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Comment on se-2021-73

Francesco Pavano (Referee)

Referee 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-RC1, 2021

I concluded to read the manuscript submitted to Solid Earth by Boncio et al., entitled "Late Quaternary faulting in southern Matese (central Italy): implications for earthquake potential in the southern Apennines". I appreciated the opportunity to read this manuscript.

Overview

Moved by the tragic events occurred recently in Central Italy, and pointing out the implications in the seismic hazard assessments of the study area, the authors try to address the issue of the definition of the seismic potential of the southern Matese area (central Italy), where slowly-slipping faults, with long return periods of > M 6.5 earthquakes, occur. The authors address these topics by combining geological, paleo-seismological and geomorphological approaches and also considering the available data about both the historical seismicity record of the area and the up to 30m-deep drill holes (Plate 1). The work benefit of a supplementary 1:20,000 scale geological map, equipped with several geological cross-sections, where the geological and some morphological information are presented.

Several age determinations have been performed (e.g., $^{40}Ar/^{39}Ar$, ^{14}C) by analyzing several samples collected at specific horizons (e.g., tephra, paleosols), in order to reconstruct a history of events (e.g., deposition, weathering, erosion), inferring potential surface faulting episodes suggestive of the occurrence of past earthquakes. These data are used by the authors to characterize the seismic potential of the normal faults of the Southern Matese Fault system (APMF and GF) that control the southern slope of the Matese Mts., trying to associate to them some historical, sometime still poorly constrained, earthquakes (e.g., 847 CE).

General comments

I found the manuscript suitable for publication on Solid Earth, and the work has great potential in contributing in the definition of the seismic potential of a still poorly-studied

fault system portion of the NE-SW-stretching southern Apennines, with great repercussion on the seismic hazard assessment of the area. In this regard, the provided ages determinations represent useful anchor points to reconstruct the morphotectonic evolution of the study area and to attempt to associate past strong earthquakes to this fault system.

Anyway, as it is, the manuscript needs a general reorganization and a more convincing way to present, interpret and discuss the collected data.

I suggest to the authors to emphasize the main goal of the work in the introduction;

Especially through the sections 4 and 5, it is not clear what are the new and the already available data/information, and sometimes there are some not-univocal interpretations of the presented data;

The discussion section strongly would benefit of a revision, mainly focused i) on the interpretation of data in the light of the initial hypotheses/aims and ii) on arguing any conclusive statement with the strong support of any of the obtained ages data, field evidences and available/previous information.

The data presented in some figures (e.g., Fig. 11b-c) are barely discussed in the main text, and they misleadingly could appear of secondary relevance.

Given the potential of this work, the suitability for publication on SE, and given the amount of work requested for the revision, I suggest that for the final pubblication, the manuscript should be reconsidered after major revisions.

Below are some general comments for single sections, then line by line comments follow.

General comments by sections:

Section 1: in this introduction section I suggest to the authors to give more emphasis to the issues that they want to address in the study, remarking its main goal.

Section 2: except for few comments on both the main text and the related figures (e.g., Fig. 1) this section is well written.

Section 3: Sub-section 3.1 actually could serve as a general introductive paragraph of this section rather than a description of the carried-out field work. Except for additional few comments and suggestions, the rest of this section is well written.

Section 4 and 5: Through the text, it seems that the description of data is sometimes mixed with their interpretation. Also, somewhere it is not clear in the description of the

different fault sections what data are just descriptive, from previous studies, and what are new. I think that the authors could clearly distinguish previous information (moving them in a more general introductive description) from their new data.

Furthermore, I think that the classical structure in "Results" and "Discussion" could help in this case.

Section 6: This section is expected to be the strongest and the main data-supported section of the manuscript, giving space to data interpretation, and their implications, to sustain any hypotheses presented in the introduction of the manuscript. Any hypothesis or statement in this section are expected to be convincingly supported and well constrained by systematically recalling the study's results. Actually, as it is now, this paragraph appears to be weak in this regard and somewhere the discussion sounds as it was randomly or weakly argued in the light of the new data. I think that the produced data, presented through the previous paragraphs, are not appropriately used to strengthen the statements done, too quickly sometimes, in this Section 6. Somewhere through the text (e.g., at the beginning of paragraph 6.1 or paragraph 6.2), the discussion sounds like out of place, fragmented and/or poorly convincing.

Section 7: some revision is recommended in the light of the comments and suggestions provided for the previous sections.

Comments line by line:

Line 25-26: CE 1293; CE 1349; CE 847

Line 52: Refer to Fig. 2 for the APMF and GF.

