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Editors-in-chief
Solid Earth

Dear editorial board,

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

We cordially thank the editor and referees for their efforts during this second round of reviews that put the finishing touches to our manuscript. We would especially like to express our gratitude to Luigi Ferranti for a set of brilliant comments that gave us excellent thought-provoking impulses even "on the last lap" of manuscript preparation.

In that sense, we significantly edited the input parameters and argumentation concerning our (especially long-term) slip rate calculations, which is most notably implemented in manuscript sections 3.1.1. and 5.3. In Section 5.2, we added paragraphs on a possible relation between the fault scarps with compatible historic earthquakes, as well as an absolute maximum age of normal fault initiation. Furthermore, we did our best to improve our argumentation in terms of responsible tectonic drivers and the underlying geometries of geological structures in order to present a clearer overall standpoint (Sections 2, 4.1 and 5.4).

On the following pages, we list our adaptions in more detail by answering the two sets of reviewer comments point-by-point.

Best regards on behalf of all co-authors

Peter Biermanns

Detailed reply to the review of an anonymous referee (Report #1)

1) […] Yet, I am still quite puzzled by how the manuscript is structure to some degree, the model ages help define the slip-rates, so it would make sense that these model ages are discussed together with the rates, not the other way around. Furthermore, there are some inconsistencies & approximations in the data analysis that if corrected or improved upon might help to consolidate the conclusions.
→ The slip rates are intentionally discussed first and independently from the ages to stay focussed. The uncertainties of the slip rates are included in the age uncertainties. Slip rates, along with the morphology of the mountain chain (and mountain building processes), are important geomorphic factors and should not be neglected. Earthquake recurrence periods are highly variable (by the way. at many faults in the Mediterranean…including Italy). Independently, the earthquake ages and the resulting recurrence intervals allow information on the seismic history.

2) First, why start by stating that the paper presents "two-previously unreported normal faults", while these faults have already been discussed in your earlier paper (Biermanns et al., 2019) that describe Tectonic geomorphology and Quaternary landscape development in the Albania - Montenegro border region – there are several paragraphs discussing these faults & figure already shown, (e.g. Fig 4h) in the earlier paper. This is clumsy at best. This paper is a follow-up analysis of the kinematic and role of these faults in the region deformation and seismicity. The current manuscript should be consistent in that regard and integrate previous publications, including their own.
→ We thank you for this very valuable comment, as also other readers might have been bothered about the wording. The fault scarps were superficially introduced in our 2019 paper when work on this manuscript was already ongoing. Now, we removed "previously unreported" in the Abstract (line 10) and in chapter 1. We also replaced ambiguous wording such as "introduced" by "described" (e.g., line 33 f.).

3) Furthermore, the data description & quantitative analysis done on these faults is rather superficial and should probably be done prior to any comparison to other fault systems in Italy or Greece. There is a great diversity in the growth and seismicity of normal faults and an extensive literature for not assuming that they are all following the same activation time & seismic behaviour to cite the two characteristics considered in the paper. Furthermore, most of these normal faults are in extension setting and not associated with some extrado deformation of active fold and trust belts, a setting that likely influence their growth, segmentation & seismicity or lack there is. These faults are particular and although examples of on-shore surface ruptures related to such secondary normal faults are rare, especially at that scale, the noteworthy El Asnam earthquake in 1980 earthquake is one famous example for having highlighted that surface ruptures along normal faults can occur as a result of thrust events.
→ We agree with you in terms of many aspects:
Yes, our study is quite "superficial" but this is something that we frankly communicate by using expressions such as 'first-time description' (line 37), 'basic studies' (line 38), low spatial resolution (line 40 f.), 'hitherto inchoate understanding' (line 59) etc. In addition, we extensively discuss and scrutinise the reliability of our methods (Section 5 and sub-sections) - something we already highlighted during the first review round. We do not really see this as a

significant weakness of our paper as we believe that none of the already "well-known" fault scarp examples was perfectly described all at once in just one paper. You will probably agree that the available data on well-studies single fault scarps was accumulated throughout several papers, often from different authors. As we already said: This is a "kickoff" study on a set of fault scarps that has never been described in detail and whose context and genesis are not perfectly clear yet (at least from our understanding). We also agree with you that certainly not all (normal) fault scarps follow the same patterns when it comes to growth, timing etc. However, it is enigmatic to us why this should generally prohibit comparisons. Although we clearly favour a syn-contractional formation of the NFS in our manuscript, we present two possible formation mechanisms and compare our fault scarps with two known settings (Italy/Greece and El Asnam). In response to the comments by you (thank you!), L. Ferranti and L. Benedetti, we now explicitly allege the El Asnam example (line 416). However, El Asnam misses a lot of extension and extensional features. The fact that the Montenegrin fault scarps were probably not formed by the same mechanisms like the examples in Italy makes it an interesting observation that they share astonishing similarities, however avoiding the rare and very special "deep seated landslide" hypothesis. Crete and Greece is another story, and very comparable (think of the 365 CE Crete thrust earthquake. and along many, many normal faults in the thrust top). As we clearly differentiate "visual" and "genetic" comparisons, we do not see any risk of readers being misled. We would rather have the feeling of restraining useful information if the comparisons are not drawn.

