Solid Earth Discuss.,
doi:10.5194/se-2016-77-RC2, 2016

Solid Earth Discussions

Interactive comment on "Defining a Mid-Holocene earthquake through speleoseismological and independent data: constraints for the outer Central Apennines (Italy) seismotectonic framework" *by* Alessandra Di Domenica and Alberto Pizzi

Anonymous Referee #2

Received and published: 31 July 2016

Dear Editor, I carefully read the paper by Di Domenica A. and Pizzi A. It deals with the identification and analysis of karstic features such as broken speleothems, sudden collapse of ceilings and fall of stalagmites along preferential orientation in a central Apennine cave (the Cavallone Cave), defined to by caused by past large magnitude local seismic events, that is, speleoseismological evidence. The paper presents nice evidence and data, which are clearly separated by their interpretation. The text is quite well written (some modifications are required in the annotated version of the

manuscript, appended) and organized. Nonetheless, below you can find some major concerns on both general and specific aspects that make the paper not acceptable for publication in its present form, but only after major revisions are made. Indeed, some conclusions of the work are not supported by data. Other minor comments are reported in the annotated manuscript.

1) The short, low-displacing and down-dip (few tens of metres depth) ending normal faults reported in Figures 2a and 2b can represent extensional features (trenches and fractures) related to the occurrence of large scale gravitational mass movements that affect the southern slope of the Maiella Massif. In fact, these are comparable by all means to other similar features identified by Bianchi Fasani et al. (2005) few hundred meters southwest-wards, just upslope the head of the Lettopalena rock avalanche. These elements have been interpreted as related to of ongoing tensional gravitational stresses acting on the portion of the slope not collapsed yet. The author should discuss about this aspect, as it could definitely be the cause of some of the observed deformations in the Cavallone cave, such as tilting of speleothems and ground cracks and fissures, horizontal displacement of stalagmites, thus suggesting recent large scale gravitational instability also in this portion of the Maiella Mts. Southern slope. In my opinion, this could be a very interesting outcome of the work.

2) The authors defined that time interval (derived by radiocarbon dating; see figure 6) during which the analyzed stalagmite should have been broken by a seismic event matches with that of 1) an activation event of the Sulmona fault defined by Galli et al. (2015) and 2) the occurrence of the Lettopalena landslide, defined by Paolucci et al. (2001). Nevertheless, in the latter case, by calibrating the radiocarbon age (defined as "about 4800$\pm$60 BP") reported by Paolucci and co-authors, it derives a time span which does not fit that of the studied stalagmite breakage. Indeed, in their original paper, Paolucci et al. (2001) did not state that the reported age was a calibrated age. Instead, since it has been expressed as "age before present (BP)", dating has to be conceivably considered as "radiocarbon age". It follows that:

- Lettopalena landslide (Paolucci et al., 2001): 4800±60 BP (radiocarbon age) => 5651-5448 BP/5386-5328 BP, calibrated, 2σ

- Broken stalagmite (this work) => GC/1 (pre-seismic): 4815-4525 BP, calibrated 2σ GC/2 (post-seismic): 4840-4645 BP, calibrated 2σ

No overlap between the chronologies of the two phenomena appears!

Based on this, the association of the Lettopalena landslide to a seismic event that might have also caused the stalagmite breakage has to be removed. Otherwise, the authors must provide robust references alternative to the interpretation above, being this a fundamental aspect for the work objectives.

3) The authors defined the activation of a single 40 km-long seismogenic source, comprising the Sulmona fault, the Western Porrarra and the Palena segments, during a supposed ∼M 7 earthquake, as causative both of the dated stalagmite breakage and of the Lettopalena landslide. This is not acceptable because based on at least questionable hints, as I explain in the following:

a) The authors defined that, according to the available literature, more than 40% rock avalanches all over the world are triggered by M>6.5 earthquakes. According to Gori et al. (2011) and Galli et al. (2015), the 23 km-long Sulmona fault is potentially able by itself to produce M 6.6-6.8 earthquakes, that is, large enough to trigger rock avalanches such as the Lettopalena one. As a result, there is no "seismological need" to invoke the synchronous Sulmona-Western Porrarra-Palena faults activation as 40 km-long seismogenic source.

