Reply to reviewers

RC1

Reviewer comment:

"The paper by Grützner et al. on Slovenian faults is well written and illustrated. The methods used are complementary and adequate to the purpose of finding hints for the Holocene activity of the two faults studied. Results are globally presented in a rigorous and detailed way. However, the study of these faults is not easy because of their low slip rate, the presence of vegetation over most of their trace, and of the anthropic action. Keeping this in mind, and not expecting the same quality of evidences as for San Andreas Fault, I still find that the arguments to demonstrate primary surface rupturing on these faults are not convincing and partly contradictory.
On the Predjama Fault, both morphology and trenching do not bring substantial evidences of tectonic deformation. The scarp is very small in height (0.5 m) and in length (200 m), and even not visible on the DEM (is it the one with 5 cm resolution?). But above all, a systematic offset of 0.5 m is not compatible here with the dextral kinematics of the fault."

Answer:

Dear reviewer,
Thank you very much for your very detailed comments and for your constructive review. Indeed, the faults are slow-moving, the area is vegetated, precipitation is rather high, and strong earthquakes are rare. This is why it is so difficult to find concrete evidence for past large events.
The scarp that we discovered along the Predjama Fault is small and short, and it is also overprinted by the dirt track that partly runs on top of it. However, it clearly shows an offset of the entire slope, which means it can not be attributed to local modification of the slope only (following the argument in Copley et al., 2018). Furthermore, there is no evidence for slope movements visible in the field or in the DEM. The scarp is actually visible both in the 1 m LiDAR DEM and in the drone DEM, but you are absolutely right that we failed to properly show it on a figure, only in the profiles. This is due to the inappropriate lighting from the NW. We added a figure highlighting the scarp in the DEM (Fig. 6a). Please also find the drone DEM now published at https://portal.opentopography.org/dataspace/dataset?opentopoID=OTDS.032021.4326.2. The DEM includes the open trenches, so that the relationship between the trenches and the scarp becomes clearer. We also added a sketch that illustrates the difference between a locally modified slope due to road construction and a systematically offset slope, similar to Fig. 2 in Copley et al. (2018), because we realize this is needed to illustrate our point without having to refer to another publication (Fig. 6b).
You are also right that the vertical offset is not what is typically expected for a strike-slip fault, but small vertical components of motion have been observed in recent large strike-slip earthquakes, too (e.g., 2010 Darfield Earthquake – Quigley et al., 2010; 2010 Yushu Earthquake - Lin et al., 2011; 2016 Kumamoto Earthquake - Shirahama et al., 2016). These are either due to the fact that the entire fault system is under transpression, or by small local bends in the fault trace, or by a combination of both. Although prevailing vertical motion is not expected, a vertical component of slip can be due to an irregular, bending fault trace. For the purpose of the national seismic hazard model, Atanackov et al. (2021) estimated a rake of the Predjama Fault between 140°-180°. In our case, we can exclude the possibility that the vertical separation is due to the lateral shift of pre-existing morphology because the slope is smooth. Therefore, we do not think the occurrence of vertical offsets is incompatible with strike-slip faulting. Moulin et al (2016) have also shown that the Dinaric strike-slip faults produce long-term vertical motion, and we see topography associated with the faults in the landscape.

Reviewer comment:

"The trenches do not show any clear rupture. Depositional and erosional processes may determine this geometry, especially by the accumulation of gently dipping slope material locally truncated by runoff processes. The youngest unit (U6), interpreted as a sag pond deposit, lays on both sides of the break in slope: in this configuration it cannot post-date tectonic deformation as it is suggested. Geophysics puts in evidence a quite localized resistivity contrast at depth. However, this limit does not seem to correlate with the scarp position at surface, since it is in average situated a few meters to the south. An idea of the geology of this zone, which clearly lacks, may help to interpret these data."

Answer:

We agree that the Predjama trenches do not show a clear rupture and, in particular, not a sharp and distinct fault trace. We have also long thought about possible non-tectonic mechanisms as the cause of the observed deformation features. For the following reasons we think that depositional and erosional processes are less likely to have caused what we see in the trenches:

(i) We did not find any evidence for slope-parallel motion in the trench, such as shallow sliding planes.