That raise many questions: Are these features only transient, superficial and by-products of the folding, propagation and/or segmentation of the fold & trust belts or can these normal faults develop over time and create longer-term relief? If the thrusts are active, as suggested by the instrumental earthquake focal mechanisms of the region, one might need to integrate & discuss the implication of such setting, especially the geometry of the system that ought to the limit of the potential surface rupture of the normal faults given the superficial depth of ramps in fold & trust systems. In any case, without a detailed analysis of these structures, geometry, segmentation, growth and propagation, the discussion remains quite superficial on that front.

→ Thank you for this comment which partly corresponds to the very constructive first comment by L. Ferranti. In Section 5.4 we now added a few statements on possible thrust/ramp/flat geometries that may have initiated NFS formation (lines 414 ff.). Additionally, we elaborated on the possible depth extent of the normal faults (lines 364 ff., 429 ff.). By analysing what tectonic units they cut/overprint, we derived an absolute maximum age for the faults (Section 5.2, line 364 ff). For further detailed information on the geometries of underlying thrusts, we refer to Schmitz et al. (2020) from our own working group (e.g, lines 95, 100).

4) Regarding the estimation of the slip-rates (SR) on these faults, the methodology used here is still rather crude in my opinion.

→ Indeed, we decided to use a very basic modelling method. This was chosen since it is only a preliminary sampling for a rough estimation on the slip rates and earthquake ages. The used 5 samples are not sufficient for precise results in any modelling method. To avoid over-interpretations, we kept it simple. In this context, we kindly ask you to please keep in mind that our study had some "pilot character" which involved also extensive and detailed field mapping of the documented NFS for the first time. A denser sampling for TCN dating at a certain

point became simply beyond our capacities.
The fact that most direct ages gathered on active normal fault surfaces in the literature are Holocene in age and therefore younger than the Last Glacial Maximum or the Younger Dryas, does not imply that all these faults activated necessarily as a result of any climate forcing.
→ We do not assume a postglacial activation. Also, we do not say that the faults were activated in response to climate forcing. We only state that the morphological expression of the faults is strongly contingent upon erosion, which is in turn a result of climatic conditions. To make this clearer at an earlier stage in the paper, we moved a large text block from Section 5.3 to 3.1.1 (lines 114 ff.). In response to L. Ferranti's comment #3, we also re-did parts of our LGM-normalized calculations and accordingly re-arranged parts of the manuscript. We are confident that the now highlighted coherence between 36Cl-based slip rates and LGM-normalized slip rates (Section 5.3, lines 385 ff.) is convincing.
In fact, previous detailed analysis of the seismic cycles of numerous normal faults in Italy concluded that there was no climate control of fault exhumation given the asynchronous high slip-rate periods along these faults (Cowie et al., 2017). Other studies that concluded that their local fresh fault scarps were close in age to the last glacial maximum had some evidences, offset glacial deposits (i.e Papanikolaou et al., 2005), dated surfaces (i.e Tesson et al., 2020) or they fully argue their case (i.e. Tucker et al., 2011).
Long-term slip-rate estimates along active faults require both well-defined offsets and an argumentation on the age of these offsets, which is not always straight forward.
→ (i) We do not say that there is climate control on fault activity, but on surficial expression of faults (see also previous comment). This view is well-acknowledged and reproduced by numerous studies throughout the last decades (Papanikolaou et al, 2005 – just to name one. There are MANY). (ii) No, in the methodology we used, the ages are not required to derive the slip-rates (see Section 3.2.2). (iii) Concerning the offsets, we used all available and reliable markers. Due to the available local setting, there are not many possibilities. Anyways, we added a few passages with more detail on this topic and also removed the less reliable value of 400 m (Section 4.1, lines 256 ff.; see also our responses to your comments #12 and 13.)
Whatever your argumentation is, you have to present it clearly. After reading the manuscript, it was still unclear until section 4, what was the timescale cover by your data, that is the Cl36 model ages.
→ This is correct as it is. The ages are results and hence, they need to be introduced in the results section which is section 4.
The model ages should be presented together with the SR derived using the Cl-36 data.
→ We feel that this would confuse the reader. As of now the text is clearly structured, also thanks to the first review round. We renamed section 4.2.2 to "Earthquakes ages obtained from ribbons and ^{36}Cl dating".
And there are no 36Cl model ages without discussion on the erosion rate.
→ In the passages were we quantify erosion rates, we mention the abundant and well-preserved slickensides (up to ca. 3 m free face height). Where the slickensides are still well-visible, erosion rates logically have to be very low (e.g. Mechernich et al., 2018, 2022). We also argue with rock surface relief in the higher regions of the free-face, which we "mapped" and estimated on-site. As these are actual observations, there is no need for a discussion.
Assuming that all parameters are the same as that of others studies in the Mediterranean, by default, is rather superficial.

→ We only assumed the chemical composition of the soil of the Greek Pisia fault. These values do not have a significant effect on the modelling results anyway as extremely different other values yield only slip rates variations of a few percent and hence within the slip rates uncertainties. From our supplementary material it should become very clear that we did not extrapolate “all parameters”.

