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**MACQUARIE**  
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Sydney, 31<sup>st</sup> July 2015.

Dear editor and referees,

Please find attached the revised version of our manuscript. First of all, we would like to thank the referees and editor for the considerable time and effort they've given to the critical but overall positive assessment of our manuscript. Their constructive comments and enquires about the details of the numerical model used and suggestions for improvement have greatly helped to improve this contribution and we hope you agree the manuscript in its revised form is now acceptable for publication in *Solid Earth*.

In the revised version, we have taken the detailed comments and questions by the reviewers into account. Many of the comments stem from misunderstanding the numerical set-up, hence we have taken more care in explaining in detail how and why the model is set up the way it is. We have extended our introduction and discussion of possible numerical models and reasoning for the use of our model. In addition, we have toned down the emphasis of brittle failure as a prerequisite for pinch and swell development, emphasising that brittle failure with subsequent viscous flow is one of many viable softening mechanisms. Suggestions by reviewer 2 on how to convincingly test the numerical stability and accuracy of results have been incorporated, for example we now provide a supplementary figure showing the effect of mesh size on model behaviour. Furthermore, we have recalculated the multilayer model to correctly incorporate gravity with depth.

We have also expanded the field example documentation and discussion considerably by providing an additional field example, as reviewer 1 had some issues with this.

As you will see, we have conducted thorough revisions including new improved numerical models, with additional model and field information to support our conclusions. Please find below, details on how we addressed the general comments and specific questions/suggestions raised in the reviews.

Thank you for your patience.

With kind regards,

Robyn Gardner, Sandra Piazzolo and Nathan Daczko

## Reply to Reviewer 1

### Major comments

#### 1) Microstructures

The interpretation of conjugate sets of brittle fractures of mode-2 (shear fractures), referred to as "through-going micro-shear bands" (1534, 3), based on the microstructural observations and the photographs of outcrop-scale structures presented in Figure 1, are not convincing and the following points should be addressed:

(i) Where exactly are the conjugate sets of shear fractures, i.e. a set of two perpendicular planes oriented  $45^\circ$  to the boudinaged layer, visible on the outcrop scale (Fig. 1a,b,c) and on the micro-scale (Fig. 1e)?

RESPONSE: We agree that in the field, the brittle structures are not conjugate sets *sensu stricto*. Therefore, we have removed the term *conjugate* in the text at 1522, 13; 1523, 27; 1524, 21; 1536, 13; as conjugate indicates that the angle from the primary stress direction is  $< 45^\circ$ . In our observations of the St Anne Point samples these are in fact  $> 45^\circ$ . Note that as shown in our models, the angle between initially conjugate sets increases with increasing strain as the brittle-then viscous planes rotate.

We've added XPL photomicrographs for both Fig. 1d and e (now Fig. 2d and e) with an expanded view (PPL and XPL) of the thin section to show the brittle structures in more detail. In Fig 2e (with yellow lines indicating the brittle failures), we now show four amphibole grains with brittle fractures. The sigma 1 stress direction that initiated the brittle failure has been added to the Figure for clarity.

(ii) Where is proof that these shear fractures actually penetrate the entire mafic layer, on the microstructural or the outcrop scale? With this respect, drawing red lines on a field photograph is definitely not enough for such an important statement.

RESPONSE: In Fig. 1c (now Fig. 2c), a brittle fracture that cuts near-continuously through the entire mafic layer at the outcrop scale is shown (and now highlighted). An inherent problem to show "just" brittle structures, stems from the fact that in our view after brittle failure, fluid influx causes softening through viscous flow thus partially obliterating the brittle nature of the structures. The connected fractures that become key failure planes and sites of strain localisation have since formed the necks between the swells and their brittle pre-history is difficult to recognize. The fractures shown in Fig. 2e are examples of these residual brittle structures, but the example shown did not localise enough strain to penetrate the whole layer and allow the formation of a neck (hence they are still present as near-brittle structures). We now comment on this in the figure 1 and 2 captions and text in Section 2.

For example, the caption for (now) Fig. 2 has been reworded to indicate that these are residual brittle fractures which did not localise enough strain to form a neck, but which we infer to show the process that initiated the localisation that caused the necks to form.