(ii) We do not see any evidence for run-off processes or slope-parallel motion in the two high-quality DEMs (1 m LiDAR and drone DEM). The slope is very smooth apart from the scarp, there are no landslide scars or 'wrinkles' that may point to the toes of slides etc. Since the sediments are of Holocene age, we might expect to see such features in the morphology.

(iii) The vertical separation of units U1 and U2 across what we interpret as the fault zone is not easily to be explained with gravitational processes only, because the downslope side is uplifted.

However, we do also realize that we cannot fully exclude the possibility that depositional and erosional processes have led to the present-day configuration. We therefore discuss this possibility in the revised manuscript in detail and leave it to the reader to draw the conclusions. Abstract, discussion, and conclusions have been modified accordingly.

We thank the reviewer for pointing out that unit U6 is not likely to be a sag pond deposit. In the revised version, we removes this interpretation.

We agree that the sharp resistivity contrast in the geophysical data is located a few meters to the north of the scarp in some (e.g., profile 8), but not all profiles (e.g., profile 10). This contrast is not only visible in single profiles, but it is a consistent feature that can be traced for at least 60 m in consecutive profiles. We cannot expect a single sharp fault zone that separates blocks of intact bedrock on either side. This is what we see in our trenches, if one believes that there is tectonic deformation in them, and this is also what can be observed in fault outcrops. For example, the quarry that we describe in Fig. 3c exhibits a ~30 m-wide shear zone. A 3D-model of said quarry can be found here for inspection: https://sketchfab.com/3d-models/predjama-fault-in-an-abandoned-quarry-slovenia-44b631f0b48046c3a653edebeac631a2. A wide shear zone also characterises the Idrija Fault as detailed in our manuscript. Thus, we argue that it is not surprising that the sharp resistivity contrast does not perfectly match the scarp location and the deformation zone in the trench. We are rather surprised that we do see this sharp contrast right at the fault trace or within a few metres distance only.

Thank you also for the suggestion to add more details on the geology surrounding our study sites. We added three figures with geological maps of the study area(s), which have the same extent as Fig. 2 including the two insets (new figs. 3, 4, and 9). This also answers your comments in the annotated manuscript. Unfortunately, there are no natural outcrops that expose the fault rocks close to our trench site. The quarry mentioned above is situated a few hundred metres away and exhibits the fractured limestones, but without a suitable Quaternary cover that would resemble the situation in our trench. A large roadside outcrop is situated just 70 m south of our trench site, but these Jurassic limestones do neither exhibit a fault zone, nor are they covered with clay. We added the description of this outcrop to our manuscript. In our trench we find partly weathered Cretaceous calcarenites. According to the 1:250,000 geological map of Buser (2009), the Predjama Fault juxtaposes these two different lithologies at our trench site.

Reviewer comment:

"On the Idrija Fault, even though morphology is flat at the fault trace, stratigraphy reveals possible deformation due to tectonics. In this context, as authors say, the ERT data were crucial for the choice of trench location. Nevertheless, to me two questions remain: firstly, is it really primary rupture that we observe? The absence of sharp planes and typical features of sand dyke (Unit 9) rather than fill fissure make me more thinking about liquefaction due to local shacking. Authors reject this interpretation, but it would be interesting to know more about the composition of the unit and its relation with overlaying Unit 10."

Answer:

