniformly regularized by erosion/deposition, is anachronistic. Indeed, erosional/depositional dynamics along mountain slopes, even during a glacial period, is a function of the global but also of the local (regional) climatic and geomorphic setting: erosional/depositional dynamics along slopes are influenced by latitude, altitude, direction of slope facing, proximity with sea/ocean, proximity with glaciers, even during global climatic forcing. This implies that the climatic morphogenic effects vary from a region to another, from a slope to another, even close to each other. Thus, assuming that the fault exposure has a post-LGM age (post 18ka) is too simplistic and, let me say, no more acceptable, because conditions that can have influenced morphogenic processes at regional and local scale do not allow to consider the assumption as reliable and robust. The above indicates that the evaluation of the fault vertical throw rate by simply performing even detailed morphological profiles across the fault scarp is based on a critical chronological assumption. Moreover, the authors do not correlate across the faults the same correlative features (such as the same deposits or landforms displaced across the fault), but they only consider local topographic offset. This is a very risky way to proceed since, for instance, the footwall may be affected by erosion, whereas deposits may accumulate at the fault hanging wall, at the base of the scrap, thus resulting in different origins and ages of the current topographic profile across the fault. This influences slip and slip rate estimates. Moreover, the total throw estimated at line 167 (200 m) is proposed only for one of the faults examined (KFS) and not for the other strands (BFSn and BFSs), and also along just one site.

→ We do not fully agree with the general statement as this is not what many scientists all over the world – especially in the Mediterranean – have measured and observed. Nevertheless, we added very critical paragraphs, detailedly scrutinizing the 'LGM-normalised' method and its weaknesses: Chapter 3.1.1, lines 117 ff. ; Chapter 5.3, lines 347 ff. and 364 ff. In our final response (toward this comment) and in our responses to L. Benedetti's comment #5, we justify and detailedly explain why we still use and publish the results of this method but clearly fade it to the background in our revised manuscript version. The reason why offset estimation is restricted to one profile across KFS, is simply a lack of other suitable markers. Instead, we extended our argumentation by a knickpoint- and topography-based estimation (lines 211 ff.)

41) the supposed common and ubiquitous earthquake free-face exposures (drawing of most of the dashed lines in figure S6) appear very speculative in many of the showed cases. Most of them appear faint or not objectively distinguishable at all. Moreover, very critical appears lateral extent of the supposed earthquake ribbons, being up to few tens of cm long in many cases. Hence, tectonic origin is very hard to believe.

→ This comment perfectly corresponds to comment #23 by reviewer Benedetti. We apologize for the quality of the images. However, this is something we cannot really change, unfortunately. Many of the photos were taken in heavily forested terrain, which has an effect on both, the state of the ribbons as well as the lighting. Moreover, we are dealing with relatively narrow ribbons that are often not perfectly preserved or even defaced (probably different from what you know from Italy/Greece). We openly address and discuss this issue in the manuscript (e.g., lines 220 ff., 301 f.). To us, it is not the lateral extent and perfect discriminability per location that makes the ribbons reliable proof and markers of earthquake activity, but the constant widths of up to 5 ribbons that are traceable across 48 locations on the fault scarps. This coherence is illustrated in Figures S7 A-C, third column. We further expanded our elaborations on the ribbons and offer a hopefully more convincing, improved argumentation (e.g., lines 217 ff, 221 ff., 286 ff.)

42) The Wells and Coppersmith (1994) regression allow to estimate maximum expected magnitude from fault geometric and slip parameters, only if a given fault is supposed to be a primary earthquake fault. Secondary features are not accounted in the regressions as parameters can scale differently with magnitude. In this perspective, authors do not prove that the faults the investigate are primary faults or secondary structures associated to a primary seismogenic thrust fault (see my comment at point 1). Therefore, any inference about seismic potential associated to the investigated faults must be taken and dealt with great caution at least, because the genesis of the extensional structures is not fully demonstrated, given the compressive active tectonics of the region. If the investigated extensional structures are secondary features, they only activate when the primary thrust fault activates. They do not release earthquakes by themselves but they only accommodate passively part of the overall deformation.

→ We openly address that the Wells and Coppersmith regression has significant weaknesses when applied to our structures (short rupture length, low magnitudes; lines 304 f.). Still, we highlight that we regard it as an adequate tool to at least roughly estimate the magnitudes that our structures could be related with. In lines 306 ff. we added a short passage on possible magnitudes on secondary faults.

43) the sole presence of a cataclastic bend along a fault zone, not characterized in terms of microstructures, is not indicative if taken by itself of seismic slip. In this term, I would suggest to consider the work of Del Rio et al. (2021), in order to evaluate the possible origin as large-scale gravitational features of the investigates structures, as secondary structures associated to primary seismogenic thrust faults.

→ We extended our argumentations against non-seismic formation mechanisms (e.g., chapter 5.1, lines 286 ff.) and particularly against gravitational sliding Section 5.4 (lines 391 ff.) We again refer to our 'final response' (concerning this comment) and to our response to your comment #38 in which we justify our belief that the DSGS theory or the thoughts of Kastelic (2016) are not applicable to our sites. Microstructural analyses would possibly add to the quality of our paper. However, this cannot really be expected as a necessity in an already multifaceted tectonically/structurally-focussed work and first-time introduction of the normal fault scarps. We believe that our detailed descriptions (particularly in chapter 4) combined with a thorough discussion (chapter 5) sufficiently justify our interpretation of the normal fault scarps as active seismogenic features.