Line 78: This fault system is not shown in Fig. 1. Is this system partially reactivated as a transfer fault, now forming the Presenzano-Ailano transfer?

At least the Garigliano Graben should be labelled somehow in Fig. 1, since it is discussed in Section 6.

Line 104-110: somewhere cite Fig. 2 for location of the fault sections.

Line 120-121: cite Fig. 1 for earthquakes' locations.

Line 144-152: This paragraph sounds like a general, introductive text rather than a description of field geology methods' explanation. A brief description of how locations,

lengths and depths of the trenches have been chosen could be added to this section.

Line 157: I think that the reader could benefit of some more detailed information here about the adopted approaches. For example, why the glass fragmentation would be good to analyze? Why it was important to sieve clasts at 1 phi interval?

An eventual, detailed and more technical description of the dating techniques (additional to the paleosol analysis) could be provided as supporting information or supplementary data.

Line 161: The pre-treatment (e.g., washing, sieving the sediments, sanidine phenocrysts extraction) was performed at the University of Wisconsin-Madison?

Line 209-210: cite Figure(s) where these locations are reported.

Line 231-233: It could be useful to add any more detailed information on the sedimentary facies, structures and textures of Unit 3 and 4 in this paragraph 4.1.

Line 244: Figure 5 show very beautiful and net rejuvenated fault scarp. In addition to the plots, is there any picture showing some kinematic indicators of the striated fault planes?

Line 247: "south of Fig. 5c" is clearer than "S of Fig. 5c".

Line 247: cite the Figure that shows the location of Criscia.

Line 254-256: any references about this statement?

Line 259-260: It is difficult to see Early Pleistocene slope breccia deposits in the footwall of the San Potito Fault in Section 2 of Fig. 4. Do you refer to Profile 12 in Figure 7? A figure showing these data needs to be cited.

Line 261: Refer to section C-C' of Plate 1, instead of "section C".

Line 264: cite Fig. 2a for the location of San Potito Sannitico.

Line 271 and elsewhere: LiDAR instead of LiDaR

Line 274: Show M. Olnito in Fig. 4 and cite the Fig. 4 in the text for location.

Line 274-275: is there any picture of the faulted valley? What is the condition of the fault-related knickpoint? What the offset of the valley bottom? Is this 3m, like the cumulated fault scarp? These are additional, important geomorphological elements. As described in the text and as showed in Fig. 4, it seems that the small valley is incised on the colluvial deposits sd2. Since the LGM, the fault-related knickpoint should have been moved upstream of a distance commensurate mainly with the drainage discharge, the channel slope, the uplift rate and the rock (sd2) erodibility; thus, the occurrence of a knickpoint at/close the fault trace could mean that it did not move enough upstream and, thus, potentially it could be an indication of a relatively recent surface faulting event. I do not ask the authors to perform a new drainage system investigation, but just to show, if any, some picture/scheme of this geomorphic marker and to describe/acknowledge in the text this eventual occurrence.

Line 275: it is better to avoid the use of the term "footwall" associated to a fault scarp.

Line 281: explain how a post LGM age has been inferred for the 2.5 m-high small fault scarp.

Line 284: probably "(Fig.s 4 and 5d)". Note that in the text, the reference to Fig. 5d appears after the reference to Fig. 6. The citation order of figures in the text would need a revision.

Line 342: unrealistic: this point could benefit of a more detailed discussion, e.g., simply remaking that there is no record of that event in the instrumental seismicity data set.

Line 345: "a fault zone of F5" or "a splay of F5"? We already are in the Fault Zone 2.

Line 345-346: A description of sedimentary facies, structures and texture of Unit 6 lacks. This is particularly needed if it indicates that Unit 6 derives from the erosion of Unit 4. It is not clear how the geometry of Unit 6 (as shown in Fig. 9) would suggest that Unit 6 sedimented after a period of erosion of both Units 4, 5 and 4b (fault zone) but, at the same time, Unit 6 formed at expenses of only Unit 4.

In addition, Unit 4b has been not described before.

Line 347: As presented in the interpreted log of Fig. 9, why Unit 6 could not be alternatively interpreted as the filling of a small channel, incised along the fault's strike? Growing stratification is more distinctive of syn-tectonic sedimentation than a triangular-shaped package of deposits.

Furthermore, what about the prolongation of the NE-dipping normal fault? It should be downthrown by F5. Any evidence of that? Could that be represented somehow, or partially, by the contact between Unit 6 and Unit 4 in the hanging wall of F5?

Line 347-349: this statement is poorly constrained and sounds like a speculation.

Line 349: Why "western splay of F5"? It seems to be sealed by Unit 6. Perhaps the authors mean the eastern splay.