Did you extrapolate your Cl36 SR to LGM times?

→ Based on the five measured samples, we inferred a constant slip rate (although we are aware that this is not necessarily the case). The termination point of the modelling is limited to the scarp height (degraded scarp) and not to a specific age. The resulting “duration” of the slip rates can be seen in Fig. 7B. The obtained modelling results strongly suggest that the fault scarp formation onset occurred shortly after the LGM demise.

If so, what evidence did you gather at your sites that the fault scarps (free-face or degraded one) are as old as the LGM? That is the key question here, one that needs to be tackled in the manuscript. At this stage, I’m puzzled by the fact that the authors choose to put forward the results from a rather debated correlation that is taken as a given evidence thorough the manuscript rather than discussing first the results based on new data & evidences gathered at their sites. The problem is not that the conclusion is that these normal faults were activated post LGM, but it is an issue that there is very little data analysis that lead to that conclusion in the current manuscript.

→ As already pointed out, we do not propose anywhere in our manuscript that the faults were activated post LGM. Climate forcing does not affect fault activity but the surficial expression of (active or temporally inactive) faults. The faults were certainly active before, during and after glaciation, which is clearly shown by the morphology of the mountain front (i.e., for a text book example see Morewood and Roberts 2002 for the South Alkyonides Fault). The relief of the Rumija mountain (>1000m) clearly indicates uplift (like many other features in the study area, see e.g., Biermanns et al., 2019: anticlines, terraces,…) Exhuming something for 1000 m needs time and earthquakes and NOT glaciations/climate change (which may influence rates). We are therefore not talking about a “key question” but a rather trivial coherence. From our point of view, we described the fault plane/free face in detail and we present adequate and acknowledged dating methods, including a simplistic (but also acknowledged) approach in our manuscript.

5) Line 106 were selected to collect samples for 36Cl dating (site BFSN only; see following Section 3.1.2) and to estimate low-resolution long- term (post-Last Glacial Maximum, LGM) slip rates based on topographic profiles across the fault scarps for comparison

Since the Cl-36 data are not of LGM ages, the sentence is puzzling. The Cl-36 SR are based on low-resolution sampling data, and covering a shorter timescale than that of LGM. The link between your data and that timescale need to be done first. Clarify or just remove the () at this stage.

→ Thank you for this comment! In the concerned sentence we unhappily mixed two aspects (36 Cl dating and profile-based dating). This admittedly awkward setup was certainly misleading and ultimately made you misunderstand the sentence. We tried to highlight that we are actually talking about two different approaches by inserting a numeration (i/ii, line 112) and by moving a descriptive sentence from Section 5.3 upward (lines 114 ff.). We are sure that this makes things way clearer.

6) Line 115 two sets of generalized post-LGM (18 ± 3 kyr, e.g., Papanikolaou et al., 2005; for discussion see Section 5.3) slip rates for each site: (i) A conservative one, only considering slip on the visible free-face and (ii) a progressive one, incorporating the degraded NFS in prolongation of the free-face (Table S10)

   Maximum ages lead to minimum SR whether define using the free-face or older eroded fault surface. It would be more logical to first define the ages, then the offsets to estimate SR before projecting the SR to longer timescales such as LGM times. In the Cl-36 section, the free-face is 8.8m and model age is 6.4ka, the 22m degraded scarp have a modelled age of 14.8 ka assuming a constant SR & constant low erosion of 1mm/ka, not 18ka. That's an issue that should be resolve before defining SRs using the same 18ka LGM climatic correlation on other fault scarps.

   → We allow ourselves to fully refer to L. Ferranti's comment #3. By following his guidelines, we achieved a very consistent (and logical!) "interplay" between LGM-normalised and 36Cl-based slip rates. Eventually, we fully agree that the initial setup and storyline in our previous manuscript version was slightly chaotic and possibly misleading. Therefore, we fully understand your comment and hope that we managed to solve the issue to both reviewers' full satisfaction.

7) Line 120 Despite these weaknesses and the availability of an absolute 36Cl dating for site BFSN (see Section 3.1.2), we still consider the comparison of topography-based slip rate estimations a reasoned benchmark in the frame of our study.

   The Cl36 ages are not absolute ages; they are modelled based on assumptions on the erosion rate, scaling schemes, 36Cl production rates… but at least, they are model ages derived from the studied sites that can be fully discussed.

   → Thanks for this comment. We removed the information that they are absolute ages (line 128). However, we think that your statement is at least arguable. From our point of view, the term "absolute ages" describes actually "dated" ages, no matter if there is a possible error to them. We interpret absolute ages as the counterpiece to "relative ages" in which we only describe a relation "older/younger" (e.g., ribbons, or the geomorphological expression of a fault scarp in our case).

8) Line 161 36Cl scarp modelling method and parameters

   The model age analysis is missing in this section and should be presented at the very least together with the SR defined using the code of Schlagenhauf et al. (2010). There are several codes available to calculate the Cl-36 model ages such as https://stoneage.ice-d.org/math/Cl36/v3/v3_Cl36_age_in.html, pending the shielding correction is gather form the codes provided by Schlagenhauf et al. (2010).

