

Interactive comment on “Strain localisation in mechanically Layered Rocks, insights from numerical modelling” by L. Le Pourhiet et al.

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I'd like to take the advantage of open journal and interactive discussion review process to point out that all the study mention in this review except Jessel are either analytical or performed with the same numerical approach by the same group of researchers. We use a numerical method, which solve equation on a regular mesh, and we cannot assess most of the reviewer request which require to use a code similar to his own code.

We do agree with Stefan Schmalholz introduction and point 7 that the numerical method we use here is not appropriate to make a careful analysis of growth rate and to compare them with analytical solution or to try to develop some semi-analytical solutions and therefore we didn't attempt to do it and therefore we didn't compare with

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papers, which focused on it because that would have been a scientific crime from our side.

However, we report that our algorithm suffers from numerical diffusion due to remeshing and that the markers based method allows to reduce that problem but not to remove it completely. Even with a "contour-based re-meshing method", we could not avoid numerical diffusion issue, simply because our code uses a regular grid. The reader can therefore see the "zig-zag", and zooming more on the structure would not ameliorate the picture. That is why we characterize mullions and boundins based on finite strain orientation and intensity computed on the markers rather than by the layer themselves and not on the mesh. Note however, that these mullions and boundins are not primordial to the analysis of the shear banding orientation when strain finally localizes and note again that we are not trying to characterize the growth rate of these structures.

In response to point 5 and in echo with the anonymous reviewer, we can indeed rewrite the paper by making clear that we are interested in understanding strain localization near kilometric scale detachment zone in the middle crust and make a clear statement that the results obtained may not be completely generic because the boundary conditions and initial conditions implies that strain localization is driven by the presence of a rigid wall at the top of the model. BUT, we fully disagree with both the reviewer that this has no geological meaning. It is all very relative to the scale, and most of the time, strain localizes because of rigid heterogeneities and not self consistently out of viscous instability at layer interfaces.

In this paper we consider that the rigid heterogeneity that causes strain to localize is by far larger than the layering in the medium where strain localizes. We indeed do not target to study the growth rate of the folds and boudins that form at small scale, we just mention them as different mode of deformation in the layer.

But contrarily to the reviewer idea of what this paper should be about, we are not studying self-consistent localization of boudins and fold in a homogeneous infinite crust

and trying to understand the growth rate of these instabilities as a function of the initial orientation.

We instead are trying to understand the structure we see on the field close to exhumed detachment faults and trying to understand if we can decipher the kinematics of the detachment from the micro-structures that form in its vicinity or understand what was the initial orientation of the layering in order to reconstruct regional geological history.

We do also comment on the integrated strength of the structures with time and on the local pressure gradient near the structure and their evolution with strain localization and in the discussion we do acknowledge pre-existing specialized papers on the subject, but we do not target to write a paper that quantifies this precisely because our numerical scheme is not accurate enough and we fully realize it by mentioning in the text the issues with remeshing.

I still think the results obtained are valid and that running similar experiment with a more accurate method would reach similar results in term of orientation and order of magnitude of stress drop within the shear zone or temporal evolution of pressure gradients/Jumps. The timing and rate could probably be more accurate using a method similar to the group of Stefan Schmalholz but one may wonder, could we actually measure these rates accurately in the rocks and therefore validate these models?

In response to point 1. We also think that using different kind of localizing rheologies (mises, high stress exponent non linear viscosity) would probably reach similar results concerning the orientation of the structures because as the reviewer state it well, we imposed kinematic boundary conditions and the layering imposed a kinematic forcing. Therefore, stress concentration will occur at the same place and what ever is the weakest weakening mechanism, the orientation of the structure and the whole kinematics will not be affected especially the orientation of the secondary shear zone. Yet we believe that pressure dependant plasticity is a more realistic representation of rock rheology which implies friction at the brittle ductile transition. Moreover, using pressure

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dependant rheology enable extra structural softening/hardening, that is not captured by visco-plastic flow rule.

In response to point 2, we can affirm here that Mohr Coulomb rheology used in its pseudo static formulation is not more brittle than Mises or any power-law rheology with very high stress exponents. None of these are brittle, they are all describing localizing rheologies. The choice of "brittle" in the introduction was a bad one, we will replace it by non associated plastic rheology and discuss the equivalence and difference with using high stress exponent or any localizing rheology to the one we used.

The reviewer considers it as bad initial choice but we consider it as a simplifying choice that had to be done. For this reason we didn't discuss anything related to shear heating because the models had no temperature.

