Dear editors, dear reviewers,

we would like to thank the Editorial Board and editor F. Rossetti for guiding us through the complex and sometimes astonishing process of Solid Earth. We very much appreciate your comments and try to answer the raised issues point by point.

1) 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).

☐ Admittedly, the underlying thrust faults (blind and offshore, but well documented by seismicity) on top of which the normal faults are supposed to form and grow) are not extensively discussed in our manuscript. However, this is effectively not the focus of the present paper. From our point of view, we provide all relevant information on the regional geology, including the Dinaric thrusts. For additional detail, we refer to literature detailedly elaborating on local thrusting activity. This also includes a paper from our own group, dealing with the immediate vicinity of the fault scarps (Schmitz et al., 2020). By answering particular comments of reviewer Bendetti (e.g., her comments 1,4,8,11,17) and implementing many of her improvement suggestions in our revised manuscript, we were already able to more clearly exclude non-seismic formation mechanisms. After the third review, we will be happy to add even more arguments, particularly against a
landsliding-related genesis (e.g., no internal deformation of the slide mass, no toe deformation, microseismicity, drainage networks, lithology – as we are dealing with limestone, not e.g., flysch). This combined, and with all due respect, we do not see how the frameworks presented in Galadini et al., Zanferrari et al., del Rio et al. or Lettis et al. should be comparable to our setting in Montenegro altogether. We argue that up to the present day, there is no scientifically solid argumentation available on how (deep-seated) landslides could be able to create such large-scale surface ruptures/fault planes (see above comments as well). The referenced papers cited themselves admit that no distinction of deep-seated landslides from seismically controlled surface ruptures is possible (del Rio et al.). In our paper, we present a stressful line of argument on why we assume a co-seismic origin of the presented structures (i.e., ribbons, 10 m high (!) free faces with striations, geomorphic landforms). The use and interpretation of these features is validated by numerous well-acknowledged studies that are (in contrast to Galadini et al., Zanferrari et al., Kastelic et al. and Lettis et al.) very well comparable to our study sites in terms of the overall setting (including bedrock, fault scarp and slope morphology etc.). The according references are clearly cited in our manuscript. Crete is one of the best examples studied by our own group (Mason et al. 2016, 2017). We are aware of the work of Kastelic et al. (2016) in the Appenines, where we had the opportunity to visit all earthquake sites of 2009, 2016 and the surface ruptures. Those were generated by the earthquakes and are seismogenic. And of course, there is some compaction/erosion going on on the hanging walls of normal faults. However, the authors fail to present convincing evidence that the bedrock-colluvium interface has not been modified by other processes: animals, humans compacting by walking at the borderline? Wash-off by torrential rains at the hardness contrast of both materials? Plant growth and bioturbation? Remember: these faults move ca. all 500-1000 years. A modification is very likely. However, many observations show that the scarps are generated by earthquakes. A note on “deep-seated” landslides, as this is also discussed: Why do those move only in earthquakes? And not seasonally? Of course the “free faces” are modified in their many 500-1000 y long life, but according to the throw during an earthquake we do not believe that this is the case. The slip history of normal faults during earthquakes has been discussed by many (Benedetti, Roberts, our working group) by cosmogenic nuclide dating of the scarps. So, yes, we acknowledge the precautionous note by the reviewer and the Kastelic et al. (2016) publication; but in our case, the experience teaches something different (Reicherter et al. 2003, 2010, Reicherter and Peters 2005; Mason et al. 2016, Mason and Reicherter 2016; Schneiderwind et al. 2016, Mechernich et al. 2018; Wiatr et al. 2013, 2014.) and of course all related publication especially of Rev 1 and 2; and many others.

