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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 #2 - Kurt Decker

General Comments. The paper provides an interesting new interpretation of some aspects of the l'Aquila fault system explaining the surface ruptures during the earthquakes (e.g. at the Paganica fault) by surface bending above a blind normal fault at depth. The authors conclude that slip during the main event occurred at a "blind" fault, which is upwards confined by a shallow-dipping former thrust fault. Faults above that discontinuity,

which extend to surface, are interpreted to result from surface bending. Their l'Aquila model, i.e., the finding that the earthquake occurred on blind fault and the interpretation that all surface ruptures are only indirectly related to the seismogenic master fault, leads the authors to an interesting discussion of the significance of surface faulting for seismic hazard assessment. The authors base their L'Aquila model on the reviews of the impressively large data set available from the L'Aquila earthquake, and the comparison of the observed faults with the results of analogue models. Modeling results (models WK1 and WK2) are used to show the possible effects of pre-existing shallow faults on a normal growth fault nucleating at depth and growing towards the surface. The paper shows convincing similarities between the evolution of the analogue model WK2, which essentially results in two separate faults/fault arrays at dept and at surface, and the fault reconstructions from the fore-and aftershocks of the L'Aquila earthquake.

Response. We thank Kurt Decker for his comments, which forced us to reconsider weak or potentially unclear aspects of our work.

Following are our replies to the issues that have been raised.

Comment #1. Although I generally accept the arguments leading to the l'Aquila model, some parts of the ms. not very convincing. These are: 1) The interpretation of a shallow-dipping previous thrust fault from aftershock lockations (Fig. 3). It is not stringent to draw the orange shallow-dipping fault in Fig. 3. Is there additional evidence from first motion studies or moment tensor solutions that highlight the activation of such a shallow-dipping fault? Do constructed geological cross-sections indicate a thrust of that orientation and at the indicated depth? (I assume that the cross section in Fig. 6 are models rather than data driven sections).

Response #1. Concerning the shallow-dipping structure locate at 3 km depth we wrote that: "Close to the upper portion of the major planar surface (at ∼3 km), some investigators recognized a sub-horizontal thrust plane inherited from the Cenozoic compressional phase (e.g. Bianchi et al., 2010; Chiaraluce et al., 2011; Valoroso et al., 2013)

(Fig. 3)." We agree with the Referee that in the present form this part of the manuscript is rather unclear. We will add a more accurate description about the evidence for a pre-existing low-angle structure. Our statement on the shallow-dipping structure at 3 km depth, however, is not an original interpretation but is based on previous studies that addressed this topic. Such interpretation is based on: 1) aftershock locations coupled with moment tensor solutions (Chiaraluce et al., 2011; Herrmann et al., 2011; Valoroso et al., 2013); 2) receiver function analyses that confirmed the presence of a discontinuity striking N 334° and dipping 20°. We coupled these seismological data with observations from recent geological maps (e.g. Vezzani et al., 2009) to propose simplified geological sections (Fig. 6) showing the low-angle structure.

Comment #2. 2) The interpretation of the second set of analogue models performed with sand needs to be elaborated. It is unclear why the authors used a completely different model setup for the kaolinite and quartz models. I see more differences than similarities between the models WK2 and quartz 2, which both should support the overall conclusions of the paper.

Response #2. It is true that WK2 and QS2 use a different setup, but we do not agree that the two experiments show different results. We believe that the WK2 and QS2 show similarities, for example when the upward propagation of the master fault in WK2 is stopped by a low-angle discontinuity. From 10 to 15 mm of total displacement (Fig. 5d and 5f), the deformation of the hanging-wall block of the low-angle discontinuity is dominated by bending rather than by simple shearing produced by the master normal fault. This is the phase that we reproduced with QS2. We will fix any possible misunderstanding, clarifying our assumptions on the initial setup of the experiments. In particular, we will extend the discussion about differences and similarities between WK2 and QS2 (see Response#8 for an example).

Comment #3. 3) In the overall conclusions the authors differentiate 5 categories of faults with relevance for hazard assessment. Although generally correct, I doubt that the classificationis widely applicable: Categories I (seismogenic master faults) and II

(inherited subsurface faults) are distinguishable e.g., in the Alps (virtually all seismogenic faults are Miocene structures), the Variscan basement N of the Alps (all seismogenic faults are Variscan structures), or the Rhine graben (Oligocene faults). A distinction makes no general sense. Categories III to V may be very hard to distinguish from each other, and from 1 and II, although their identification would be very useful for hazard assessment.

