Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-212-RC2, 2021

Solid Earth
Discussions
Open Access

Interactive comment on "Distributed faulting following normal earthquakes: reassessment and updating of scaling relations" *by* Maria Francesca Ferrario and Franz Livio

Anonymous Referee #2

Received and published: 16 February 2021

Review of the paper: Distributed faulting following normal earthquakes: reassessment and updating of scaling relations by M. F. Ferrario and F. Livio

The paper presents new (updated) empirical regressions for distributed surface rupturing during normal faulting earthquakes based on an updated dataset of normal faulting case studies. The results have implications for probabilistic fault displacement hazard analysis (PFDHA).

The basic work on PFDHA is Youngs et al. (2003), where the empirical regressions for distributed faulting were based on a limited number of normal faulting historical surface ruptures. Since then, there are not published works that implement such empirical re-

Printer-friendly version

Discussion paper

gressions for normal faulting. The work presented here is certainly of interest in the international community working on PFDHA, and in general in the Solid Earth community interested in natural hazards. Therefore, in my opinion the paper deserves to be published.

The paper is well organized and well written. The figures are good and self-explanatory. I appreciate that the basic data are made available as shapefiles (some comments below).

Please, consider the following comments during the revision of the paper:

1) Nomenclature: Primary vs Principal. The reference literature on PFDHA uses this nomenclature: Principal and Distributed (Youngs et al 2003; Petersen et al., 2011). Though this is a very minor comment, it would be nice if all the specialized literature will use the same nomenclature.

2) Method (gridding) (Lines 100-102, Fig 2a). In order to make the results reproducible, can you be more detailed in describing the geometry of the grid and the method for calculating distances? Did you consider the same maximum distance from PF for all the events? How far from PF (20 km?)? Which criterion guided the choice? Was the grid (and the sides of the squares) always horizontal/vertical? Or rotate with fault strike? PF-distance: is that the shortest distance between the PF line and the centre of the cell? Adding a real case in Fig. 2a can be explicative and help the reader.

3) Extrapolation of regressions (lines 174-175). Why did you extrapolate away from observations?

4) Role of dataset age and M. You found no systematic bias between the pre- and post-2000 datasets (lines 179-181). Looking at your data, I agree with this observation. But I suspect that you do not see the differences in the analysis because of the small number of modern data compared to older data and the coarse grid size, which smooths differences. I suspect that this can influence the possible dependency on M, as well. I

Printer-friendly version

Discussion paper

think this point (possible bias due to methodology of analysis) should be addressed in the discussion.

4) Distributed faulting in the near-field (< 1 km). The regressions are cut at distances shorter than 1 km for mathematical reasons that I can understand. But, what about the 0-1 km distance, where the highest number of distributed ruptures are observed (highest hazard)? In the discussion you deal with this point (lines 210-220), but it is not clear to me the message: are you suggesting the empirical-probabilistic approach from global data is not applicable in the 0-1 km distance? From your results, what is the suggestion to practitioners for the 0-1 km distance in a probabilistic approach? Please note that the cited references (Teran et al., 2015; Gold et al., 2015; Loukidis et al., 2009; Treiman, 2010) mostly refer to rupture zone widths that are much narrower than 1 km.

5) High values at 7-12 km distance and antithetic faults (lines 242-243). Interesting observation. Did you verify if the 7-12 km ruptures effectively correspond to antithetic faults? Please, can you cite the cases where they correspond?

6) Title. I suggest to modify into 'Distributed surface rupturing during normal faulting earthquakes . . .'.

7) Data (shapefiles and Table 1): - 1915 Fucino M 7.1: the San Benedetto dei Marsi fault is considered distributed. I think you should reconsider this choice (maximum coseismic displacement was there). - 1954 Rainbow Mountain + 1954 Stillwater (Pezzopane and Dawson, 1996): The principal faults of the two events overlap for a large portion, but they have different traces (mainFaults in the shapefile). Is there something wrong? - 1980 Irpinia, 1997 Colfiorito, 2009 L'Aquila, 2016 Amatrice, 2016 Norcia: you should cite the source of the original rupture maps, as stated in the caption of Table 1. - 2009 L'Aquila: please note that there is a database on-line the line-work of which appears more detailed than that reported in your DB. See https://ingv.maps.arcgis.com/apps/webappviewer/index.html?id=05901efc172e489f8db4198bc43bf507

Printer-friendly version

Discussion paper

(it is already in Baize et al. 2019) - 1980 Gulf of Corinth: the association of the surface rupture to the second shock only (Feb. 25 M 6.4) or to both the first and second shocks (24 M 6.7 + 25) is not straightforward. See e.g., Hubert et al, 1996 EPSL. - 1980 Mammoth Lake: very complex event. How could you identify the main fault? - 1987 Edgecumbe: the rupture sections are not in the online DB. Did you use Baize et al. 2019?

8) Equation 1: specify that ‘x’ is in km; ‘Log’ should be ‘Ln’.

9) Table 1, caption: cite Equation 1.

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-212, 2020.