Solid Earth Discussions

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Comment on se-2021-7

Anonymous Referee #1

Referee 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-RC1, 2021

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. 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.

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. 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.

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.

Specific comments are in the attached file

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2021-7/se-2021-7-RC1-supplement.pdf