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Interactive comment on "The role of mechanical stratigraphy on the refraction of strike-slip faults" *by* Mirko Carlini et al.

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We are thankful to the first reviewer for his/her interest in our paper and for his/her helpful and constructive comment. We will do our best to clarify the parts of the manuscript that, in the light of the reviewer's comments, resulted more confused and/or generated some misunderstanding, we will also enrich the photographic material that helps supporting our model. We report below the reviewer's comments followed by the authors' answers.

reviewer: The strike slip origin of the faults is, at the best, not convincing. The intersection between the pink left lateral faults and green right lateral faults, as seen in the stereoplot of figure 2, exactly lies along the bedding plane and the striations along

the planes of both systems are at 90 from this intersection zone. This could be taken as a textbook example of a pre-folding tilted extensional conjugate system (as shown in figure 8d, where the data are displayed after unfolding). Your paleostress analysis provides slightly different indications (i.e. Sigma 2 not lying along the bedding plane), but this could merely indicate that some of the assumptions behind the stress inversion are wrong (e.g. you have lumped in the inverted dataset structures pertaining to different events). The study of additional outcrops, with different bedding dips, is required to solve the problem: the orientation of the strike slip faults is constant at different bedding dip value = they are post folding; the orientation changes but the angular relationships with the bedding is constant = they are pre to syn folding. As pointed out by the authors, if an extensional origin is assumed the derived extension direction would be NW-SE. i.e. parallel to the trend of the belt. The authors claim that (Page 6 line 27) "the derived extensional deformation phase would have ensued during a pre- or synfolding time and within this portion of the Northern Apennines this is not supported by any independent geological evidence, neither in literature nor from our own field observations". This is unfair with respect to the thousands of works that have documented pre to syn-folding extension oriented parallel to the trend of foredeep and anticlines in FTBelts (starts with Sterans, 1968; Dietrich, 1989), including the Apennines (Doglioni, 1995)."

authors: The idea that the studied faults cannot be interpreted as rotated normal faults is based on two main arguments: - First, the average dihedral angle between the two conjugated fault families (as identified and suggested by the reviewer) is very large, more than 80°, which would imply strongly non-andersonian conditions during the extensional event; - Second, the kind of faults studied and described in our manuscript are spatially restricted to just an area of few hundreds m2 right at the front of the Palazzuolo anticline, thus strongly suggesting a genetic relationship between their nucleation/development and the shortening that created the fault-related fold. Based on these observations, although we cannot possibly exclude the presence of extensional structures related to pre- to syn-folding extensional tectonics within this portion of the Northern Apennines, we firmly believe that the studied features are indeed strike-slip faults linked to the formation of the antiform, and that they are not indicative of regional-scale, pre- to syn-folding extension or even lithospheric-scale syn-contractional transtension. Concerning this topic, we would like to remark that our speculations were strictly referred to the studied portion of the Northern Apennines, not to the entire chain nor to orogenic wedges in general. In a possible revised version of our manuscript we will add more photographic documentation of the strike-slip kinematics of both the sinistral and dextral faults.

We believe that the reviewer's deduction suggesting that our paleostress analysis may be faulty (derived from his/her idea that the faults should be extensional) is based on a rather circular argumentation, and we invite the reviewer to provide a more direct evidence of the faulty assumptions at the base of the analysis presented by the authors. We have followed a robust scrutiny during the analysis of the fault-slip data and have taken great care while sorting the bulk fault-slip data into mechanically compatible subsets, which were further utilized in the assessment of states of paleostresses.

reviewer: The idea that faults has nucleated in weak layers is not supported by the presented data. Whatever the origin, post-folding strike-slip or pre-folding extensional, field photographs suggest that fault propagation has occurred throughout the linkage of pre-existing fractures, as commonly observed during fault growth (e.g. Healy et al., 2006).In detail, looking at the photographs, many could say that fault propagation has occurred by the linkage of the pre-existing (or precursory in the case of pre-folding extension) high angle to bedding fractures (those affecting the stiff layers).

authors: It is not clear to us what the reviewer has in mind as to which field data should suggest the linkage of pre-existing tensile fractures and not support the nucleation within weak layers. The rare tension gashes observed within strong layers are never observed to link up through by shear fractures within weak layers, while, on the contrary, clear field data show well-developed shear faults within weak layers propagating through strong layers, but without completely cutting through them. We will, yet again, provide better photographic documentation and apologise if our descriptive base in the

paper was note sufficiently clear.

reviewer: The authors suggest a different and counterintuitive history, with the nucleation of faults within the weak layers and propagation (with consequent refraction) in the stiffer layers. Honestly, there is not enough field material to discriminate between the two hypotheses (the presentation of some displacement-distance diagrams could help). At present, the evolutionary model proposed by the authors is based only on an analytical model explained in figure 9: stiff layers require higher tensile stress for failure and, if one assumes that the stress tensor is the same for adjacent layers, failure must occurr before in weak layers and then, after stress build up, in stiff layers. This is a weak argumentation, as the assumption of a constant stress tensor in adjacent layers with different mechanical properties is highly questionable: what about uniaxial or biaxial strain conditions?

authors: We assume a uniform stress applied to the whole multilayer pack, but this initial condition changes immediately, as deformation proceeds. The initial uniform stress, in fact, immediately differentiates, following different paths within the two mechanical layers. This is the core of our proposed conceptual model and we emphasize that we do not assume constant stress tensor except at the very initial state, prior any brittle deformation has taken place. The clear nucleation and propagation structures observed in the studied faults, as mentioned before, do not lend any support to alternative evolutionary conceptual models (such as linkage of pre-existing tensile fractures within stiff layers or even the opposite, that is, linkage of pre-existing shear fractures within weak layers).

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