Response #3. We believe that the concerns expressed by the Referee about Category I and II are mainly due to the name of Category II. We probably phrased it poorly, but we never intended to state that inherited faults cannot be themselves seismogenic. On the contrary, we agree that several inherited faults are seismogenic and able to generate severe earthquakes as the active faults in the Alps. In Category II we meant to include only inherited faults that play a passive role because they are not favorably oriented with respect to the active stress field. We will modify the description of this category and introduce this much needed clarification. As for the actual applicability of our categories, we wish to stress that at this stage our main goal was to work on the theoretical basis for such a classification, and we all know that going from theory to practice may be difficult (if not hopeless). Just to stay in our playground, we all know what is the difference between an active and a dead fault, at least in theory, but we also know that sometimes being able to tell which is which may be very hard. Going back to our scheme, we believe that distinguishing among I, II, III, and IV is hard but not impossible. In the Apennines we observe extensional faults that are certainly inherited from the Mesozoic extensional phase, or that were generated during the Cenozoic chain buildup, and are now well exposed at the surface (see Section 2 for a discussion). Some of such faults exhibit a stratigraphic throw of several kilometers, which is inherited itself, and definitely inconsistent with the strain rate of Quaternary extension multiplied by the time elapsed since inception of the current regime. We believe that introducing examples to support the applicability of our results, as also suggested by the other Reviewer, will help clarifying some potentially unclear aspects of our speculations.

Comment #4. Chapter 4.1 2053 Line 5-9 It is unclear how the experiments were done. Have several runs of the same setup been done stopping at different cumulative displacements? Or is the sequence of deformations shown in Fig. 4 result of a single experiment, and pictures were made through a glass window laterally confining the experiment? Such a window would probably induce unwanted boundary conditions: please add explanation.

Response #4. We conducted four experiments. The WK experimental apparatus has the top and the side free. In the QS box, two lateral glass walls confine the quartz sand. The sequence of deformations shown in Fig. 4 is the result of a single experiment, and the deformed experiment is photographed after every 1 mm of displacement. To prevent undesired boundary effects, in the QS experiments we reduced the friction by polishing the glass wall with graphite powder. In the WK experiment this was not necessary because the lateral side of the experimental box is free. We will add this information in the next version of the manuscript.

Comment #5. 2053 Line 29 What is the evidence that the fault at the surface of the models are interpreted as Mode I fractures?

Response #5. We based such an interpretation on D.I.C. analyses. Through a specific software (PIVLab: http://pivlab.blogspot.it/) we calculated the displacement distribution of particles within image pairs, and then derived several parameters, e.g.: strain distribution, shear stress, etc.. The displacement field (see an example in Figs. 5a and b) shows that some structures are characterized by an opening mode (Mode I, i.e. the tensile stress is normal to the fracture plane). This is not surprising because we find these structures in the extrados of the monocline. To prevent ambiguities, in the next version we will add a figure showing this mechanism.

Comment #6. 2054 Line 24-26 A marked difference between the final products of the Kao experiments may be the location of the surface breaking fault; unlike in the no-discontinuity model (WK1) experiment the surface breaking fault in the discontinuity

model WK2 is located on the top of the “scarp” in the surface topography. Comment #7. 2055 Line 16 The wide open gash in Fig. 5g may result from strain compatibility above the concave-up fault bend of the final connected normal fault.

Responses #6 and #7. We thank the Referee for these comments and agree with him that these aspects deserve to be more clearly elucidated. We will add these observations in the discussion of modeling results.

Comment #8. Chapter 4.2 The authors state that the quartz sand experiments were performed to evaluate eventual unwanted effects of high cohesion of the wet Kao model. However, both, Kao and quartz models use quite distinct model setups which may question the authors’ conclusions. Results for the no-discontinuity model are admittedly very similar. However, results for the models WK2 and quartz #2 are not. At close inspection the experiment quartz sand #2 even shows reverse faults in the “hangingwall” in front of the bending antiform. It is difficult to believe that this should only be due to the different material cohesion and that the model setup has no influence.

Response #8. In the present version of the manuscript we did not state that the setup of the WK and QS models is the same. Our modeling approach uses two different materials “to evaluate if rheological differences . . . (e.g. cohesion) may affect our observations”(2052 Lines 17-18). Nevertheless, we fully understand the concerns of the Referee. The WK1 and QS1 use the same experimental box, hence the results are easily comparable. We maintain that also WK2 and QS2 show similarities. An example is given by the reverse faults: as pointed out by the Referee, in QS2 some high-angle faults show reverse sense of slip. These curved faults have been described both in analogue models and in natural examples as “precursor faults” (e.g. Mandl, 2000). They represent the first step of brittle deformation stage which initiates close to the buried tip of a master fault. These structures are also seen in the WK2 experiment. In particular, in Fig. 5d the high-angle fault located above the low-angle discontinuity is a “precursor” fault. We will emphasize this analogy in the next version of the manuscript.