   → Thank you for highlighting, that this was missing in section 3.2.2. It is now added (lines 214 ff.). We did not use an ^{36}Cl age calculator, as it does not work on moving surfaces. Hence, we used the slip rates which were derived from the Schlagenhauf code together with the amount of coseismic offset from the ribbon locations. Based on the equation slip rate = offset/age, we calculated the earthquake event ages.

9) Line 178 and integrates over a time-span of 17.3 kyr, which is appropriate for our postglacial focus

Why 17.3ka? is your Cl-36 model ages reach this timescale? Best is to estimate the model ages first and correct for the production rate for the proper time-span of each sample, or at least the older Cl-36 model age at ~6ka.
→ 17.3 kyr is the time the Ca-spallation rate of Stone (1996) is integrating over. As our study focusses on the postglacial time, we argue that this is an appropriate reference. We are not familiar with any Ca-spallation rate integrating over ~6 kyr.

10) Line 180 Scaling with respect to latitude and elevation was performed using the Stone (2000) scaling scheme assuming a constant geomagnetic field intensity.
You mention the opposite earlier in line 174 "Furthermore, production rates have to be scaled appropriately to the local and distant shielding of the site from cosmic rays and to changes of production through time due to geomagnetic field effects." Explain or clarify.
→ Thank you for highlighting this. The changes of the geomagnetic field are not considered in the Stone (2000) scaling scheme. Hence, we removed the second part of the sentence in line 174 (now in lines 183 ff.).

11) Line 188 Assuming this as the minimum amount of erosion and using a preliminary calculated 36Cl age of ~15 kyr at 8.8 m height, we estimate an erosion rate of ~1 mm/kyr at our sample locations
Why 15ka? Preliminary, do you mean zero-erosion model age? is your Cl-36 zero-erosion model ages reach this timescale? If you have considered 2 to 8 mm in 15ka, that would give 0.13 to .53 mm/ka of erosion rate pending the sampled free-face reach the 15ka timescale. If you older zero-erosion model is in fact ~ 6ka, then the erosion rate would be higher and ranging from 0.33 to 1.3 mm/ka. Why not discuss & use your estimates instead of ~ 1mm/yr? Please update.
→ Thank you for pointing this out. The text was adapted accordingly (lines 197 ff.)

12) Line 211 The total throw of the NFS is estimated to max. ~200 - 400 m. The 200-m frame is based on the offset of stratigraphic markers across KFS (see cross sections in Fig. 4 C).
This is unclear how the throw rate is estimated. Is it really from the low-resolution topographic profile of a geological cross-section? This is not the classic way of doing it, please explain/clarify.
→ The 0-200 m throw estimate of the NFS was conceived by the consideration of structural data, outcropping width, and geometry (hence, a classical geological cross-section). The topographic profile is not connected to the throw estimate. We added some explanation to the text (Section 4.1, lines 256 ff.).

13) Line 212 The 400-m frame is based on an analysis of topographic cross sections across BFS, where a clearly perceptible knickpoint (~850 m a.s.l.) marks the NFS ~400 m below the overlying highest parts of the Rumija ridge (~ 1250 m a.s.l.)
Any figure to refer to?
→ While elaborating on comment 12, we decided to clearly shift the purely topographic estimate to the background (Section 4.1, lines 265 ff.) as it is less reliable compared to the geological-geometric estimate, and to avoid unnecessary confusion. The reference to a figure is thus no longer necessary.

14) Line 249 sets of conservative and progressive minimum slip rates
Both are based on a climatic correlation implying a maximum age for the free-face surfaces leading to a minimum SR… "conservative" & "progressive" adjectives in that context are rather subjective.
→ Also here, we would like to refer to L. Ferranti's comment # 3 for details. We removed the differentiation between "conservative" and "progressive" from our manuscript.

15) Line 261 The modelling of the 36Cl concentrations on the BFSN free-face highlights that the measured 36Cl pattern can be generated by a constant slip rate of 1.5 ± 0.1 mm/yr (Fig. 7 A).
The SR rate based on your data should probably come before the climate correlation together with an argument for projecting your CL-36 rate to the longer LGM timescale…
→ (i) We re-interpreted the correlation between 36Cl-based data and LGM-normalized studies and achieved a significantly more logical "interplay" (Section 5.3, lines 386 ff.). When the 1.5 mm/yr slip rate is extrapolated over the degraded part of the scarp, we now reach the full ~22m height within a post-LGM period of 15 kyr. For more details, we kindly refer to our reply to comment #3 by L. Ferranti. (ii) The assumption of a constant 1.5 mm/yr slip rate was also chosen in default of other evidence ("Ockham's razor").

16) Line 267 The retrieved slip rate suggests that the 8.8 m-high free-face was most likely exhumed within the last 5.9 ± 0.4 kyr (Fig. 7 B) and the according fault scarp age is presumably 14.8 ± 1.0 kyr (Fig. 7 B).
Thanks for sharing the finally timescale of your data... So, the fault scarp is ~15ka based on the Cl-36 SR define over the last ~6ka, assuming a constant rate over time. First, you use a preliminary (Line 188), i.e zero-erosion model age for the age of fault scarp, adding some erosion to the model ought to get an older age, yet the preliminary and corrected ages appear to be the same. How do you justify using 18ka for the minimum SR, since you have an estimate of ~15ka? Why not estimate a long-term erosion rate using the 6ka-SR together with the geometry of the fault plane, pending the geometry is well established (See Tucker et al., 2011)? Then speculate on the longer timescale SR using some revised erosion rates? Please consolidate and clarify.
→ We allow ourselves to fully refer to L. Ferranti's comment #3 (- we adjusted the age to 15 kyr). We think that we served your request by implementing the according changes.

