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Solid Earth
Discussions

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

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The paper by Le Pourhiet et al. presents numerical simulations of the deformation of multilayers in simple shear. The simulations are performed to study the evolution of geological structures such as folds, boudins and shear bands (e.g. S-C structures). The applied rheology is viscoelastoplastic; the viscosity is linear (Newtonian) and the plasticity is described by a Mohr-Coulomb yield criterion. The main focus of the study is the control of the initial orientation of the multilayers with respect to the simple shear orientation on the developing structures. The numerical results are visualized by displaying mainly the distribution of finite strain. The systematic analysis and the classification of the results into four general types are useful to better understand the control of the initial orientation on the developing structures. However, I find that the paper in its

current form has a number of serious shortcomings which I will mention and discuss in the following comments. I have a number of general comments, specific comments and some technical remarks. Because this is an "open-access" review, the comments are also intended to inform interested readers about related and existing studies which have not been mentioned by the authors. Also, because I worked myself on the development of ductile structures, I will also refer to several results of my own studies, simply because I know these best.

General comments:

1) The authors investigate the formation of folds, boudins and shear bands in multilayers. For multilayer folding, there exist a large number of both laboratory and mathematical studies (e.g. the textbook by Johnson & Fletcher, 1994; works by Ramberg & co-authors etc.) but nearly all studies, and especially their results, are ignored by the authors. For example, the theoretical results for the dominant wavelength and dominant growth rate in viscous multilayers (e.g. Johnson & Fletcher, 1994; Schmid & Podladchikov, 2006) are essential to understand the developing wavelength of the multilayer folds, also for overall simple shear. The magnitude of the growth rate determines what strain is necessary to generate folds with finite amplitude. For continuous boudins (or pinch-and-swell), there exist also studies providing the dominant wavelength and growth rate (e.g. works of Smith 1977, 1979; Schmalholz et al., 2008; also experiments by Ramberg). For example, recently we showed in Schmalholz & Maeder (2012) that boudins and localized shear bands can develop in viscoplastic multilayers. However, we used a power-law viscous flow law and the plasticity was described by von Mises plasticity (or, alternatively, by high power-law stress exponent). A brittle rheology was not necessary to form localized shear bands; a result in contrast with point (6) stated in the abstract of Le Pourhiet et al.. For shear bands or S-C structures, there exist also a number of studies investigating, for example, the deformation of particles in viscous shear zones (e.g. Jessel et al., 2009; Dabrowski et al., 2012). These studies show the formation of S-C structures for particles and not for multilayers in a shear

zone. All these studies and their results, which I think are quite relevant for the results presented by Le Pourhiet et al., are neither mentioned nor discussed.

2) A main conclusion of the authors is that a brittle rheology is necessary to form localized shear bands in the ductile regime. This is, however, only true for the specific numerical model that was applied in the study. There are several other mechanisms which have been studied and proposed to form localized shear bands in the ductile regime, such as viscous shear heating (e.g. Kaus & Podladchikov, 2006), alignment of particles in shear zones (e.g. Jessel et al., 2009), grain-size reduction (for scenarios far away from equilibrium; e.g. Braun et al., 1999), high power-law stress exponents in multilayers (crystal plastic flow, von Mises plasticity; Schmalholz & Maeder, 2012) or deformation of poro-viscous material (e.g. Fletcher, 1998). I think the paper by Le Pourhiet et al. would be improved if such other studies and alternative mechanisms for shear band formation in the ductile regime would at least be mentioned and discussed a bit.

3) The impact of different spacing of the strong layers is not investigated and discussed although several studies on multilayer folding indicate the importance of this spacing on the dynamics of multilayer folding. The authors argue that shear stresses in the weak layer are negligible; however, in linear viscous weak layers the shear stresses can become significant if the layers are much thinner than the strong layers. In some of the natural examples shown in Fig. 10 it looks like the weak matrix is indeed significantly thinner than the strong layers. Schmid & Podladchikov (2006), for example, have investigated the impact of spacing on the multilayer folding dynamics.

4) Viscous shear heating is neglected, but for the model configuration viscous heating could play a significant role. The applied background strain rate is 10ˆ(-13) 1/s which means that strain rates in the developing shear bands can be easily one or two orders of magnitude larger (or even more). Deviatoric stresses are in the order of a few 100 MPa. Therefore, the heat due to shear heating (stress times strain rate) is easily two orders of magnitude larger than the heat generated in the crust by radioactive decay. One could

determine the critical length scale of shear heating for the applied parameters. For this, the authors should state the value of the strain rate within the developing shear bands. Also, over 1 km the temperature can easily vary by around 20 °C and if the shear zone is horizontal then the temperature at the base would be warmer and the viscosity correspondingly weaker. This could cause more strain localization at the base of the model. Neglecting viscous shear heating and temperature variation across the shear zone can be justified for the purpose of investigating first-order mechanisms during multilayer deformation; however, such model does then not support the statement of the authors that their results are scale independent. The presented results cannot be applied equally to the kilometre and the meter scale, because the model does not consider thermal effects. Viscous heating and strain localization has been investigated by many studies (e.g. Kaus & Podladchikov, 2006; Braeck et al. 2009) which provided dimensionless parameters that allow determining if viscous heating is important or not for certain parameters. The authors could do such analysis to show the importance or unimportance of viscous heating for their results.