It is right that we do not see a sharp fault zone in the trench. There are four main arguments that led us to interpret the observations as primary ruptures: (i) The open fissure filled with overlying material, typical for strike-slip earthquakes; (ii) the vertical terminations of layers at said rupture, which indicates lateral motion; (iii) vertically aligned pebbles; and (iv) a large ruptured clast. This interpretation is supported by a sharp resistivity contrast in greater depths, which shows that the trench is located right above a fault, and by the fact that the trench is situated along strike of the mapped fault zone. The fissure is filled with dark, organic-rich material. The base of the layer above is the only one in the trench that also yields this dark layer rich in organics, and the fissure reaches the base of this layer. This indicates that the fissure was filled from above and later became covered by the thick clays that make up the top part of unit U10. If the fissure were instead a sand dyke that propagated upwards, we should see the source material of the dark fissure fill at its base, which is not the case. Furthermore, we do not see any liquefaction or soft sediment deformation structures (SSDS) elsewhere in the trench (flame structures, warped layers, mushroom structures, pseudonodules, broken layers, ball-and-pillow structures etc.). We thus think that it is very unlikely that the observed deformation features are merely due to liquefaction. We expand our documentation and discussion on the absence of other SSDSs to make this point clearer, and we also add the argument that a sand dyke would either need to root in its source material or penetrate all the way to the base of the trench to a lower source level.

Reviewer comment:

"This brings me to the second question: Unit 10 is described as having a different composition, but Unit 9 is filled with Unit 10 material. Moreover, if Unit 9 is filled by Unit 10 material it means that the dating of Unit 10 pre-dates the deformation event, not the contrary. On the other hand, how the fissure can open just below Unit 10 without affecting it? This part definitely needs some clarification."

Answer:

Thank you very much for pointing out this contradiction. We improved the description of Unit U10. This unit has at its base a composition (silty-sandy clays) and colour (dark grey) that resembles that of the fissure (U9). This lowest part is rich in organics and dark. It then changes upwards into massive grey and brownish clays with a minor gravel component. It is not possible to draw a sharp boundary between the dark, sandy clays and the greyish-brownish clays with gravel because the transition is gradual. However, we have to map these deposits as one single unit because it clearly covers all the other fine and coarse units in the trench and because the internal changes are so gradual. We interpret that the fissure fill was sourced from the lowermost parts of U10.
Sample SLO18_SK6 is a charcoal from the transition zone of unit U10. It is situated just above the dark basal zone of U10 that resembles the fissure fill. Therefore, you are right that we must assume that the sample SLO18_SK6 pre-dates the formation of the fissure. However, this charcoal sample is older than the samples from stratigraphically lower

positions. We thus have to take into account that it has a complex history. We discuss these issues now in greater detail.
Actually, all charcoals may have had a complex history, which is why their use is limited. The best constraint comes from sample SLO18_SK11, which is rather young (492-315 cal BP). This age tells us that U10 cannot be older than 492 cal BP. The older ages of the other charcoals must therefore be due to a complex transport history of the charcoals. They do not tell us much about the earthquake timing. It is unfortunate that the other samples of unit U10 do not stem from the very base of U10, which would have helped to better inform about the age of the deformation, but most of the (charcoal) samples that we took turned out to be of insufficient quality after pre-treatment in the lab. We changed our description in the revised version of the manuscript.
Please note that we also uploaded a high-resolution DEM of the Idrija Fault trench site to OpenTopography.org, in which the trench location is documented:
https://portal.opentopography.org/dataspace/dataset?opentopoID=OTDS.032021.4326.1

Reviewer comment:

"If authors succeed in bringing new lights on these points and overcome the incoherence of some interpretations, the discussion about the deformation history could be more robust and convincing. In any case, I think that looking at the data there is no argument to discuss fault magnitudes in this study. I would forget this overinterpreted part that particularly fragilizes the rest of the paper. Identifying regional structures that have ruptured in Late-Quaternary times and having an age range of the latest events on them is already a valuable issue."

Answer:

Thank you very much! We agree that discussing magnitudes weakens the paper. Accordingly, we removed this part from the manuscript. We also happily incorporated all the other comments raised in the annotated PDF, which mainly refer to clarifications, more precise wording, and some changes to the figures:

- page 2: phrasing may be improved here

Done.

- page 3: the regional geological history is described. However we lack a map of geological formations, which would help to understand the relationships between structures geometry and active tectonics.

Done – added three new figures.

- page 3: since when?

"Moulin et al. (2016) concluded that the strike-slip movement on the fault probably originated in the Early Pliocene."

- page 3: not clear, lower than 10 km? How much?

2 km.

- page 3: please define "short-term".