17) Line 268 Since the slip rate of 1.5 ± 0.1 mm/yr is very high
Is it? Compare to what? maybe the erosion rate use for defining the 6ka-SR is underestimate. Carbonates in the Mediterranean can have erosion rate up to ~25mm/yr (i.e. Ryb et al., 2014), so there is room to discuss this parameter and revise a SR to more reasonable values.
→ Since the area is dominated by contractional tectonics we did not expect such a high slip rate. Hence, we replaced "very high" with "higher than expected". According to our response toward your comment #4, we justify our impression of low erosion rates with abundant and well-preserved slickensides and a thourough mapping of the fault scarp (free faces). These slickensides would not be visible if there was an erosion of 25 mm/yr. In lines 295 ff., we included a sentence explaining this.

18) Line 274 To be as open-minded as possible, we used any hypothetical scenario, without correlation to the local mapping.
???
→ We deleted the second part of the sentence as it is was confusing (line 297).

19) Line 280 Hence, we highlight that the slip rate of the Bar fault during the last ~6 kyr was surely higher than ~1.15 mm/yr and presumably around 1.5 ± 0.1 mm/yr.
Sure, neglecting the inheritance would increase the timescale and thus reduce the rate, but the inheritance is derived from the data gathered from the colluvium wedge, so that's not really a reasonable option. However, the erosion is more open to debate as pointed out earlier.
→ A higher erosion rate is highly unlikely due to the preserved slickensides as explained in comment #17**.**

20) Line 281 Also, alternative interpretations of the few 36Cl data points is conceivable, but they are significantly more complicated, not underlined by field findings and hence considered less likely.
If you don't present these alternatives, there is no point in that statement.
→ We deleted this statement. To explain why we are not mentioning any alternatives: It could basically be any reason, no matter how unlikely/"crazy". A debris flow could have covered the fault scarp temporally but was eventually washed away by torrential rains; a windstorm could have covered the fault scarp with disrooted trees for several hundreds/thousands of years etc. etc. etc. All of these would hamper production rate and make the scarp appear younger. As possibilities are infinite, but there is no evidence of such scenarios, we leave out the statement, as you suggested. Thanks!

21) Line 338 To enable a comparison of the different (structurally and exposure-related distinct) sections of the fault scarps nonetheless – and to provide at least one benchmark for the obtained 36Cl dating results – we invoke the rather simplistic technique of fault scarp profiling (see also Sections 3.1.1 and 4.2.1) for slip rate derivation. For this technique, it is assumed that the preservation of NFS initiated around the LGM.
The topographic profile perpendicular to the fault scarp are done to gather the geometry (dip angles) of the fault & to define the offsets. To use of the LGM age as the age of all these offsets, there is a need to first justify the use of LGM age on the free-face sampled for Cl-36 analysis.
→ As you may have noticed, we justified the use of a post-LGM time frame during the last round of reviews already (e.g., comment # 40). We would like to again stress that the hindered (erosion-induced) formation of pronounced fault scarps before the end of the LGM is a well-acknowledged and well-funded hypothesis based on the findings of many (cited) studies, particularly in the Mediterranean. In sections 3.1.1 and (particularly) 5.3, we openly discuss the obvious weaknesses and error-proneness of the method, including the possibility that the expected effect is not applicable to our location at all (!!): " *[…] three main error sources: (i) The local impact of LGM climate on erosion at all, (ii) the exact timing of initiating fault scarp preservation – in case it was effectively impeded during glaciation […]* " (lines 372 ff.) Although we thoroughly scrutinise the method and the role of the LGM, we are convinced that a process valid in all of the surrounding regions (Italy, Greece) is likely valid for our

study sites as well. We therefore introduce and discuss this approach (see also: comments/responses # 22/40 of the last review round).

22) Line 411 capable of triggering earthquakes up to Mw≈7 ± 0.5 , yet within the text the "Derived magnitudes range from Mw≈5.3 to 6.5."
Given the logarithmic scale used for earthquake magnitudes, it is best to stay within the range of Mw calculated or use the mean Mw of 6.3 ± 0.1 as defined in table S9. A Mw 7 event release ~15 times more energy than a Mw 6.5…
→ It is proven that the mentioned "higher-order thrust faults" create magnitudes in that range. The Montenegro 1979 (M = 7.1) earthquake is a prominent example. See our earlier Schmitz et al. (2020) contribution.