Reference: Mandl, G., Faulting in brittle rocks. An introduction to the mechanics of tectonic faults, Berlin, Springer-Verlag, 436 pp., 2000.

Other comments and related responses

Comment. Chapter 5. 2057 Line 2-4 Please give more explanation to the statement "the size and the shape of the basin related to the growth of upward-propagating faults depends on the growth rate of the faults": First, why growth RATE? Second, the basin shape cannot be seen in Fig. 5, as the experiments WK1 and WK2 start from different nonplanar model surfaces: WK1 starts from higher topography above the footwall, WK2 from a higher topography above the hanging wall. It is therefore not straight forward to compare the final "basin topography".

Response. Concerning the grow rate of the faults. In the WK2 experiment we observed a temporal variation of the spatial propagation of the fault, i.e. a temporal variation in the growth velocity and size of the faults due to the interaction between new and pre-existing faults. We call this phenomenon as fault growth rate. Concerning the basin shape. We analyzed the basin shape with D.I.C. analyses which results are not shown in the present version of the paper. This technique is able to reconstruct the displacement of the particles of the analogue material during the experiment, and the displacement of the shallower part of the experiment allow us to reconstruct the basin shape. We will add these analyses in the next version of the paper both in the text and in the Figures. The initial topography of the WK experiments is not planar. This is characteristic of wet kaolin experiment because the initial flattening of the surface is a difficult process (see for example Withjack et al., 1990 AAPG Bulletin).

Comment. 2057 Line 7-9 ": : :when a propagating failure meets a mechanical discontinuity, such as a weak layer or a pre-existing fault, the failure may stop, penetrate it, or be deflected along it": ok, this sentence lists all possibilities, but does not help understanding the models.

Response. We will delete this sentence.

Comment. 2058 Line 8 ff "Unfortunately, neither field nor trenching observations allow the nature of the Paganica fault gouge to be assessed": This is surprising. No trenching results and no high-resolution reflection seismic is available from that fault to show whether it extends to depth or not?

Response. Paleoseismological investigations across the Paganica fault used standard trenches dug in loose Quaternary deposits up to 4 m depth (e.g. Cinti et al., 2011) which did not reach the bedrock (i.e. the Mesozoic carbonates). Digging deeper trenches is much more expensive and requires a totally different setup, but it is indeed a fact that a depth of 4 m in that specific geological setting is insufficient even to rule out that the observed faults are rotational slumps rooted at very shallow depth. To understand fully the mechanical behavior of this structure one would need to observe the fault directly in rocks similar to those existing at seismogenic depth. As for the high-resolution seismic data, the only available work is a joint seismic refraction-resistivity survey that has been published by Improta et al. in 2012. We used their results to constrain the location of the Aterno basin depocenter, but its characteristics and its effective penetration depth (around 300 m) do not allow firm conclusions to be drawn on whether and how the surface ruptures extend at depth.

Reference: Improta, L., et al. (2012). High-resolution controlled-source seismic tomography across the Middle Aterno basin in the epicentral area of the 2009, Mw 6.3, L'Aquila earthquake (central Apennines, Italy). Ital. J. Geosci. (Boll. Soc. Geol. It.), Vol. 131, No. 3 (2012), pp. 373-388,

Comment. Chapter 5.3, Line 17 ff "Lower seismicity cut-off (9–10 km): can be interpreted as due to the presence of another inherited thrust surface": this interpretation should be supported by other data, e.g., a regional cross section showing the basal detachment of the Apennine fold-thrust belt.

Response. See Response #1.

Comment. Figure 2. The figure is not well legible, especially 2a (contour lines) and 2c (photographs of surface ruptures etc.). The photographs should be enlarged arranged in a separate figure.

Response. This is a good point. We will arrange the photographs of the surface ruptures in a separate figure.

Comment. Figure 3. The interpretation of the shallow-dipping discontinuity (drawn in orange) based on the distribution of the aftershocks is not stringent. The authors should include additional evidence to support their interpretation, e.g., a regional geological cross-section to show that a thrust fault is expected in that depth.

Response. See Response #1.

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