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Discussions

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

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From the exhaustive review performed by an anonymous reviewer, several points arise, which we definitely will take into account during a thorough rewriting of the submitted manuscript. Through the unnecessarily provocative tone of the review, we can group useful comments in two categories : some relate to the legibility, form and structure of the manuscript, some relate to the computing method we used and its applicablity domain. Concerning the form and structure of the manuscript, we would like to apologize for unsufficient editing (comments 1, 2, 5, 6 partly, 12, 13, 16, 23 , 45, 47, 50, 53d, 54, 55, 60,) or awkward terminology (Sn Âń business Âż, comment 33, pressure jump instead of gradient, comments 26, 58). The paper will be restructured as advised with a description of the case studies and field examples we use as natural reference

coming first. This will likely help understanding the choice of initial conditions. Code description and models computed will be preceded by a discussion of the critical dimensions and the delimitation of the results space we aim at investigating in our study, and to what extent this problem is Âń scale-independent Âż. The description of the results themselves will be developed in a more linear manner, with a clear definition of the different types we can distinguish. The discussion will be refocussed on the effects of initial geometry on structures development in simple shear. Figure 2 will be moved to the discussion (general section and comments 22, 25) and a plot of the orientation of shear bands as a function of delta0 will be added (comment 34).

Concerning the method we used, we acknowledge (comment 32) that in the precise case we are interested in, no analytical solution is possible. We indeed focus on the development of shear bands in a simple shear domain, such as the top of a detachment footwall, clearly stated as Âń a case in which, at the upper surface, the material above a horizontal plane, generally cutting through the layers, is converted to a perfectly rigid solid, and the condition at the surface is a perfect coherence Âż by the reviewer herself. The location of the result domain (comment 17) at the boundary is therefore relevant in this perspective. Since, we believe that these boundary conditions are geologically relevant and that since we cannot find an analytical solution, they constitute a perfect problem to be treated with numerics. Numerical solutions are indeed a way to solve geologically relevant problems, which do not have analytical solution. Our work was to treat such a problem for visco-elasto-plastic layers with pressure dependant yield criteria and not the localization in an infinite visco-plastic media with mises yield criteria for which analytical and semi-analytical solution already exists.

1 More detailed response to scientific comments

1.1 Related to boundary conditions:

The model and its numerical implementation produce a most peculiar result – a concentration of deformation at a horizontal boundary. No rationalization of this result is given in the paper. This result points to either an error in the implementation, or the specification of an unrealistic boundary condition. In fact, you are really aiming at an infinite medium in bulk simple shear. Since you can't do that, you should pick a "result domain" that stays away from the boundaries. In Figure 1, you put the results domain in the wrong place – smack up against the upper boundary! Why? (What does σv signify in this figure?) σ_v is the confining pressure.

Ans: We are not aiming at an infinite medium in bulk simple shear. We were just concern that the zone of shear localisation close to the rigid boundaries is very long as compare to its thickness. A good way to deal with that would have been to use periodic boundary conditions on the lateral side of the models. However, because the media is layered, it is not really realistic to transfer the stress from a strong layer on the left to a weak layer on the right. Therefore, we employed a very long modeling box and only examine the center of the box to stay away of the left and right boundary. Similarly, we only represent the top part of the model for reason of symmetry.

We observe the strain localisation close to the rigid boundary as expected, that is the reason we details the deformation pattern near this boundary. Geologically speaking, what is really interesting is the thickness of the zone affected by shear beneath this boundary as a function of the initial angle of the layering.
The rigid boundary represents a less layered and stronger tectonic unit. We do realize that we should rewrite the introduction and make a more specific target of

detachment zones such as the Tende and the Betics (cf the picture of figure 10) and other detachments in the Aegean or in the Apenins. This will justify the scale at which the models were run.

20. I have no idea how many bells and whistles your code has. I presume you're always being deliberate! The simplest formulation is to assign a lithostatic pressure corresponding to the depth of interest to the basic state. I guess that's what you do, but there is no need to talk about gravity and thermal gradient and temperature in this way.

Ans: We extended a little on the subject to say that we deliberately neglect these effects to simplify the problem, we were concern about reviewers who would ask us about these effects like S. Schmalholtz.

1.2 Related to "dynamics":

If, to simplify, a mean stress independent "von Mises" yield stress, K, is introduced, the corresponding multilayer in simple shear is described by only 7 parameters: the two viscosities, η_1 and η_2, the two layer thicknesses, h_1 and h_2, the yield stress, K, the inclination of the layers to the shear plane, δ, and the rate of shear $\frac{d\gamma}{dt}$. The parameters that determine the behavior are reduced to 4: δ is already dimensionless; $R = \frac{eta_1}{eta_2}; T = \frac{h_1}{h_2}$; and, say, $S = \eta_1(\frac{d\gamma}{dt})/K$. We immediately see from these groups that the behavior is length-scale invariant. This is the power of dimensional analysis, which is not made use of in the present paper. A choice of particular values of η_1, $d\gamma dt$ and K is irrelevant – all that matters is their combination S; similarly with R and T. If the authors want to use a Mohr-Coulomb material, the result would likely be somewhat less simple. If, in the present case, R and T are fixed, one may determine by the analytic treatment of the basic state the domains in δ, S – space in which yielding *(shear bands ?) and no yielding would occur. It is unlikely that the elastic properties that come with the model have any significant effects. This could at least be verified.*

Ans: Elastic properties do have an effect, which will enter in term of relaxation time for the characteristic strain rate and should live in your S term. They also limit the stress term in the strong layer so that S is not properly $\eta_1\dot{\gamma}/K$ but probably a term that limits η_1 should be added, you can see that increasing η_1 does not result in significantly different results and that localization saturates at one point when viscosity increases. This is an elastic effect. I am not sure the initial state governs every thing either because some viscous deformation occurs in the layer prior to yielding. Yielding occurs once the layers have start to deform and structural weakening as already occurred. It occurs ealier in extension than in compression due to the use of pressure sensitive yield criteria and that is why shear banding and strain localisation occurs when the folds are overturned in the case of initial shortening.

