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Interactive comment on "Fracturing of ductile anisotropic multilayers: influence of material strength" *by* E. Gomez-Rivas et al.

E. Gomez-Rivas et al.

e.gomez-rivas@abdn.ac.uk

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We thank the reviewers and editor for their comments and suggestions, which help improving the manuscript. We are pleased to see that both reviewers are supportive of the ideas and results presented in this manuscript, and that they recommend acceptance after minor revisions. The reviewers and editor have raised some questions and suggested changes, most of which we have applied. In this document, we first type the question raised by the reviewer/editor (in italics), and then we reply to it explaining the changes made to the manuscript (in regular font).

Reply to reviewer #1 (G. Zulauf):

General comments

The present paper is focusing on the experimental deformation of multilayers to demonstrate the transition from ductile to brittle behavior with changes in viscosity of the individual layers. The paper is well written and should be of interest for many structural geologists dealing with brittle to ductile deformation of crustal rocks. I think the paper could be published after minor revision. There are some weak points, which should be considered when revising the Ms. I have listed these below and have indicated these in the paper (pdf).

Specific comments

The material used for the experiments is not sufficiently described concerning material anisotropy. First of all I suggest to clearly define the meaning of "ductile" and "viscous" in the introduction section (see reference in the pdf). It is obvious that anisotropy is very important for the development of the deformation structures. However, there are not only two but at least three types of anisotropy in your experiments, which are important for the results: (i) anisotropy of the plasticine itself because of possible plate-shaped filler components, which might rotate during progressive strain resulting in strain hardening and different viscosity in different directions with respect to the principal strain axes; note that an anisotropy would also be produced in cases of isometric filler components because of their change in site (with progressive strain you have a denser distribution parallel to the Z-direction and a less denser distribution parallel to X direction; the quality of the present paper would probably increase if the type of fillers and their behavior during progressive strain is known; (ii) anisotropy caused by the paper flakes, which rotate during progressive strain; it is of interest if these paper flakes collide during rotation/translation or if these are rotating passive markers during the entire run; (iii) anisotropy because of the different layers, which *have different viscosity. The viscosity ratio of the individual layers should be listed in a table and also in the figure where the deformed models are depicted. This viscosity ratio is important for the nucleation of pinches. The pinches and necks are weak zones where shear fracture could nucleate. Thus, the formation of macroscopic shear fractures and boudinage is probably intimately related. Pinch-and-swell structures are particularly well developed in Model B, but some are also present in Model A, where deformation is almost entirely homogeneous. May be that these few pinches in Model A result from heterogeneities in the material (artefacts, such as air bubbles). I suggest to describe the sequence of structures (pinch-and-swell, tension gashes, shear fractures), which develop with progressive strain during the individual experimental runs, in order to check, which structure is influencing the other.*

We agree that the system is complex. We have characterised it following the methods and procedures previously published in similar studies that used plasticine as a rock analogue, including those authored by the reviewer. We cannot provide information on the specific composition of plasticine, since the manufacturers do not make it public. However, we used two types of commercially available plasticine and our tests indicate that they behave isotropically when there are no layering or preferentially-oriented second phases. Therefore, we doubt that this potential third type of anisotropy (associated with plasticine components) plays a role in our experiment, if it exists.

The effective viscosities of each mixture are listed in Table 1, all of them calculated at 10% shortening for different strain rates (as in similar studies). As explained in the manuscript, the viscosity ratio between alternating layers is very low and difficult to note. This very small contrast is due to the addition of dye to white plasticine, which is the way used by the manufacturer to produce coloured mixtures. However, the method used to characterise the materials is not precise enough to allow detecting such small differences using the effective viscosity parameter. The stress gauges are

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not able to detect such small variations with enough precision. We cannot therefore list the viscosity ratios for each model. In any case, we clearly explain in the text that the viscosity ratios are remarkably low, and that structures nucleate in and affect the different layers in the same way regardless of their colour. We agree with the reviewer that the formation of part of the shear fracture network is related to the development of pinch-and-swell and boudinage-like structures. In fact, this is one of the main points of this study, since we explain and illustrate with experiments how these structures can give raise to the development of conjugate shear fracture sets. We interpret that these structures form due to small irregularities in the layers. The sequence starts with pinch-and-swell developing at the onset of deformation. Tension cracks and associated boudinage-like structures start to be visible after 15-18% shortening in most models (except in model A, when they start later). Then, shear fractures form: (i) from pinch-and-swell structures, (ii) from the collapse and coalescence of cracks and voids and (iii) directly without precursors. This is widely explained in Sect. 3 and discussed in Sect. 4. We have made some small modifications to the manuscript, to better report these events.

We agree that pinch-and-swell and tension cracks form in model A from heterogeneities. However, we do not think air bubbles play a role at all, since we have not identified them in the experiments. We must bear in mind that heterogeneities also exist in rocks, therefore determining where new structures nucleate. Our models show how layers start pinching in some areas, probably where layers are slightly thinner or where microscopic cracks have already formed. We have added some sentences in Sect. 3 to better explain how pinch-and-swell and boudinage-like structures form in these models.

