Response to Reviewer 1 (Francesco Pavano)

Black: original comments by Reviewer

Red: response by Authors

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, paleoseismological and geomorphological approaches and also considering the available data about both the historical seismicity record of the area and the up to 30 m-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., 40Ar/39Ar, 14C) by analysing 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.

We thank the Reviewer for the detailed review and for constructive comments. In the following, the point by point response to the comments:

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.

1) Good suggestion. We will remark the main goal in the Introduction, by adding the following from line 51:

This paper focuses on the normal faults cropping out along the southern slopes of the Matese Mts., named Southern Matese Fault system (Ailano – Piedimonte Matese and Gioia Sannitica faults; Fig. 2). The main goal is to determine if the fault system must be considered active, possibly seismogenic, and with what seismogenic potential. We seek to achieve the expected result by performing an earthquake geology study aimed at mapping the fault traces in detail, collecting field evidence of recent activity, particularly Late Pleistocene – Holocene, looking for evidence of earthquake-related surface faulting episodes, and combining all the evidence in a consistent seismotectonc frame for estimating the likely earthquake potential. We start studying in detail the geology of the Gioia Sannitica normal fault, which is the less constrained fault of the system. The results are described in Section 4, and a detailed, 1:20,000-scale geologic map of the Gioia Sannitica normal fault is attached as supplementary material (Plate S1). Fault scarp heights are recognized, carefully selected to avoid scarps of non-tectonic origin, measured using high-resolution topography, and used to derive fault throw rates. In sub-section 4.2, we describe evidence of Holocene surface faulting, discovered both on the Gioia Sannitica and Ailano – Piedimonte Matese faults. For the first time, we show clear evidence of late Quaternary and Holocene faulting, thanks to detailed field analyses of fault zones in dated (14C and 40Ar/39Ar) Quaternary sediments. In Section 5 we integrate our new data with data deriving from previous geological works on the Ailano – Piedimonte Matese fault (Boncio et al., 2016). Our results are then discussed in the light of a recent geomorphology and Quaternary stratigraphy study of the Southern Matese mountain front (Valente et al., 2019). Finally, the new and pre-existing data are discussed together in terms of present activity and overall seismogenic potential of the Southern Matese Fault system.

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.

2) We prefer to leave sub-section 3.1, which will be integrated with methodological details on fault scarp selection and measurements. In particular, we will specify that sites have been selected in order to avoid erosional exhumation processes.

3.1 Field mapping and fault scarp measurements

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.

3) Suggestion accepted. The revised paper will be organized in Results and Discussion:

Note that all the information provided in former sections 4 and 5 are new and original (i.e., derived from our field survey). There are a few exceptions, with information deriving from the literature and included in sections 4 and 5 because considered relevant for the described subject. When the information is from the literature, the original source is always acknowledged. If there are not any references, it means that this is original work, as usually done in scientific contributions.

Anyway, it is evident that the present organization is not satisfactory, not sufficiently clear. So, we will be careful in removing from the Results section all the unnecessary data deriving from the literature, and better organize the structure of the paper as follow:

4. Results

4.1 Geology of the Gioia Sannitica normal fault from field mapping

4.1.1 Geomorphology and stratigraphy of the southern Matese piedmont along the GF

4.1.2 Geometry, kinematics and fault scarp morphology

San Potito fault section

Castello di Gioia fault section

4.2 Late Pleistocene – Holocene surface faulting

4.2.1 The San Potito site on the Gioia Sannitica Fault

Tectonic interpretation

4.2.2 The Sant'Angelo d'Alife trench site on the Ailano – Piedimonte Matese Fault

Tectonic interpretation

5 Discussion

5.1 Architecture and kinematics of GF and SMF system

5.2 Activity of SMF system, throw rates and throw rate variability

5.3 Seismogenic potential

6 Conclusions

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.

4) Suggestion accepted. We are confident that the new organization in Results and Discussion will improve the paper. The Discussion will be better developed, and more convincing, we hope.

Comments line by line:

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

5) In general, in seismologic or seismotectonic papers, when dealing with historical earthquakes, "CE" (or BCE) is used only for ancient events, in general before 1000 A.D.

Anyway, considering that all the cited events are CE, we will remove "CE" from all the earthquake dates. This is consistent with the standard of the historical earthquake catalogues we used.

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

6) Accepted. We will modify the manuscript accordingly.

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.

7)Accepted. We will add “Garigliano graben” in Fig. 1.

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

8) Accepted. We will modify the manuscript accordingly.