Detailed reply to the review of L. Ferranti (Report # 2)

1) Normal faults in the contractional setting
I believe, given the geological context and the seismicity and geodetic data that the simplest and more logical explanation for the normal faults studied here is that they formed coeval with, and in the hanging-wall of a thrust stack. This interpretation is presented in the paper along with the alternative scenario that hinterland extension is opening its way into the Dinarids. I think the latter scenario fits less with observations for two reasons. First, the existence of an active thrust documented by seismicity and geodesy beneath the antiformal stack means per se that hinterland extension has not encroached this area yet. Otherwise, the basal orogenic thrust should have been broken across by normal faults and be no more active, unless we are looking at the unlikely situation that contractional and extensional systems share the same regional detachment. Note that the authors state in the abstract "the NFS are incipient extensional structures that postdate growth of the fault-related anticlines on top of which they occur". This statement is misleading because the NFS cut the uppermost thrust stack (the Budva-Cukali thrust sheet and of course the overlying High Karst thrust sheet) but not the basal orogenic thrust, which is still active as documented by seismicity and geodesy. Therefore, the NFS postdate only the higher thrust unit emplacement but is coeval to the younger thrust stack at depth.
Second, if formation of the BSF and KSF was triggered by hinterland extension, one would expect NE-directed collapse (i.e. toward the hinterland), but instead the two NFS displace toward the Ionian Sea. Perhaps the extensional faults formed because the fold has grown past the ramp of the orogenic detachment and its higher part is collapsing above the upper flat (see also the comment by L. Benedetti and the comparison with the El Asnam 1980 earthquake). Something akin to this latter explanation is almost sketched in Fig. 10, although I am not convinced by portrayed depths, where major thrust earthquakes are around 10 km depth, so: the normal faults root at around the same depth into the regional detachment, or they stay confined at some shallower detachment level (but beneath the Budva-Cukali thrust)? What are the age constraints on the Budva-Cukali thrust (your maximum constraints for NFS onset)? These issues should be expanded and more carefully discussed. In addition, I would like to see in the discussion a first-choice interpretation (fold-collapse? It is your choice) followed by an alternative view (hinterland extension propagation).
Regarding the model, I would like to see more info about NW-ward migration of extension behind the thrust belt. Are there independent data to support this NW-ward migration? What we know about? The Hellenic trench is migrating south faster than the Dinarid trench, and differential motion is accommodated by the Scutari-Pec line. In this long-established frame there is no need for extension migration to the NW, rather hinterland extension should stop at the Scutari-Pec line. Is my view correct?
→ Thank you for this valuable set of comments which we highly appreciate.
(1) Actually, we never considered that the normal faults cut the active basal thrust. Although we (accordingly) never claimed this in our manuscript, we agree that several passages may have been slightly misleading in that respect. Our initial wording in the abstract "[…] fault-related anticlines on top of which they occur" was intended to refer to the High Karst and Budva-Cukali zones (which is also described in chapter 2). We now clearly point out that we consider only the uppermost thrusts in the stacks to be affected (lines 19 f.) by extension up to an unknown depth.

(2) We fully agree with your arguments why a syn-contractional genesis of the NFS is the significantly likelier option. Therefore, we now recite the majority of your arguments in Section 5.4 (lines 425 ff.). We also followed your suggestion to re-arrange the discussion chapter by first presenting a first-choice interpretation followed by the alternative view (lines 411 ff.). Both are followed by a final list of arguments in favour of the "syn-contractional hypothesis". Additionally, we clearly highlight our support for this hypothesis in the Abstract already (lines 22, 28).
(3) Besides our own work (this manuscript and Biermanns et al., 2019) there are no datasets proving the migration of extensional tectonics directly into the Dinarides. The already cited papers (e.g., Dumurdzanov et al., 2005; Handy et al., 2019) as well as our implications from Figure 1 and 9 suggest that an extensional domain has constantly been approaching our study area and is at least very close now. Your evaluation that the Shkodra-Peja fault (SPF) is "absorbing" the southern extensional tectonics is per se correct. However, we already indicated in our 2019 paper that the region at the SW end of SPF seems to be more complicated than expected, featuring a wide variety of tectonic styles that are not straightforward and (to be honest) still keep us musing. We added our observation from Biermanns et al. (2019) that weak indicators of extensional tectonics can be found close-by (lines 434 ff.). Nevertheless, we now clearly highlight that syn-convergent development of the NFS is almost irrefutable. According to the basic statement in your comment and due to a lack of convincing "pro-extension" arguments, we decided against significantly extending our discussion on that inapplicable hypothesis.
(4) Based on the observation that the normal fault scarps clearly overprint ("cut") the Budva-Cukali zone and seem to extend into the Kruja-Dalmatian Unit, we now present an absolute maximum fault age in Section 5.2 (lines 364 ff.). We thank you for the suggestion to present such maximum frame, although we expect significantly younger ages of fault formation as derived from the slip rates and offset markers.

2) Earthquake age
Your modelling results in earthquake ages of 100 ± 14 yr (EQ1), 173 ± 24 yr (EQ2) and 210 ± 29 yr (EQ3) (Fig. 8). Considering the significant modelled magnitude (Mw 5.3-6.5), and the young age of events, you should have an historical record of these earthquakes even in a remote region like this. Did you check it?
→ Thank you for this brilliant and truly justified comment! Very obviously, this has to be mentioned and discussed. A quite detailed and useful earthquake record is available starting from ca. 1900. Therefore, we found well-fitting possible agents for the two younger proposed earthquakes, while a match for the 210 ± 29 yr horizon was admittedly less successful. We now discuss the possible matches with reported earthquakes near Shkodra and Durrës (1905, 1926, 1855) in chapter 5.2 (lines 353 ff.). The 1851 earthquakes near Vlora/Berat in fact fit temporally but a connection is considered unlikely in the given tectonic context.