We have not provided information about passive rotation of flakes and their potential collision. In principle, regions with higher density of paper flakes, or where they collide with others, could be stiffer and play a role on determining strain localisation. However, we have not observed this at all. Paper flakes are very well distributed within the model, and they were initially oriented parallel to layering, and layers parallel to the extension direction. Therefore, they cannot rotate much, since they are already parallel to *X* from the beginning. This means that their potential passive rotation cannot play a role.

The reviewer also asks to provide a definition of *ductile* and *viscous*. We use the term *ductile* to define the ability of materials to accommodate continuous deformation without development of discontinuities or fracturing. This term is not associated to any particular constitutive relationship (e.g. viscous or plastic behaviour). We use the term *viscous* when the deformation stress is dependent on the strain rate. Therefore, if the differential stress is not strain-rate dependent but the material deforms without fracturing, then the material is ductile by plastic behaviour (e.g. Burov, 2007, Treatise on Geophysics, Elsevier, pp. 99-151). We observe this dependence of strength on strain rate with high stress exponents n=3-5. We have now better defined these terms in the introduction, as suggested by the reviewer.

Another weak point is the lack of confining pressure. I know that most of these types of analogue experiments are working without confining pressure, but this should be mentioned at least in the Discussion section. Note that confining pressure would suppress the formation of open fractures, which are present in your models. These open fractures, particularly tension gashes, are not common in rocks, where the overall deformation is viscous, at least when the rocks are dry. In nature, such fractures develop in the ductile (viscous) level because of elevated pore pressure, which, however, is not possible in your models.

At the beginning of deformation, our models are subjected to confining pressure.

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However, we have to open the plates of the deformation cell in the *X* direction at the same rate as we close them in the *Z* direction. The third dimension (*Y*) keeps the sample confined, but the reinforced glass cannot support moderate high pressures. This is why the device does not allow keeping the sample strongly confined in *X*. This of course has an influence on the type of structures that nucleate, especially on tension cracks. We have added some additional text in the discussion, to better address this limitation.

Although our experiments are at a different scale, we think that the processes resulting in the formation of open spaces might be equivalent to the cavitation processes inferred from natural shear zones samples (e.g. see Rybacki et al 2008; High-strain creep of feldspar rocks: Implications for cavitation and ductile failure in the lower crust, Geophysical Research Letters, 35(4)). Observations indicate that this phenomenon might be not only be related to pore fluid pressure, but could also depend on local stress concentrations and strain compatibility problems at grain boundaries, triple junctions or matrix/particle interfaces. In our case, potential sources of instabilities are surrounding flakes and layer interfaces by local problems to maintain the strain compatibility.

Apart from that, open cracks and voids are more common in rocks deformed in ductile conditions than we normally think. Structures associated with their collapse can be widely seen in rocks deformed in the middle and lower crust in many field areas (e.g. see Bons et al., 2008 and references therein). We agree that there are limitations when directly extrapolating experimental observations to nature. However, despite the differences in the mechanisms of nucleation formation of such cracks between the experiments and nature, the processes of collapse and linkage associated with progressive deformation are very similar. Our models illustrate very well these processes, and therefore constitute a new piece of work that can help other scientists understand how fractures develop in viscous media.

We have included additional text in the discussion section to address the low confining pressure issue, and how it relates to the opening of cracks and voids.

Page 421 – comment 1: Don't forget to mention the following paper: Rutter, E.H. (1986): On the nomenclature of mode of failure transitions in rocks.- Tectonophysics, 122: 381-387.

Although we agree with the principles discussed by Rutter (1986), we think that the terms ductile shear zones or brittle-to-ductile transition are now widely accepted in the community. We therefore prefer to use those terms, which are properly defined in the introduction.

Page 423 – comment 1: You should also mention here if strain was plane or non-plane, i.e. true 3D strain.

We have added "and plane strain conditions".

Page 424 – comment 1: You should describe the method and apparatus used to produce these layers with constant thickness. You should also list the standard deviation of the thickness.

We used an adjustable hand rolling pin for engraving art, which is quite precise once adjusted. We have included a more detailed explanation in the text. Layers were meant to have a thickness of about 4.25 to 4.50 mm, but paper flakes can alter this

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thickness up to ± 0.5 mm along each layer. We have included this information in the text.

Page 424 – comment 2: Note that flattening with a rolling pin results in strain, but most plasticine types show significant strain hardening. This problem has to be addressed when talking about material properties. The viscosities of the material listed have probably been determined without pre-straining the material.

The mechanical properties of the experimental materials were determined using cubes of different mixtures. These cubes were assembled by compressing the plasticine with a wooden plate in order to form six faces. These cubes were then cut with a saw to get the appropriate size (10 cm long sides). The resulting pre-strain of the mixture is probably of the same order than the one applied by the hand rolling mill. Anyway, it is not possible to detect the strain hardening effect potentially induced when assembling the models because of the relative low precision of the stress cells (which have a precision of 1 kg). We have included new information on the procedure to build the cubes in Sect. 2.3.

Page 424 – comment 3: Such type of intrinsic anisotropy could already be present in the plasticine because of pre-straining it when forming the thin layers. It is possible that the filler of the plasticine consist of plate-shaped components. Did you investigate the filler type?

As explained before, we do not know the exact composition of these two commercial plasticines. We only know that the filler is an inorganic mixture made of clay, paraffin and oil. But we ignore the exact composition or the volume content of each material. However, our tests indicate that these mixtures behave isotropically when there are no layering or preferentially-oriented second phases. We made no changes here, since we think this point is already properly explained in the text.

