Response to Reviewer 2 (Ettore Valente)

Black: original comments by Reviewer

Red: response by Authors

Dear Authors,

you will find attached my comments. Overall, I think the paper is pretty interesting and may be suitable for publication in Solid Earth. It ayway deserves partial re-thinking and re-organisation of the Results and Discussion section, which should be more clearly distinguished.

I hope you will find my comments useful to increase the scientific interest of your paper.

Ettore Valente

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

The paper deals with active tectonics along a sector of the Southern Matese mountain front that has been believed to exert low tectonic activity in the late Quaternary.

The theme is of high scientific interest, as very recent activity of this fault strand has not been proved to date. In addition, the authors propose a correlation between the Gioia Sannitica fault activity and some poorly known historical earthquakes (e.g., the 346 and the 1293 events).

English style is fine, I just highlighted very few corrections at the end of this file (see section "Technical correction") but I'm not an English mother tongue, so I may have missed something.

I think the paper may be of high interest, but it needs to be re-thought in the light of general and specific comments. In particular, I encourage the authors to clearly distinguish results and discussion section, which are not easily identifiable in this current version, and to focus the discussion on the comparison between APMF and GF, and not only on the seismogenic potential of GF. So, despite the high interest of the argument, I think the paper needs relevant modifications before acceptance. I highlighted several points that are listed below, and my decision is to reconsider it after major revision.

1) Concerning the re-organization of the paper, we will modify the manuscript separating more clearly Results from Discussion. Interpretations will be moved to the Discussion. The new organization will be:

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

One of the main criticisms is that authors show evidence of recent tectonic activity along the GF but they also mentioned that this portion of the mountain front is mature by referring to Valente et al. (2019). A mature mountain front would imply either an inactive mountain front or very low fault slip-rate overwhelmed by erosional processes. I never found I clear discussion of these contrasting data, which is just shallowing approached in Sections 6.2 and 7.

2) Criticism accepted.

We will deepen this point in the new discussion section. The main results from Valente et al. (2019), to be honest already acknowledged in the present version of the paper, will be more exhaustively (we hope) summarized in the introductory geologic context (2.2 Quaternary tectonics), and then further discussed in the Discussion. The differences will be discussed in dedicated sub-sections (new sub-section 5.2).

We think that the discrepancies between our results and Valente et al. (2019) results are only apparent. Probably they are due to different scales/detail used in the analysis: at the scale of the mountain front in Valente et al; at a detailed scale from field mapping and high-resolution topography of fault scarps from LIDAR in our paper. I think the two results must be integrated. We tried to give this message in present section 6.2 but evidently our attempt was not efficient. We will get it better in the revised version.

Furthermore, proving the tectonic activity of a mature mountain front is a very interesting issue, but a comparison with similar case studies in different tectonic and climate context is missing. I encourage you to address this issue, which would increase interest of the international scientific community towards this paper. This should be discussed in a separate section before the Conclusion.

3) We agree that this topic can be of great potential interest. But, we think that a discussion of global significance, with comparison with other cases worldwide, is beyond the scope of the present work. Please, consider that the paper is already quite long, and it was submitted to a special issue: “Tools, data and models for 3-D seismotectonics: Italy as a key natural laboratory”

Evidences of recent tectonic activity of GF include some meter-high scarp detected by Lidar data at the base of the mountain front. By the way, this scarp occurs close to the alluvial fans’ topographic apexes, where the alluvial fans are strongly dissected (up to several meters) by channels feeding youngest fans (channels are very clear by Lidar in Fig. 2b). So, you should consider the hypothesis that this scarp may be due to erosion and not to fault activity. Furthermore, post-LGM throw rates are not convincing to the south of GF (see specific comments).

4) The first, very important, activity for performing fault scarp analysis is the selection of appropriate sites. The sites must be unaffected by erosional exhumation processes. We agree on that.

We performed our scarp analysis after a careful selection of the sites. We disagree with this very generic, poorly motivated criticism.

The fault scarp illustrated in Fig. 6 is a nice 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. 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.

To enforce your hypothesis that this scarp is due to fault activity, you should provide other geomorphic markers of recent tectonic activity of the GF, such as knickpoints. Given that the scarp cross over adjoining drainage basins, I expect that river long profile should have a knickpoint in the surroundings of the scarp. You have Lidar data so river long profile should highlight the presence of some meters high knickpoints. If not, the scarp may be the result of differential erosion, as it seems to be.

5) We agree that analyses aimed at identifying other geomorphic markers of recent tectonic activity is a 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; see reply to comment n. 4), can provide good estimates of throw rates.

Anyway, please have a look at Figure A below. The figure shows the area where the small scarps in Holocene alluvium, shown in profile 4 of Fig. 7 (total throw of ca. 4 m), are located. The scarps are clearly visible on LIDAR DEM.

Figure A 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 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 the figure as supplementary material. The graphics will be improved before submitting the revised version.

