

Solid Earth Discuss., editor comment EC5  
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## Comment on se-2021-73

Frank Pazzaglia (Editor)

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Editor comment on "Late Quaternary faulting in southern Matese (central Italy): implications for earthquake potential in the southern Apennines" by Paolo Boncio et al., Solid Earth Discuss., <https://doi.org/10.5194/se-2021-73-EC5>, 2021

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I have now received and read the detailed response to reviewer comments by Boncio et al. For the SE manuscript "Late Quaternary faulting in southern Matese (central Italy): implications for earthquake potential in the southern Apennines". I am pleased to recommend that Boncio proceed with all of the suggested revisions and post a revised manuscript. Assuming that manuscript will incorporate all of the changes indicated in the reviewer response letters, and it also follows my enumerated suggestions below, I would judge the manuscript suitable to be accepted for publication.

First, I would like to thank and commend the authors and the three external reviewers for a professional, constructive discussion that will result in a much improved manuscript, with high scientific impact. I judge the few examples where the authors disagree with reviewer comments to mostly arise from honest misunderstandings, or in limited cases, from vague expression that will be resolved in the revised manuscript.

Second, I agree with and echo the author's commitment to expanding the scholarship of this manuscript, particularly in the discussion section, so that the work can be placed in the broader context of what is already known, and how that knowledge may need to be modified given the novel discoveries and data.

Third, perhaps the most important addition and improvement to this manuscript will be in the Supplementary Material. The authors have indicated that they will add three new figures and improve the quality of the maps and cross sections. This is really important. May I ask why figures D and E, provided to Reviewer 1, will also not be added? Figures D and E are excellent and should be part of the Supplemental Materials in my opinion. All supplemental figures need detailed explanatory captions. I encourage the authors to clearly show the locations of all geochronologic data, including the ages and their associated uncertainties, right on the maps and cross sections. In addition, I think that it is advisable to include all of the raw data and lab-generated tables/data for all of the geochronological samples. For example, radiocarbon analyses from Beta provide full reports, in addition to the final age and uncertainty. Similarly, the lab methodologies, and standards for the tephras should be collated and presented; for example these methodologies would directly address the comments of reviewer 1 for line 157 in the manuscript. I recognize that this is a tedious request, but data management is an increasingly important component of our science that is critical for reproducibility and

credibility. The supplementary materials data and tables are already excellent. Adding the five new figures and lab methodologies and full reports will make them even better.

Lastly, here are some additional point by point impressions that I had reading through the reviewer comments and author responses that may be of use during the revision process.

The revised introduction material following line 51 is excellent; however it still lacks a clear rationale for why this particular fault system was investigated, other than the fact that it is always good to characterize under-studied regions in order to better constrain seismic hazards. But if there is something special or unique about this particular fault system, here is the opportunity to state it, which raises the impact of the study.

(New subsection 5.2) The perceived disconnect in inferred slip rates derived from mountain front morphology, which integrates fault slip over long time spans, and the Late Pleistocene-Holocene fault scarps that integrate slip over short time spans may speak to an earthquake clustering process in space and time. The manuscript is mostly silent on this topic but I think that it has potential for discussion.

(Lines 334-338). The overall properties of the paleosol and the numeric age indicate weak pedogenesis over a short period of time. But soils can be younger than the deposits they cap, when the top of the deposit undergoes erosion. So, there is no preservation of sediment from 508 to 6 ka, but in fact, there could easily have been erosion into a deposit that is 508 ka, then quiescence and subsequent pedogenesis. The relationship between the soil, its much older underlying parent material, and likely unsteady surface erosion of that deposit should be clarified.

(Lines 357-359). In summary, I think that the approach you want to take is simply the most parsimonious descriptions and interpretations. There are many possible scenarios, but few of them are falsifiable. Therefore you appeal to the most parsimonious interpretation, and let others collect the data to falsify it.