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Comment on nhess-2021-399

Anonymous Referee #2

Referee comment on "A morphotectonic approach to the study of earthquakes in Rome"
by Fabrizio Marra et al., Nat. Hazards Earth Syst. Sci. Discuss.,
<https://doi.org/10.5194/nhess-2021-399-RC3>, 2022

The paper addresses a topic that is relevant, due to the great vulnerability and exposure that characterizes Rome from a seismic risk perspective. The high number of people who lives, works and spends holydays in this city, the critical infrastructure that characterizes the Capital of Italy, along with the value of its ancient building stock and cultural heritage, makes a detailed evaluation of the seismic hazard of this area very important to be carried out.

For this reason, the paper is in principle of interest to the nhess audience and would deserve to be published. However, it is not ready for publication, as described below. In general, more care is needed in the drafting of the text, the citation of bibliographical references and the content of the figures.

General issues

1.

The general frame depicted in Section 2 would benefit from a wider description of the seismic events that hit Rome in the past, also including those occurred in "the period of ancient Rome, as well in the Early Middle Ages" (lines 84-86). This information can be easily retrieved in the available seismic catalogues and, in general, in the literature. This would show that comparable damage (e.g., intensity VI-VII) has been caused both by strong earthquakes with a far epicentre, e.g., in the Apennines chain, and by moderate events much closer. For this reason, defining the potential size of these moderate events significantly contributes to a better definition of the seismic hazard of the area.

I suggest also adding a new figure with a graph or a table representing the seismic history of Rome.

In the same line, it would be useful to know the magnitude of the instrumental earthquakes. Are there any comparable with the 2020 MI 3.3 event? In case, they could be outlined in Figure 1. Concerning this figure, there are also some details that need to be fixed: the blue star of the 2020 event is not so evident; there are letters A-B and C-D that are not defined in the caption; in the legend, Bulletin has two t; neither in the caption nor in the text a definition of G.R.A. is reported (only in Figure 8, at the end of the paper). Could you add the stream of the Aniene river? It would help compare this one with the other figures.

2.

The description of the structural setting could be more precise, even without being longer, and for this purpose an improved Figure 2 would be of great help. In general, this figure needs to be rethought for an international audience unfamiliar with the tectonics of Central Italy. Which is the age of the tectonic features reported (thrusts, normal and strike-slip faults)? Which are active today and which not? Why the extensional stress field in the Apennines has a different graphic than that along the Tyrrhenian margin? Is the stress field of the strike-slip faults no longer active (the retrieved focal mechanism has an opposite kinematics)? In the text, you talk about the volcanoes of the "Roman Province", whereas in this figure you represent the volcanic districts of the Tyrrhenian Sea margin. Could you homogenize the names, also highlighting the Colli Albani? Could you add a box corresponding to figure 1 and a graphic scale? Could you add any references in the figure caption?

I suggest redrafting Figure 2 and then rewriting coherently the structural setting.

Concerning the morphological setting, Figure 3 is not centred on Rome and does not include the 2020 MI 3.3. I suggest reframing the figure, expanding it to the North and to the West.

3.

The way the seismicity is addressed in the paper should be better organized. The seismicity of the area is described in Section 2. Section 4, called Seismicity, describes the data collection, but it also shows a part of methodological description, in particular the hypocentres relocation. It does not mention, however, the computation of the moment tensor solution, that is addressed directly in the Results, but is shown in Figure 4, to which Section 4 refers. Now it seems that the focal mechanism in Figure 4 comes from the literature.

Moreover, why only two out of the four networks described in the text at page 8 are shown in Figure 4? I suggest including all the networks in the figure 4, distinguishing them with different colours and/or symbols in the maps.

I also suggest adding a table with the list and the parameters of the MI 3.3 event and the 4 aftershocks (magnitude, depth, etc.) mentioned at page 14. Why only 2 out of these aftershocks are shown in Figure 4?

This example highlights, as a more general point, the need of a clearer organization of the text. I suggest reviewing the Table of Contents of the paper, separating better the introductory framework, the data used, the methodologies adopted, and then results and discussion. Within each of these general topics, subsections regarding the different disciplines (seismology, geomorphology, tectonics, etc.) need to be included. Otherwise, as it happens now, you have a mix of literature, data and interpretations in many parts of the paper, and this does not help the reader.