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

9) Accepted. We will modify the manuscript accordingly.

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.

10) See reply n. 2. The location and size of the Sant’Angelo d’Alife trench site will be added. Please note that the San Potito site is not a trench, it is a road cut.

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?

11) The tephrostratigraphic approach on distal volcaniclastic deposits embedded in Middle-Early Late Pleistocene successions, which are often deeply altered, is generally not so straightforward. As a matter of fact, you generally are able to distinguish in the field the volcaniclastic nature of the deposits, but the following analytical phases are complicated by the deep alteration of the pristine glass fraction characterizing the layer. Obtaining chemical data on the latter, both in terms of major and trace elements, however, would make it possible an attribution to a possible source and, even, to a specific eruptive event. This is the reason why the samples are pretreated with several washing phases and sieved at different grain-sizes, in the hope to find, at least in the finer fraction, a little amount of fresh glass. Unfortunately, this was not the case of our samples, for which only a preliminary characterization of lithological and crystal component could be achieved. Anyway, we are available to better explain the rationale of the procedure, as the reviewer asks.

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.

12) We think this is not necessary. Used 14C and 40Ar/39Ar methodologies are rather well-known in the geologic community. The cited references to basic papers and/or to Lab. web sites will address readers interested in technical details.

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

13) At the University of Naples “Federico II”. This will be specified.

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

14) Accepted. We will modify the manuscript accordingly.

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.

15) Accepted. We will modify the manuscript accordingly (consistent with the description in Plate S1).

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?

16) Slickenlines are reported as slip vectors in the stereonets.

A photo with a detail of the fault plane and slickenlines will be added in Fig. 6.

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

17) Accepted. We will modify the manuscript accordingly.

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

18) Accepted (Fig. 4). The locality Criscia will be removed from the text (this is not necessary).

Line 254-256: any references about this statement?

19) This is an original observation from field evidence. Please, see Plate S1. We will explain in the text more clearly, referring to Plate S1.

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.

20) Accepted. This is the sd1 cropping out close to the trace of cross section 2 in Fig. 4, in the footwall of GF. This will be specified in the text. The outcrop of sd1 will be made more visible in Fig. 4.

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

21) Accepted. We will modify the manuscript accordingly.

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

22) Accepted. We will modify the manuscript accordingly.

Line 271 and elsewhere: LiDAR instead of LiDaR

23) Accepted. We will modify the manuscript accordingly.

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

24) Accepted. We will modify the manuscript accordingly.

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.

25) We agree that drainage analysis aimed at identifying knickpoints is a very useful tool. In this paper we focused mostly on fault scarps (identified and measured on high-resolution topography) because we think that:

- fault scarps, if measured in proper sites (away from erosional exhumation processes), can provide good estimates of throw rates;

- the fault scarp illustrated in Fig. 6 is a good example of post-LGM fault scarp. We used this area for calibrating our observations. The fault is entirely in Unit sd2 (i.e, no different lithology across the scarp). It is clearly a fault scarp, as it is just along the fault. The bedrock fault planes crop out in the eastern and western (see stereonet) sides of the scarp. A photo of the fault plane will be added in the revised version of the manuscript. Part of the scarp (western and eastern sides) is modified by anthropogenic activity, and we carefully avoided those sites. Because the scarp is entirely in the sd2 deposits, and considering that the dip of the post-LGM deposits (dated) is nearly coincident with the dip of the topographic surface (photo in profile 7), a non-tectonic origin, by e.g. differential erosion, stratigraphic features or other non-tectonic processes, is not reasonable. The fault offsets the topographic surface and the underlying stratigraphy, and this displacement is post-LGM. Therefore, the fault scarp is a good feature to estimate post-LGM throw and throw rate. Please note that the valley in the central part of the figure, entirely within the sd2 unit, is more incised upslope the scarp (i.e., in the footwall of the fault). This is consistent with footwall uplift due to faulting.

Concerning the small scarps in profile 4 (total throw of ca. 4 m), we totally agree with you: *"… 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 …"*.

The scarps are visible in the field, but unfortunately we could not capture a good photo showing the entire scarps, due to logistic difficulties and vegetation. But, the scarps are clearly visible on LIDAR DEM.

Figure A (below) shows: a shaded relief of the LIDAR DEM without geology, the same with geology and a long profile across the Gioia Sannitica fault close to profile 4. In the long profile, the knickpoint (we name this "knick zone") is clear. The knickpoint height (elevation difference between crest and toe) is 3-4 m. By considering the long profile slope and the fault dip, the throw is on the order of 2-3 m. This is less than the cumulated throw measured on profile 4. This is consistent with profile 4, as the knickpoint should have registered younger slip compared to the top of the Holocene alluvium (al unit).

