

Comments and Responses

Comments:

You respond that your goal is not seismic hazard (response 1, reviewer 1), yet your abstract concludes with “Our work can provide a perspective time-independent seismic potential of faults” (l.16-18, p.1), which is a key ingredient of seismic hazard assessment. You claim - and your simplified method attest - that major implications for seismic hazard for Italy are not justified. I agree with that. So why are your last claims in abstract and conclusions about seismic potential relevant for seismic hazard assessment rather than about testing the usage of easily accessible fault databases on a national scale. I think such key pieces of formulation should be updated to reflect your goal better. I would suggest to not try to suggest implications beyond what the quality of your tests allows and remember the societal and political sensitivity such statements carry. Such worries are clearly stated by Valensise et al and reviewer 1, so please ensure that you remain concise and accurate about what your very simplified method can do and what it can not, particularly in the conclusions and implications you draw.

Done. We have reworded the final part of Abstract (Lines 14-19 on Page 1) and Conclusions (Lines 5-6 P. 17) omitting concepts that may be somehow connected with the seismic hazard.

You write that you “benchmark” scaling relationships against catalogued magnitudes (l. 3, p.1), but I do not think you reach such a high level related to a scientific interpretation of that word. I rather think wording like “compare” is more appropriate.

Done. We have used “compare” instead of “benchmark”. Please, see Line 3 P. 1.

p. 6, l. 11: You write you consider “all known faults”, while it is very clear from the long list of references from reviewer 1 and SC1-Valensise (p. C4-C5) that this is not the case. I think all descriptions of your fault database should be updated to reflect the actual situation accurately, while acknowledging other approaches and literature. For clarity, I do not think your database needs to be updated as you need to make assumptions to generate a database on some grounds. However, I think your text should be distinctly updated to acknowledge that you miss or miss-represent a distinct number of otherwise known faults. The worries of these experts should not be dismissed, but rather acknowledged appropriately.

Done. We have better explained what we have excluded from our database. Please, see Lines 14-17 P. 6.

From that same perspective I think the worries expressed by Valensise as summarized in the centre paragraph on C5 (SC1) should be better articulated in the paper. For example, for assumptions 3 and 4 in section 4.2.

Reviewer 1, Comment 6

I think it is fair to answer to the critique of missing faults with careful examples given by the reviewer. I understand your simplified method resolves the double counting aspect, but still there are two examples well total length may be underestimated. Addressing this limitation in the paper also gives the reader a better perception of the completeness of the databases you use. Here just saying they are as complete as possible is not really meaningful. Rather write exactly what you do (as you do), but than acknowledge potential though understandable flaws properly.

Dear Editor, the limits connected with our methods are largely addressed (Pages 9-11) and have now been improved following your comments (please, see Lines 12-23 P. 11). Valensise et alii mention two earthquakes and related faults.

The Messina 1908 earthquake possibly occurred offshore (Messina Straits) in a region of high erosion and deposition due to steep sides and related slides and turbidites. The causative fault has never been found with certainty (Billi et al., 2008, GRL, doi: 10.1029/2008GL033251) and hence I do really think that this is not a good case to mention to emphasize/explain our limits. Moreover, the magnitude of this earthquake is an assessment.

Concerning the 1915 Avezzano earthquake, we would like to stress (1) that the related magnitude 7.0 is an estimate based on damages and not an instrumental measure, and (2) that the causative fault in our database (the Fucino Fault) is 15.85 km long, corresponding to a FLEM (Mw) of 6.25 (according to Leonard, 2010). This second case has now been properly mentioned in the manuscript as you suggest (Lines 17-23 P. 11).

The double counting problem is addressed at Lines 5-10 P. 9 and the completeness of our fault database is now addressed/improved (following your comments) at Lines 14-17 P. 6.

Your analysis does not account for rupture jumps that could happen from one fault to the next, as suggested by the "segmentation" examples of e.g. 2016 M7.8 Kaikoura earthquake. That would mean that earthquake with larger magnitudes than you estimated could occur. This is quite a likely scenario with major implications if that is too happen. However, you describe this limitation in only line (p. 11, l. 4) without any scientific references or appreciation of the process or consequences. I think that should be included.

Done. We have better explained this problem and mentioned the related paper (Cesca et al., 2017). Please, see Lines 12-17 P. 11 and the added reference to Cesca et al. 2017 work on EPSL. Moreover, also the Fucino case (see our previous response) is a case of multiple coseismic rupture. Also this concept has been now integrated in the new version of our manuscript (Lines 20-22 P. 11).

I also very much appreciate you openly sharing the updated Italian fault database. However, I am also not able to find and download the fault database from the link given in the manuscript and rebuttal letter. Could you please ensure a long lasting and secured access?