Line 351: it is not clear why the wedge of Unit 7 thickens in the footwall of F1. I suspect that the authors mean F5, instead of F1.

Line 354: "a period of erosion" is to be explained and discussed in more detail. For example, why did the erosion actually not impact on the fault scarp's steepness and shape (as it is drawn in Fig. 9)?

Line 357-359: I think that without a reference layer within Unit 3 of Fig. 9, it is extremely speculative to estimate the vertical displacement.

Furthermore, the offset of Unit 3 by the NE-dipping normal fault (hereafter **NEDNF** just for simplification) seems to be an important offset (~ 1 m). The **NEDNF** does not offset significantly the fault strands of F4 and their upward prolongation across the **NEDNF** is drastically reduced and difficult to trace. This would mean that F4 is younger than the **NEDNF**, which anyway could represent an important structure, potentially related to some past relevant seismic event, pre-dating F4 (and F5). The authors do not discuss about the role and the deformation history of the **NEDNF**, thus it is not clear if, for example, the portions of the F3 and F4, depicted in the hanging wall of the **NEDNF**, have been actually offset from the F5 and F4, respectively, occurring in the footwall of **NEDNF**.

In addition, it is difficult to understand why Unit 2 disappears in the hanging wall of the **NEDNF**. An estimation of the displacements along any single fault splay would need a revision in the light of all these elements of uncertainties.

Line 367: indicate the sub-figure where the location of sample C1_D-E is shown. The same for sample C8_D-W in Line 368.

Line 371: How to explain the steps that both Units 4 and 5 form? Could this be the evidence of a splay of F1? Is there evidence of a sedimentary growing structure within Unit 5? Why to exclude the occurrence of Unit 5 deposits at the footwall of F1?

Line 383-385: the paragraph 6.1 starts in a strange way, with just a quick statement saying that some data are shown in Fig. 11a and 11b. Then the discussion passes to describe data of Fig. 12. In addition, it is confusing to understand if the authors want to start the discussion about the GF, the APMF or the general SMF.

I think that the first three lines of this paragraph could be erased, or moved (?), since they are not a strong way to open a discussion section.

Furthermore, Fig. 12 is cited before Fig. 11c. In this regard, I think that this latter could be moved below as a new Fig. 13.

Line 383-395: this paragraph looks fragmented, more descriptive, just rigidly listing a series of data. The strength of the discussion reached in the previous paragraphs seems diminishing here, yet this is one of the conclusive sections of the paper. I suggest to move this description somewhere in the previous sections 4 or 5.

Line 387-390: it is not clear how the fault strike's trend has been evaluated. Actually, the data collection is strongly influenced by different factors, such as the outcrops availability and accessibility and the distribution of the measuring points. Thus, I suggest to calculate such statistics by using a population of several smaller segments, of equal length, obtained by splitting any fault segment.

Line 391: This statement is not clear. Simply by reading the plot of Fig. 12c it is difficult to recognize clusters of vectors plunging at 200-240° (SW) and 110-180° (SE). An important cluster of data falls within the range of 180-220°. How do the prevailing plunging of slip vectors have been measured?

Line 395-403: these statements are too speculative here, since they are not supported by an appropriate kinematic analysis of deformation history carried out along the fault planes. For example, such a kinematic evolution would commonly result in the overlapping of different families of kinematic indicators on the striated fault planes, useful elements to discriminate the relative age of the tectonic deformation stages underwent by the studied region.

Furthermore, why a fault plane showing SE-plunging slip vectors, associated to early Pleistocene stage of deformation, should be considered as belonging to an active (e.g., late Pleistocene-Holocene) fault segment (as shown in Fig. 11b), which accommodate the NE-SW-trending extension? Why could they not be considered as exhumed fault planes? Otherwise, if this is not the case, this means that the occurrence of SE-plunging slip vectors needs another explanation that would deserve to be discussed in the text.

In addition, provided that the slip vectors data would enable to evaluate the real total

offsets along the faults, rather than the apparent ones calculated by considering just the vertical component of movements, so if the prevailing slip vector on a fault plane would allow to associate an age to the fault scarp, this would have an overall wide impact on the throw rates estimations.

All of these uncertainties above could be addressed in the text and discussed/acknowledged if such a statement (i.e., Line 395-403) is done.

But again, detailed structural analyses can help to support the statements done in the second half of the paragraph 6.1 (I recognize that this is not the goal of the study).

Line 405-413: This paragraph actually does not address a discussion concerning the throw rates. Why not discuss in detail the data presented in the inset plot of Fig. 11b?