Page 425 – comment 1: replace "notice" with "note"

Done.

Page 426 – comment 1: This sounds interesting. What is the reason for the different stress exponents. Are the paper flakes acting as passive markers without colliding during the deformation procedure used for measuring the rheological data?

The differences in stress exponents between mixtures composed of pure plasticine and those made of plasticine plus flakes is interpreted here as consequence of the increase of strain partitioning/heterogeneities of the flow induced by the presence of paper flakes. It could potentially happen that local slip between paper flakes plays a role, but we have not observed this phenomenon. The only reason we can observe is how plastic behaviour is enhanced by the presence of paper flakes. We have added a sentence in Sect. 2.3 to address this issue.

Page 426 – comment 2: replace "the ones composed of" with "those"

Done.

Page 426 – comment 3: As has been mentioned already above, you should describe which type of filler is included in the plasticine. There is a difference in mechanical behavior depending on the type of filler (see Zulauf and Zulauf, 2004).

We have addressed this question above.

Page 426 – comment 4: Note that most of your rheological tests indicate strain hardening of the plasticine used, which also has to be addressed.

We already mention this in the previous paragraph.

Page 427 – comment 1: May be it is appropriate to distinguish between microscopic and macroscopic anisotropy. Note you have:

- probably anisotropy in the paper-free plasticine because of pre-straining during the layer production;

- added anisotropy because of paper flakes, which rotate as passive markers during progressive strain;

- added anisotropy because of the different plasticine layers.

We clearly discuss through the manuscript the role of composite anisotropy (due to layer stacking) and that of composite plus intrinsic anisotropy (due to layer stacking and preferred orientation of paper flakes). We do not think anisotropy associated with material pre-straining during layer production makes a difference, compared to the other two types of anisotropy. Additionally, we cannot verify whether the first type of anisotropy suggested by the reviewer actually exists. However, we think it does not exist, as demonstrated by the uniaxial compression tests.

Page 427 – comment 2: In this section, you should also consider the paper flakes. Do

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they act as phyllosilicates compared to natural rocks? It is important to know if these paper flakes collide with each other with progressive strain.

We address this question below, when replying to the comments by the editor. We cannot include the potential role of phyllosilicates and paper flakes in the dynamic scaling equations. This scaling only tries to show that the balance between strain rate, stress and viscosity is of a similar magnitude both in rocks and these experiments. However, we have re-analysed sequences of images from the experiments to detect potential collisions between neighbour flakes, and we have not seen evidences of this process. For example, flake collision cannot be seen at all in the sequence of images displayed in Fig. 8j-l. This process is even less likely to happen during fault growth.

Page 427 – comment 3: You should also present the viscosity ratios between the layers as this is important for the results. The viscosity ratios could be listed in a table.

We have replied to this question above.

Page 427 – comment 4: shear fractures?

Done.

Page 428 – comment 1: as all of your plasticine shows strain hardening, a steady state should not be expected.

Our experimental materials show hardening when they are deformed in homogeneous cubes under uniaxial compression (up to 20-25% shortening). At this amount of deformation there is no failure. On the contrary, when anisotropy is present fractures start forming in the models at about 15-18% shortening. The stress-strain curves of models C and D clearly show softening after yielding. We are just describing the behaviour of the experiments here. We discuss these issues in the discussion section.

Page 428 – comment 2: you should mention the viscosity ratio between the layers.

We have replied to this question above.

Page 428 – comment 3: after 40% and until 40% is not compatible. I guess that these structures formed because of the high strain magnitude, which led to strain hardening of the plasticine.

We wanted to explain here that structures of both types (tension cracks and shear fractures) started to be visible after 30%-40% shortening, but tension cracks only formed until 40%, and after that only shear fractures formed. We have rephrased to clarify this observation.

Page 429 – comment 1: Is there a viscosity ratio between the layers?.

We have already replied to this question before.

Page 429 – comment 2: replace "plascitine" with "plasticine".

Done.

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Page 429 – comment 3: boudinage is only possible if a viscosity ratio between the layers exists.

Strictly speaking, yes. But boudins form here when several tension cracks nucleate in the same layer, even without viscosity contrast. In order to avoid nomenclature problems and genetic connotations, we have replaced "boudinage" with "boudinage-like structures" through the whole manuscript. We now better explain this formation mechanism in Sect. 3.

Page 430 – comment 1: Why did no boudins develop in this experiment? What is the viscosity ratio between the layers. This is important, as the boudins and pinch and swell structures have an impact on the shear fractures.

Boudins did not develop in this experiment because boudins in our models form by the formation of small (restricted to ca. one layer) and consecutive tension cracks along layers. Since tension cracks in this model were longer than that, no boudins were visible. We have added a sentence to clarify this. As explained before, the viscosity contrast between alternating layers of different colours is very low (lower than 1.4 in any case), if we look the viscosities observed from uniaxial compression tests. These are therefore way too low to develop any real pinch-and-swell that could produce ultimate failure and associated boudins. There is no systematic difference between layers of different colours, and boudinage/tension cracks are observed in both types of layers in all models. This reaffirms our observation, using compression tests, that the mechanical behaviour of different coloured layers is indeed very similar.