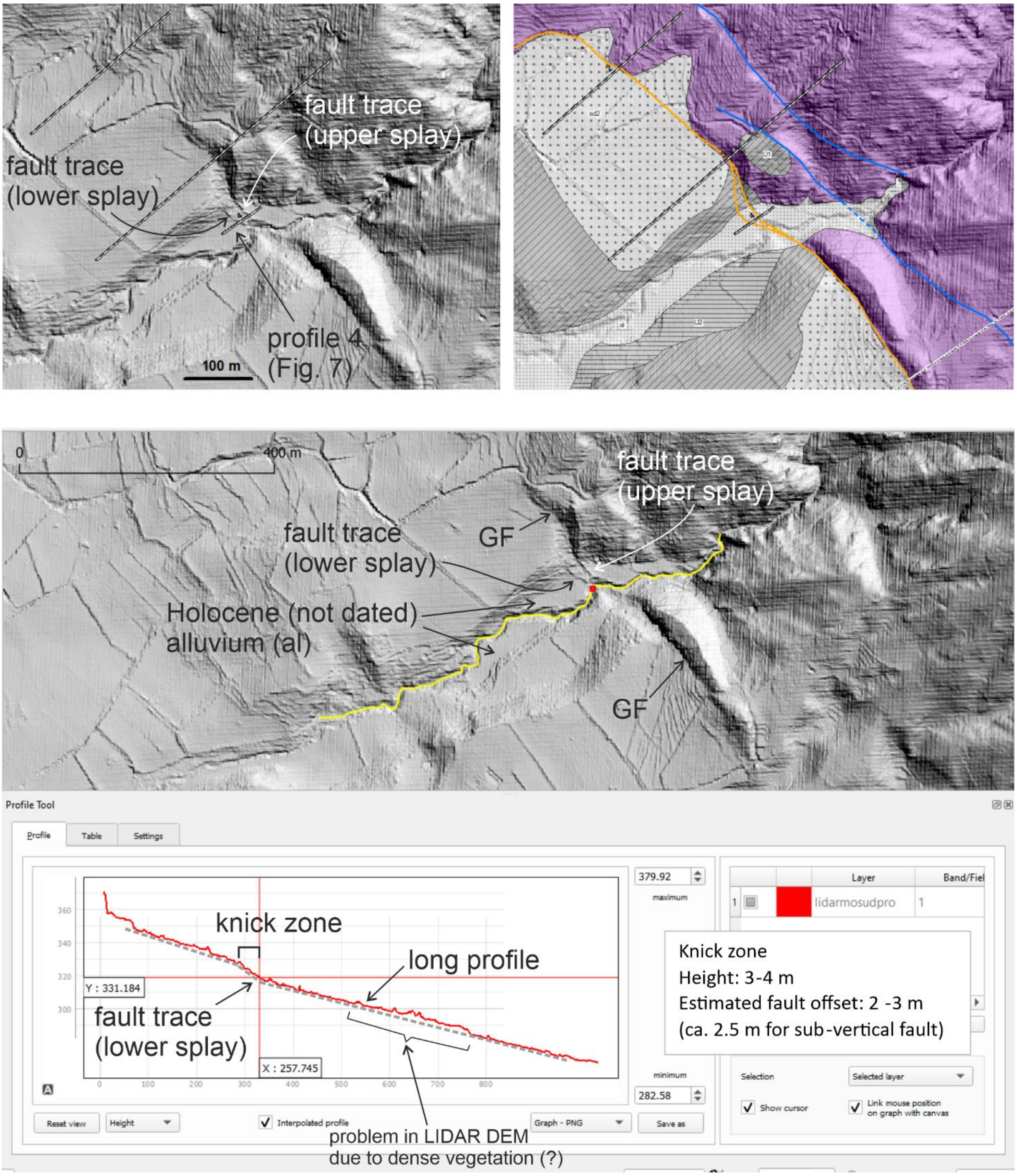


Figure A (the dashed grey line in the long profile has been drawn for highlighting the scarp in the knick zone).

Regarding along strike variations in throw rates, you mentioned that the APMF has no evidence of recent tectonic activity. Anyway, I think that throw rates you estimated near Sant'Angelo d'Alife are supported by strong field evidence (Fig. 10), whereas throw rates along GF are not very clear and may lead to possible alternative interpretation (see comments in the entire file about throw rates). In my opinion, your data highlight a more recent activity of the APMF than the GF, which would imply that APMF is more active than GF, according to already published data. I think this is a crucial point, and your discussion should be addressed towards comparison of the two study areas, and not only in addressing the seismogenic potential of GF.

The seismogenic potential of GF should be re-thought according to the previous comment.

6) This criticism cannot be accepted.

Probably the Referee is confused. We did not write " … *that the APMF has no evidence of recent tectonic activity*". Where did you read this?

Please, note that at lines 425-427, we state that …. **the Piedimonte Matese section** of the APMF has **poor** geologic and geomorphic evidence of recent activity. We confirm this statement, because we did a careful field mapping.

In the paper, we discuss the activity of the entire Southern Matese Fault system, including APMF and GF. This is clear from the text (section 6.3), from Fig. 11 and from Tab. 4, we think. Please, note that the central part of the APMF is from red (Late Pleistocene – Holocene activity, dated) to orange (post-LGM from fault scarp).

Therefore, your criticism cannot be accepted, because unmotivated.

Please, also read carefully at lines 104 – 110:

The SMF can be divided into the Ailano – Piedimonte Matese fault to the NW, and the Gioia Sannitica fault to the SE (Boncio et al., 2016). The 18 km-long Ailano – Piedimonte Matese fault is in turn divided into the Raviscanina and Piedimonte Matese fault sections. The Raviscanina section is 11.5 km long, strikes NW-SE and progressively bends to ~W-E in the southern part (~1 km SE of the Sant'Angelo d'Alife village). The Piedimonte Matese fault section is 7 km long and strikes from W-E to WSW-ENE. The eastern part of the Piedimonte Matese fault section, striking ~ W-E, has strong geomorphic evidence of Quaternary activity, while the western part, striking WSW-ENE, is less evident due to cover deposits and larger mountain front sinuosity.

Stratigraphical and structural setting in the Mt. Erbano embayment is very tricky. Firstly, the presence of the varicolored clays (intended as a lithological unit, not like a Formation) within the Meso-Cenozoic succession of the Camposauro and northern Matese ridges has been proved (Vitale and Ciarcia, 2018). So, are you sure that Vitale and Ciarcia (2018) setting cannot be exported to your area? If so, you do not need a back-thrust, but the varicolored clays may stratigraphically overlain the Miocene deposits.

7) Thank you for the suggestion. In the Mt. Erbano embayment, bedding and structural data are a few due to poor exposures, especially for arenaceous and clayey deposits. According to the literature and geological considerations at smaller scale (Sannio-Matese), we have interpreted the contact between varicoloured clays (obviously intended as a lithological unit) and the Miocene turbidites as a folded thrust. However, taking into consideration the suggested reference (Vitale and Ciarcia, 2018), interpreting the varicoloured clays as an olistostrome within the Miocene arenaceous deposits (Caiazzo formation) is plausible. We will take into consideration this suggestion during the revision.

Secondly, the dashed projection of the GF towards south-east is not convincing as you do not show evidence of recent fault activity in this area. In fact, profile 12, which is the only profile in this sector of GF, show an 85 m high scarp carved in the carbonate bedrock. This would imply that the scarp is due to a lithological contrast between the bedrock and the debris deposits. The apparent offset of unit sd1 may due to erosion. In fact, profile 12 is very close to a deep incision, which may have lead erosion that caused the outcrop of the buried bedrock, with debris on top of the bedrock being simply a remnant of the Quaternary cover.

8) We agree that this is not the best place. We selected the location of profile 12 in order to avoid erosion from nearby channels as much as possible. But, we are aware that erosion might have had a contribution. This is the reason why in the text we say only that unit sd1 is hanging of 80-100 m in the footwall of the fault. Without further speculations.