In response to point 3, We indeed didn't adress the question of layering and layer relative thickness in this manuscript. That would have been to many model to present in a trying to be concise way. In the models we presents, the weak layers are relatively thick, i.e. as thick as the strong layers and the stress is indeed negligible. We do not denie that for thinner layer the stress would be much higher as it was shown by Schmid Podladchikov (2006). We will include that point in the discussion of the revise version of the mansucript.

The shear heating point raised both in point 2 and 4 is however a good one, when we started the study, we deliberately dismissed the temperature in order to simplify the models and particularly in order to get rid of the orientation of the thermal gradient as compare to the applied boundary condition. Note again that we do agree that viscous heating might be important at the scale considered here, although in the model, localization is achieved through plasticity. Before it occurs, the strain rate is small in the strong layer and heating is not important, and once it has occured, 1) the strength drops and shear heating might becomes less important 2) the length scale is imposed by plasticity and shear heating is not going to change the resulting kinematic. More

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over, if one consider field data, shear heating doesn't seem to be an important process near the brittle ductile transition near detachment zone. We believe that including fluid flow in the problem would be much more important and completely annihilate the shear heating issue.

We believe this paper is yet useful for a wide community of people, i.e. field geologists, who are interested in understanding regional geology from field observations without entering in the details of the deformation of one layer and trying to quantify viscosity contrast from wavelength of the structure that form. These study rarely consider the impact of pre-existing structures and wavelength like sedimentary bedding, or architecture of platforms, which are involve initial variation in thickness of the layer and sometimes strong periodic forcing and would control the wavelength of the growing fold and boudins much more than the viscosity contrast.

1 Answer to specific comments that are not related to typo.

1.1 specific comments with long answers

Section 5.2: This section needs significant improvement. First, that the pressure depends on the strength of the strong layers has been shown and analysed by many other studies, but none are mentioned, implying again that this is a new result of the study. Second, how can one "report" overpressure? This is usually an interpretation and I would rather say that the study of Vrijmoed et al. (2009) is one of the few studies that interpreted their field data by overpressure. I think the same field area has been interpreted before without overpressure. Most studies simply do not consider overpressure at all and therefore they do not "report" it. The word "reported" is misleading here. The same applies for the statement "proven examples of tectonic over/under pressure". Third, one can of course also generate significant overpres-

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sure in the weak layers if, for example, folds become tight and isoclinal. There are several studies that quantified this (e.g. Schmalholz Podladchikov, 1999).

Ans: I think the editor should be made aware that this over pressure debate could somehow be mentioned as a conflict of interest between Paris group and Oslo or affiliated to Y. Podladchikov groups.

I, as first author, have tried and almost manage with this work to actually get the Paris group to acknowledge that over pressure may exist and in a way to conciliate people. I cannot get my co-authors to sign what Stefan Schmalholz is asking.

I can mention the paper by Schmalholz Podladchikov, 1999, we also do report over pressure in the weak layer in some of the cases but I need to leave this part as open discussion in the text or half my co-author will not sign the paper.

I personally do agree with Stefan Schmalholz that over pressure are indeed probably not reported, but my co-authors who do the field work and the petrology do not agree. Are over pressure a bias of over simplified models or do they truly exist is completely behind the scope of the paper and I personally believe that each participants of the "conflict" should discuss more openly and more peacefully (which I have tried to do in this part of the paper).

Appendix B: It is not clear to me why the authors used this particular method to calculate the finite strain ellipse. The orientation and aspect ratio of the finite strain ellipse can simply be calculated from the eigenvalues and eigenvectors of the Cauchy-Green tensor which can be easily calculated from the strain rate tensor which exists for every numerical node (or integration point). We did this, for example, in Frehner Schmalholz (2006). The method applied by the authors appears unnecessarily complicated.

Ans: The method we use is by no mean more complex than the one described in Frehner and Schmalholz 2006, it is actually the most straight forward method to com-

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pute finite strain. It was even considered as so obvious and simple by the anonymous reviewer that he would like us not to even mention about it.

It just consist at computing the finite strain directly from the marker field. Instead of integrating on time the derivative of the velocity field (by the way, you take the gradient of the velocity field and not the strain rate tensor), we use the marker to integrate the incremental of displacement

To say the truth both methods suffer from the same bias, except our method is general and can be applied within any discretisation (we use it in 3D with Gale in a paper published in G3 this year) while your method requires to use the finite element method and works best with high order elements.

