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Final response on RC2

Peter Biermanns et al.

Author comment on "Aegean-style extensional deformation in the contractional southern Dinarides: Incipient normal fault scarps in Montenegro" by Peter Biermanns et al., Solid Earth Discuss., <https://doi.org/10.5194/se-2021-97-AC5>, 2021

Dear Prof. Benedetti,

we would again like to thank you for the comments on our manuscript. In our author comment 'AC2' from Nov.5, we responded to your individual comments already to encourage further discussion. Since we posted that author comment, our conception of the manuscript has not significantly changed so that the present "final response" corresponds 95% with the cited earlier author comment. We only specified a few of our statements. Your overall comprehensive review is certainly valuable and will undoubtedly add to the quality of the final outcome. However, we were surprised to be accused of a 'provocative and assertive tone' of our contribution. In terms of content, a detailed look at particular points of the criticism expressed left us with the impression that our manuscript might have been (at least in parts) misunderstood (see responses below). Many aspects that are allegedly missing or misreported, can clearly be found in the manuscript. Nonetheless, we emphasise that we are in no way resentful, but highly motivated to use all of your comments to complement and re-arrange the text in order to highlight several aspects and conclusions. In the following, we are responding to individual comments:

1) *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)*

□ In chapter 2 we give an outline of the seismotectonic and kinematic setting in our study area and its surroundings, including a comparison with the Italian side. We also highlight that the location of the discovered fault scarps is very close to an already known and well-approved (the according references are presented) transition between (a) extensional tectonics in Eastern Albania and (b) contractional tectonics along the coast. The reasons why we interpret the discovered features as active normal faults are detailedly presented in chapter 4.1 and further scrutinised/discussed in chapter 5.3. We are puzzled about the reproach of not considering normal faults in compressional contexts, as we repeatedly bring this explanation into play (e.g., LL 27 ff.; very clearly in LL 252 ff. and 272 ff.) Still, we take this as an opportunity to make our points even clearer and expand our elaborations on the regional tectonic setting!

2) *The tone of the paper somehow provocative and assertive [...]*

□ This was not our intention and we will certainly look for passages that could be 'mitigated'. Still, we are surprised about this comment as we especially tried to formulate the more 'daring' statements with extreme caution (e.g. L 22 - '...suggesting that...'; L 24 - '...might be induced...'; L 47 - 'possibly seismogenic' etc.). Appositely, we bluntly discuss more than one possible formation mechanism of the introduced structures which (in our opinion) is anything but provocative and assertive, but rather well-balanced.

3) *On Figure 4, the colour are difficult to distinguish but the normal faults appear to correspond to the contact between Mesozoic 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 agree that the colours of the official geological map in Fig. 4 are hard to differentiate. We will adjust the colours and add signatures to distinguish the units more clearly from each other. However, the misinterpretation that the scarp corresponds with the tectonic contact of the nappe probably goes back to the map scale. We will therefore create an inset 'zooming into' the direct vicinity of the fault scarps. One of the scarps (KFS) is close to a Mesozoic/Eocene thrust contact, but clearly does NOT coincide with it. The KFS is in the middle of the Triassic units as illustrated on the map. The Quaternary deposits are present (described in chapter 4.1, LL 158 ff.), but are too small to be illustrated in the chosen map scale in Fig. 4. Both aspects will be visible by adding a larger-scale map, which we will readily do.

4) *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 of the carbonates and its relation to the fault planes is clearly described in chapter 4.1, L157, and shown in the cross sections (Fig. 4 B and C). Since this specification was apparently hard to find, we will highlight it more clearly and we will add the according explanation to the legend of Fig. 4.

5) *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 ^{36}Cl 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 mentioning 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 agree that the obtained absolute dates are the more 'solid evidence'. Therefore, we will move this part of the study clearly to the front and present the second approach only for comparison – as you suggested. In terms of the LGM timing, we will keep our 18 ka frame for the same reasons explained in response # 4 of Prof. Roberts' comments. First of all, the exact timing seems to be a matter of debate, as can already be seen by the discrepancy between two reviewers. We chose 18 ka in all conscience, after (from our point of view) synthesizing an ideal age from works in the surrounding areas. In section 5.1, we discuss the reliability of this age which is, e.g., also used by Papanikolaou et al.

2005, or Giraudi and Frezzotti, 1995. As our slip rate calculations based on the proposed LGM age are easily reproducible and "convertible" from the provided tables, we prefer to maintain our 18 ka age – also as a compromise between both reviewers.