We are still convinced that the throw measured in profile 4 is better for estimating the cumulated throw and throw rate (post Holocene alluvium), also because the knickpoint has no chronologic constraints and because of river erosion and retreat. Moreover, there is some noise in the LIDAR data in the long profile, probably due to dense vegetation (in general the vegetation is denser along rivers compared to the alluvial planes). But, we agree that the data shown in Figure A can help in convincing sceptical readers. Therefore, we decided to add this figure as supplementary material. The graphics will be improved before submitting the revised version.

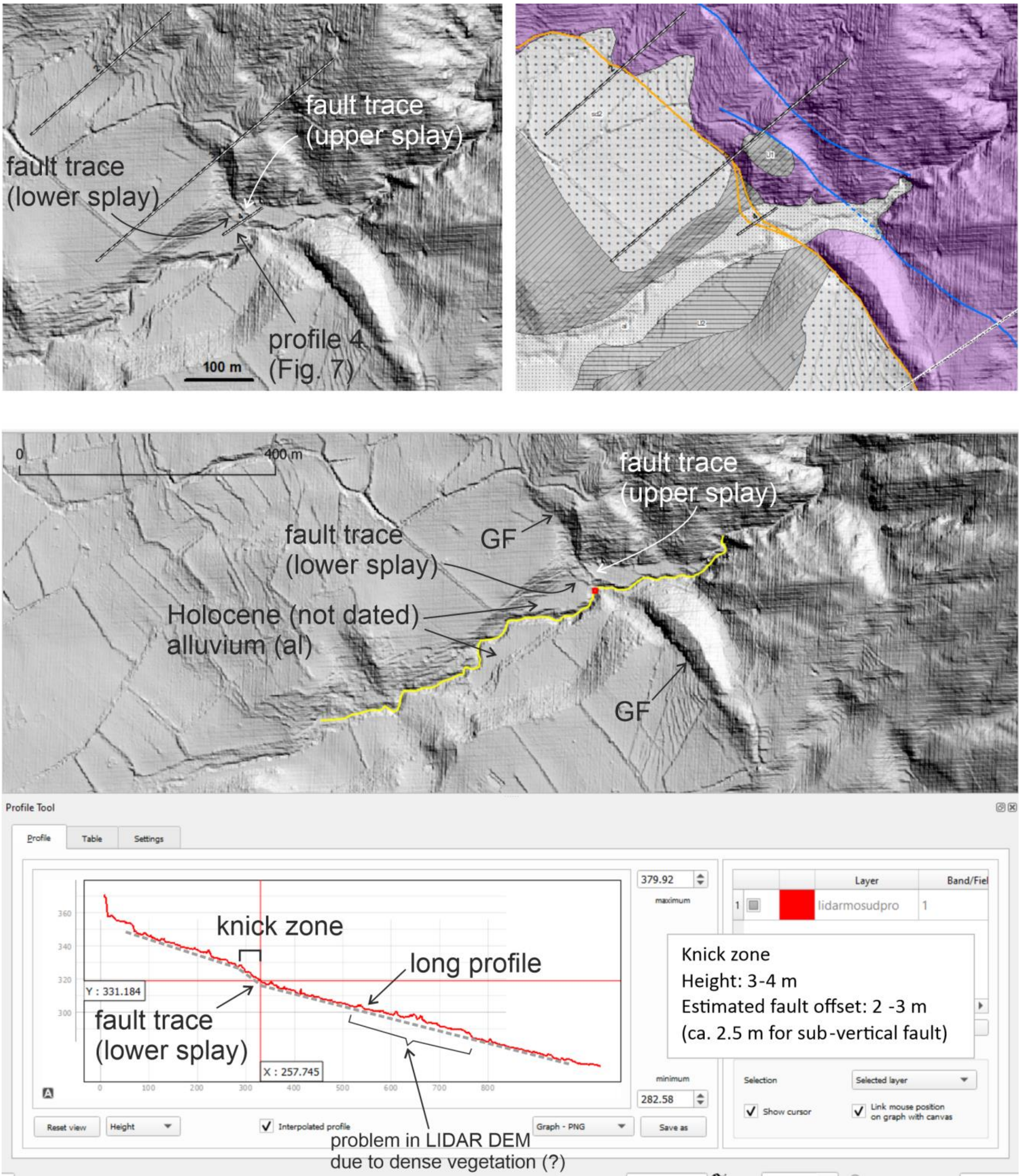


Figure A (the dashed grey line in the long profile has been drawn for highlighting the scarp in the knick zone).