"(~15 ka)"

- page 4: add a legend for the size of focal mechanisms

Done.

- page 5: it would be nice to see the fault trace on the original DEM (without interpretation)

Done – new fig. 6a.

- page 5: it's the latest strong EQ in the region, give its Mw and indicate it on the map.

Done - We added the magnitude and changed the caption of Fig. 1 so that the Friuli sequence can be identified.

- page 6: date?

1895.

- page 7: the drawned fault hide the trace that should be visible on the DEM

Done, fig. 6a

- page 7: I think "prominent" is a bit overestimated!

Changed.

- page 7: I understand the difficulty to work in such a slow deformation context and vegetated environment. However, both morphology and trenching do not bring substantial evidences of tectonic deformation here. The scarp is very small in height and in length, and above all the systematic offset of 0.5 m is not compatible with the dextral kinematics of the fault. The trenches do not show any clear rupture. Depositional and erosional processes may determine this geometry.

Additional data and discussion added, see comments on the main points.

- page 7: a view of the 5 cm drone DEM is required. Th DEM in figure 4 has a too low resolution (1 m? more?) and no break in slope may be detected.

Done – added new fig. 6a and made the drone DEMs available via opentopogrpahy.org.

- page 10: there is not corrspondence between the position of the interpreted fault and the break in slope, which is in average situated a few meters to the south.

See comments on the main points.

- page 10: how do you explain this stratigraphy? It is important for the tectonic interpretation! To me it seems a series of colluvial units sub-parallel to the slope, truncated by oblique runoff features. It is no obvious to see vertical displacements since the units may not correlate along the trench.

We added a detailed description of thew units and the stratigraphy and we critically discuss the possible mechanisms that could have created the observed features. See comments on the main points.

- page 13: Unit 6 lays on both sides of the break in slope: if it were a sag pond post-dating tectonic deformation it would have deposited only upstream of the break in slope.

We removed this interpretation.

- page 13: where on the map?

We did not annotate Moulin et al.'s morphological markers because our figure would become too crowded. It may also be misleading for the readers, who might think that we interpreted the offsets or that we support this interpretation. The reader is referred to Moulin et al.'s paper.

- page 13: an idea of the terrace age?

No, as far as we know, our study is the first to date the fluvial units.

- page 18: but unit U10 has another composition. And how the fissure can open without affecting U10?

See comment on the main points above. The most reasonable interpretation is that the base of U10 filled the fissure.

- page 18: the shape of U9 (composition?) looks more like a sand dyke, truncated and sealed by U10. This is for me an indirect evidence of local shaking. However, relative chronology between U9 and U10 must be clarified.

Done, see comment on the main points above. We think it is impossible that this is a dike, because of the lack of source material, its composition, and the lack of any liquefaction features.

- page 19: the bend looks very small.

True, but there is topography across the fault (Moulin et al., 2016) and the rake varies between 140°-180° (Atanackov et al., 2021). Other recent strike-slip earthquakes resulted in vertical displacement, too.

- page 19: difficult to correlate U1 and U2 from north to south of the trench, units are not well preserved and no absolute dating support this interpretation.

True. We added a much more detailed description and discussion in the revised version. Also see comments to the main points.

- page 19: not consistent with the scarp position

See comment on the main points.

- page 19: To claim the rupture of the fault up to surface a clear trace should be observed through the trench.

True, but we do not observe a sharp fault. We base our interpretation on a number of arguments, among which the scarp points to surface rupture. See comment on the main points.

- page 21: if fault is strike-slip, the height of the scarp cannot be an offset. The offset depends on slip vector (unknown here) + topography.

We deleted this section.

- page 21: this should be demonstrated with more elements, it is a main issue!

Done.

- page 22: in the description of the trench, you say that U9 is filled with material from U10. In this case deformation is post-deposition of U10, not before it. What is true?

See comments above.

- page 23: to me, there's no argument to discuss fault magnitudes in this study. I would forget these parts that fragilize the rest of the paper.

Deleted.