To further emphasise the relevance of brittle structures as precursors of viscous softening, we now provide another representative field example: a pinch and swell structure from Wongwibinda, New England, NSW, Australia (now Figure 1). These Wongwibinda

structures formed at  $\sim 600^\circ\text{C}$  and  $\leq 200$  MPa (Craven et al., 2013). The brittle deformation is easier to see due to the simplicity of the plagioclase mineralogy and minor fluid influx.

(iii) I do not find the occurrence of single biotite grains indicative of brittle failure of the entire ca. 10 cm-thick mafic layers. Is the slight trace of an oblique (to the main foliation) oriented biotite clast indicative of a shear fracture that penetrates through the entire mafic layer? To me, the trace of the suggested shear band (dashed line in Fig. 1e?) is not clear. I cannot observe biotite precipitated along this zone.

RESPONSE: To clarify the issue raised by the reviewer, we've now added labels and arrows in (now) Fig. 2e to indicate the original biotite ( $\text{Bt}_A$ ) and Quartz ( $\text{Qz}_A$ ) and the phases which we interpret to have precipitated during/after the brittle deformation event ( $\text{Bt}_B$  and  $\text{Qz}_B$ ). The bulk of the quartz and biotite is original ( $\text{Bt}_A$  and  $\text{Qz}_A$ ), with the smaller biotite and quartz ( $\text{Bt}_B$  and  $\text{Qz}_B$ ) crystals grown in the fractures after brittle failure.

(iv) Is the amphibole clast, suggested to be intragranularly fractured (missing asterisks in Fig. 1e?), evidently fractured? Its thickness left and right of the supposed shear plane does not fit.

RESPONSE: Yes, we are sorry for this omission, the asterisks/stars were missing and are now added. In the expanded thin section image four examples of fractured grains are highlighted (star pairs) (Fig. 2e). The mismatch of the thicknesses across the fracture can be accounted for by a small amount of rotation of the broken pieces (in 3D). The latter comments are now added to the figure 2 caption.

(v) The suggested plane (dashed line; Fig. 1e?) runs vertical to the suggested extension direction, so is this a mode-1 structure? I suppose it is not, as I cannot observe any extensional features. Biotite should precipitate within such a structure in agreement with the direction of stress or shear. The orientation of this assumed failure plane is not in agreement with mode-2, nor am I able to follow the shear plane across the structure based on this micrograph. Moreover, I do not observe a conjugate set of these planes on the micro-scale.

RESPONSE: This is a misunderstanding. Thus, we have rotated the image so that foliation is horizontal, allowing for easier analysis of the near-conjugate set of brittle structures, and we have indicated the original vs. the newly precipitated biotite and quartz and the inferred stress directions in Figure 2e.

(vi) Mechanically weak phases are abundant all around the boudinaged layer (e.g. see biotite grain that defines the foliation in Fig. 1,d). Furthermore, according to Klepeis et al. (JSG 1999), the meta-sedimentary matrix of the St. Anne point host rock is composed of garnet, quartz, amphiboles and rutile. Does biotite also occur in the host rock (matrix)? Based on the micrograph taken at the outermost rim of a swell, close to the necking area, biotite seems to be also present in the host rock (Fig. 1e?), forming the foliation. Under the considered metamorphic conditions, at least quartz and biotite act as weak phases. For this reason, this kind of heterogeneous mineral distribution could also be treated as a homogeneous feature of the surrounding matrix and therefore be excluded for introducing instability. For clarification, please add a short description of the mineral composition and deformation mechanisms of the matrix. The latter will make the discussion of the results obtained from the sensitivity study of the power-law exponent more practical. As the identification of shear fractures is of great

importance for the numerical modelling scheme and the general concept, more insight and discussion have to be added. Please consider providing more indicative microstructures.

RESPONSE: Our terminology has caused confusion. For clarification Fig. 1e does not depict the layers surrounding the mafic layer, it shows only the neck area of the mafic swell. This has now been clarified in text and figure captions. As the reviewer indicates, the metasedimentary surrounding layers do include quartz and biotite as weak phases, but near the pinch and swell structures depicted here, these layers do not include any amphibole. Typically the metasedimentary layer quartz, biotite and plagioclase form S-C fabrics rather than brittle fractures, with the quartz commonly forming ribbons of recrystallised grains displaying grain boundary migration and minor undulose extinction. The modes of quartz and biotite in the matrix are much higher than in the mafic layers. Garnet grains in the matrix can be fractured and/or have strain caps and shadows. To clarify this issue further, an additional description and figure of the surrounding metasedimentary layers have been added to the St Anne Point Petrology supplementary data. We have also checked the use of matrix throughout the manuscript to avoid confusion.