In the revised version of the manuscript we will stress that this site can be "contaminated" by un unquantified amount of exhumation due to erosion, preventing reliable throw rate estimates.

A point that deserves further discussion is the apparent increasing throw rate from the Middle Pleistocee (<0.1 mm/yr in the last 450 kyr) to the Upper Pleistocene and the Holocene (>0.2-0.4 mm/yr in the last 15 kyr). Several factors can underestimate long-term fault slip rates. One of them refers likely to geological processes such as compaction of accumulated sediments and/or erosion of marker beds. You should also discuss this point.

9) We agree that the paper can benefit from a deeper discussion of this apparent increase in slip rate. We will discuss this in the revised version.

The age of the paleosol on top of tephra layer in Fig. 9 rise some questions. It would imply that there has been no sedimentation from 508 ka to 6 ka, and that the mountain front has been not active in this very long time interval. Consequently, units 4 to 7 accumulated since the Middle Holocene. This contrast with the age you assigned to Quaternary units as no Holocene deposits have been mapped in this sector of the pediment. Also, the story these data would imply is:

1) tectonic quiescence from 500 ka to 6 ka; 2) accumulation of units 4, 4b and 5; 3) faulting of these units; 4) erosion and accumulation of units 6; 5) faulting of this unit (you mentioned it at line 349, but I disagree with your hypothesis as the colluvial wedge seems accumulated in a topographic low due to erosion. I don't see any thickening of the colluvial wedge towards F5. Again, its geometry may be due to erosion); 6) accumulation of unit 7; 7) formation of the modern soil. Maybe, it is a bit too much to occur in just 6 ka! Is it possible that this very young age is due to contamination from the overlying deposits?

10) The paleosol is 8,460-8,200 years old (6,510 – 6,250 yrs BCE).

Yes! We are aware that this is a young age. This is the reason why we did not stop at the 14C dating, but we performed a paleosol analysis (please, read Text S1), in order to verify if the characteristics and maturity of the paleosol are consistent with the obtained ages. The paleosol shows weak andic properties and poor pedogenetic evolution, suggesting a young age (please, read lines 334-338).

Therefore, two data from independent analyses show consistent results. The obtained ages are a matter of fact; this is not an opinion. We cannot omit this fact. We must take this into consideration, and provide a reasonable explanation.

About the " *... it is a bit too much to occur in just 6 ka!*":

What do you mean for "a bit too much"? Please provide specific, quantitative arguments.

Moreover, we have estimated a minimum vertical displacement of the paleosol of 2.4 m in 8,145 years. Considering displacements per event at the site on the order of 50 cm, as suggested by the colluvial units (see also below), we can obtain the total displacement with 5 surface faulting events, with average recurrence interval between consecutive events of >1600 years. This is not "too much".

About the "*This contrast with the age you assigned to Quaternary units as no Holocene deposits have been mapped in this sector of the pediment*": you are right. The Holocene deposits are missing due to a mistake. They will be added in the map.

About Unit 6 and the story of the outcrop of Fig. 9, please consider that:

- 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.

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.

We will add the figure B below as supplementary material.

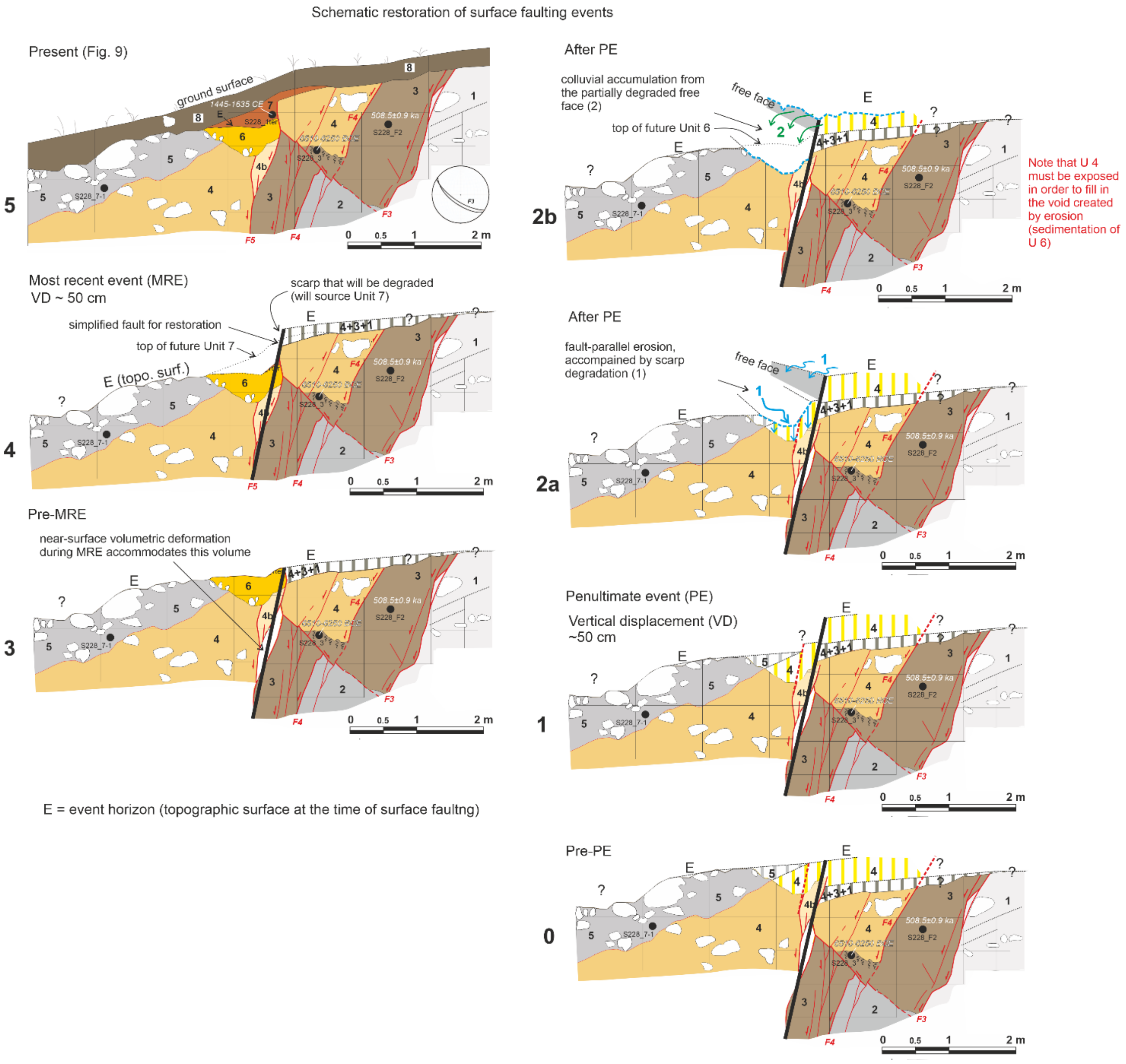


Figure B - Schematic restoration of surface faulting events

SPECIFIC COMMENTS

MAIN TEXT

TITLE: Central Italy and Southern Apennines sounds in contrast. The Matese ridge is part of the Southern Apennines and study area is geographically constrained to the Southern Italy.