- page 25: not so obvious to distinguish between rapid and slow deformation

Yes, that’s right. We phrased more carefully.

RC2

Reviewer comment:

"The authors present a comprehensive paleoseismological study of the Holocene activity of the Dinaric Fault System, which threatens major cities in the region such as Ljubliana or Idrija. They used a large number of methods to obtain their data in a difficult environment with low tectonic activity, in addition to fluvial deposits, forests, intense anthropogenic activity, ... The opportunity for such a study to be undertaken is noteworthy and the use of the several approaches to the topic adopted is mostly appropriate. The paper has a good organization showing a well-organized research work by the authors, providing all kind of information (figures, complementary images, dating samples, ...), for which the authors should be complimented. The writing style is clear and easy to follow. The resulting paper will provide a worthwhile input to the future SHA of the region. However, the manuscript, in its present state, would benefit from further attention and tightening up technically. For my part, the manuscript requires major/moderate revision.
Although the following comments, along with those included in the revised pdf, may appear to be challenging or negative, they are intended to be constructive, as the nature of the topic and how it is being addressed make it worthy of such attention from my pointof-view."

Answer:

Thank you for the very positive and constructive feedback.

Reviewer comment:

"In general terms, further geological description of the sites must be done, especially at Predjama fault : type of sedimentary units, the environment of deposition. A geological map must be included. An adequate description of the sedimentary units exposed on the trenches walls must be also done."

Answer:

Thank you for the suggestion to add more details on the geology. We add three figures with geological maps of the study area(s), which have the same extent as Fig. 2 including the two insets (Figs. 3, 4, and 9). We also added a more detailed description of the geological units found in the trenches.

Reviewer comment:

"Predjama Fault: My main concern of the interpretation of the trenches in this fault is about the possible pedogenic development on the exposed materials. I am not sure whether the dating is giving the age of a sedimentary deposit or the progressive and continuous process of edaphization. Besides this, the interpretation of unit U6 as sagpond seems to me rather unrealistic. See detailed comments on the pdf."

Answer:

Thanks for the detailed comments here and in the annotated PDF. We provide more detailed descriptions of the geological units that we encountered in the trench. We interpret units U3 and U4 as weathered calcarenites. Unit U5, however, also contains sand, fine gravels, and charcoal fragments at its base (where it is in contact with U4). Thus, U5 is not just the product of weathered bedrock, but also involves slope deposits. This lowermost part of U5 parallels U4, which is why we interpret the base of U5 as having been involved in the deformation. Therefore, sample SLO18_BAN3 should pre-date the deformation. However, we agree that non-tectonic processes may have also contributed to the features that we see in the trench, and we realize that we must discuss possible alternative interpretations and processes in more detail. We therefore discuss these possibilities in the revised manuscript in detail and leave it to the reader to draw the conclusions. Abstract, discussion, and conclusions were modified accordingly. We thank the reviewer for pointing out that unit U6 is not likely to be a sag pond deposit. In the revised version, we will remove this interpretation.

Reviewer comment:

"Idrija Fault: the analysis of the stratigraphic relationship with a potential earthquake is too speculative. The uncertainty of the ages (both, epistemic and methodological) obscure any reliable analysis."

Reviewer comment in the annotated PDF on the same subject:

"The two other samples collected from U10: SLO18_SK13 and SLO18_SK6 give a consistent age around 2300 ka. Taking SLO18_SK11 as representative for the U10 would give an unrealistic young age for the unit and the tectonic activity.
2300 ka is also consistent with the age of the fissure filling. The last tectonic event must occur before that date, and maybe after 2645."
"An alternative dating method as OSL would have been valuable (U10 looks really adecuate sediment for that method)"

Answer:

