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Solid Earth Discussions

Interactive comment on "Switching deformation mode and mechanisms during subduction of continental crust: a case study from Alpine Corsica" *by* Giancarlo Molli et al.

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Bologna, 25 February 2017 Dear Editor, I found the manuscript "Switching deformation mode and mechanisms during subduction of continental crust: A case study from Alpine Corsica" by Giancarlo Molli and coworkers a well thought-through and inspiring piece of research and I recommend it without any hesitation for publication in Solid Earth. The study deals with the (somehow poor – see below) description, analysis and interpretation of potential evidence of viscous-frictional cyclicity under blueschist facies conditions connected with the Alpine age subduction that affected part of Corsica. The authors develop a robust conceptual model that accounts for the documented meso- and microscopic structural evidence and, by means of a paleopiezometric approach

and critical utilisation of literature data, they also derive quantitative constraints on the actual mechanical/dynamic constraints at the time of deformation in the subduction channel. The authors are to be commended for their efforts to elevate field and microstructural observations to a more "numerically constrained" level, thus helping to bridge the difficult-to-fill gap between field observations and seismological constraints. The paper is actually slightly heterogeneous when it comes to the way it is written, with the quality of the text and of the English increasing as the reader goes through the pages and approaches the "core" of the study, with the main results and the discussion being particularly clear and well written. A minor linguistic review will no doubt harmonize the linguistic. I attach a Word file with several inputs, comments and suggestions for the authors and would like to ask them to consider them carefully so as to produce an even stronger end-result. Some moderate revisions (although conceptually minor) are in my mind necessary. I limit myself here to the analysis of only a couple of issues, while more comments can be found in the provided, directly annotated text. 1) Abstract: This has to be completely rewritten. As it stands at the moment, it is not a concise summary of the main results of the study and an indication of how the authors used them to elaborate and propose a solid model. It reads instead more like a totally data-barren rationale to a scientific proposal. Only generic statements about the broad theme of viscous-frictional cyclicity are made and no data/hard facts and interpretations thereof are proposed. This has to be changed.

2) As for the scientific component of the study, I do believe that the authors "read" correctly the available structural evidence and that they interpret that properly to generate a robust evolutionary model.

Unfortunately, though, I was disappointed by the level of documentation of the key structural features, which, especially in the case of the microstructural description, is very deficient. Let me elaborate this further by referring to the text of lines 417-427. This is THE CORE of their work. Take this away and the reader is left with nothing to ponder and no model can be elaborated and proposed. Indeed because of the cen-

trality of this suggested reconstruction, the reader needs to be taken in much detail through the available evidence that underpin the proposed evolutionary model. The sequence events 1) to 3) is poorly elaborated and documented, I believe, and the evidence that would demonstrate beyond doubt the cyclicity between the frictional and viscous evolution and the overprinting relationships is, at best, quite poor. Cyclicity is a "hot topic" these days and is being actively described in many scenarios. Not always, though, is the proposed documentation convincing enough. The cataclastic portion of the rock is not documented, for example. The authors should prepare photographic tables documenting the microstructural characteristics of the cataclasite and describe much more accurately the clasts and their compositions. The text, for instance, reports that the clasts are of fractured K-Fs and qtz. What quartz is that? Is it deformed? Is it equivalent to that of the protomylonite (which would help convince the reader of the relative age of cataclasis) or are there also clasts of ultramylonite? Better documentation is needed, in synthesis. When offering the "time" sequence of deformational episodes, Molli and coworkers, moreover, need to list ALL the existing evidence in favour of their hypothesis in a clear and convincing way, such that the readership are not asked to "believe" what they conclude, but can easily come to the same conclusion (easly because of appropriate documentation and clear text descriptions). I refer the authors to the many comments and inputs in this sense that I have added to the Word document, highlighting the parts of the text that, in my mind, require more robust data and structural characterization.

3) I am intrigued by the discussion about a possible "background" flow stress value that would steer the overall grain size of the dynamically recrystallised quartz in the ultramylonite and in the sealed microcracks. While I follow the reasoning behind this (and share the proposed view), I'd invite the authors to also consider that other factors might control the final grain size of the nucleated grains in the fractures, such as the initial aperture of the (micro)cracks and the starting grain size of potental micro-gauge fragments in the dilatant space of the fractures. Are these (and potentially other factors) relevant? If yes, how? And if they are not, why? A few lines discussing these aspects

could be useful. With only modest revisions I am convinced that the impact of this study will become much more significant. Your sincerely.

Giulio Viola

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/se-2017-11/se-2017-11-RC1-supplement.pdf

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