b) No presently available geological data define that the Sulmona fault, the Western Porrarra and the Palena segments are the expression at surface of the same seismogenic fault, able to rupture with M 7 earthquakes! Indeed, no paleoseismological data show synchronous activation of these structures, neither structural hints suggesting hard linkage between them exist. Instead, the available literature depicts the Sulmona

fault as ending close to Pacentro.

c) The fact that the Western Porrarra and the Palena segments are closer to the investigated sector than the Sulmona fault cannot be the ground to suggest the synchronous activation of these structures for at least two reasons: first, as authors exposed, peculiar seismic response in the analysed cave and the presence of structural weaknesses in the stalagmite structure may "amplify" the effects of a distant seismic event; second, by taking a look at the distribution of the archaeoseismological evidence provided by Ceccaroni et al. (2009), the II century AD earthquake, referred to the activation of the sole Sulmona fault (Galadini and Galli, 2001; Galli et al., 2015), caused heavy damages to the Roman settlement of Iuvanum. The site was located even farther to the south-east with respect to the cave, and along the same direction with respect to the Sulmona fault.

d) The attribution of a Maw of about 6.3 to the II century AD earthquake (and claimed by the authors to be the magnitude attributable to earthquakes potentially caused by the Sulmona fault) has to be taken with caution, as it is a rough estimation referred to a seismic event dated back to about 1800 years by the present, just based on epigraphic sources. Instead, the archaeoseismological data of Ceccaroni et al. (2009) and paleoseismological data (Galli et al., 2015) refer this event to the activation of the Sulmona fault, responsible for about 1 m mean surface offset per event. This observations, coupled with the surface fault length (Gori et al., 2011), provide estimates of the maximum expected magnitude in the order of 6.6-6.8.

Therefore, in the whole, because of the too vague and weak hints that would support the hypothesised Sulmona-Western Porrara-Palena structure activation during the supposed ∼4700 yr BP seismic event, my comment is to remove this part from the manuscript, being it just based on too speculative and incomplete assumptions. This is relevant for the strong implications that such an hypothesis could have in terms of the seismogenic potential of the Maiella Massif and Sulmona region. The authors should simply state that the observed speleoseismological observations are consistent with

the occurrence of the Lettopalena landslide in terms of potential triggering magnitude, as they both suggest the occurrence of M>6.5 earthquakes in the area.

4) The absence of rock avalanches triggered by the 1706 earthquake (Mw 6.83; Locati et al., 2011) (page 10, lines 20 to 29) to claim that the supposed 4770±30 yr BP earthquake (supposed by the authors to have caused the Lettopalena landslide) has had a larger magnitude is not acceptable. Apart from my chronological concerns at point 2, catastrophic rock slope failures occur when the internal cohesion forces of rock masses are overtaken and an earthquake, even strong, may or may not cause collapse, depending on the stress conditions of the "shaken" rocks at the moment of the earthquake occurrence, or on the distance from the epicentral area (a moderate earthquake just underneath a gravitationally unstable rock mass may cause landsliding whereas a large earthquake several km far may not). Within this light, for instance, it is worth noting that the Mw 7 1915 earthquake, in central Apennines, did not trigger rock avalanche as large as that of Lettopalena, and the earthquake has had a magnitude similar to that hypothesised by the authors for the supposed 4770±30 yr BP seismic event. Therefore, this part should be removed as too speculative.

5) The fact that the observed coseismic secondary effects "HAVE TO" be referred to the activation of extensional faults rather than of compressive faults in this part of the Apennines deserves a much much higher degree of uncertainty. Indeed, according to the available information, debate on the possible source of the 1706 earthquake (Mw 6.8), that had epicenter exactly coinciding with the area under investigation, is underway. Some authors, indeed, tentatively refer this event to the activation of the Abruzzo Citeriore Basal Thrust (DISS Working Group, 2015, and references therein), a deep thrust plane underneath the Maiella anticline. Robust geological data supporting this hypothesis are not provided to date, though. Therefore, this part of the manuscript should be changed accordingly, in order to make it sound just as an hypothesis.

Sincerely

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Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/se-2016-77/se-2016-77-RC2-supplement.pdf

Interactive comment on Solid Earth Discuss., doi:10.5194/se-2016-77, 2016.