

Interactive comment on “On the complexity of surface ruptures during normal faulting earthquakes: excerpts from the 6 April 2009, L’Aquila (central Italy) earthquake (M_w 6.3)” by L. Bonini et al.

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Referee #1 – Anonymous

We thank Referee #1 for his/her comments, and we respond as follows.

General comments. The manuscript se-2013-52 of Bonini et al. addresses the issue of surface faulting during the 6 April 2009 earthquake (M_w 6.3) of the Abruzzi – Apennines region of Italy. The authors collect geological, seismologic and geodetic data to analyze the fault geometry at depth (3 to 10 km) and at the sub-surface (0 to 3 km). The

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authors consider that the 2009 coseismic ruptures and related surface breaks do not represent the earthquake faulting and cannot be considered further in any seismic hazard analysis. In order to support their inferences, they perform analogic modeling with two alternatives using wet clay and dry sand media. From their modeling results, they suggest a nomenclature of 5 possibilities for the surface geometry of normal faulting earthquake taking into account their relationships to the inherited geological structures.

Reply. We never wrote that the Paganica ruptures are not coseismic breaks. We question their nature, which is an entirely different problem. Similarly, we did not state that the 2009 ruptures cannot be considered for seismic hazard analyses: on the contrary, we believe they can be used, but with much caution. We claim that the Paganica ruptures are surface faults created by coseismic surface folding, in its turn caused by the fault at depth: as such they are caused by slip along the master fault at depth, but they are disconnected from it as they nucleate at the surface and work their way downward. For this reason they can hardly be used in empirical relations because their throw is only weakly related to slip at depth; rather it is very sensitive to local geological conditions that have nothing to do with slip at depth. We believe that the nature of the Paganica fractures explains the hidden nature of the earthquake causative fault prior to 2009.

Comment #1. The presented ideas are quite attractive but flaws appear quite often and I did not go along with the data analysis, text wording (too verbose) and field data analysis. The modeling and related inferences are, unfortunately, not always supported by the data. In several part of the article, the presentation of data and interpretations are mixed in order to justify their inferences. In the absence of a thorough analysis of their analogue modeling (the main new contribution for the 2009 Aquila earthquake studies), I cannot for the moment recommend this article for publication and I suggest major revision.

Response #1. We will improve the text style and will separate better the actual data from their interpretation, especially in Section 3.

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Comment #2. In page 2 (last paragraph), they state that “The 2009 event is the best documented continental extensional earthquake worldwide . . .”. This statement seems to me excessive since other normal faulting earthquake in the Gulf of Corinth (Greece), Basin and Range (USA) and East African Rift System may challenge their point of view. On the same kind of reasoning, in page 4 (second paragraph) they come back with the “an extraordinarily detailed double difference catalogue of relocated event . . .” which is another exaggeration in the wording since hypoDD and tomoDD are now used for most earthquake aftershock studies. This style is quite often used throughout the text and affects the clarity of the article.

Response #2. What we meant by that sentence is simply that the amount of instrumental observations available for the L'Aquila event is larger than any previous normal faulting event. East African Rift System earthquakes may have been more spectacular, but we doubt they could have been recorded by the many tens of seismometers, GPS receivers and, satellites that recorded the 2009 event and its effects. At any rate, we accept the suggestion: in the next version of the paper we will rephrase this sentence and will remove any excessive wording (e.g. “extraordinarily detailed”).

Comment #3. (page 5) The authors do not seem to understand the complexity of earthquake faulting associated with moderate earthquakes ($M_w < 6.5$) and the threshold for earthquake ruptures to reach the surface. I really fear that their criticisms concerning the mapping of earthquake faults associated with moderate earthquakes do not appear as a neutral analysis of field data with consideration of the nature of superficial geological units. Similar remarks are addressed to the geodetic studies (InSAR and GPS) concerning their assumption in their modeling of a planar fault geometry reaching the surface and coseismic slip distribution; here again, their statement that the coseismic rupture did not reach the surface in 2009 suffers no alternative for them. In contrast, they provide no alternative modeling with curved fault rupture.