Line 407: explain better how the bedrock indicate that the GF has a long tectonic history. As it is now, this sentence alone is a little confusing. I guess that the authors are referring to the juxtaposition of bedrock's limestones with different ages.

Line 412-413: why in any case the throw rate is "sufficient to determine clear morphotectonic markers of young fault activity"? What data do support this statement? This implies that the slope erosion rate is negligible with respect to the throw rate. The authors could usefully argue here the competing along-slope erosion rate and the relief-building forces' rates (i.e., tectonic).

Line 425: note that the length of the APMF reported in Table 4 is 18.5 km.

Line 428: actually, for consistency, in table 4 the APMF + GF reach a total length of 30 km, not 19.5 km.

Line 432-433: This statement could be argued more in detail recalling the age determination results.

Line 433-435: as for the colluvial wedge of Unit 5 in Fig. 10, actually its occurrence at the top of the footwall of F1 cannot be definitively excluded, since it was not dug. This could be acknowledged.

Line 435-436: the comparable age of the CWs does not necessarily support the idea of the occurrence of an earthquake before this depositional event. It could be suggestive of climate conditions replacing those promoting both the removal of sediments from slopes and their downstream delivery by the drainage system. This could be discussed more in

detail.

Line 438: based on what is stated two lines below about the distribution of damages, an activation of the APMF and/or GF during the 1349 event can be excluded. Why here it "cannot be excluded"?

Line 446-447: Why assuming a reactivation of APMF + GF if the historical seismic data already do not fit this hypothesis? A more data-supported way to argue this point could be necessary.

Line 449: what does "high-energy moment" mean during a period of limited removal and delivery of sediments from slope?

Line 450-451: Why during a period of erosion, occurred before the deposition of Unit 6 (spanning in time between 346 and 1445-1635 CE), the fault scarp maintains its steep shape?

Line 457: the data from Bottari et al. (2020) should be shown in Fig. 11a.

Line 458-459: this sounds like a quick statement, as if it was the conclusive sentence of a detailed discussion occurred above. Actually, this discussion lacks and it is better if the hypothesis of the plausible connection between the "ages of San Potito and Sant'Angelo d'Alife sites" and the 847 CE earthquake was argued accurately and in a more convincing way.

Line 461-465: although already existing, it could be useful to introduce this issue in the introduction section of the manuscript with more emphasis, since this is an important motive of the study, to be addressed and figure out in the discussion section. But, again, this latter would be better if written in a more focused and more convincing way.

Line 468: colluvial.

Line 469: Just for consistency, what is the length of the SMF? 30 km (like in Table 4) or 29.5 km?

Line 470: actually, the along-strike slip rate varies also for reverse and strike-slip faults; it

is not diagnostic of a normal fault.

Line 471: as shown in the inset of Fig. 11b, the thrown rate does not exceed 0.6 mm/yr.

The average value could be also reported here.

Line 473-474: no mention about these slip episodes appears in the discussion section of paragraph 6.3. Actually, this would be very interesting to argue exhaustively.

Line 475-481: This final statement in the Conclusions section sounds as if the paper is a little inconclusive with respect to the initial problem that the authors want to figure out. All the historical seismic events seem to be compatible with both the two scenarios of i) ruptures along the APMF and GF, separately and ii) ruptures along the APMF + GF as a single fault segment.

FIGURES:

Figure 1: In the legend, part of the caption of the white unit (Marine and continental deposits) is missed. Due to the transparency of the layers, the "Marine and continental deposits" layer should be shown in light grey rather than in white.

In addition, in the legend, indicate that the dashes and the triangles of normal faults and thrusts represent the downthrown and the uplifted block of the faults, respectively. In the legend or in the figure caption, indicate what the numbers within or near the white squares represent. In the caption, briefly describe the plot in the inset.

Insert coordinates and north arrow in the main map.

In the location map circles indicating historical earthquakes with M ≥ 6.0 (CPTI15) are too small and difficult to see.

The year of the low magnitude earthquake southwest of Isernia is missed.

Figure 2: In Fig. 2b a thin dotted line tracing the fault line could be helpful. Furthermore, try to avoid red drawings in a green background for colorblind readers. If possible, changes red/green colors also in Fig. 11a and 11c.

Figure 3: Another schematic section (crudely parallel to the mountain front) showing the

lateral relations between the different Units could be useful.

A more exhaustive description of what is shown in this figure could be added in the figure caption.

Figure 4: Change "**ve**" in "**vc**" in the main map.