11) Accepted. We will change in southern Italy.

Line 17: structural data in Supplementary File "Tab.S3_Data points with structural data" refer to the entire Southern Matese Fault System, and not only to the Gioia Sannitica fault. You should either add location of structural data in the main map or add a field in the excel tab to specify to what sector of the Southern Matese Fault System the data are referred.

12) Good suggestion. We will add a field in the excel file.

Line 19: are you sure "mature geomorphology" is the correct term? It may be used to indicate a tectonically inactive area, so its use may lead to misunderstanding.

13) Accepted. We will check and use the most appropriate term.

Lines 22: I guess you reported here the average values listed in Tab. 4. If so, average value along GF is 6.2 and not 6.1.

14) Yes, it is a mistake. We will change in 6.2.

Line 25: the 1349 event is not poorly known such as the 346, the 847 and the 1293 events.

15) Accepted. We will modify the manuscript accordingly.

SECTION 1 - INTRODUCTION: the authors should introduce papers that discuss very recent activity of the Southern Matese Fault System (e.g., Ascione et al., 2018; Valente et al., 2019). It seems that you addressed the Introduction towards your findings, which is to prove the very recent activity of the Gioia Sannitica Fault, without referring to papers that contrast with your working idea. Furthermore, in Boncio et al. (2016), you already mapped the APMF (Ailano-Piedimonte Matese Fault) as an active structure and the PMGF (Piedimonte Matese – Gioia Sannitica Fault) as a possible active structure, so you should also introduce this point.

16) Accepted. We will better summarize and acknowledge the work by Ascione et al. (2018) and Valente et al. (2019).

See also replies to comments N. 2 and 6.

Line 34: add some reference to support this sentence.

Lines 63-65: add references about the morphotectonic setting of the Matese massif.

17) Accepted. See reply to comment N. 16.

Line 68, lines 72-74 and lines 81-82: refer to scheme showing tectonic evolution from Valente et al. (2019)

18) Accepted. See reply to comment N. 16.

Line 86: Ailano is not mentioned in Fig. 1, maybe you referred to Alife

19) Yes, the correct locality is Alife. We will correct the manuscript accordingly.

Lines 90-93: references listed in this section do not talk about the presence of this tephra layers within the Middle Pleistocene deposits, but refer to the volcanic activity of these volcanic areas. You should refer to papers that suggest the presence of the Roccamonfina and the Campi Flegrei products within the Middle Pleistocene deposits.

20) We had listed those papers because they were referred to the available age constraints for the whole of the Roccamonfina activity and for the main markers from Campi Flegrei (CI and NYT). We agree with the reviewer that in the present form the sentence could be confusing, so we will rephrase the statement and separate the references for previous tephra findings in the area (quoting the right papers) and age of activity at the hypothesized sources.

SECTION 2.2 – QUATERNARY TECTONICS: you should mention the paper by Ascione et al.(2018) that show evidences of recent activity of faults bounding the southern slope of the Matese massif.

21) Accepted.

Line 95: Pay attention, you wrote that you mapped these Quaternary faults, and this sentence sounds like you are introducing your results whereas you are still in Geological Setting.

22) Accepted. We will pay attention.

Lines 97-98: connection between the San Pietro Infine fault and the SMF is not discussed in this paper, so you should add adequate references. I think your paper Boncio et al., 2016 is one of the papers, or even the first one, where this connection is hypothesized.

Lines 99-104: you should also add references to papers that highlight recent tectonic activity of the SMF (e.g., Boncio et al., 2016; Ascione et al., 2018; Valente et al., 2019).

Lines 108-110: add reference to justify along strike variation in fault activity along the SMF. I guess it should be the paper by Valente et al. (2019). If so, the sentence at lines 116-118 is a repetition of this sentence.

Line 131: add reference to Galadini and Galli, 2004_Annals of Geophysics.

23) Accepted. Additional references will be added.

Line 135: you mentioned that the 2013 event had a magnitude of 4.9 whereas you indicated a magnitude of 5.2 in Fig. 1. Rovida et al. (2020) indicate magnitude 5.16 for the 2013 Matese event. Please, make the main text and the figure consistent. Furthermore, detail of the 2013 event are also reported in Valente et al. (2018), so you should also refer to this paper.

24) The correct magnitude is Mw 5.2, according to RCMT catalogue (http://rcmt2.bo.ingv.it/). We will correct the manuscript accordingly.

Lines 138-141: why did you refer to the 2016 event? As you correctly stated, it occurs NE of the Matese area. It testifies that earthquakes in this sector of the Apennine chain occur along NW-SE striking normal faults, but this event is not due to activity of the faults bounding the Matese massif.

Or, do you think this event is due to activity of the Northern Matese Fault System? Do you have data for supporting this hypothesis? If yes, please add further details to support it. If not, you should not mention the 2016 event in this section.

25) We mentioned the 2016 earthquake simply because it is in figure 1. We do not draw speculations, inferences or similar. Why we should not mention that event? Instead, it provides insights on the area currently undergoing SW-NE extension.

Line 146: what does CTR means? I know, but you should specify it for international readers not used to Italian acronyms.

26) Accepted. We will specify this acronym (Carta Tecnica Regionale, CTR: Italian name and acronym of the 1:5,000-scale topographic map of the Campania Regional authority).

SECTION 3.2 – SAMPLE DATING: you mentioned that you performed tephrostratigraphic analysis, but I did not find any tephrostratigraphic analysis both in the main text and in the supplementary data.