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

26) Accepted. We will change with "upslope".

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

27) This is inferred to be post-LGM because the topographic surfaces upslope and downslope the scarp can be correlated (restored). Keeping in mind the observations from Figure 6 (see response n. 25), it seems reasonable to infer

that the offset of the topographic surface is post-LGM, as for Figure 6. Please note that we acknowledge that this is “inferred”, because we have no firm constraints.

In any case, considering also the comment/response n. 3, we decided to leave in this section only the description of the fault scarps (features, throw …). The constraints and inferences on the derived throw rates, and the obtained values and uncertainties, will be discussed in the new Section 5.2.

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.

28) We will correct.

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.

29) Accepted. We will integrate.

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

30) Splay of F5 is good. We will modify the manuscript accordingly.

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.

31) Units 4b and 6 are, very synthetically to be honest, described in the legend of Fig. 9. But, ok we will add additional description.

Concerning the relations between unit 6 and units 4, 4b and 5:

- the bottom of U 6 truncates the underlying units. I think we agree on that. So erosion (truncation), must be considered;

- the sedimentary facies of U 6 is very similar to that of U 4, but U 4 lies on a paleosol and U 6 does not. Moreover, in the hanging wall of F5, U 4 crops out below U 6. A plausible explanation is that U 4 sourced U 6. Moreover, in U 6 there are elongated clasts with the long axis plunging sub-vertical, which suggests colluvial sedimentation from a nearby source (as for scarp-derived colluvium, within which clasts plunging downslope at the foot of the scarp are typical);

- U 6 pinches out (i.e., zero thickness) at a distance of ~1 m from the fault; it is 30 cm-thick at the contact with fault F5. This indicates thickening towards F5. This suggests a strict relation with the fault;

- please, note that U 6 is faulted. So, you have to consider its original position before the last faulting event;

- The possible explanation is (please, see figure B below):

1) faulting along F5: a fault scarp forms, with a free face exposing U 4 in the footwall; in the hanging wall, there is a sequence formed by U 4 (already previously faulted by N events) and U 5 on top;

2a) erosion started after the faulting event; possibly by water running parallel to the fault scarp and cutting down the underlying stratigraphy for a maximum of about 50 cm; the formation of a coseismic scarp, a depression at the foot of the scarp immediately after faulting, and/or open fissures that are typical during surface faulting, would have favoured linear erosion, aided by the presence of a deep valley to the SE (see Plate S1) that would have driven surficial running water. In this stage, it is likely that part of U 4 in the footwall was removed by erosion.

2b) River incision ceased for some reason, but the degradation of the coseismic scarp continued (U 4 exposed in the free face), sourcing U 6 with colluvium (scarp-derived colluvium) (stage 3);

4) after some time, a second surface faulting event faulted U 6; a new coseismic scarp formed which sourced U 7 (a scarp-derived colluvium; i.e., colluvial wedge sensu Mc Calpin, 2009; see reference below).

We are aware that the interpretation of U 6 is not easy as it must take into account erosion and then sedimentation. The uncertainties in the interpretation are always considered in the present version of the paper (e.g., question mark in Fig. 11c). But we judge our reconstruction (figure B) plausible.

<u>We agree that the overall tectonic interpretation of the trench wall can be improved, and the uncertainties in the interpretation of unit 6 emphasized. For the sake of clarity, we decided to add a sub-section entitled: Tectonic interpretation.</u>

<u>We will add the figure B below as supplementary material.</u>

<u>We are also planning to add a photographic documentation of fault F5 in the supplementary material (see Fig. C below).</u>

