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Comment on se-2021-97

Anonymous Referee #3

Referee comment on "Aegean-style extensional deformation in the contractional southern Dinarides: Incipient normal fault scarps in Montenegro" by Peter Biermanns et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-97-RC4, 2022

Dear Editor,

I carefully read the paper by Biermanns and co-authors "Onset of Aegean-style extensional deformation in the contractional southern Dinarides documented by incipient normal fault scarps in Montenegro".

It deals with the presence of some supposed normal faulting scarps in the Montenegro, at the southern end of the Dinaric chain. The authors proposed that the presence of these tectonic features could represent the effect of an early stage of tectonic extension in a region dominated by compressive deformation. Dating fault scarps exposition by mean of cosmonuclides permits the authors to claim discrete events of fault activation, as response to earthquakes occurring along these structures.

Although I found the paper appealing in terms of tectonic interpretation, the work has some substantial weaknesses that prevent to accept it for publication.

I list my major concerns below:

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).

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 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.

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.

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.

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.

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 investigates structures, as secondary structures associated to primary seismogenic thrust faults.

Regards

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