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Interactive comment on "On the mechanical behaviour of a low angle normal fault: the Altotiberina fault (Northern Apennines, Italy) system case study" *by* Luigi Vadacca et al.

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This paper proposes to model the rather fast interseismic deformation in the region of the Alto Tiberina (ATF) fault using a 2D mechanical modl . The deformation rate around this major low angle normal fault, is provided by a dense cGPS array, and the detailed structure and fault geometries are provided by geological studies, deep boreholes, and avtive seismics. The authors use an elastic model for the whole crust, imposing horizontal extension at constant velocity. The model considers not only ATF but also smaller faults rooting in it, in particular the antithetic Gubbio fault . To numerically simulate the creep with the COMSOL software, they reduce the shear modulus to 0.01 GPa, in order to concentrate the elastic deformation close to the fault. They tried a

number of model, with varying locking depths of the various faults. Their conclusion is that for explaining the GPS records, the ATF must be creeping at depths larger than 5 km , and that the secondary faults play a significant role in the deformation by creeping. A second important result in the conclusion is that their model is consistent with the absence of reported moderate earthquakes in the last 1000 years.

General comment:

The topic is very interesting, and important, both for the mechanical problem of low angle normal faulting, and for the question of hazard assement related to the seismic potential of this major fault. The area is also the target of a Near-fault observatory, a densely monitored area, with multiparametric recordingÂă: this mechanical model is expected to provide a new frame for interpreting some of these data. However, the conclusions on the above questions are questionable: the purely elastic modeling may be leading to biased and possibly unrealistic results, and the inferred relatively moderate seismic potential of the studied faults may not be safely justified.

My main detailed comments are listed below.

(1) the introduction should quote published mechanical models of the crust with active faults, involving elasto-visco-plastic rheologies (e.g., Cianetti et al, GJI, 2008), and discuss the expected differences with their simplified, elastic model. Did the authors make some numerical comparison with simple fault/crustal modelsÂă to validate their simplified approach?

(2) in order to simulate creep on the major faults, the reduction by a factor of 1000 of the Young modulus results in a reduction by a factor of 1000 of the shear modulus (hence the creep), keeping the Poisson ratio constant (0.25). But this also reduces by a factor 1000 the incompressibility K: which means that not only the fault zone easily shears (mode II), but also easily compacts or expands (mode I). This may strongly alter the strain pattern outside the fault zones. This local mode I elastic strain is not a desired process (real creep should produce purely mode II slip), and may significantly

change the stress-strain transfers between neibouring faults, and with the more rigid blocks around. The authors do not mention nor quantify this expected side-effect of the model. I believe that a reasonable elastic simulation of creep should keep a large value of K in the fault zone, while decreasing significantly the shear modulus (by a factor of 10 or more). This can be done by adjusting the Poisson coefficient, taking it larger than 0.25. The relevance and optimization of such parametrization should then be tested in simple geometric, with the criteria of a neglectable mode I strain component on the fault zone - and numerical stability.

(3) A few locking depths have been tried, as shown in Figure 4 and 6. The main, sharp step between 65 km and 70 km varies strongly, depending on the model: Some models produce a larger step than the reported one from GPS, and the other produce a smaller step. Surprisingly, no model is presented with a proper adjustment of this main feature of the GPS record. I would imagine that some models with intermediate parameters would do the job (intermediate locking depth, and some active secondary faults) would provide a better adjustment to the reported step; but the authors do not mention this possibility: did they try?

(4) The Figure 1 shows that the deep microseismicity coinciding with ATF is not shallower than about 9 km in depth. The shallower seismicity appears off-fault, and close the Gubbio fault. The authors should comment on this apparent change in the microseismic regime, as with a creeping ATF up to 5 km, one would have expected a clear microseismic cloud attached to the fault up to this depth. Is it related to magnitude cutoff in the selected seismicity for the figure? This question relates to the previous one, as a locking depth deeper than 5 km may provide an acceptable or even better fit.

(5) The lateral boundary conditions looks odd to me. The text writes that the SW edge is moving to the SW at the rate of 0.5 mm/yr, whereas the NE edge has a rate of 3.5 mm/yr to the NE. This is clearly seen on Figure 5 (ATF1 and ATF3) where the Von Mises stress is uniform on the vertical edge. However, a realistic mechanical model should have a non uniform displacement rates at the NE edge, with a 3.5 mm/yr above

ATF, and some much smaller velocity beneath it (as the deepest crust there is part of the SW block). This problem is related to the specific geometry of the fault in this purely elastic modeling, which leads to unphysical boundary conditions. The way to correctly deal with this is not clear to me: what is the rate to be taken at depth beneath ATF on the NE border?If the ATF reaches the NE border precisely at the deep angle of the rectangle model, would this stabilize the problem? What could be specified as boundary conditions if the ATF reaches the horizontal, lower border? Probably, the safest way to solve this problem is to work with a viscous layer simulating the lower crust, as is usually done.

(6) The authors do not discuss the faulting process of the upper, locked part of ATF. Is this locked part seismogenicÂă? If yes, what is the expected magnitude? If one takes 5 km as the locking depth, as suggested by the authors, the shallow ATF has a width around 15 km, and a width 60 km, thus leading to a potential of a few magnitude 6.5, possibly close to 7 if it breaks in one single rupture. The slip could be in the 1.5- 2.5 m range. Thus, with a average ATF slip rate of at least 4 mm/yr, such an event should occur very roughly every 200-400 years – which is contradicted by the the historical records. This gets worse if one accepts that a deeper locking depth (7, or even 9 km) remains plausible (ie, fits equally the GPS data, as suggested above). Indeed, magnitudes of 7 and above would then become possible, and the absence of historical earthquakes could then suggest that the fault is in its latest part of its seismic cycle. Of course, alternative models may be proposed, like episodic aseismic stress release of ATF in its shallow part, which would not have occurred in the short time window the GPS monitoring – or a GPS strain rate much larger than the average interseismic one. Clearly, this is a major issue.

To conclude, my impression is that the purely elastic modeling brings a number of difficulties which makes the detailed mapping of stress and strain rate quite uncertain. This in turn makes the final conclusion, that the locking depth of the Alto Tiberina fault is about 5 km, quite uncertain as well, and more convincing tests should be done, in particular to exclude locking depths of 7 or 9 km. The details of the calculated interaction between ATF and the secondary faults may also be questionable, due to the non physical strong mode I strain of these faults. Also, it seems that the possibilty of destructive, rare earthquakes from the locked ATF fault zone cannot be excluded.

Thus, some of the main conclusions of the paper are yet not well supported by the presented analysisÂă; the latter should integrate a more physical modeling approach, and a more carefull discussion, as suggested in more details above. This seems requested before going to a 3D modeling; the alternative being to include a standard, non-elastic rheology.

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