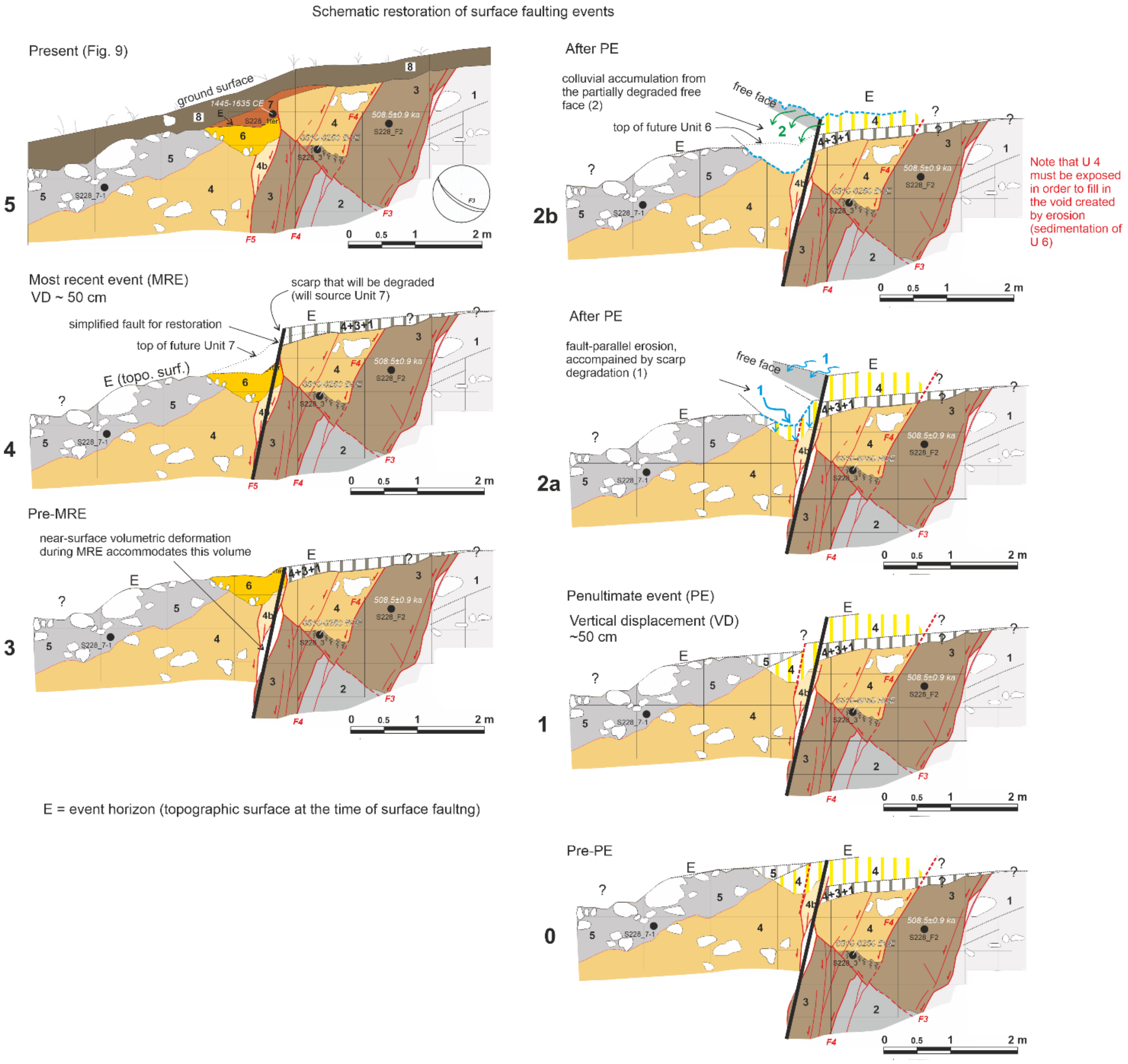


Figure B - Schematic restoration of surface faulting events

Photographic documentation of fault zone 2 (Fig. 9 of the main text) during different stages of wall cleaning

Figure C - Photographic documentation of fault zone 2 (Fig. 9 of the main text) during different stages of wall cleaning

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?

32) We agree that the erosion can be due to a small river, but there are not the sedimentary features of an alluvial unit. The erosion by a small river does not preclude a scarp-derived origin of U 6 (i.e., colluvial wedge). See above.

Growing stratification is more distinctive of syn-tectonic sedimentation than a triangular shaped package of deposits.

33) We refer to the classical "colluvial wedge" model. Please, see Mc Calpin, 2009 – Paleoseismology. Academic Press. Chapters 2A and 3.

Furthermore, what about the prolongation of the NE-dipping normal fault? It should be downthrown by F5. Any evidence of that?

34) We agree. The antithetic fault is offset and downthrown by F5. There is no evidence for the antithetic fault in the hanging wall of F5. This means that the antithetic fault is somewhere below the base of the trench wall, in the hanging wall of F5. In the hanging of F5, at an unknown depth below the exposed wall, we should find the antithetic fault with U 4 in its hanging wall and the paleosol/tephra in its footwall (same structural relations seen in the footwall of F5). This is in agreement with our estimate of the minimum vertical displacement of the paleosol (>1.3 m by F5).