27) As a matter of fact, we wrote "Three tephra layers interbedded within faulted alluvial fan and colluvial sediments were sampled for tephrostratigraphic analysis and $^{40}Ar/^{39}Ar$ dating" and "Unfortunately, no well preserved glass fragment survived washing pre-treatment and hence chemical composition of glass could not be achieved", which is the reason why no chemical data has been reported. For the sake of shortness, as required by the journal, we did not either add the description of the results of lithological and mineral component analysis achieved on the dated and other sampled tephra, which we used to make at least some inferences on their possible attribution. In the revised version of the manuscript, we plan to add a new supplementary section, containing further information on tephra useful for the time constraining of the identified units.

Lines 182-183: see comments to Supplementary Data.

Lines 222-227: is the correlation between tuff interlayered in the U2 deposits and the WTT supported by tephrostratigraphic analysis you mentioned in the Methods section? Or, is it just a working hypothesis?

28) The age is assigned on the basis of field observations, field lithological characterization and correlations with previously published papers. We agree with the reviewer that a precise attribution to WTT Formation could be quite speculative in the absence of precise age determination and chemical data on fresh glasses (see also replay to comment n. 26), so we decided to change the possible correlation referring to Roccamonfina pre-caldera activity (the dated layers pertaining to U1) and Roccamonfina post-caldera activity (the leucite free tephra pertaining to U2). This only slightly predates the inferred age of U2 unit to ca. 430 ka, but does not change the stratigraphic setting. It will be specified more clearly and the references cited.

Line 247: refer to Plate 1 to locate Criscia.

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

Lines 298-302: estimated post-LGM throw rates are not convincing. In fact, profile 13 shows a 6.5 m offset in Sd1 unit, which you constrained to the late Lower – Middle Pleistocene, whereas profiles 14 and 15 show offset U1 unit, dated at the early Middle Pleistocene. Throw rates should be referred to these chronological intervals, which may lead to values like those one obtained by profiles 3, 9, 11 and 12.

30) We agree with the reviewer that the age of fault scarps is not constrained there. We infer a post-LGM age because of:

- the scarp offsets the mountain slope in a similar way, independently from the underlying carved unit (U1 or sd1), suggesting the scarp formed after the shaping of the mountain slope;

- similarity with the area of Fig. 6, in terms of both the scarp size (height) and geometrical relations between the scarp and the mountain slope.

But we agree that the minim value (post-U1) is a more conservative value, and in any case a correct one. We will change the present value in the minimum throw rate value, as suggested by the reviewer.

Line 355: if the scarp is due to erosion, as you mentioned, then it is not a fault scarp and you cannot derive throw rates.

31) This criticism is unacceptable.

At line 355 we do not say " … *the scarp is due to erosion* …".

At line 355 we write exactly this: "A ~1 m-high eroded scarp across the F3-F5 fault zone is visible on a ….".

Once a coseismic surface faulting occurs, a coseismic scarp forms, if there is vertical displacement (e.g., normal fault). After surface faulting, the fault scarp starts to be degraded and eroded, and the scarp retreats. The size and shape of the eroded scarp depend on several factors, including: size of the coseismic displacement, single-event or compound scarp, lithology, climate, erosional processes, time after surface faulting, etc.

This process has been described in papers that are milestones of the earthquake geology literature. Please, read:

- Wallace, R. E. (1977). Profiles and ages of young fault scarps, north-central Nevada. Geol. Soc. Am. Bull. 88, 1267–1281.

and/or

- Mc Calpin, 2009 - Paleoseismology. Academic Press. Chapter 3

- Yeats, R.S., Sieh, K., Allen, C.R., 1997 - THE GEOLOGY OF EARTHQUAKES –Oxford Univ. Press. Chapter 7.

SECTION 6.1 – OVERALL ARCHITECTURE AND KINEMATICS: you should also refer to scheme showing tectonic evolution of the SMF proposed by Valente et al. (2019), and not only to papers by the authors.

SECTION 6.2 – THROW RATES: regarding mature geomorphology of mountain front, you did not introduce any data supporting this hypothesis, so you should just refer to Valente et al. (2019).

32) See replies to comment N. 2.

You should also consider the possibility that the scarp you used to derive throw rate is only due to erosion (see general comments).

33) See replies to comments N. 4, 5 and 31.

Line 405: compare your values with values from Cinque et al. (2000) and Ferranti et al. (2014).

34) Accepted. We will compare with values from Cinque et al. (2000) and Ferranti et al. (2014).

SECTION 6.3 – SEISMOGENIC POTENTIAL: this section should be revised according to my comments on how you estimated throw rates.

Table 1 and 2: you should add coordinates of sampling site and show their location in some map, at least in Fig. 4 and Plate 1.

Regarding samples in the surroundings of Sant'Angelo d'Alife, you may add their location in Fig. 2.

35) Accepted. We will add coordinates in Table 2.

The Sant'Angelo d'Alife site is already located in Fig. 2. Please, see Fig. 2.

FIGURES

Figure 1: correct "Northern Maters Fault System".

36) Thank you, we will correct the name.

Why did you place the 346 epicentre in the core of the Matese massif? Galli and Naso (2009) and Galli et al. (2017) placed it in the surroundings of Ailano. You also mentioned, in the main text, that the SMF may be the source area of the 346 earthquake, so it looks a bit in contrast the location of this event in the core of the massif. What data do you have to place it to the south of the Matese Lake?

37) This is the epicentre from the CFTI5Med catalogue (Guidoboni et al., 2019). We will explain in the text and in caption.

Furthermore, are there marine deposits within the Quaternary basin? Are you sure? If so, cite papers mentioning these marine deposits within the main text. Also, the upper age limit of this deposits is missing.

Add coordinates and north arrow.

Figure 2: fault traces should be thicker.

All the above suggestions will be adequately considered during the revision.

Figure 3: you show that units U3 and sd2 contain the NYT and the CI tephra layers. Anyway, you never presented data showing the presence of this tephras both in the main text (see lines 228 to 234) and in the boreholes section of Plate 1. What data do you have? Please, introduce them in the main text.

The occurrence of these tephra in the outcropping and borehole successions has been reported in a PhD thesis (Leone, 2018), with full chemical data supporting this attribution. We will make full reference on the occurrence of these tephra in the new supplementary section. See reply to comment N. 28.

The data are available in the appendix sections of the PhD thesis "Leone, 2018. Studio dell'evoluzione quaternaria di alcune conche intermontane dell'Appennino campano-molisano, a supporto della pianificazione e gestione del territorio e della prevenzione del rischio sismico. https://iris.unimol.it/handle/11695/75943#.YSxoUY4zZPY

Figure 4: revise accordingly to comments at geological map and cross-sections in Plate 1.