31. What is the measure of "softening?" Do I have to guess? It is not described in the text, but I see some jagged lines in Fig. 3, falling with shear strain. Presumably, it is the shear stress required to maintain a constant shear rate.

Ans: Softening materials are materials, which exhibit a decrease of strength with strain according to mechanical textbook. Here, we indeed measure softening by the drop in average shear stress (second invariant) within the strong layers with shear.

For the basic state, with no perturbations, a complete analytical solution may be obtained by elementary means. For a viscous multilayer, the mean stress difference – negative of the pressure, between the stiff (η_1) and soft layer is (($\eta_1 - \eta_2(\gamma)\sin 2\delta$), where γ is the shear rate. In layer-parallel extension, its positive – the pressure is

greater in the soft layer, and in layer-parallel shortening, it's negative. Robin (1979) and possibly Treagus (19??) obtained such results.

Ans: And they are indeed now well accepted. We can cite these references in the description of the results although we found this was not necessary, because it was obvious.

The effect of elasticity – likely completely insignificant, and plasticity could be incorporated with some additional work.

Ans: You have no scientific argument to say that the effect of elasticity is insignificant, it might be correct for a layer at 1e21 Pas but in the experiment with higher viscosity contrast it is not and the initial pressure contrast will be limited.

32. The results are given in a domain that extends $\frac{1}{2}$-way through the layer, and so the mean velocity at the base is zero. This, however, is not a surface at which boundary conditions are applied. I presume that the solution domain passes to the base of the layer. One would then expect to see the symmetric counterpart of the structure at the top at the base. However, we are not provided with this information.

Ans: There is one we just do not plot it as represented on the model set up figure.

The question of why the profile bends over remains. Apparently, none of the authors are disturbed by this, either because it might represent an error in the code or, otherwise, because it represents an unrealistic boundary condition at the horizontal surfaces; I suspect the latter.

Ans: The boundary condition is indeed rigid, as drawn on the figure, whether this is unrealistic or not, that is the boundary condition we wanted to apply on the problem and as a result it bends and we are not disturb by this because it is what we expected from the boundary condition we apply.

That boundary condition may be described as follows. Let there be a domain of a multilayer subject to simple shear, as the authors posit, but let that domain be unbounded. In this case, one may apply the analytical treatment suggested. Now, however, consider the case in which, at the upper surface, the material above a horizontal plane, generally cutting through the layers, is converted to a perfectly rigid solid, and the condition at the surface is a perfect coherence.

Ans: It is exactly the boundary condition that is applied and what we draw on the figure.

This, or a discretized and numerically-implemented variant of it, is the model treated by the authors. Indeed, an analytical solution likely is not possible. It is possible in the limit that the layer thickness, relative to the scale of interest, goes to zero – i.e., the anisotropic viscous fluid.

Ans: Thanks for agreeing with that and to therefore provide an explanation to the other people who commented on the paper on whether we did not do it. At the same time, you also provide a good argument on why to do it numerically and not use analytical solution or compare directly to existing analytical solution. What we do not understand is the need to provide an analytical solution. Most of the real earth problems cannot be treated with analytical solutions. Should we restrict ourselves to problem with an analytical solution or try to solve problems with realistic boundary conditions.

1.3 Related to petrology:

The authors' discussion of "pressure gradients," actually discontinuous pressure jumps, is covered in most facets by Robin (1979) Theory of metamorphic differentiation and related processes, which treats the pressure jump between stiff and soft layers and its role in driving segregation; an equally pertinent paper is Sawyer Robin (1986) The subsolidus segregation of layer-parallel quartz-feldspar veins in greenschist to upper amphibolite facies metasediments. Chapman (1950) Quartz veins formed by metamorphic differentiation of aluminous schists shows remarkable and beautifully-illustrated examples of veins formed by segregation, and intuits that compression plays an essential role in their formation. See also van der Molen (1985). Such a sub-topics, are not discussed in adequate detail, nor with adequate reference to illuminating earlier work. It is pointless, if not embarrassing, to say that "we postulate...veins could form by diffusive small scale mass transfer..." when this proposal has been made over 30 to as much as 60 years ago, and a detailed discussion of the process, together with detailed observations, have been given. Alternatively, one might much better focus on the main thesis, and leave out other material.

Ans: We actually thank you very much for this insightful literature review, which I would take great care in reading before correcting the manuscript. I was personally sure that this subject was covered in the literature and I recon I am an ignorant on the subject, and yes, we did probably reinvent the wheel because we found that logical from first principle of thermodynamics and as it was a subtopic we didn't expand on doing the literature review. These references are going to be of great help to improve this part of the discussion. We will replace pressure gradients by pressure jumps.

1.4 Related to "wording":

33. (8, 27) Drop this “Sn” business! One is dealing with discrete layers, and with structure that is developed at the scale of the layer thickness. Because of this, the bending resistance, as well as other qualitative features in the layer deformation, come in.

Ans: We will use the term of rheological layering.

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