In fact, we acknowledge that the role of the antithetic fault has not been adequately described. We will improve this part.

But, please consider also that the restoring of the wall of figure 9 is a conundrum and we don't want to go too much into speculations. We prefer to stay within reasonable interpretations.

See also the reply to comment N. 39 (below).

Could that be represented somehow, or partially, by the contact between Unit 6 and Unit 4 in the hanging wall of F5?

No. It is not possible. F5 postdates the antithetic fault. Please note that in the footwall of F5, the antithetic fault places in contact U4 against the tephra/paleosol. So, we must find the same structural relations in the hanging wall of F5.

Moreover, there is no evidence of shear between U 6 and the underlying U 4.

We are sorry, but you should trust a little bit our descriptions/interpretations, otherwise everything is potentially questionable.

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

Please, see reply n. 31.

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

35) This is a mistake. We are sorry, eastern splay is correct. We will modify the manuscript accordingly.

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.

36) This is again a mistake. Thank you. F5 is correct. We will modify the manuscript accordingly.

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)?

37) Please, note that is: “… period of erosion or with negligible pedogenesis”. In any case, faulting occurred after this period of erosion or negligible pedogenesis.

We will explain more clearly in the text that: a) the contact between U 7 and U 6 is the event horizon (i.e., the topographic surface at the time of the faulting event; see Mc Calpin, 2009); and b) the sequence is: 1) period of “erosion/negligible pedogenesis”; 2) faulting and formation of a coseismic fault scarp offsetting the event horizon; and 3) start of the scarp degradation and accumulation of U 7.

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.

38) We agree. Actually, we estimated only the minimum displacement, without going into further speculations. We are firmly convinced that estimating the minimum vertical displacement is possible and necessary.

Please, note that the paleosol is clearly faulted in the hanging wall of F4. It is not visible in the F4 footwall. Therefore, the minimum vertical displacement is 1.1 m, as stated in the text.

The paleosol (and the antithetic fault offsetting the paleosol/tephra) must be in the hanging wall of F5, below the outcropping wall. Therefore, the vertical separation is at least 1.3 m.

We agree that a better description of the tectonic interpretation will help better understanding also this part. See also replies to comments N. 31 and 34.

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.

39) We disagree that the antithetic fault is so large as suggested. We recall that the restoration of the wall is really a conundrum and we want to stay on the most convincing evidence. The careful field analysis says (see figures D and E below):

- the antithetic fault offsets the paleosol and F4 (the thick F4 red lines in the hanging and footwall, called here F4hw and F4fw, respectively). There are several secondary faults (thin lines). Two secondary faults offset the antithetic fault, one in the hanging wall of F4fw and one in the footwall of F4fw. We understand that this latter small secondary fault is nearly coincident with the trace of F4hw, and this might originate confusion. But this is the fact. Moreover, please note that there is some uncertainty here on the exact location of the F4hw trace (dashed line).

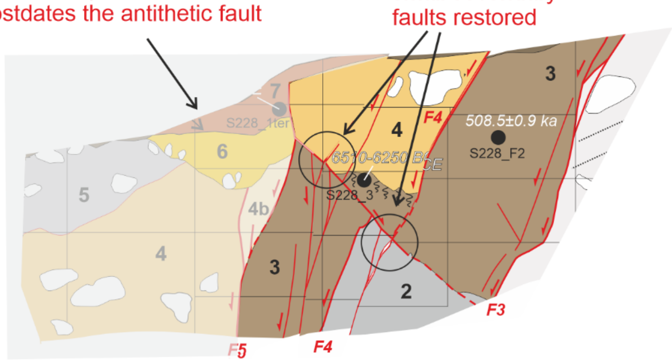
- once restored the small displacement on secondary faults, the estimated displacement on the antithetic fault is about 25 cm (see figure D below).

In the schematic reconstruction of the relations between synthetic (F3, F4, F5) and antithetic faults shown in figure E, there is the possibility that the antithetic fault, in the hanging wall of F5, is somewhere within U4. Possibly, where U4 is very coarse-grained, and therefore the fault is difficult to identify. But this is only a hypothesis.