Figure 6: the outcrop of Triassic bedrock close to the fault trace and the lateral variation in scarp height, which is maxima where the bedrock outcrops, suggest that the scarp is due to differential erosion and not to fault activity.

See replies to comments N. 4 and 5.

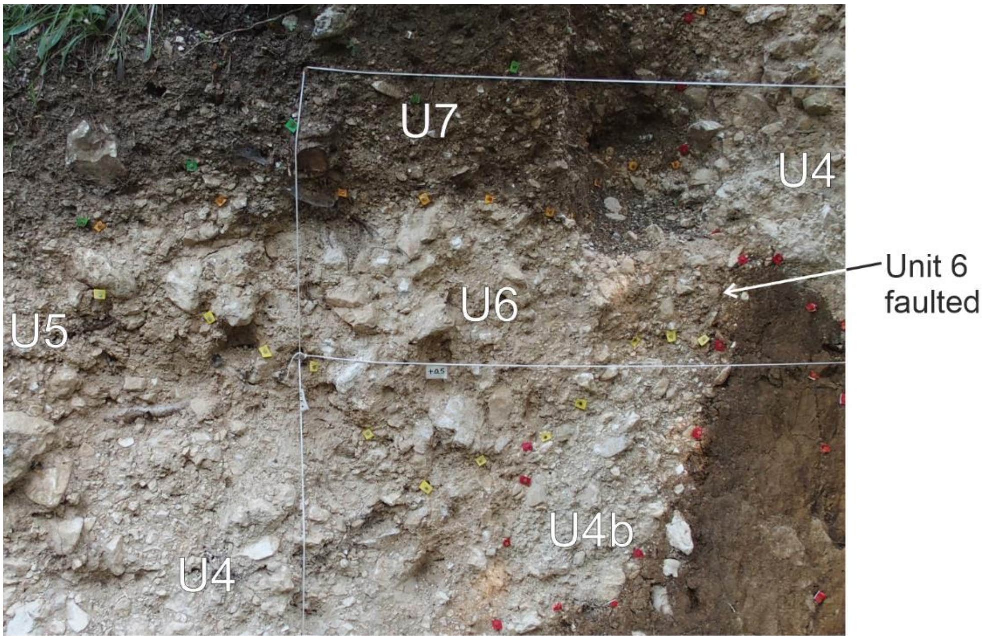
Figure 7: specify, in the caption, if fault colours are referred to fault colours in Plate 1. If not, add a small legend to specify what different fault colours indicate.

Accepted.

Figures 8 and 9: to facilitate readability of these figures, authors should complete details of faulted deposits with reference to units mapped in the geological map (Fig. 4 and Plate 1). Unit U1 is present in this sector of the study area, which should refer to units 1 to 3 in both figures, whereas C14 data indicates that units 4 to 7 are Holocene in age, but there are no Holocene deposits in this sector of the pediment. Furthermore, it seems to me that unit 6 in Figure 8 can be prolonged towards the NE and may cover the fault trace, but maybe this different interpretation is due to colours in the picture.

Accepted. We will show the relations between the units in the logs and the units in the main map.

Unit 6 is clearly faulted. No questions about that. Perhaps the Reviewer used a low-resolution image. We are planning to add a photographic documentation of fault F5 in the supplementary material (see Fig. C below).



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

Figure 10: age of unit 4 should be "BCE" and not "CE".

Figure 11: I missed in the main text Middle Pleistocene to 15.3 ka throw rates for the Raviscanina and Piedimonte Matese areas. I just found throw rate >0.35 mm/yr during the late Holocene (see line 379 of the main text).

All the above suggestions will be adequately considered during the revision.

SUPPLEMENTARY DATA

TAB. S3_Data points with structural data

- See comment to line 17.

38) General reply to specific comments to Plate S1 and geologic sections (below).

Geologic maps and sections are always a combination of: 1) firm constraints (outcrops); 2) geometric reconstructions according to stratimetric rules and structural geology; and 3) reasonable interpretations. Al the geologists doing field mapping know about that.

In the map and sections proposed as supplementary material, we did our best on the basis of months of field work and reasoning. Certainly they can be improved. But we are sure to have done the best with a scientific approach.

We are happy to modify and improve map and sections if they conflict with documented field constraints, or if there are errors, geometrical problems, inconsistencies.

But, if the asked modifications are merely aimed at proposing alternative interpretations, without constraints, we need to be free of rejecting them.

PLATE S1

MAIN MAP

- The Gioia Sannitica fault passes, laterally, to a generic Quaternary fault to the north of Calvisi. How it is possible?

Accepted. We will correct the map.

- The tectonic setting near Curti, Petrella and Caselle is very tricky (see comments in the "General comments" section)

- The Quaternary faults to the west of GF are not supported by your data (see next comments), so I suggest removing them and modifying the cross-sections accordingly

See specific reply.

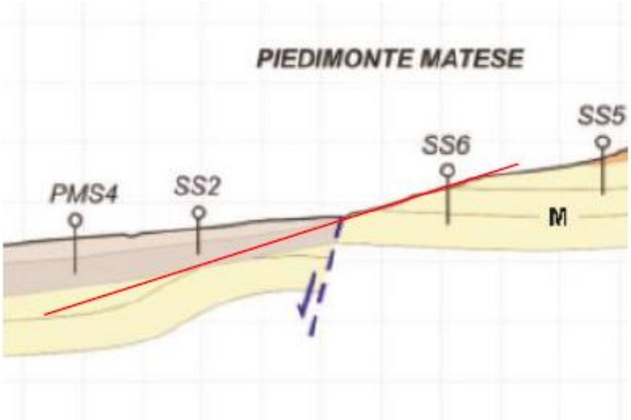
CROSS-SECTION

Enlarge text on the X and Y axes. Add "Elevation (m a.s.l.)" to the left of the Y axe and "distance (m)" below the X axes.