Page 432 – comment 1: You should depict the viscosity ratios of the different layers to the models. The same holds for the ratios of G. It's too late to emphasize these ratios *in the discussion.*

These are not viscosity ratios. We are just comparing the mechanical properties of the two commercial plasticine types: OCLU-PLAST (models A and B) and JOVI (models C and D). The aim of these sentences is to discuss why the mechanical response and resulting structures of models A+B versus C+D are indeed very different. Since we addressed above the issue of viscosity contrast between alternating layers, we do not made changes here.

Page 432 – comment 2: what does this mean 'statistically oriented parallel to layering'? Is the paper-flake shape fabric present already in the initial sample, or do the flakes rotate during progressive deformation resulting in a shape-preferred orientation of the flakes?

Flakes are statistically oriented parallel to layering because we oriented them when building the models (the fabric is predefined at the initial stage). We have added some words to clarify this point.

Page 432 – comment 3: Note, there is probably a third type of anisotropy related to the plasticine and its filler components. Given these fillers are plate shaped, they will rotate as passive markers resulting in a SPO, which is responsible for strain hardening.

We have already discussed this issue.

Page 432 – comment 4: What is the degree of SPO of the paper flakes during progressive strain?

The development of SPO would imply that paper flakes deform. However, they do not deform. We have not measured their rotation rate because the vast majority of them were already oriented parallel or subparallel to layering during sample preparation, and therefore do not generally rotate during deformation.

Page 433 – comment 1: This is probably not the case. It should be analysed if transverse fractures or pinch-and-swell structures develop earlier during progressive strain.

We have removed these sentences, as suggested by reviewer #2.

Page 433 – comment 2: However, compared to nature, your models do not imply a confining pressure, which might impede formation of fractures and related volume increase of the model.

We have replied to this question above. And following the advice of reviewer #2, we have removed these sentences.

Page 434 – comment 1: is this possible?

We believe it is. Even if there is hardening in the uniaxial compression tests, the multilayer models do not show it. It is clear that the systematic formation of small-scale fractures in this model prevented it from suffering marked hardening, and therefore only a slight hardening was registered. This is exactly what we discuss.

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Page 434 – comment 2: Do you speak about your models or of formation of tension fractures in nature? As your models are free from confining pressure, I guess you are talking about natural examples. However, in your scaling section the confining pressure has not been considered.

We are talking here about tension cracks forming in our models, which are indeed not free from confining pressure although it is low. We address the scaling issue when replying to the editor's comments below. We have added a clause to remark that these statements refer to our models. We have also now better discussed the confining pressure issue (see replies above).

Page 435 – comment 1: Tension cracks could also have formed by the layer parallel drag of the less viscous material on the stiffer layers, which also results in pinch-and-swell and boudinage structures.

This sentence does not refer to the models presented in this paper, but to those discussed in Gomez-Rivas and Griera (2011). The formation mechanisms of different structures depending on strain rate are not the matter of the present manuscript.

Page 436 – comment 1: I think that the asymmetry is caused by heterogeneity in the sample. I suggest to analyze the initial samples using CT to show possible air bubbles.

We cannot perform this analysis, because the experiments have deteriorated since they were made and we do not currently have access to a CT for performing this type of analysis. However, we think the reason proposed by the reviewer is not the cause of the predominance of the dextral fracture array. This model behaves significantly more brittle than the other three. It happened that a few dextral fractures could propagate and link, and therefore they dominate the system. This also happens in nature.

Page 438 – comment 1: This development of different structures, however, has not clearly been described in the present version. This sequence of structures is very important for understanding the final deformation pattern with its macroscopic shear fractures.

We do not agree with this statement. In the results and discussion sections we clearly explain how tension cracks and pinch-and-swell structures first nucleate and then they give raise to two conjugate sets of shear fractures. We think we properly document the sequence of structures and illustrate it with many photographs of the experiments (e.g. Fig. 8).

Table 3: is confetti the same as paper flakes mentioned in the text?

Yes, we have replaced "confetti" with "paper flakes".

Figure 2: In the figure captions you are talking about principal stress, whereas in the figure the principal strain axes are depicted. This might be confusing for the reader.

We prefer to orient the figures with respect to the principal strain axes. In this case the principal stresses (both applied and resolved) are parallel to the principal stress axes. In case of oblique layers this might not be the case, as in the models published in Gomez-Rivas and Griera (2012), where the resolved principal stresses differ from the applied ones (and are not parallel to the principal strain axes). We therefore prefer to leave most of the figures as they are, since we think the orientation of both axes is clear enough to the reader. Following the reviewer's advice we have included sigma 1 and sigma 3 in Figure 2.

Figure 4: add dimension of viscosity and add dimension of strain rate. Remove "plot" from the caption.

Done.

Figure 5: you should also depict the viscosity ratio between the individual layers for each model. These are important for the development of pinch-and-swell structure.

We have replied to this question above.

Figure 7: Given this is a XY section, it should be parallel to the layer. Is this the case? I guess that the fractures were recorded in XZ sections.

Sorry, this is a mistake. It should be *XZ*. We have corrected it.