Finally, in the description of your method, one can see that some of the non linear terms related to large strain have been dropped so that the numerical integration in time for the velocity gradient tensor might be inaccurate if you are not considering the deformation at a point but for a finite volume as we do.

1.2 specific comments with short answers

P1169L15: The authors should provide some justification for these viscosity values and cite some studies or provide some arguments to justify these values. I think that effective viscosities could easily also be 10^{22} Pas for the strong layers, depending which flow law is assumed and what geotherm. Also, the physical unit of viscosity is Pa times s not Pa divided by s.

Ans: Sorry for the typo in the units. Some of the run were done with $1e22$ Pas. This enhances plasticity effects but does not change the orientation of the structures (figure on scaling). The viscosity were chosen to display visco-plastic behavior for the strain rate imposed at the boundary. As long as the weak layer is effectively viscous due to

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its thickness or its viscosity the results are unchanged.

P1170L25: What is a false S-C structure?

Ans: I will remove the adjective true

P1171L11: I do not see boudins in Fig. 4a. I think it is essential to show a figure which indicates the layer boundaries with a line and shows a zoom displaying 2 or 3 boudins only together with the stress or strain rate field. The results displayed in Fig. 4a are extremely patchy and a boudin geometry is not visible; see general comments.

Ans: I agree for the 10° model, for the 20° and 30° boudins are the blue structure which are not deformed... and as I said in the general answer I cannot do better with the numerical method I use.

P1174L22: This higher pressure in the strong layers during folding has of course been documented in many studies before, but the authors never mention such studies so that the reader gets the impression that this is a new result discovered in this study.

Ans: We do mention some of them in the discussion but we could have done a better job for the literature review it is true. I in general separate the literature review from the pure description of the results. I really don't think we postulate it is a new discovery, maybe on the one hand I assume that this has become textbook materials and on the other hand, for some of my co-authors it is yet an issue. What we really point out, is rather that although these over pressure do exist and the place where strain localizes is always in the weak layer which in only very few cases do present over pressure.

P1176L2: What is "large" wavelength folding. There is an enormous literature that studied the dominant wavelength in ductile multilayers and usually for folding a wavelength is compared to a theoretical dominant wavelength. The word "large" means nothing here. The manuscript would benefit if the results would be described more quantitatively and put into context to existing results, e.g. dominant wavelength theory of multilayer folding.

Ans: By large wavelength, we mean compare to the layering scale or compare to the scale of the model, it is just meant to describe the largest scale we can produce given the size of the model, we will clarify it in the revised MS.

P1177L9: Where exactly did the mullions develop? Again, I think it is essential to show more zooms of the numerical results to better see the developing structures; see general comments.

Ans: See answer to general comments

P1179L24: What is a false plastic deformation? I find the usage of the adjective "true" confusing, because I wonder then what a false plastic deformation is and that it apparently can occur in the simulations because otherwise the word "true" would not be necessary.

Ans: Agree with that I will clarify, by true plastic I just meant plastic or localizing. It is always an issue with the definition of plastic. I will remove the adjective "true" from the text and reformulate.

P1180L4: Finally, the non-newtonian rheology is mentioned. However, not a single study that investigated the formation of folds, boudins, and shear bands for non-newtonian rheology is cited. The impact of Peierls creep on the formation of folds and

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boudins has been studied by Schmalholz Fletcher (2011).

Ans: Yeah, it is mentioned in the discussion and not in the intro because it is beyond the scope of the study. However, following your review we will discuss in more details the other localizing rheology, Peierls, high stress exponent and how they compare to coulomb or mises so I can cite all these papers.

P1180L15ff: I think that this depends on the spacing of the strong layers. If the weak layers are much thinner than the strong layers and if the rheology of the weak layers is linear viscous, then significant shear stresses can be present in the thin layers. For example, Schmid Podladchikov (2006) showed that if the thin layers become significantly thinner than the strong layers, then the multilayer stack behaves effectively as a single layer. The spacing of the layers has not been investigated but has a considerable impact on the results, especially for linear viscous rheology.

Ans: see general answer to comments

P1181L21: I do not understand why the folding is in braces after hardening. Maybe the authors could explain this better.

Ans: because we observe hardening when layers are folding, I understand it might be disturbing compared to your results comparing pure shear and folding, but folding do result in hardening result in simple shear.

Interactive comment on Solid Earth Discuss., 4, 1165, 2012.