4.

The morphotectonic analysis of the drainage network (Section 6.3) is a huge work, supported by several detailed figures. However, the tectonic lineaments that are present in all these figures do not allow a proper view of the data analysis, whose details are masked by the black lines. Therefore, on the one hand, I suggest removing the tectonic lineaments from Figure 6 a) and b) and Figure 7. On the other hand, the Authors should add a new figure where all the main results from the previous figures are reported (fluvial elbows, knickpoints, etc.) along with the interpreted tectonic lineaments.

Moreover, it is not clear why the Authors draw a N-S lineament near the MI 3.3 epicentre even though in figure 5 A) it falls within a zone characterised by NE-SW streambed analysis (see also lines 346-349 at page 15).

5.

The most critical point, in my view, is in the concept of "seismic intensity" that, according to the Authors, the analysed faults have now compared with that they had in the geological past. This concept, along with the seismic intensity of the area related to the Pleistocene stress field, is present since the beginning of the paper (page 7, from line 153), up to the Discussion.

I think that this concept should be totally revised. In general, seismogenic faults are not characterised by a "seismic intensity" but, rather, by a "seismic rate", that can be estimated if you are able to recognise one or more seismic events that they generated in the past (for instance from historical and/or palaeoseismological record), associated with a "geological slip rate" from structural, geomorphological and stratigraphic data.

In this study, the faults analysed are buried and blind, there are only hints of their activity at surface. Therefore, there is no information to assess which is their current and past activity and seismic rate.

Moreover, there are no data to discuss the "dimension" of the current and previous stress fields, although it is clear that the Middle Pleistocene tectonic activity, also responsible for the development of the volcanic district, was much more developed than the current tectonic activity.

I think that a scheme or a table is needed reporting (with refs) the orientation, kinematics and age of the different stress fields (including the strike-slip one of figure 2) that affected the study area through time and that are relevant to this study. Based on this, the inception, development, segmentation and possible reactivation of faults can be framed and discussed. This will allow the Authors to strengthen their idea that segmented faults with limited tectonic activity can be assigned a seismogenic potential for events with moderate magnitude.

Details

Page 3, lines 79-81

I suggest describing what the Greater Rome is: the Province of Rome?

Page 6, lines from 139

The name of the volcanic complex should be added.

Page 7, lines 148-151

This part should be described better, and some references added.

Page 7, lines 157-159

This sentence (Moderate earthquakes ... almost exclusively ... in the volcanic area) is not supported by the current figure 1, where moderate earthquakes are reported also in correspondence with the city. Maybe, the modified Figure 1 could help clarify this part.

Page 7, line 170

"At depth": which depth? Could you characterize better the third dimension of these faults?

Page 8, line 186

The Italian Strong Motion Network (RAN) should be mentioned along with its owner/operator, as you did for RSN and INGV, RSA and Lazio and Abruzzo regions, IESN as an amateur seismic network. RAN is operated by the National Civil Protection Department.

Page 10, line 206

"Carried out" rather than "carried on".

Page 11, Figure 5 B)

Add in the caption what the light blue and yellow lines are.

Page 12, lines 240-249

Here the Authors could explain the reason why a role played by lithologies can be ruled out. This concept, without explanation, can be read at page 16, line 367. Maybe a figure with a geological sketch could be added.

Page 14, lines 296-306

At page 7, line 170, the Authors state that the fault planes at depth do not propagate to the surface. Therefore, here some explanation is needed on the mechanism causing fault induced disturbance on some elements of the drainage network.

Page 14, line 325

I strongly discourage the use, here and in other parts of the text, of the term "Antiapennine" as a synonym of a NE-SW direction, and of the term "Apennine" as a synonym of a NW-SE direction. Please, refer to NE-SW, etc.

Page 16, line 363

Check the number of the figure.

Page 16, line 375

Check the orientation.

Page 20, line 396

Remove "by the INGV": references are already there.