Reply to reviewer #2 (T. Duretz):

General comments

This contribution presents analogue experiment of multilayer deformation. The authors utilises layered elastoviscoplastic composite materials, which are subjected to bulk pure shear deformation. They report a transition from non-localised (distributed) to *localized deformation, discuss the role of mechanical anisotropy and the resulting fracture patterns. In general, the paper in a pretty good form, with only few typos. The analog experiments seem relatively well designed and calibrated. The model results are described into detail and are quite instructive. However I found that some results/statements are quite confusing and that some aspects of the model description are missing. Therefore I provide some comments, questions and suggestions that the authors can address prior to final publication.*

Specific comments

1) p. 424. You mention the density of the paper flakes. Are they neutrally buoyant?

The flakes do not raise or sink when immersed in plasticine. They just stay where they are. The mixture and its components behave as a solid. We now mention the bulk density of these plasticines in Sect. 2.2.

2) p. 424. Stress and viscosities are important quantities. Do you refer to bulk normal stress (probed along the pistons) and bulk normal viscosity? For plasticines with paper flakes, was the compression direction normal to the statistical orientation of the flakes?

Yes, we refer to normal stress and normal effective viscosity. The equations used are listed and explained in Gomez-Rivas and Griera (2011). We decided not to repeat here the explanation of the equations, since the reader can easily find them in to the references. The compression direction in multilayer models containing paper flakes was normal to layering and flakes. We think this is explained in the text, but we have added a sentence in Sect. 2.2 to clarify this point.

3) Another important parameter is the volumetric fraction of flakes. What is the approximate value of this volumetric fraction of flakes in the presented experiments?

It is 10%, as explain in Sect. 2.2 of the manuscript.

4) Table 1 and 2 contains rheological data (power law parameters and estimated shear moduli). Since the models deals with plasticity and crack formation. It would be nice to list the complete list of parameters (moduli for volumetric deformation, tensile strength, cohesion. . .).

We do not have all these data. A limitation of our uniaxial compression tests is that samples did not reach brittle behaviour. From these test results, we were able to calculate the main elastoviscous parameters such as viscosity, stress exponent and shear moduli, but we could not estimate tensile or shear strengths. We agree with the reviewer that it would be interesting to know other mechanical parameters but this would require the design of a completely new set of mechanical tests, and we are not able to do it at this stage. However, we have characterised the mechanical properties of the different plasticine mixtures following the same procedure as that of similar studies published (see references in Sect. 2.3). The resulting variables are listed and discussed in the paper, in the same way as in previous similar studies. We have now added a new parameter (volumetric strain) in Table 1. This shows how the material slightly loses volume (1-5% vol. at 10% sh.) when subjected to deformation.

5) p. 427 You mention a "degree of anisotropy". This should be important to interpret the results of the experiments. Can you introduce this measurement and give estimations for the different experiments? Is this anisotropy estimation valid for models characterized by intrinsic and composite anisotropy?

Determining the degree of anisotropy is not straightforward. We could only estimate the degree of anisotropy from the difference of effective viscosity between experiments with layers oblique and parallel to the deformation axes. This estimation is listed in the manuscript and results in a degree of anisotropy of ca. 6. These calculations are shown in Gomez-Rivas and Griera (2009), and discussed there.

6) p. 427 - 5 In the model scaling section, you mention dynamic scaling but I don't find information about the gravity. Are the models scaled with regard to the gravity stress? Please add information about this and give the complete scaling including flow stress/gravity stress ratios. This is also why the density needs to be mentioned in the parameter table.

The material behaves as a solid, there is no density contrast between alternating layers and flakes do not sink or float. We are using plasticine, not analogues such as PDMS that can be strongly affected by gravity. The scaling just tries to show that relationship strain rate vs viscosity is of a similar order both in experiments and nature. We have not scaled gravity, and we assume a similar gravity for our models and nature. Attending to the scale of our models (cm scale) and the low velocity, we have assumed that inertial effects are negligible. In order to answer the question raised by the reviewer, we have improved our explanation of the methods used to perform the dynamic scaling of the models. Moreover, the density of plasticine is now listed.

7) The values of schist viscosity are calculated for temperatures ranging between 500-700 C, it seems a warm for the middle crust (see table 3 and p. 427-15). Characteristic viscosities of order 10^{18} *or* 10^{19} *Pa s for the middle crust is extremely and would be likely results in purely viscous deformation and not brittle structures.*

Maybe the author could use more representative crustal viscosity values for the scaling.

We used these values, which we obtained from the references listed in the caption of Table 3. However, we agree with the reviewer that these values of viscosity are probably too low because they imply too low flow stresses. For this reason we have taken more representative values (ranging from 10^{19} to 10^{20} Pa s, using Talbot, 1999 as a new reference) to compare the scaling in our models. We now mention that the viscosities we obtained are lower than the typical natural ones for schists, although are still within the range of published values. However, it is important to note that most of the deformation in these experiments (except in Model D) is accommodated by viscous flow, while brittle structures are restricted to stages when strain is relative high. Therefore, we should expect corresponding relatively low stress values in equivalent natural rocks.

8) p 428-20 - The notion of "void" is introduced here. What does it actually mean?. It seems that these cracks are not related to bubbles since the plasticine receives a special treatment to avoid bubbles. What are the voids filled with? Are these voids corresponding to the cracks themselves?