3) Slip rates estimation
I am puzzled by the extrapolation of slip rates in the long-term. Based on the 36CL data, the Authors provide a ~6 ka age for the slickenside surface with ribbons (the "free-face"), which allows them to derive a 1.5 mm/yr slip rate in the last 6 ka (at one site, but this is fair). When

considering the degraded scarp above the free-face and assuming a ~18 ka age for commencement of preservation of the scarp (what they call “regular” approach), the slip rate derived from slope profiling at the four studied sites falls to ~1-1.2 mm/yr for BSF-N and KFS and to ~0.4-0.6 mm/yr for BSF-S.
The Authors also compute what they call “conservative” slip rates by assuming that slip was arrested between 18-6 ka and only resumed at 6 ka, forming the free-face, which provide very low slip rate values for the last 18 ka. In this interpretation, the degraded scarp would be the result of pre-LGM faulting. Then how could it be preserved from erosion? An explanation would be that the degraded scarp is not a product of faulting (non-tectonic origin) after the LGM, by then what process shaped it?
To me, a simplest alternative would be that until the LGM any record of previous slip was cancelled by erosion and there was not a scarp emerging from the levelled slope. The post-LGM and >6 ka motion should have produced the now degraded scarp, and the post-6 ka motion formed the free-face. The fact that there is a sharp morphologic contrast between degraded scarp and free-face, which probably prompted the authors to claim for quiescence during 18-6 ka and resumption of clustered faulting afterwards, might be explained for instance with better climatic conditions after ~6 ka (see Lambeck et al., 2011; Boulton and Stewart, 2015), so clustering would not be a need.
Notwithstanding, using the “regular” approach there is still a mismatch in slip rate pre- and post-6 ka, which itself could call for clustered slip after 6 ka. This latter claim is likely affected by uncertainties regarding the commencement of development of the degraded scarp, which in case the scarp is tectonic could affect the constancy in rate estimation. I have done my calculation with 18, 15, 12 ka (i. e. 15 ± 3 ka) possible ages of fault scarp preservation, and results document that for BSF-N and KFS the LGM-6 ky rates are equal (using 15 ka as onset age) or even larger (using the 12 ka age) than the slip rates for the last 6 ka. For BSF-S the intermediate-term rates are lower than the short-term; however I note that BFS-S is the less representative strand of the NFS to compute slip rates insofar it is trending oblique to the broad NE-SW extension expected to be parallel to contraction, as supported by the different strike and related lesser pitch value compared to other segments. This exercise demonstrates that 18 ka may be not a valid choice for onset of the fault scarp preservation, and younger ages would ameliorate the mismatch. Notably, the 15 ± 3 ka value for which slip rate during the 15-6 ka and 6-0 ka intervals are equal, perfectly matches the ~15 ka age for the scarp at BSF-N estimated from extrapolation of the 1.5 mm/yr rate derived from 36 CL analysis.
→ We cordially thank you for this extremely well-funded and versatile comment and for your effort to “play around” with our data. Indeed, this was kind of an eye-opener for us. The shift of our proposed LGM date from 18 to 15 kyr was already suggested by previous reviewers. As your argumentation now really convinced us, we finally did so and re-calculated the “LGM-normalized” slip rates. Accordingly, we adjusted all relevant passages and data in our manuscript (e.g., in Table S10, the Abstract, Sections 4.2.1, 5.3 and 6). Also, we really liked your interpretation concerning the morphology of the free face and degraded scarp in a post-LGM frame. We therefore added some according passages in the discussion chapter 5.3, lines 382 ff. (largely reproducing your view). As the new explanation approach clearly suggests that both, degraded scarp and free face were formed after demise of the LGM, we removed our differentiation between “conservative” and “progressive” slip rates in the frame of our profile-based dating.

Annotations within the PDF file (L. Ferranti)

4) you should not declare the age of faulting before you illustrate the data that allow you to derive the age

→ We are slightly surprised about this comment, as it refers to a passage in the Abstract. From our point of view, an Abstract does commonly not include a detailed/extensive presentation/illustration of data but rather summarizes the key aspects and most important results of a paper. Anyways, we removed the marked text passage as you additionally saw a conflict with our statement in line 16 (see #5).

5) above you declare Holocene faulting and niw you state a different age

→ You are right that referring to both "the Holocene" and "the post-LGM period" is confusing. We therefore removed the time designation previously in line 13 ("the Holocene").

6) why do not simply say "suggests.....during the last 6 kyr" ? This is data and then initiation of resumption of faulting is a matter of interpretation

→ As you can see from our last manuscript submission, we particularly inserted this wording in response to a comment of G. Roberts (his previous comment # 10). However, we now adapted your suggestion.

7) How do you use this info for your purposes? *(referring to the calculated total throw)*

→ This is rather a result of our study, not input data.