Restoration of secondary faults (thin lines)

ignore this part because F5 postdates the antithetic fault

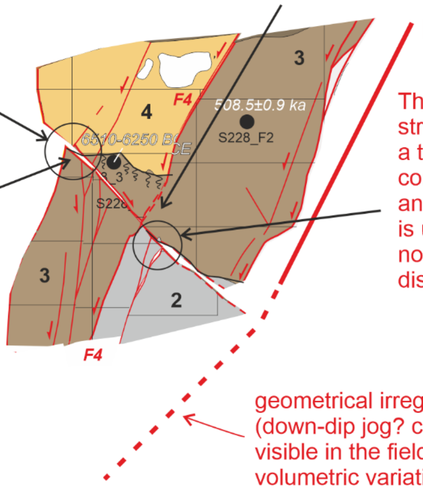
small secondary faults restored



Restoration of antithetic fault

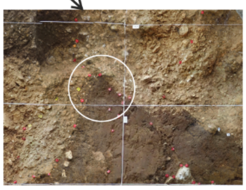
25 cm of net slip on antithetic fault restored

This small displacement (25 cm) on antithetic fault is sufficient to restore the displacement of the paleosol, within the uncertainties of a few centimetres and considering that there is some volumetric distortion that should be considered.



This could be the original stratigraphic contact U3/U2 (U3 is a tephra, the geometry of the contact is hard to predict). The antithetic fault is dashed (i.e., there is uncertainty) because shear is not so evident, suggesting small displacement.

This is the dark part of the tephra in the photo: i.e., part of the paleosol in the footwall of the antithetic fault.
We will highlight this information in the revised version of the figure



geometrical irregularity at depth (down-dip jog? change in dip?), not visible in the field, produced volumetric variations accommodated by antithetic faulting.

Figure D – Restoration of antithetic faulting

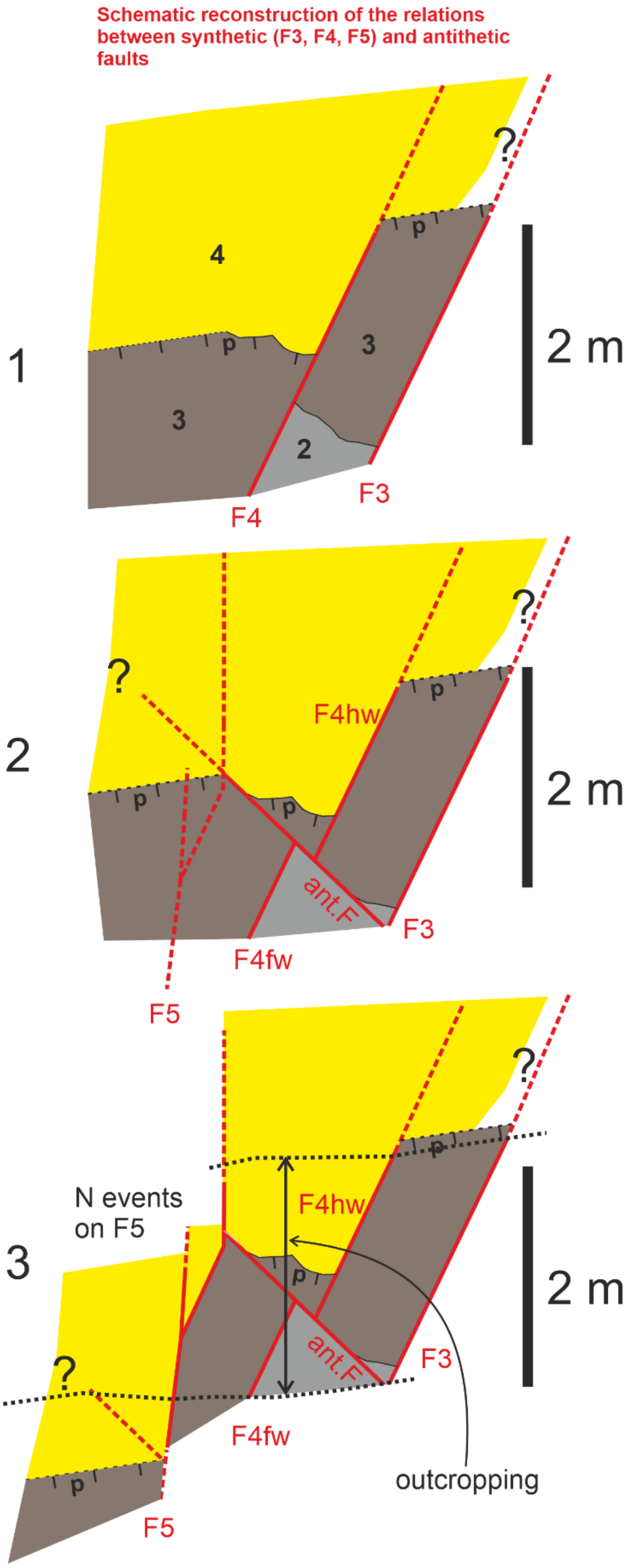


Figure E – Schematic reconstruction of the relations between synthetic (F3, F4, F5) and antithetic faults. P = paleosol.