We agree with the reviewer. We have used the terms tension cracks and voids to refer to cracks formed due to layer extension in *X*. But this is confusing. Therefore, we have removed the term "void" in the whole manuscript. All these structures are in fact tension cracks, but they might have different geometries.

9) More generally is the process of void/crack collapse relevant for natural rocks since cracks are generally filled with minerals and therefore not willing to collapse?

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Cracks collapse in nature. There are several publications describing this phenomenon in the middle to lower crust. In the discussion section we provide references to publications addressing this issue, and discuss how our experiments relate to it (p. 435, 5).

10) p. 430-20 The sentence about length scales and strain localisation is unclear. Do you mean that the shear zones do not cut across the entire model?

Yes, this is what we wanted to mean. We have added a clause here to clarify this point.

11) p. 432-20 If I understand well, strain localisation did not occur in plasticines containing randomly-oriented flakes nor in those containing aligned flakes ("…samples did not fracture during uniaxial compression", p. 426). Heterogeneities (randomly oriented or aligned flakes) did not produce stress perturbations that were large enough to trigger yielding. Strain localisation only occurred in multilayer cases (composite anisotropy), is that right? If yes, then it is not consistent with the sentence l. 24.

We did not explain it well here. Strain localisation did not occur in the uniaxial compression tests because they were isotropic and they only reached 20-25% shortening. On the contrary, failure occurs in anisotropic experiments at 15-18% shortening. This indicates that anisotropy indeed enhances failure and strain localisation. We have added a clause in this sentence to make this point clear.

12) p. 433-10 The statements about tectonic underpressure are vague and qualitative - or not well formulated. In Mancktelow (2008), underpressure is shown within an already formed and filled neck (non void) subjected to extension (suction effect, see fig 6). I am not sure there is a statement that underpressure is the reason for forming the crack. I would suggest to either rewrite or remove this part of the discussion. I would

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rather discuss the role of stress variations around hetegeneities. In general, stress variations (and hence pressure variations) occurs in the vicinity of material interfaces and heterogeneities. Stress variations are indeed proportional to rheological contrasts and geometries. In the presented models, stress variations due to flakes did not promote localisation in non-layered experiments. In the layered case, stress variations due to mechanical layering are sufficient to trigger the development of structures (i.e. pinch and swells) within the timeframe of the experiments.

We agree with the reviewer that this is a bit confusing, and therefore have removed these sentences following his advice.

13) Strain localisation in viscoplastic multilayers (hence in the absence of any elastic effects) was documented in the numerical models of Schmalholz and Maeder (2012). The experimental conditions are fairly similar to those of the present study, I would suggest the authors to discuss these results.

We agree with the reviewer. We have added a sentence introducing these simulation results in p. 434, 5.

14) p. 437-25 the fractures are considered as "easy slip". Does it mean that, within the duration of an experiment, once a layers breaks (losing cohesion), it cannot heal (by bonding the same way you assemble the multilayer)?

We have not observed healing. A very small number of fractures may keep their length and displacement through the experiment, but the vast majority of them continue being "active" (by different combinations of slipping, linking and propagating), although propagation takes place at a small rate resulting in high maximum displacement versus length ratios. We have added a clause to clarify the potential healing issue.

15) From figure 8 (type A) it appears that all the layers do not have the same thickness. Does this affect the model results?.

Layers are meant to have the same thickness, although the method of building them does not allow an extremely high precision. It has to be noted that these photos already show deformation stages at 30%, 40% and 50% shortening. Therefore, layers already underwent extension in *X*, and therefore start pinching. There are some apparently thicker layers (twice as thick) in all the models. These are in fact two consecutive layers of the same colour, and were introduced as reference layers, to help identifying the structures and layers. Their behaviour is exactly the same as the others. We have added a sentence in the Sect. 2 to explain this.

Reference: S. M. Schmalholz, X. Maeder, Pinch-and-swell structure and shear zones in viscoplastic layers, Journal of Structural Geology 37 (2012) 75-88.

We have included it.

Technical corrections

1) In the assembled pdf, all mathematical symbols appear as squares (minus signs, multiplications, tilde).

This must be a problem associated with the PDF reader the reviewer has used. We can perfectly see all the symbols, and we understand that reviewer #1 and the editor can also read them since they have not complained.

2) p. 425-16 It is written "both authors" but three authors are co-signing this contribution.

Yes. This is a mistake. We have corrected it.

3) p. 429-10 - "plasticine" instead of "plascitine".

Done.

4) p. 429-25 the symbol "n" has two meanings, one for stress exponent (p. 426), one for fracture counts. Please use different symbols.

We have fixed this. We now use the symbol N for the number of fractures per set. We have modified Table 4 and Figure 7 accordingly.

5) p. 435-12 "mixed-mode" instead of "mixed-more"

Done.

6) In general figure 8 contains a lot of detailed annotations, it is however not easy to distinguish all the subtleties. For example, I do not see much at the tip of the arrow #1. It could be useful if the authors would select some key features and provide enlarged images of them.

The purpose of this figure is to summarise, with examples, the different types of structures forming in our experiments, and how they evolve to form conjugate shear fracture sets. We think this figure zooms enough in the selected areas, since zooming much more would result in pixelated images. Arrow #1 is showing how a shear fracture nucleates from a pinched layer. We have corrected this in the figure caption.