8) Actually you prove that the NFS cut the uppermost thrust stack (the Budva-Cukali thrust and of course the overlying High Karst thrust sheet) but not the basal orogenic thrust, whihc is still active as shown by seismicity and geodesy. So the NFS postdate only the higher thrust unit emplacement but is coeval to the younger thrust anticline

→ This comment has a lot of overlap with your comment #1. Therefore, we kindly refer to our response in #1.

9) geodetic data are obviously "recent" for a paper dealing wth Holocene faulting

→ This is true. We therefore removed the word "recent".

10) givene the sparse nature of seismic and geodetic data I would not be so imoperative- "broadly" would sound better

→ Again, you are right. We adapted your improvement suggestion and replaced "exactly" by "broadly" (line 23).

11) are there independent data to support this NW-ward migration? What we know about? The Hellenic trench is migrating south faster than the Dinarid trench, and differential motion is accommodated by the Scutari-Pec line.

→ Also this comment has a lot of overlap with your comment #1. Therefore, we kindly refer to our response in #1.

12) Actually continental subduction and related frontal thrust belt motion has recenty (ca. 600 ka) stopped in the southern part of the Apennines (Patacca and Scandone, 2007) and current convergence deformation is accommodated by strike-slip faults that involve both lower and upper plate (Di Bucci et al., 2006, Seismotectonics of the southern Apennines and Adriatic foreland: Insights on active regional E-W shear zones from analogue modeling. Tectonics 25, TC4015)
→ Thanks a lot for this comment and the according references. We added a sentence describing this circumstance as well as the suggested reading (lines 76 ff.). Please understand that we are not going into much detail, as our focus is on the Dinaric part.

13) I understand you adopt the "conservative" approach because the degraded aprt of scarp may be pre-LGM, but I think this is less constrained that admitting the degraded scarp is post-LGM and pre-6 ka; see my general comments
→ This comment has a lot of overlap with your comment #3. Therefore, we kindly refer to our response in #3.

14) (with slip clusters) [rearrange sentence]
→ Thanks for your improvement suggestion which we gratefully adapted (line 127). This makes the sentence way smoother.

15) Later you state that the 15 kyr age concerns the full slope (free-face plus degraded scarp) with hight of 22.2 m
→ We edited this sentence and highlight that this is only a first assumption to get an idea of the amount of erosion on the free face (lines 197 ff.; adapted together with point #11 of reviewer 1).

16) so far you used the label BSF, and niw on you turn to the full name. Please homogeneize
→ We use BFS when talking about the Bar Fault SCARP. Here we intended to talk about the fault (not considering the geomorphic landform 'scarp'). However, you may be right that this is confusing. We therefore inserted BFS at the marked spots. Thank you!

17) These are called "regular" in Tab. S10...why this difference? / separates two paragraphs here
→ We kindly refer to your comment #3. We removed the differentiation between "regular" and progressive slip rates.

18) you do not need to say this here
→ We added this statement in order to satisfy L. Benedetti's review by highlighting the superiority of the 36Cl method as opposed to the profile-based method. However, we now follow your suggestion and removed the brackets.

19) you should have an historical record of these earthquakes even in a remote region like this.
→ This comment has a lot of overlap with your comment #2. Therefore, we kindly refer to our response in #2.

20) so why do you compute conservative rates meaning that the degraded scarp formed before 18 kyr?
→ We removed our differentiation between "conservative" and "progressive" rates as your comment #3 fully convinced us. The concerned statement (Section 5.2, lines 359 ff.) now perfectly links to the proposed age of the full fault scarp.

21) you should add a caution note saying that this estimation is too young insofar it does not account for the quiescence intervals in the long-term.
→ Thank you for this comment. We added the according statement at the end of Section 5.2 and added a maximum age for normal fault initiation according to your comment #1(lines 363 ff.).

22) Exchange of expressions "following demise of" instead of "around"
→ We adapted your suggestion (line 115; the whole text block was moved up).

23) how do you know this?
→ The concerned text passage has been removed. We believe that this comment is now obsolete and sufficiently answered through our response toward your comment #3.

24) This is true, however BFS-S is the less representative strand of the NFS to compute slip rates insofar it is trending oblique to the broad NE-SW extension expected to be parallel to contraction, and as supported by the lesser pitch value compared to other segments.
→ Thanks for this very interesting comment. You are completely right that the BFS_S orientation does indeed differ from the overall Dinaric/Hellenic strike. However, it is approximately parallel to almost all other geologic/geomorphic features in the area, e.g. the southerly adjacent anticlines. This is also the case for KFS, making up the major part of the full mapped fault scarp length together with BFS_S. That said, BFS_N is the actual outlier, although it best corresponds with the expected Dinaric strike. We inserted these thoughts and discussion into our manuscript (lines 394 ff.).

25) I do not agree with this interpretation or at least there may be an alternative one, see my general comment
→ We adjusted our statements and believe that this comment is now obsolete and sufficiently answered through our response toward your comment #3.

26) you stated in text that you only sampled location BFS-N
→ Correct! Thank you. We used this as a location for fault scarp profiling but did not date samples from this location. "Sampling location" was therefore the wrong term. We fixed this!