In the figure caption cite the refence(s) for the Fault Activity age constraints. Here, these latter are presented as they were available data. The ages attributions of fault activity would deserve a clear, dedicated paragraph. A clear explanation of the method used to perform this attribution actually lacks or this information is sprinkled through the different (sub-)sections of the manuscript. In addition, note that when the Fig. 4 is cited for the first time in the text, the ages determination data are nor presented and discussed exhaustively yet.

Figure 6: I suggest to use different letters (a to d) for the different panel of the figure. In addition, in the inset picture of Profile 7, it is better to label the layers above the buried paleosol as "sediments deposited during the Last Glacial Maximum (LGM)".

Figure 7: What is the nature of some additional topographic drops shown along the following profiles:

- Profile 4 at distance of ~ 55 m
- Profile 5 at distances of ~ 310 m and ~ 340 m
- Profile 9 at distances of ~ 260 m and ~ 310 m
- Profile 13 at distances of ~ 100 m, ~ 110 m and ~ 130 m
- Profile 14 at distances of ~ 85 m and ~ 155 m
- Profile 15 at distance of ~ 145 m, ~ 165 m and ~ 180 m

If anthropic in nature, it needs to be indicated in the profile (like for Profile 1 and 10) otherwise, if natural, they could be considered in the computing of the total surface faulting and the estimations of throw rates.

For the faults cutting the topography, I suggest to use the same colors used in Figure 4.

Figures 8, 9 and 10: The authors could try to use the same colors for the corresponding Units between the different interpreted logs. For example, the "Altered trachytic tuff" represents Unit 2 in light yellow in Fig. 8, but it is the Unit 3 in light brown in Fig. 9. I recommend a revision and reorganization of both colors and units' attribution in order to avoid confusion.

Figure 9: It is not clear how Unit 2 continues upward. It is truncated by a NE-dipping normal fault but does not appear in the downthrown block between Unit 1 and Unit 3.

Does Unit 2 correspond to U2 of Fig. 3, Fig. 4 and Plate S1? This question is just for consistency reasons and to avoid any confusion.

Are Units 5, 6 and 7 buried by late Holocene colluvial units so that they do not outcrop in the study area? If it is so, even though they are not shown in the map of Fig. 4, they could be still included in the morpho-stratigraphic sketch of Fig. 3.

Figure 10: the use of F1 and F2 for the Raviscanina Fault section could be confused with those used in Fig. 8 for the San Potito Fault section. Why not using F6 and F7?

Figure 11: the information shown in Fig. 11a actually it is better if presented before, as a figure for introducing the seismotectonic setting of the study area, in the paragraph 2.3. Probably, this figure could be move above as Fig. 1b.

In the map of Fig. 11b the different colors actually would represent the "Fault activity age constraints".

In the inset plot of Fig. 11b, what do the dark blue triangles represent? They are not described in the legend.

Figure 12: In Figure 12c the data need to be distinguished by symbol's color or shape between those collected for the present study and those from Boncio et al. (2016).

TABLES:

Table 3: why do Pr.s 6 and 8 not appear in the table?

PLATE 1:

It is useful to show the 1:20,000 scale of the Geological Map.

The measure stations for structural data should be reported. Alternatively, a map with the location of the measure stations for structural data could be useful as an additional supplementary material.

In the Legend, use "Fluvial riser's edge" instead of "Fluvial raiser".

Why the Fluvial riser are confined to the downthrown block of GF? Are they evidences of fluvial re-incision just where soft rocks and clastic deposits outcrop? Any evidence of fluvial re-entrenchment on the bedrock outcropping at the uplifted block of the GF? These important geomorphic features would deserve space in the main text of the manuscript.

The labels in the Cross Sections are too small if compared to the font sizes of other elements of the Map. Anyway, in general, revise the font sizes used in the map.

Furthermore, sometimes the axes' labels are missed (e.g., section C-C'). The label of the x axis should be "Distance (m)" and the label for the y axis "Elevation (m a.s.l.)".

How does the manuscript benefit from the cross-sections other than C-C'? They are not described/referred in the main text.

Why the ~ 350m-deep drill hole "S93" does not occur in the drill holes' panel? Revise this and other potential missed data.

Some trends of the boundary between different geological units would benefit of some revision. This is the case, for example, of i) the contact between K Carbonates and M Siciliclastic deposits at Auduni, where the Carbonates seem to overlap the Siliciclastic deposits, and ii) the contact between K and J Carbonates units at Mt. Erbano, mostly traced pretty straight, suggesting a nearly vertical trend of the stratigraphic contact.

I want to assure the authors that all my comments above are written in the spirit of providing a helpful feedback from the perspective of a skeptical reader. I hope that they will find them useful.

Best wishes

Francesco Pavano