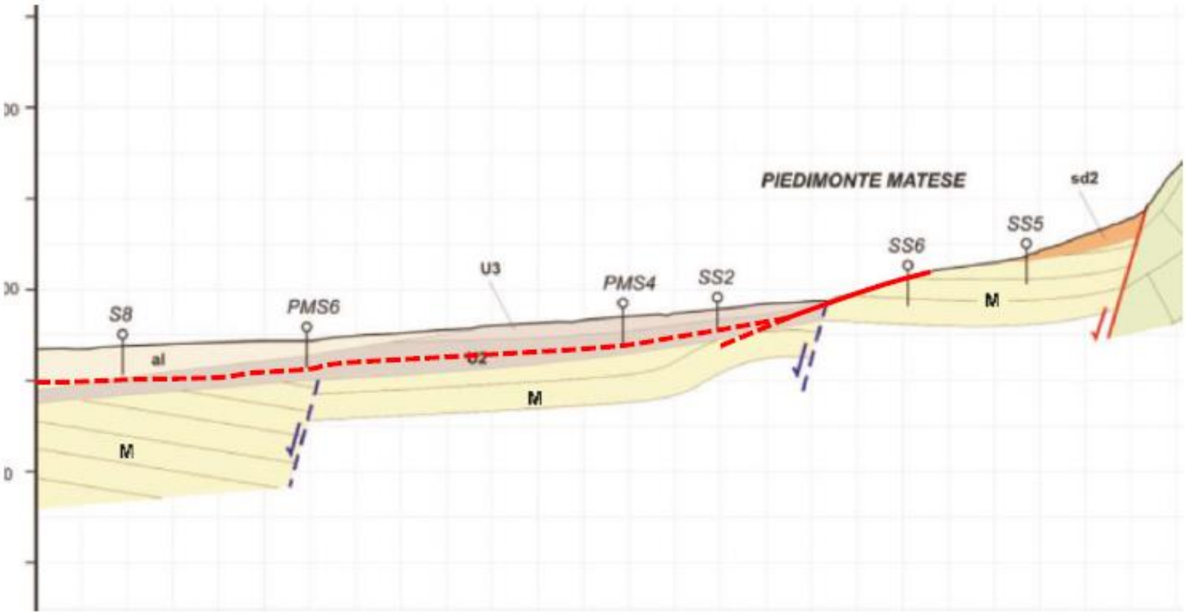
Accepted. We will modify the sections.

CROSS SECTION A-A'

- I have some doubts about blue faults you reported in this cross-section. Borehole data do not show any offset, as borehole SS6 drill the Miocene unit whereas boreholes SS2 and PMS4 drill alluvial units. If you project the topographic surface near borehole SS6 south-westwards, you do not need a fault (see red lines in the figure below). So, it is easier to interpret the contact between alluvial fan deposits and Miocene deposits as stratigraphical.



- Furthermore, borehole data do not allow to infer the presence of another fault, near borehole PMS6, because there are no offsets in the Quaternary units. In this area, borehole data do not allow to infer both the thickness of the alluvial fan deposits and the depth of the bedrock, so the lower part of the cross-section is not supported by data. I suggest ending the cross-sections at the base of boreholes (a possible solution is provided below).



39) Partially accepted.

The debated faults are dashed faults. This means that they are not well constrained (dashed if inferred or buried). But there are reasons for their presence.

The fault close to PM6 is drawn in order to justify the difference in elevation of the top of the carbonate bedrock in the hanging wall (deeper than -200 m a.s.l. from geophysical data in Corniello and Russo, 1999; see section 1 in Fig. 4) compared to the footwall (outcropping Cretaceous limestone at elevations higher than + 175 m a.s.l.; see outcrops in the geologic map).

The fault between SS2 and SS6 is drawn (again dashed) because of the higher elevation of Miocene sediments in the footwall compared to Quaternary sediments in the hanging wall.

But, we agree that we have no constraints for inferring the displacement of the Quaternary units. We will modify the section adding a dashed line along the bottom of Quaternary units, as suggested, and a question mark at the intersection with the inferred fault.

- I also found some incongruences about Miocene deposits dip in the hangingwall of red fault (e.g, the Gioia Sannitica fault). In fact, Miocene deposits dip towards SW in cross-section A-A' and D-D', towards NE in cross-section B-B', C-C' and F-F', and are horizontal in cross-section E-E'. Few bedding data on Miocene deposits reported in the main map do not allow to support such variations, so the authors should re-draw this part of the cross-sections.

- Lastly, the Cretaceous units dip 40° towards the SW. This is the only bedding data reported in the main map and it does not support folding of this units as reported in cross-section A-A'.

40) Accepted.

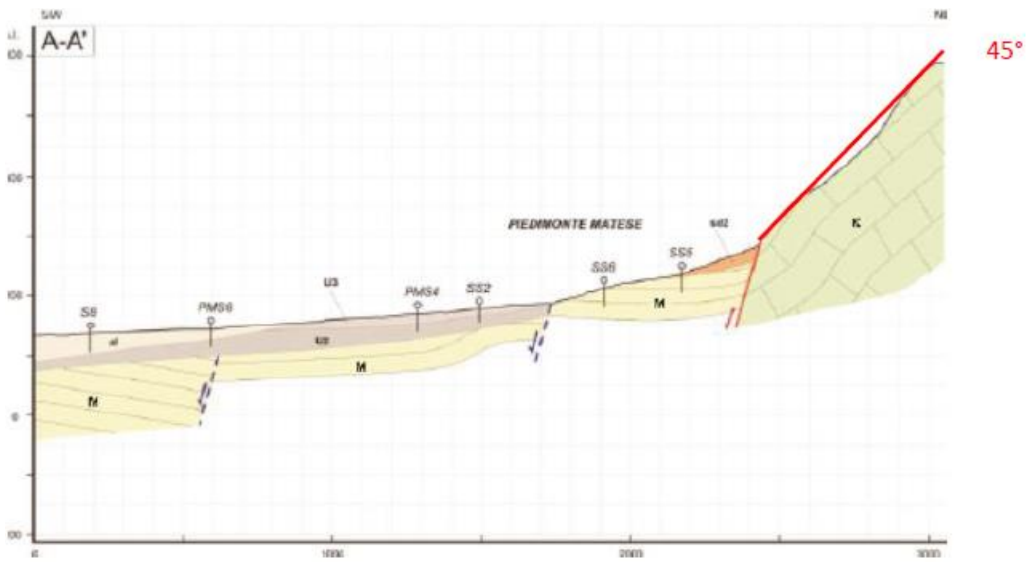
Unfortunately bedding data are only a few, due to poor exposures. We agree that sections are poorly constrained from this point of view.

The attitude of bedding has been drawn and laterally extrapolated/interpreted in order to preserve as much as possible the cut-off angles across faults. This is a basic structural geology rule.