6) *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 discutabile and should not be presented as straightforward. 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 distinguished ?*

With all due respect, we disagree. As a look at chapter 4.1, LL 163 ff. and chapter 5.3 shows, it is not true that a precise description of the ribbons and a discussion of their formation mechanisms are missing. Still, we are willing to further specify and expand these aspects. To us, the constant widths of the up to 5 ribbons (also illustrated in Figs. S7 A-C) across 48 locations on the fault scarps exclude other (erosional) formation mechanisms. Especially snow (as suggested) is regarded as extremely unlikely: (a) Snow as an extremely short-lived phenomenon in this coastal climatic setting at low elevation will probably not leave such distinctly visible marks. (b) Snow would possibly never create such uniform ribbon widths, as snowdrifts would certainly yield variable thicknesses of the snow blanket. We will add some of these arguments to the manuscript.

7) *Moreover, the way they are presented in the abstract is misleading because it suggests that the slip amount and age is deduced from the ^{36}Cl 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.*

We agree that both approaches and their results could be separated more clearly in the text. We are aware that five ^{36}Cl samples yield a relatively low resolution, but as the other reviewer, Prof. Roberts, recognized in his review: This is only a first approach on which further studies may build upon. Even if we agree that a higher sampling density would bring "nicer" results, we do not expect any other conclusions compared to a sparser sampling, as we think the results are very clear. We will emphasize this in the text.

8) *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.

On the one hand, this shows us that we may need to consider a re-arrangement of the text blocks and given information or at least add aspects to the introduction. On the other hand, we are surprised about the suggestion to present the normal faults in a compressive regime as 'puzzling', because this is exactly (literally!) what we do in L. 41. Also, we are of the opinion that our expressions like 'possibly seismogenic' (L. 47) or 'the occurrence of these newly discovered structures is still fully unexplained' (L. 43) could hardly be more cautious. However, we will try to rephrase these sentences with even more modesty. Initially referring to the introduction it is suggested that aspects should be detailedly described and discussed which we think is better preserved in the results and discussion chapters. In exactly these chapters (especially 4.1 and 5.3 and 5.4) we describe and discuss why exactly we interpret the structures as active normal faults and which formation mechanisms could be responsible.

9) 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.

Again, with due respect, this is not speculative at all, but proven by numerous (cited) publications. The transition from a (close-by) hinterland extensional (slab-tearing-induced) to a compressional domain near the coast is well-acknowledged (e.g., Dumurdzanov et al., 2005, Handy et al., 2019) for a long time already, while recent publications (Pondrelli et al., 2021) suggest that this transition could even be closer to our study area than hitherto expected.

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

True. We will change this.

11) 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).

See #1. We repeatedly introduce this explanation approach.

12) 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.

This is exactly what we say. We literally speak of both (LL 53 and 56), the Apennines and Dinarides. Furthermore, we do already cite the work of D'Agostino. Of course, we will add Nocquet as well.

13) 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 are still not sure where we present an opinion: The transition between an extensional and a compressional domain in the Hellenides/Dinarides is a well-established observation backed by numerous published (and cited!) geological and seismotectonic studies – and increasingly also by geodetic ones. The same is true for the abundance and size of normal fault scarps in the mentioned regions. The current view that normal fault scarps are non-existent in Montenegro is now obsolete due to the discovery of the presented fault scarps. Although we are not conscious of any mistake, we will readily

rephrase the sentence, as it apparently might have deemed you provocative, which was certainly not our intention.

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

In fact, contrary to your statement, ³⁶Cl dating is possibly affected by vegetation according to Dunai et al., 2014 (Quat.Geochron.). But you are right that this is not really what we wanted to refer to, so we will remove the brackets and their content.

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

See chapter 5.1 where we discuss the origin in the discussion chapter. We will add the Papanikolaou paper as a reference here as well to prevent confusion. See also response #5 and response #4 to Prof. Roberts' comments.

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

We will rephrase the sentence.

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

Gravitational collapse is discussed (LL 258 ff.). Also, the bedding is specified (L 157). We will elaborate more on this point and emphasize our arguments against a landslide feature.

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

In chapter 5.3 we discuss in detail why we exclude other formation mechanisms. Figure S7 shows a remarkably constant width of the (up to 5) ribbons across 48 locations (see also LL 183 ff.). See also response #6.

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

Again, the bedding IS specified (L. 157) and illustrated (Fig. 4).

20) *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.*

See response #1. We are constantly discussing 2 different approaches, starting in the Abstract. Still, we see the necessity to make this clearer. We will be happy to implement this in a final version.