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Discussions

Interactive comment on "Width of surface rupture zone for thrust earthquakes. Implications for earthquake fault zoning." *by* Paolo Boncio et al.

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General comments

Boncio et al. propose a statistical analysis of the surface rupture distribution and an evaluation of the most probable width of surface rupture, occurring during thrust earthquakes. This approach, used for probabilistic fault hazard assessment of distributed faults, is a well-established practice previously performed mainly using strike slip and normal faults datasets. A systematic data review for thrust faults alone was still lacking in literature (partially made on Japanese earthquakes) a thus this work present a novelty aspect that must be considered. The approach is similar to previous studies and no particularly innovations have been proposed from the methodological point of view. An adequate discussion on fold-related faults (i.e., flexural slip faults and bending-moment

faults) has been introduced even if I would have found interesting a statistical approach also on these structures (see below) – even to exclude that a PDF can be invoked for their distribution in space. In summary, I found this work a first interesting (and necessary) review of some well-documented case studies of thrust surface faulting. These observations are resulting in a first proposal of setback distances from fault traces, that should be taken into considerations for siting purposes and by public administrations providing guidelines for land-planning. Neverthelss, some major revisions should be made. In the following, some specific scientific issues are opened to the discussion (Specific Comments) and several notes are made (technical corrections).

Scientific Comments

Firstly, I strongly suggest the Authors to add as supplementary Material the georeferenced maps they used. Ideally, the trace of the main and distributed faults could be provided, as georeferenced shapefiles or .kmz files. This could provide the original datasets that can be used by other scientists for further analysis, data checking etc. and it is one of the main objective of this kind of "data mining" papers. At the moment, no further inspection on the used dataset can be made and this is one of the major faults of the paper in the present form. A note on the methodological approach used for measuring distances. The approach depicted in Figure 1 could result in some biased measurements In fact, it is depending on the azimuth of the main fault strike, in turn derived from the chosen fault tips, fault segmentation etc. This is working well for distributed fault striking parallel to the main one but can be misleading for non-parallel faults. Why not to use a GRID-based approach (like in Petersen al. or in Youngs et al.)? This would also assure data comparison with previous works. At lines 167 – 173 some characteristics of the bending moment faults (BMF), significantly contributing in widening the WRZ, are described. Regarding this point, wavelenght of the thrust-related fold can be considered in order to recognized distant ruptures due to BMF but has not to be taken alone: these secondary structures a more related to hinge zones (and thus geometric characteristics of the fold i.e., curvature of the fold, thickness of the folded single-layer etc.) than to wavelength alone. Distances proposed to distinguish between pressure ridge anticline vs larger scale structures are just cutoff distance not discussed in their significance. Moreover, I find hard to distinguish between the two of them at intermediate scale. I think that the choice to exclude these structures from a probabilistic analysis can be right but further discussion or objective criteria are needed in order to correctly hierarchize thrust-related faults. Some attempts can be made considering structurally derived cutoff distances: e.g., depth of the sole of the thrust, axial planes (i.e. possible hinge zones) predicted by kink band modeling, etc. In any case, the Authors should provide schematic cross-sections of the considered case studies presenting BMF, so that a direct comparison can be made with the schemes in Figure 2. The best probability density function (PDF) of the distributed faults has been obtained through a commercial software (lines 174-177) but no detailed information is available on the procedure of fitness testing used by the software (a Kolmogorov-Smirnov test is cited but no scores are reported). Maybe this information should be provided as Supplementary Material. A quantitative comparison of the different tested PDF should be provided. Did you test only unimodal distribution or also multimodal? Did you tried to include also bending-moment and flexural-slip faults and fit the entire dataset with a multimodal PDF? Very few people know the Birbaum-Saunders distribution (originally thought to predict the life of mechanical parts subject to stress before failure). Some consideration should be made on the chosen PDF. I found that the statistical analytics are not well explained in the present form of the text and that maybe some other ways of data fitting should have been tested. Lines 185-197 (Figure 4) briefly describe the trend of the fitted PDF considering distances corresponding to progressively increasing cumulative probabilities of occurrence. Here, a strong statistical approach is lacking in transforming cumulative probabilities in distances proposed for setback etc. a qualitative approach is used. The Authors state that “90% probability . . .seems to be a reasonable value to cut the outliers” (line 185-186) and “. . .40% probability bounds reasonably well the zone where the most of the ruptures occur”. These statements are not quantitively constrained. If you use a PDF like the Birbaum-Saunders, that can

be characterized by a strong skewness and a long right tail, maybe outliers have to be evaluated with cautiousness. Vanegas, L. H., Rondón, L. M., & Cysneiros, F. J. A. (2012). Diagnostic procedures in Birnbaum–Saunders nonlinear regression models. Computational Statistics & Data Analysis, 56(6), 1662-1680. Provide a review of the tests that can be performed of this PDF in order to identify outliers.

Minor points: Lines 88-93: here, a brief summary of the main pertaining references is given. I suggest to add the following work: - Takao, M., Annaka, T., Kurita, T., 2013. Application of probabilistic fault displacement hazard analysis in Japan. J. Jpn. Assoc. Earthquake Eng. 13 (1), 17–36. Line 249: “and parallel to the anticline hinge”; it depends: not e.g., in transpressive settings. Line 201: “total width”: do you mean maximum? Or average? Line 202: did you tried plotting net slip instead of vertical component? Maybe the median of the width could be more clustered. Data on Figure 5 are quite scattered, maybe a bilinear upper bound can be proposed with a flat top toward the right. Line 204-205: also this part is questionable. If we admit that a positive upper bound can be supposed in the lower left of the graph (i.e., less than 200m) how do you explain this threshold distance? Intercept point to ca. 20 m of width, independent from the displacement on the main fault. How do you comment this? It is and expression of aleatory uncertainty or rather related to a geologic process? In any case this result is really important! Line 252: “first order stiffness of the folded material”. I don’t get the point. What’s a “first order stiffness”? are you referring to tensile strength or other mechanical properties of the upper layers? Please, discuss this point or rather avoid this sentence that can be misleading. Line 268-269: “cold criteria” is not appropriate. Do you mean objective? Threshold values?

âĂČ Technical corrections

Abstract should be considerably shortened. I would put a major stress on the major advances of this work and novelty, in the first paragraphs. Line 58: put AP Act in refs Line 131: suggested change – “. . .faults (type i) are reverse faults. . .” Table 1: indicate also the mapping scale of each digitized map Figure 3: should be a little bit improved both in the graphing type and in the format. A vectorial image should work best. Figure 4: alpha and beta parameters of the chosen PDF are not discussed in the text or in the caption. Some additional information should be provided and discussed: e.g.., both hangingwall and footwall datasets show similar alpha values but different beta (i.e. median) parameters.

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