7) Figures 3, 4 and 6 have slightly "heavy" axis label annotations (e.g. stress (nsigma) [Pa] (x10^5) on figure 6). In general, I would rather write the mathematical symbol and its corresponding unit, I guess it's just a matter of taste. Also, units are missing for quantities stated in figure 4 (strain rate, effective viscosity).

We have modified the three figures, as suggested by the reviewer.

Reply to editor (N. Mancktelow):

Both reviewers are positive and recommend publication after minor revision, with which I agree. However, the reviewers also provide a quite significant list of queries and suggestions for improvement that should be taken into account, and responded to individually, when revising the manuscript. I have also made some comments and suggestions on the uploaded annotated PDF of the manuscript. Although this is not a third formal review, you might consider these comments in revising, especially the additional references providing field and experimental / modelling evidence for a general lack of propagation of precursor fractures during subsequent localized displacement in an overall ductile regime. The experiments are well-designed and documented and the presentation well-structured. The English is in general also very good, so my compliments for non-native speakers of the language.

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Specific comments:

Page 420 – comment 1: I am not sure what this means exactly - and I suspect many others readers of the abstract (in isolation) would not immediately understand either.

We agree that this sentence might be confusing without context. We have rephrased.

Page 421 – comment 1: remove "Mancktelow, 2006"

We have applied this change.

Page 421 – comment 2: add "Passchier, 2001; Exner et al., 2004; Kocher and Mancktelow, 2005, 2006;" and "2006,"

We have applied this change.

Page 421 – comment 3: add "Mancktelow and Pennacchioni, 2005;"

We have applied this change.

Page 422 – comment 1: (e.g. Exner et al, 2004; Kocher and Mancktelow, 2005; Pennacchioni and Mancktelow, 2007)

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We have applied this change.

Page 423 – comment 1: locally the effective stress (i.e. - pore fluid pressure) might be tensional. the total stress will never be tensional any depth in the crust.

Yes. We have modified the sentence to add this point.

Page 424 – comment 1: rather large when scaled to the natural example, even at 1:1 for length

Paper flakes had a maximum size of 2 mm, but there were smaller flakes too. We have added this to the text. We agree with the reviewer. However, the flakes were preferentially inserted parallel to the layers in order to increase the degree of anisotropy, and not to simulate phyllosilicates. We have added a sentence in the methods section in order to clarify this point. The role of these flakes will never be the same as that of planar minerals in real rocks. Therefore, a perfect geometrical scaling is impossible with analogue models. We think our results are valid and provide good insights of the influence of the degree and type of anisotropy on the resulting structures.

Page 426 – comment 1: which will promote eventual failure as the stress rises to touch the yield envelope

Yes, eventual failure could occur. But there are two factors that together prevent failure in these tests. One of them is the amount of shortening these tests reached (up to 25%). Macroscopic fractures in multilayer models are only visible at 15-18% shortening. The other factor is anisotropy. These tests were built without layering and with flakes randomly oriented. Anisotropy enhances earlier fracturing, but these tests were not anisotropic, and therefore failure would have occurred later (certainly at $>$ 25% sh.). We have incorporated a sentence in the text to explain this issue.

Page 427 – comment 1: not clear how you get to this result. Pa s is N m^{-2} s – the time scaling is given (ca. 10^{-9}), the length scaling is 1 as you state, but then you need to give the scale in applied force (N) to determine this final scaling of viscosity. Please give a bit more detail here.

We obtained these results following the principles of similitude of Ramberg (1981). Since gravity is not scaled (i.e. nature and model have the same gravity) and the length scale is assumed 1 (i.e. 1:1), then the scale of applied force between model and nature is controlled by the difference in density between plasticine and medium/lower crust rocks (e.g. schists), so that *F* = *density* x *volume* x *g*. Therefore the ratio of modelled to real stress (σ*) is the density ratio between model and nature. The equivalent viscosity can be calculated knowing the strain rate ratio (assuming a natural strain rate of 10^{-14} s^{-1}). The calculated values range between 3 x 10^{18} Pa s and 1.7 x 10^{19} Pa s for the softer and harder materials, respectively. We have modified the whole section in order to clarify how we calculated the scaling relationships.

Page 427 – comment 2: this is a rather low viscosity which would result in flow stresses on the order of 0.1 MPa at 10^{-14} s^{-1}

We agree that these values are relatively low, but they are still in the lower range of natural rock viscosities (e.g. see Talbot, 1999 or Davidson et al., 1994). We would like to remark that this series of experiments (except model D) show dominant viscous deformation, which is more coherent with low stresses. Brittle deformation in these experiments only occurs after relatively large strain, and bulk stresses in the system are mostly dominated by viscous flow. For this reason, there are no large drops of stress after fracture onset, and stress variations are somehow gradual. We have modified Table 3 to include higher viscosities.

Page 428 – comment 1: as expected - see line 17 below. Clearly "stiffer" models are more prone to hit the yield envelope and fail

Yes, this is not surprising. But we do not know what the editor proposes to add here to the manuscript. We are just describing the behaviour of the models.

Page 428 – comment 2: not surprising - clearly there is more likelihood to (locally) touch the yield envelope as the flow stress increases - eventually every material must fail

Same as previous reply.