We agree that the dating results are not as straight forward as we would have wished, and indeed we cannot narrowly bracket the tectonic deformation. We did not take OSL samples, because we found abundant charcoals in the trench. Unfortunately, most of the charcoal samples that we took turned out to be of insufficient quality after pre-treatment in the lab.
Sample SLO18_SK11 gives a very young age of 492-315 cal BP. This contrasts all other samples, which cluster around 2-2.5 ka. (An exception is SLO18_SK8, which yielded ca. 1.5 ka, but it is also a bulk age and not a charcoal age.) One possibility is that the young age of sample SLO18_SK11 is simply wrong. However, it is a charcoal sample and not bulk, and there is no indication of technical difficulties with the sample. It could have been brought into its position via bioturbation, but we checked carefully and did not find any evidence for burrows etc. It is easily possible to deposit an older charcoal in younger sediments, but not vice versa. Therefore, we must assume that the age is valid. There is practically no way to find out how long it took to deposit unit U10, because the samples SK6, SK11, and SK13 are not in stratigraphic order, but sample SK11 indicates that the deposition cannot pre-date 492-315 cal BP. However, we do agree that the clustering around 2-2.5 ka raises the question if something is wrong with sample SK11 (although, as mentioned above, there is no hint for that). The only way to solve this issue is to raise this possibility in the discussion section of the manuscript, which we did. In any case we would like to stress that we are not trying to sell a very young rupture, e.g. in the 1511 Earthquake. This is reflected in the title of our manuscript and we also very carefully discuss the pros and cons. For us, the important message is that the fault ruptured in the last 2.5 ka. Our study does not aim to find the 1511 rupture. Even if the features in the Idrija trench were not due to primary rupture, but due to seismic shaking as suggested by reviewer 1, this means that a strong earthquake occurred very close to the trench site, and perhaps on the Idrija Fault. However, we think that many arguments are in favour of a rupture on the Idrija Fault, despite the trench not showing a textbook rupture with a sharp fault.

Reviewer comment:

"Geomagnetic and georadar surveys could be removed from the paper, as they do not show any valuable results."

Answer:

Thanks for this suggestion. We would like to mention these geophysical surveys for two reasons. (i) It may be a valuable information for other researchers attempting to use GPR or magnetics in a similar setting for active fault studies. (ii) The data set is huge and freely available for everyone via the PANGEA repository (Grützner, 2020). Thus, it can be used for any kinds of scientific studies or other purposes. We would like to link the data with the trenching results for the sake of completeness. Also, the methodological description is very short and does not unnecessarily lengthen the paper a lot.

Reviewer comment:

"The analysis of the earthquake magnitude on both faults is completely speculative. The authors cannot make any reliable estimation with their data. Remove these sections. Please also note the supplement to this comment: https://se.copernicus.org/preprints/se-2021-7/se-2021-7-RC2-supplement.pdf"

Answer:

Thank you very much! We agree that discussing magnitudes weakens the paper. Accordingly, we removes this part from the manuscript.

Reviewer comment in the annotated PDF:

"As you describe later, Sava Ft. and Periadriatic F. accommodate part of this deformation. In the way that it is written, seems like all the motion were accommodated only by DFS. Could even some other not studied faults take part in this motion, also?"

"I do not understand what is the objective of this discussion. Trying to find some correlation between recurrence interval and slip rate? Please, rewrite it."

Answer:

Yes, that is right. In our thought experiment, we therefore wrote "If the entire northward motion of Adria were accommodated by the right-lateral motion on the DFS …" Thank you for pointing out that this is misleading. We added the information for the Sava and the Periadriatic Ft. and make clear that we talk about the absolute maximum cumulative slip rate that can be expected for the DFS. The entire reason for this paragraph is to make the point that the faults must be slow given the overall slow Adria-Europe convergence, and that the recurrence interval of strong earthquakes must, therefore, necessarily be long.

We also incorporated all the minor suggestions made in the annotated PDF:

- page 2: Give a representative range of values for the region and reference it.

Done. There is a lack of paleoseismological studies in the region and thus, basically no knowledge on EQ recurrence intervals. We write: "Areas with similar strain rates of a few mm/a are known to have recurrence intervals in the order of thousands to tens of thousands of years (e.g., Grützner et al., 2017)."

- page 3: Additional geological information is needed for a complete understanding of the studied sites: lithology, age of stratigraphic formations, geological maps (at least at the scale of Fig. 2).
Are the units exposed in the trenches fluvial/alluvial/colluvial deposits, or weathered bedrock?