2) 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 seem 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 regard the comment as not appropriate here, as the shown striations cannot form in
bed rock during landslides. Also, the reviewer fails to give examples of rejuvenated landslides and landforms associated. We refer strongly to the paper by Dramis and Blumetti (2005) who elaborated the “seismic landscape” concept: Dramis, F., Blumetti, A.M., 2005. Some considerations concerning seismic geomorphology and paleoseismology. Tectonophysics 408, 177-191. https://doi.org/10.1016/j.tecto.2005.05.032. And keeping erosion rates of carbonates in mind: of course triangular facets, wine-glass shaped valley are seismogenic landscape features. We have the feeling to be trapped in a non-conventional argumentation that lack geological understanding. However, from a distance we observed the discussion of tectonic vs landslide hypothesis on normal fault scarps. Judging from the comment, we would urgently recommend another detailed look at the manuscript. First of all, Figures 3 b,c and d do NOT show our sampling sites (there is also no such indication in the text or figure captions!) and Figure S1 shows that our sampling sites do certainly NOT coincide with stream incisions. This is clearly visible although the resolution at the presented scale is not even great. 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 162 f). We also clearly state that our sampling sites were selected after criteria strictly avoiding such domains (lines 85 f). 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. In response to comments 6, 17 and 18 by reviewer Benedetti, we indeed do elaborate more detailedly on possible non-tectonic/seismic formation mechanisms of the fault scarps in our revised manuscript which will hopefully reduce your scepticism. We agree that this aspect possibly came up short in the first manuscript version.

3) 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 agree with the first general statement as it is not what many scientists all over the world – especially in the Mediterranean – have measured and observed. Of course there are local variations in terms of elevation, exposition etc. This is why we applied an error. Thus, we agree that slight local and temporal variations in terms of climate and erosion may likely exist, also during glacial periods. We therefore also fully agree that the applied method and assumption is indeed simplistic and partly generalized. However, we are fully convinced that the OVERALL integrated processes and erosional behaviour during glacial/non-glacial periods DO follow the pattern described in our
manuscript. We believe that this attitude towards your concerns is justified, as our presented hypotheses, framework and methods are well-founded and have previously been proven and applied by many renowned (cited) authors. In that sense, your comment and concerns would possibly be more convincing if resilient scientific proof was available. In section 5.1, we discuss the possible weaknesses of our method. Here, we are ready to add a passage on possible small-scale local variations. According to comment 5 by reviewer Benedetti, we introduce the slip rate calculations based on scarp profiling as an auxiliary tool in our revised manuscript version. The fact that the method is thereby faded into the background to serve as a benchmark for 36 Cl dating certainly justifies its preservation in the manuscript. The reason why offset estimation is restricted to one profile across KFS, is simply a lack of other suitable markers.

4) 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 partly corresponds to comment #6 by reviewer Benedetti. In this case, 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 240f). 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 Figure S7 A-C, third column.

5) 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.

☐ First of all, we do not fully commit ourselves to one or the other formation mechanism (primary or secondary fault/movement). The view that the faults MAY be primary features therefore fundamentally justifies an application of the Wells & Coppersmith regression, even if we follow your argumentation. Let us explain it like this: the Wells & Coppersmith publication includes a data set of c. 80 earthquake ruptures, many of those are NOT normal, the publication is 25 y old, and hence should be regarded respectfully and not as "the bible". More importantly, we detailedly discuss the weaknesses of the technique and come to the conclusion that it is probably not too reliable for our setting anyway (short rupture length, low magnitudes; lines 242 f). Still, we regard it as a tool to at least roughly estimate the magnitudes that our structures could be related with. We are ready to further highlight this in a revised manuscript version. The Montenegro 1979 eq was a thrust event of Mw c. 7; our structures are well below this and prone to host a Mw c. 6. Once again, we like to refer to Crete, where Mw 8 (or higher) uplifted the western part,
whereas onshore normal faults are much shorter but also seismogenic, more or less in the range of M 6 ± 0.5. Identically we may refer to Japan, 2011 events, and Chile 2010 and many others.

6) 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 investigated structures, as secondary structures associated to primary seismogenic thrust faults.

□ Apposite to our statements at the outset, we are not willing to promote poorly underpinned theories as a base for our work and argumentation. We absolutely respect the suggested paper by Del Rio et al. (2021) but are by no means convinced of the DGSD theory and related arguments, particularly if applied to our sites. Instead, we prefer to rely on a multitude of well-established and acknowledged comparable studies (all cited in our manuscript). 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-focused work. 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. A first-time description of the normal fault scarps is combined with an introduction of different possible formation mechanisms. This makes the style of our manuscript rather open and defensive - and does not claim “the one and only” irrefutable explanation for itself.