Response #3. We thank Referee #1 for this comment. We will add these observations in the next version of the manuscript. As for the curved faults, we will add a brief dis-

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cussion about them. We believe that our analog models (WK1 and QS1) suggest that secondary curved faults directly connected with buried coseismic ruptures are high-angle curved faults. In the L'Aquila area, presumed synthetic secondary faults (e.g. the Paganica fault) are mostly located in the footwall of the master fault (see Fig. 3a and b). This is an additional observation supporting the downward-propagating nature of some of these structures (Paganica fault) or the reactivation of inherited faults (Stabiata fault). As for the InSAR and GPS solutions, we ignore why exactly all investigators have assumed a planar geometry, but we can speculate that a) very few earthquakes have been demonstrated to have occurred on listric faults, especially for in size range of the 2009 event, and b) introducing a curved rupture plane increases the number of the unknowns in the inversion (while the number of data is fixed), potentially leading to an underdetermined or unstable solution. All of these models show a drastic reduction of coseismic slip as the rupture plane (or its upward prolongation) approaches the surface, and this aspect of the 2009 earthquake has never been questioned. Our statement that the seismogenic rupture did not reach the surface in 2009 goes along with the instrumentally detected lack of coseismic slip in the shallowest portion of the crust, and is coherent with the long-term geomorphic signal recorded in the fault-related basin. We would be glad to consider alternatives to this scheme, but frankly we do not see what a different scenario would look like, assuming that the instrumental data are not cheating.

Comment #4. In figure 2a the location of cross sections S1 and S2 (in figure 3) are hardly visible. In figures 3 a and b, red lines drawn on aftershocks may not necessarily represent the coseismic fault plane and it appears as imposing an interpretation on the fault geometry.

Response #4. The red lines in figures 3a and b are based on the aftershock distribution and on the focal mechanisms of the mainshock and of the most energetic aftershocks (Herrmann et al., 2011; Valoroso et al., 2013). We will explain better our initial assumption about the buried geometry of activated faults, but it is a fact that the plane obtained

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by fitting the aftershocks has been preferred by most investigators and has been used to fit successfully the GPS and InSAR observations.

Comment #5. Section 4 (page 6 to 9) dealing with the analogue modeling is the major part of the article and the new contribution in this article with regards with previous publications on the 2009 Aquila earthquake. Although the attempt of reproducing the crustal deformation and faulting within the 10-km-thick crustal structure of the Apennines can be instructive, the modeling experiment itself is weakly tested and consists in a limited number of possibilities that may address the issue of surface normal faulting in a complex geological background. The experiment fixes the velocity of the driving motor to a single value 0.005 mm/sec. The resolution and scaling of the successive modeling steps is poorly described. Not a single diagram of results is presented (e.g., velocity versus fault rupture formation and propagation; dip angle of ruptures versus the timing of rupture propagation, etc.).

Response #5. We will improve the explanation on the scaling assumptions. Concerning the motor speed, the 0.005 mm/s value was assumed because many investigators found that it is the most suitable rate when clay experiments are carried out in order to reproduce the upper crust behavior. The mechanical properties of wet clay are indeed time-dependent, and selecting a different speed has been shown to result in unrealistic, i.e. non-analog results (for rheological testing of wet kaolin see Cooke and van der Elst, 2012; GRL). We acknowledge the need for describing in better detail the modeling experiment. To this end we will add diagrams showing the progression of the results, for example a diagram showing velocity versus fault rupture formation and propagation.

Comment #6. In their conclusion, the five categories of surface normal faulting put forward in page 13 would be quite useful if only supported by more examples and case studies of surface faulting. It is curious that in this section, no comparison is presented with surface faulting of the 1980 Irpinia major seismic event and 1997 Colfiorito earthquake.

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Response #6. We will add more examples to validate our proposal on active faults categorization. The comparison with the 1980 Irpinia and 1997 Colfiorito earthquakes is interesting and stimulating, and it is easy for us having worked on both; we did not mention them in the first version of the paper fearing they would have been seen as inappropriate examples, since we deal with a totally different event based on analogue models that are clearly not available for those older earthquakes.

Interactive comment on Solid Earth Discuss., 5, 2043, 2013.

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