Done, we added new figures and a detailed description of the units.

- page 3: Reference?

Moulin et al. (2016).

- page 4: It would be useful to incorporate within Fig. 1 a figure showing the regional tectonic framework which includes the tectonic features named in the text: Dinarides, Dinaric thrusts, Adria and Europe plates, Eastern Southern Alps, 3 mm/yr convergence, counterclockwise rotation, ...

Done.

- page 4: Include the frame of the Fig.2 for an accurate location of the study area.

Done.

- page 4: To be consistent with the subsequent Figures (and many other studies of active faults) fault traces should be in red. Int. borders in black.

Done.

- page 5: Rasa Ft. must be displayed in this figure.

Done.

- page 5: Label these two EQs in Fig.1 with the year of occurrence.

We did not label them, because we think this information does not help to better understand our study. The reader is referred to the literature, which we expanded.

- page 6: Display and/or label this fault in Fig.1

Done.

- page 6: Indicate year of occurrence

Done.

- page 8: The contrast in humidity is quite straight and seems to be controlled by the location of the fault in almost all the profiles. The shadow hypothesis is too weak. Could it depend on the thickness (or even the presence or not) of the Unit U5 exposed in the trench?

Thank you for this suggestion, which we added to the manuscript.

- page 11: Is this unit fluvial/alluvial/colluvial or is the weathered bedrock instead? It seems to have some kind of well-defined bedding and a rock mass looking. If it is a weathered bedrock, the description in the text (i.e., clast supported in a clayey matrix) and representation of U1 in Fig. 5 should not be as a kind of gravel deposit, but as a fractured and weathered bedrock.

Unit 1 is weathered and fractured bedrock, but it is so intensely fractured, that parts of this unit have a large amount of clayey matrix. We keep the signature in the trench log to emphasize this and to also illustrate that the degree of fracturing changes within the unit. A fracture signature woulöd thus be misleading in our opinion. We modified the description to be more precise and we refer to the high-resolution trench logs in the supplement.

- page 11: If units U1 and U2 are not bedrock, could the dip be depositional? Under what type of environment were these units formed? Fluvial, alluvial? This aspect is important to point out possible tectonic deformation.

See comment above.

- page 11: "Massive" is not a proper term for a clay deposit a few centimetres thick.

Changed.

- page 11: This is an important feature that must be reported in this paper since this is strong evidence of tectonic activity.

Thanks, we agree!

- page 11: Do the authors mean the isolated calcarenite blocks or the whole unit U2? Justify why younger units (U3 and U4) do present deformation while U2 do not.

Yes. They are offset across the deformation zone, but not deformed, because they are not present where we observe the bending in U4.

- page 11: Labels in Fig. 5 and Table 1 show "18". Correct discrepancy all along the text, table and/or figure.

Thanks – done!

- page 12: Fit the horizontal lines of the grid with those on the trenches dug at field for a proper location of the units and samples.

Done.

- page 13: According to the sketch in Fig. 5, Unit 5 overlies directly on unit U1 and seems to include the units U3 and U4 as lens. Please, explain this stratigraphic distribution. U5 seems to have some kind of ghost layering. It would be useful for interpretation to plot some of these layers which draw the deformation. The authors must consider the pedogenic development along the exposed units

Done – we expanded our description and discuss this issue in detail. See also comments on the main points. We refrain from drawing the "ghost layering" because we did not observe it in the trench. The exception is the base of U5 where it is in contact with U4. This case we describe as U5 being clearly parallel to U4.

- page 13: A sagpond is quite unrealistic on such a slope with easy surface drainage along the fault trace. Also, it would have developed without a clear or prominent shutter-ridge associated with it.
U6 is drawn with a shape with an erosive base, which is not proper of a sagpond. Besides this, a sagpond must have at least one of the lateral edge controlled by a fault strand. Moreover, this type of deposit should contain layers rich in organic matter that could be easily dated. U6, if is not anthropic, could be a kind of flat zone where water remains giving a local weathering or pedogenic profile.