But we understand this criticism. In the revised version, we will draw attitudes only where reasonably constrained.

Furthermore, I evaluated the dip of the Mt. Olnito slope which resulted to be of 45°. This would suggest that this slope is a dip-slope. So, if the very recent activity of the Gioia Sannitica fault is estimated by the steeper lower sector of the Mt. Olnito slope, I think this interpretation is not correct.

This criticism is not clear to us. The activity of the GF has been evaluated on the basis of all the data collected in the field and described in the paper.



CROSS SECTION B-B'

- What are the evidences to infer the location of a Quaternary fault to the NE of the Gioia Sannitica fault?

41) We have structural evidence from filed mapping (fault plane and fault zone) plus geomorphic lineaments in the LIDAR topography and U1 deposits in the hanging wall.

- Your data do not allow to infer depth of the Quaternary bedrock, so you should stop your cross section at the base of the boreholes (the same for all the cross-sections)

See reply N. 39

- Again, your data do not support the presence of an inferred Quaternary fault to the SW of San Potito Sannitico

See reply N. 39

CROSS SECTION C-C'

- Stratigraphy of the borehole S"S93" is not reported in the borehole stratigraphy figure

42) You are right. It is missing by mistake. It is a water well. It will be added in the figure.

- There are no evidences to infer the presence of the dashed blue fault.

See reply N. 39

- What data do you have to infer the tectonic contact between Mesozoic carbonates and Miocene units along blue fault?

See reply N. 39

CROSS SECTION D-D'

- Again, there are no evidences to infer the presence of the dashed blue faults.

43) The blue fault (again dashed because poorly constrained) is added in order to explain the difference in elevation between the NE-dipping Cretaceous rocks in the footwall at ~275 m a.s.l. (Auduni) and the NE-dipping Cretaceous rocks in the hanging wall ~250 m a.s.l. (east of Carattano), and considering the strike-lines from bedding attitude.

- How can you hypothesize the presence of the Varicoloured Clays? Looking at your map, they seem to be limited to the Petrella area. See also comments in the "General comment" section.

44) In correspondence of the geologic section the Varicoloured clays are covered by Quaternary sediments. So we don't know from surface geology, but according to the elevation of the boundary, the section should intercept this boundary.

In the stratigraphy of the S"S93" well, blue/gay clays were drilled. We interpreted those as VC. But, we will add a question mark in the geologic section.

CROSS SECTION E-E'

- The map shows, in the surroundings of Auduni, the presence of U2 deposits whereas U1 deposits are reported in the cross-section.

45) This is a mistake. Thank you. We will correct this mistake.

- This cross-section is not very clear as there are no bedding data in its surroundings that allow the infer the complex geological setting, with folded Miocene and Mesozoic units.

46) This is drawn in order to be consistent with the structural setting of the adjacent section D-D'. Please note that the fold in section D is drawn in order to account for the available bedding attitudes (e.g., NE-dipping K unit) in the

hanging wall of GF, and in order to preserve cut-off angles across the GF. Anyway, we will check carefully the consistency of the cross section.

See also reply N. 39.

- The Gioia Sannitica fault is inferred in the main map whereas it is certain in this cross-section.

47) Accepted. This is a mistake. We will modify the cross section.

- Why you did not project borehole GSA4 in cross-section E-E'?

48) Accepted. We will project the well on the section.

CROSS SECTION F-F'

- There are no evidences to infer the presence of Quaternary faults to the SW of the Gioia Sannitica fault.

49) The blue fault (again dashed because poorly constrained) is added in order to explain the difference in topographic elevation between the footwall, where Miocene siliciclastic deposits crop out, and the hanging wall, where Quaternary deposits accumulated.

BOREHOLE DATA

- You distinguished, in the main map, SD1 and SD2. I agree, but most of the boreholes drill another SD unit, that is not listed in the legend. Looking at both the map and the cross-sections, I guess it should be the Miocene unit. Borehole data must be consistent with geological map and cross-sections to avoid any misunderstanding.

- The unit CC drilled in boreholes S14 and GSA5 is not listed in the legend. What is it? Furthermore, authors could project borehole S14 on cross-section A-A'.

- Borehole GSA7 drill SD1 deposits whereas it drills SD2 deposits in cross-section D-D'.

- Boreholes S9, S12 and PMS2 are placed in an area where U2 deposits are present whereas they drill U1 deposits.

- I cannot find boreholes PMS6 and SS1 in the main map. Where are they located?

- Boreholes SS5 and SS6 drill SD unit in the borehole stratigraphy figure whereas they drill Miocene unit in cross-section A-A'. So, is the SD unit composed of debris slope deposits or the SD unit correspond with the Miocene unit?

- Stratigraphy of borehole GSA1 indicates that it drills, in the upper part, U3 deposits whereas it is in an area where U1 deposits are present. Furthermore, you indicated that the WTT tephra is present within the U2 deposits, whereas, in borehole GSA1, the WTT occurs within U3 deposits.

- Regarding the WTT, how can you establish that this tephra layer is the WTT? Did you sample it in all boreholes and performed tephrostratigraphic analysis? Or the classification of this tephra layer as the WTT is just your assumption?

Please, see our comment n. 27 and 28. The legend of the Figure will be changed accordingly.

- Again, what is the SD units in boreholes GSA3, GSA4, GSA5, GSA6, GSA7, SPS1, SPS2, SPS3, SPS4, SPS5, SPS6 and SPS7?

- Stratigraphy of borehole GSA6 indicates that it drills, in the upper part, SD2 deposits whereas it is in an area where Miocene deposits are present.

- Boreholes 158202, 163920 and S"S93" and reported in the main map but are not detailed in the borehole stratigraphy figure.

50) You are right. There are several inconsistencies between borehole stratigraphy in the inset and map and sections. This is due to a change in the acronym of the geological units, for making them more understandable by English readers, but that was not uniform by mistake. For example: CC (Calcari cristallini) in borehole stratigraphy is K in the map and sections; SD (Siliciclastic deposits) in borehole stratigraphy is M in the map etc.

Thank you for pointing this out.

We will check carefully all these observations during the revision of the manuscript.

TECHNICAL CORRECTIONS

Line 84: type NE-SW instead of SW-NE

?

Line 168: delete "which was".

Corrected.

Line 173: the letter "o" of Alo, Feo, Sio, should be written in capitals.

No. Lower case is correct. They are acid ammonium oxalate extractable.

Line 241: it should be Mt. Acero

Corrected.