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.

40) Accepted. We will modify the manuscript accordingly.

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?

41) The "step" could be a warp of the topographic surface, covered by the colluvial wedge. We did not interpret this step as due to faulting because we did not see evidence of discrete faulting and shear. Your suggestion is a plausible interpretation, looking at the figure, but we prefer our interpretation based on field analysis. The geometry of the colluvial wedge (Unit 5) is more clear in the opposite wall.

We have no evidence of U5 in the footwall.

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.

42) We will take into consideration this suggestion in the reorganized version of the manuscript.

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.

43) Good suggestion. We will take it into consideration in the reorganized version of the manuscript.

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.

See reply N. 42.

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.

44) These are the data we have collected. We want to show them in a simple way, without going too much into statistical speculations. Please, note that we acknowledge that there could be some bias due to under-sampling because of poor exposure. We will try to be more clear on that.

It is practically not feasible to split the faults into segments of equal size and have data for each family of different size. Moreover, following which criterion? In order to obtain what? We prefer to avoid this.

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?

45) We did not use the term "cluster". We have written "… prevailing slip vectors plunging …". In the sense that the largest part of the measured data is in that range. For example, we have 20 out of 52 slip vector data plunging between 200 and 240°, and 15 out of 52 data plunging between 110 and 180. Therefore, the largest number of data plunge to SW or to SE. But, we will be careful in describing the data, using numbers.

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.

46) For this we refer to the existing cited literature (Boncio et al., 2016; Amato et al., 2017) who used structural analysis, including relative chronology of slip vectors (please, see Fig. 16 in Boncio et al., 2016), and other tectono-stratigraphic arguments.

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).

47) The SE-plunging slip vectors are related to an older phase. Now the faults are reactivated under the SW-NE trending extension. This will be stated more clearly. Please, see also the cited references.

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?

48) Accepted. This part will be better discussed in the reorganized and revised version of the manuscript.

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.

48) Accepted. This part will be better described in the reorganized and revised version of the manuscript.

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 reliefbuilding forces' rates (i.e., tectonic).

49) Please, see reply to comment N. 25

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.

50) The correct values are those reported in Tab. 4. We will fix the inconsistencies.

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.

51) It was dug! Please see Fig. 10a. There is no evidence for Unit 5 in the footwall of F1.

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.

52) Please, see reply to comment N. 31

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”?

53) Cannot be excluded if we consider (only) the age constraints.

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.

54) We are note sure to get this criticism. We are discussing about historical earthquakes and faults. Please, consider that we are dealing with ancient, poorly constrained historical events. We cannot exclude a priori some hypotheses.

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

55) We will try to be more clear.

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?

56) The shape is not necessarily steep. It is steep now because of the second slip event.

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

57) It is shown. Please, see the caption.

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.

58) We can widen the discussion on the 847 event.

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.

59) These are concepts rather well established in the earthquake geology and seismic hazard literature.

Line 468: colluvial.

Corrected

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

The correct values are those reported in Tab. 4. We will fix the inconsistencies.

Line 470: actually, the along-strike slip rate varies also for reverse and strike-slip faults; it is not diagnostic of a normal fault.

60) We know. Of course, this is not diagnostic of a normal fault. Is this the message got from this sentence? We will be more careful.

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.

61) We will correct with the appropriate number.

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.

All the suggestions below will be adequately taken into account during the revision of the manuscript. We reply here to relevant comments.

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.

Colours refer to fault activity constraints (different colours do not necessarily mean that the faults are active in different periods). The approach is inspired to Faure Walker at al. (2021), but data are from our original work. We will explain clearly in the caption and in the text.

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.

All the significant features will be explained, implementing what already done in Fig. 7, when necessary.

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.

We will explain in caption.

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.

Yes. Must be added.

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?

We will explain in caption that the numbering refers to the site.

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.

They are upward pointing arrows. We will explain.

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.

Please, note that structural data are already provided as 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.

We focused mostly on the detailed field mapping of the fault trace. These data are fundamental from an earthquake geology perspective, and are lacking in the existing literature. We will state clearly.

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