Changed.

- page 13: The folding described for the underlying units points to an uplift of the southern block by drawing a kind of drag fold, which does not favour such a deposit.

We agree – changed.

- page 16: Could be the same unit labeled as U2?

That's what we also discussed in the trench, but then the colour was much more red and much brighter, and we therefore decided to give it a unique label. In any case, this unit is not important for the earthquake story.

- page 19: How the authors explain the downward bending located on the downthrown block. Does it not have to be the opposite as a sort of drag fold?

That's absolutely right. In the revised version, we state: "However, in such a setting one might expect a drag fold geometry. In our trench we observe the opposite. This could either indicate that non-tectonic processes were involved in the deformation or that an unknown amount of lateral motion has contributed to the observed geometry. In the first case, it is hard to imagine a process that would lead to such a configuration and to also result in a ~200 m-long scarp. Lateral motion, however, can juxtapose units that were originally located at different depths and lead to complicated deformation patterns as a result of transpression."

- page 19: Watching carefully the supplementary photos, U6 seems to be in angular unconformity over U5 at the SW (vertical 2-3 m). Some more ghost layering can be observed within U5 between 4-5 m. If no more evidence of unconformities has been identified in between, that is the relationship that should be assumed. Hence, a single event could explain the whole deformation of U5, U4, U3 and U2, at least.

We agree and write: "the simplest model that can explain all observations is a single event".

- page 20: I agree U4 is deformed, but U5 is also deformed in the same way, since U5 includes U4 somehow (See comment above)

We agree.

- page 20: No fault trace has been identified, hence it is impossible to distinguish between lateral different units within the U5.

We did not find a sharp fault trace, but several lines of evidence point towards a fault zone that is a few tens of cms wide. This can accommodate lateral motion.

- page 20: Could these samples be rest of roots?

We are sure that these were proper charcoals.

- page 20: These widely scattered ages (13-0.7 ka) with no chronostratigraphic correlation are all within the Holocene, just after the last glacial period. Pedogenic development?

We discuss this possibility in detail in the revised version. Possibly, yes.

- page 20: This section is completely speculative. The authors cannot make any reliable estimation with a supposed vertical offset measured a single site 200 m long. Remove this section.

Removed.

- page 22: The ages obtained in such a difficult fluvial environment is noteworthy, but the analysis of their stratigraphic relationship with a potential earthquake is too speculative. The uncertainty of the ages (both, epistemic and methodological) obscure any reliable analysis. Most of the ages range between 2645 and 2000 ka (except for SLO18_SK8 and SLO18_SK11). An alternative dating method as OSL would have been valuable (U10 looks really adecuate sediment for that method)

We discuss the uncertainties related to the samples in much more detail now, and we discuss the implications. In hindsight, we agree that OSL would have been a good additional constraint, because most of the many samples that we took turned out to be of insufficient quality after pre-treatment in the lab.

- page 22: The two other samples collected from U10: SLO18_SK13 and SLO18_SK6 give a consistent age around 2300 ka. Taking SLO18_SK11 as representative for the U10 would give an unrealistic young age for the unit and the tectonic activity. 2300 ka is also consistent with the age of the fissure filling. The last tectonic event must occur before that date, and maybe after 2645.

We discuss this possibility, but there are also good arguments against that view. See comments on the main points above.

- page 22: This conclusion is not supported by the data. Consider the comments above.

We discuss this, but there are also good arguments for our view. See comments on the main points above.

- page 23: Same comment as section 5.2. Remove this section.

Deleted.

- page 24: As you describe later, Sava Ft. and Periadriatic F. accommodate part of this deformation. In the way that it is written, seems like all the motion were accommodated only by DFS. Could even some other not studied faults take part in this motion, also?

Very true! We changed the figure and made this more clear in the revised version.

- page 24: I do not understand what is the objective of this discussion. Trying to find some correlation between recurrence interval and slip rate? Please, rewrite it.

The objective is to show that long recurrence intervals and low slip rates must prevail in the DFS. This is implied by our study and other data, and it explains the difficulties in trenching these faults.

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