Solid Earth Discussions

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Reply on RC1

Christoph Grützner et al.

Author comment on "Holocene surface-rupturing earthquakes on the Dinaric Fault System, western Slovenia" by Christoph Grützner et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-7-AC1, 2021

Answer to reviewer comments

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 will add a figure highlighting the scarp in the DEM. 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 will also add 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.

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. 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 will therefore discuss this possibility in the revised manuscript in detail and leave it to the reader to draw the conclusions. Abstract, discussion, and conclusions will be 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 and modify our already included alternative explanation: "We note that unit U6 is not necessarily a sedimentary body, but the distinct pale appearance may be due to post-depositional modification by increased water content, or it may result from compaction due to the occasional use of the track by the farmers."

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 will add another figure with geological maps of the study area(s), which will have the same extent as Fig. 2 including the two insets. 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. 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 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 will expand our documentation and discussion on the absence of other SSDSs to make this point clearer, and we will 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 will improve 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. We will point this out in the revised version.

All these 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 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 will change 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 will remove this part from the manuscript. We will also happily incorporate all the other comments raised in the annotated PDF, which mainly refer to clarifications, more precise wording, and some changes to the figures.

Best regards on behalf of all authors
Christoph Grützner