Page 429 – comment 1: the question is how realistic this is in relation to nature. Firstly the flakes are large in comparison to typical rock grain size and secondly most flakes (phyllosilicates) are the relatively weak phase in natural rocks.

The insertion of flakes in our models did not aim at simulating individual planar minerals, such as phyllosilicates. Instead, our goal was to define a macroscopic intrinsic anisotropy in the model, in addition to the composite anisotropy produced by layering. We do not agree with the assertion that phyllosilicates are always the relative weak phase in natural rocks. Some studies compared the stiffness matrices of quartz and muscovite. For example, Bass (1995, in: T.J. Ahrens (Ed.), Mineral Physics and Crystallography. A Handbook of Physical Constants, AGU Reference Shelf 2, pp. 45–63) observed that the young modulus in some directions of muscovite can be higher than that of quartz. From the point of view of plasticity, phyllosilicates are expected to behave in a highly anisotropic way, with a weak slip system parallel to the basal plane and hard-to-activate non-basal slip systems. Therefore, we can expect that phyllosilicates behave as a weak phase in rocks where they are randomly oriented. However, for certain orientations (e.g. normal to the basal plane) it would be difficult to activate the weak slip system and these minerals will probably behave as a hard phase. This explains, for example, why in some circumstances phyllosilicates show intracrystalline kink-folds that cannot be observed at the fine quartz rich matrix. However, we do not aim to discuss the elastoviscous role of phyllosilicates in this manuscript, since we think it is beyond our goals.

For the presented experimental conditions, where paper flakes are oriented normal to the shortening direction, it would be possible that planar components of a natural rock in a similar situation behave has a hard phase. In these settings, perhaps phyllosilicates could behave as a stiffer phase than quartz, depending on the deformation conditions. This coherent with numerical simulations by Naus-Thijssen et al (2010, J. Struct. Geol., 32, 330-341) exploring the role of phyllosilicates at different stages of crenulation development. Anyway, the role of these minerals in natural rocks can be really complex, because recrystallization probably affects quicker phyllosilicates than quartz.

Page 433 – comment 1: remove "both"

Done.

Page 433 – comment 2: As in natural examples of boudinage, the fractures will almost certainly nucleate in the more viscous ("stiffer") layers because they will touch the yield envelope first - both because of the higher deviatoric stress in these layers (larger radius to the Mohr circle) and because of the lower pressure ("tectonic underpressure" = shift in the centre of the Mohr circle for stress) relative to the matrix when stiff layers are extended

Yes, but we clearly explain in the text that layers of different colours do not have very different mechanical properties. Accordingly, we can see how structures nucleate in all layers, regardless of the colour.

Page 433 – comment 3: "well-bonded" does not exclude the pressure effect - indeed the simplest example as presented in Mancktelow 2008b assumes perfect bonding

We agree. We have removed "well-bonded".

Page 434 – comment 1: realistic?

We have addressed this question before.

Page 435 – comment 1: replace "than" with "as"

Done.

Page 435 – comment 2: replace "evidencing" with "which is evidence for"

Done.

Page 436 – comment 1: not really correct English

We think "As evidenced by" is correct English. However, we have replaced it with "As demonstrated by", which sounds better.

Page 436 – comment 2: replace "such" with "this"

Done.

Page 437 – comment 1: replace "evidence" with "show" or "establish"

Done.

Page 437 – comment 2: replace "with associated" with "associated with"

Done.

Page 437 – comment 3: replace "to propagate" with "from propagating"

Done.

According to the modifications of the text, we have added the following references to the reference list:

- Exner, U., Mancktelow, N. S., and Grasemann, B.: Progressive development of s-type flanking folds in simple shear, J. Struct. Geol., 26(12), 2191–2201: 2004.

- Kocher, T., and Mancktelow, N.S.: Dynamic reverse modelling of flanking structures: a source of quantitative kinematic information , J. Struct. Geol., 27(8), 1346–1354, 2005.

- Kocher, T., and Mancktelow, N.S.: Flanking structure development in anisotropic viscous rock, J. Struct. Geol., 28(7), 1139-1145, 2006.

- Passchier, C.W.: Flanking structures, J. Struct. Geol., 23(6-7), 951–962, 2001.

- Ramberg, H.: Gravity, Deformation and the Earth's Crust, Academic Press, London, 1981.

- Schmalholz, S. M., and Maeder, X.: Pinch-and-swell structure and shear zones in viscoplastic layers, J. Struct. Geol., 37, 75-88, 2012.

- Smithson, S.B.: Densities of metamorphic rocks, Geophysics, 36(4), 690-694: 1971. Talbot, C.J.: Can field data constrain rock viscosities?, J. Struct. Geol., 21(8-9), 949-957, 1999.

We have also removed the following references, following the reviewers' advice:

- Mancktelow, N. S.: Tectonic pressure: Theoretical concepts and modelled examples, Lithos, 103(1-2), 149–177, 2008b.

Finally, we have added the following sentence in the acknowledgements section: "We gratefully acknowledge G. Zulauf and T. Duretz, whose constructive reviews greatly improved the manuscript, together with the editorial guidance of N. Mancktelow."

Interactive comment on Solid Earth Discuss., 7, 419, 2015.

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