5) If I understood the model configuration correctly, then the top boundary is rigid and only moves horizontally. The multilayers are obliquely attached to this rigid horizontal boundary causing strong boundary effects. It seems that all displayed results are from this top boundary region, i.e. all results are controlled by the boundary effect of a rigid top which has no correspondence in nature. Why do the authors not present structures that developed away from the rigid boundary in the middle of the model? In the present form it is difficult to understand which structures are controlled by the rigid boundary and which structures really developed self-consistently as result of a mechanical instability (i.e. without the need of a rigid boundary).

6) In the presented form, the applied numerical method and the numerical resolution appear unsuitable for the performed simulations. On page 1169 Line 23 the authors report that there are only 3 elements across each layer. Several questions should be answered and demonstrated by the authors. 1) How are the initially oblique layers re-

solved? Looking in more detail in the figures it appears that the boundary between layer and matrix has a significant zig-zag shape indicating that the layer boundaries were initially not straight lines but had a step-like shape. This would cause considerable variations in layer thickness and geometry. However, instabilities that generate folds and boudins are extremely sensitive to thickness variations and layer geometry and therefore the initial shape of the layer boundaries has a first-order control on the resulting structures. 2) The authors report that their algorithm generates numerical diffusion of material due to a marker based re-meshing method. This means that the stress variation across material boundaries diffuses also; however, the sharp stress gradients control the instability. To avoid such “boundary diffusion”, we, for example, used a contour-based re-meshing method which does not have any numerical diffusion of material. We have applied this contour-based method to large strain simulations for folding, boudinage (pinch-and-swell) and shear zones in e.g. Schmalholz et al. (2008), Schmalholz & Schmid (2012) and Schmalholz & Maeder (2012). To judge the quality of the presented results, the authors should show a zoom with only a few layers in which the layer boundaries and the stress field are displayed, for several strain stages (including the initial geometry). It is essential to show figures that demonstrate the quality of the applied numerical approach, e.g. the flexural stresses across a fold hinge. Furthermore, the authors mention that boudins and mullions develop in their numerical results. However, I cannot really recognize such structures due to the strong zig-zag pattern in the displayed figures. Again, it is essential that the authors show a zoom of the structures together with a clearly defined layer boundary and with a stress or strain rate field. As a suggestion for a possible way of displaying the numerical results I added a figure showing results for boudins and shear zones in viscoplastic multilayers presented in Schmalholz & Maeder (2012).

Specific comments

Page 1167 Line 18-24: There exist a considerable number of studies that investigated analytically and numerically the ductile deformation in anisotropic material with either

anisotropic constitutive laws or with multilayers. A few more references would improve the paper; see general comments.

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 10ˆ(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.

P1170L25: What is a false S-C structure?

P1171L11: I do not see boudins in Fig. 4a. I think it is essential to a 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.

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.

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 quantitative and put into context to existing results, e.g. dominant wavelength theory of multilayer folding.

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.

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.

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 boudins has been studied by Schmalholz & Fletcher (2011).

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.

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

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

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.

Technical remarks

P1167L25: The sentence is incorrect.

P1168L3: The correct name is Mohr Coulomb.

P1170L13: The entire paragraph is repeated at the beginning of section 2.2.

Figures: It would be better if all figures have a legend with a clear description of what the colours indicate.

Best regards, Stefan Schmalholz

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

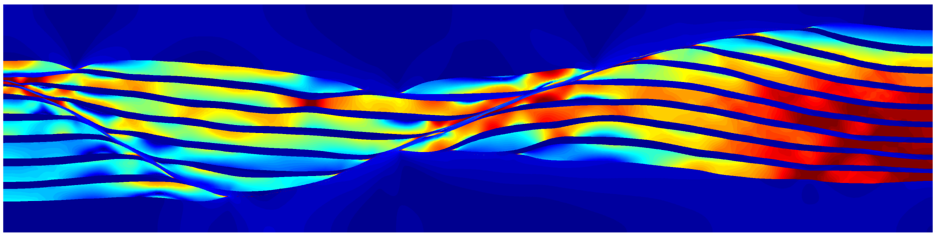

Fig. 1. Boudins and shear zones in multi-layers. Colors indicate the second invariant of the deviatoric stress tensor (dimensionless units: red=40 and blue=1). Layers and stresses are resolved accurately.