[http://pmd.gfzpotdam.](http://pmd.gfzpotdam.de/panmetaworks/review/924b171fd21c78f295d58a7e9e321e8ad07667ab6201634b23d3cb5a3f170d10/)

[de/panmetaworks/review/924b171fd21c78f295d58a7e9e321e8ad07667ab6201634b23d3cb5a3f170d10/](http://pmd.gfz-potsdam.de/panmetaworks/review/924b171fd21c78f295d58a7e9e321e8ad07667ab6201634b23d3cb5a3f170d10/)

We understand the concern. The database is now open at this link:

<http://pmd.gfz-potsdam.de/panmetaworks/review/924b171fd21c78f295d58a7e9e321e8ad07667ab6201634b23d3cb5a3f170d10/>

Moreover, consistently with the policy of the Potsdam (GFZ) Repository, this database will be freely accessible through the DOI and related reference (Petricca et al., 2018) as soon as the present paper will be officially published on Solid Earth. This is the policy of the Potsdam (GFZ) Repository. Please, note that we have used the same repository and policy for a recent paper published on Solid Earth (<https://www.solid-earth.net/10/741/2019/>).

I agree with Reviewer 2 (comment 10) and Valensise et al (comment 13) that using log-scale quantities such as magnitude introduces a bias and potential for misinterpretation of the size difference (see values in comment 13).

You agree with this comment, yet I do not see the suggested changes in the updated manuscript (l. 11, p. 13). I do not see exact numbers.

However, I find it more important that major differences often of 2 magnitude values (Fig. 10) are described as “having a good agreement ...” (p. 1.), such that “this gives credibility to the used scaling relations” (p.1, l.16) or that these “can be benchmarked” (p. 1, l. 5). Based on the information in your abstract a 2 standard deviation range includes a magnitude range of 2×1.47 is almost 3 magnitude values, which require very large fault length differences. I think this rather shows large discrepancies, which could also be used to argue that something opposed. I have the impression you take care of this uncertainty by in the discussion saying that they show “either good agreement or some differences are observed”. I do not think that is very accurate. Could you please improve your formulation in such conclusions, abstracts and in the results?

We understand the Editor point and, consistently, we have omitted all adjective and adverbs providing a qualitative and subjective opinion on our results. Please, see our changes at Lines 14-18 P. 1; 22 P. 13; 31 P. 13; 1 P. 14; 8 P. 14; 27-28 P. 16; 2 P. 17.

I also like the suggestions to you seismic moment instead, which can indeed not be calculate directly for all earthquakes, but scaling relations similar to those used also exist to portray seismic moment. Doing that would better show the variation in values and would lead to less bias.

Although we understand the validity of this suggestion, its feasibility is impossible in our case due to the fact that, for many earthquakes (particularly in historical catalogs but also in old instrumental catalogs), independent assessments of the seismic moments do not exist. This makes impossible the use of seismic moments in our approach, which includes historical and instrumental earthquake catalogs for an entire nation. In other words, the only parameter available for the entire nation over the historical and instrumental periods is the earthquake magnitude and not the seismic moment.

In response to Valensise (SC1, p. C7-C9 & comment 16) I think you should also stress more the low probability of occurrence for such large earthquakes. Common people that do not in detail read the whole paper should readily understand this low probability to prevent confusion and potential follow-up miscommunication at all levels. The emphasis on such information should thus be done in the main text at multiple key locations and by extending the disclaimer with more information than this being for scientific purposes only (e.g., the missing component of probability of occurrence and risk that is missing in all such figures). Considering the sensitivity of this topic for society I think such additions are useful and required.

Done. We have added a statement at the end of Intro and in the Acknowledgments. Please, see Lines 22-25 P. 3; 22-27 P. 17.

Your new section structure makes it hard for the reader to understand your flow and appreciate what you did. Section 5 describes “Results and discussion” all together in one section. Section 6 then describes a “Statistical test of FLEM values”, which is again an analysis of the results. Could you update your structure and section headings a more standard scientific paper setup to facilitate the reader in finding the information needed?

Done. Please, see Lines 1 P. 12; 23 P. 14.

As a follow-up on comment 17 by Valensise:

I agree lengths in Fig. 7 and 8 seem to represent the required fault length and there are no isolated cells with too large a magnitude

However, in Fig. 9 for the observations I do see quite many of such isolated cells, as I guess these values have ended up only in the cell of the hypocenter. This means that when assessing the difference in Fig. 10,11, and 12 you are also including this missing information and are thus underestimating Mcatalogue and overestimating deltaM.

Dear Editor, we have re-checked Figs. 7 and 8 and we do not see isolated cells characterized by high FLEM values (and therefore by long faults). In particular, we refer to red cells that are never isolated in Figs. 7 and 8. Please, note that cells with lower FLEM values (orange-yellow to blue cells) are characterized by shorter faults and hence can occur also as isolated. In other words, isolated orange-yellow to blue cells are compatible with the used method whereas isolated red cells would be incompatible (they are not isolated indeed) with our method as pointed out by Valensise et alii. We are obviously available for further improvements and clarifications.

- Your sentences can be quite long (e.g. p. 14 l.2-7). I recommend another look at the sentence length and language of the manuscript.

We have polished a bit the English over the entire manuscript and tried to split all long sentences.

- Occasionally (p. 16, l. 15; p.1, l. 14) you still use scaling law instead of scaling relation.

There are no “scaling laws” in our manuscript. We have found three “scaling equations” that have been changed into “scaling relationships” (Lines 16 P. 10; 22 P. 10; 28 P. 10).

Thanks a lot for your efforts on our manuscript

Sincerely

Andrea Billi and co-authors