We believe the variation of biotite and quartz modes between the swell centre and edge is of significance, indicating increased softening on the edges of the swell structure by reaction weakening through biotite growth. As now discussed in more detail the text and supplementary data, the modes of the weaker minerals (biotite and quartz) are increasing from the swell centre towards the neck suggesting that fluid influx in the initial brittle structure caused reaction weakening and subsequent viscous flow (softening).

## 2) Numerical modeling

In the numerical scheme (Moresi & Mühlhaus, PhilMag 2006), softening is induced by brittle yielding of the central layer. The sensitivity (rate) of softening is partly driven by the factor RCO. In Table 2 the authors summarize the numerical experiments for different rheologies. However, the following points have to be critically reviewed:

(i) In case of the numerical experiment with a viscous creep rheology exclusively, there is indeed no softening mechanism implemented. I miss the introduction and discussion of the classical concept of localization within rate- and temperature insensitive materials and, more generally, the common assumption that pinch and- swell structures form in a viscous manner, during continuous necking of a power-law layer, embedded in a weaker matrix (e.g. Schmalholz & Maeder, JSG 2012). These could be mentioned at page 1520, for instance.

RESPONSE: The comment by the reviewer highlights the necessity to review in the introduction in more detail the suggested prerequisites for pinch and swell structure formation. Thus this has been rectified (Introduction page 2, 31-33). Our results show that brittle failure with subsequent viscous weakening is one possibility to make pinch and swell structures without having to invoke extreme  $n$  values for viscous power law behaviour. We've found if you use a strain localising material ( $n \geq 1$ ) you can get pinch and swells to form. In our model of the exclusively viscous creep rheology we don't get any strain localisation as we don't have any geometric perturbation and our numerical/statistical perturbations are only used when modelling M-CB materials. Hence, no localisation and no pinch and swells formed. We have now added another clarifying sentence in Section 4.1 page 8, 14-18.

The authors finally imply that the initiation of pinch-and-swell structures in general is due to the brittle behavior of the material. If there is no softening in the dislocation or diffusion

creep rheology accounted for by e.g. a negative power-law exponent or viscous shear heating, how can the layer localize? I fear that the authors draw a fundamental conclusion (e.g. 1532, 1-7) based on the limitations of the numerical concept.

**RESPONSE:** We agree with the reviewer that brittle failure with subsequent viscous flow is NOT the only possible strain localization and softening process. However, we maintain that it is one viable mechanism and from field observations along with recent literature, a possibly common geological feature. The initiation of viscous softening can occur through brittle failure with subsequent fluid influx and new mineral growth/reaction. This is now explained explicitly in the text (Section 2 page 6, 19-31) and discussed in more detail in the discussion Section 6.1 page 17, 6-14.

(ii) Unfortunately, the authors do neither discuss nor introduce how the onset of localization occurs and where? In case of a rate-insensitive material, the fundamental analysis of localization is missing. According to the finite element model of Moresi & Mühlhaus (PhilMag 2006), "healing" of former fracture planes was considered. Is this the case here?

**RESPONSE:** We have not allowed any explicit healing in the numerical model, this has now been added to the description of the model Section 4.1 page 9, 23). We have expanded in the introduction the importance of strain localization for pinch and swell structure formation. In Section 4.1, we clarify why we use the brittle-then viscous behaviour set-up based on our field examples.

More specifically, the model causes the localisation via a numerical perturbation with the site of the resulting localisation being dependent on various computing variables. We ran tests varying the specifics of this numerical perturbation, they do not fundamentally impact the overall pinch and swell formation process. However, the number of swells formed can vary. As this is a limitation of the numerical model, we have not made any conclusions on the numbers of swells formed etc. Hence, our conclusions are of a general nature only. We have now added a paragraph to Section 4.1 page 10, 17-21) on this to clarify the issue.

(iii) The authors lay emphasis on a model setup, in which localization results out of the constitutive description. However, they do not define the brittle localization criterion. Actually, the sketch of the model setup (Fig. 2b,d) indicates a surface roughness of the central layer. So, the initial condition is geometrically perturbed. For this reason, the introduction is misleading. Please specify how the model is perturbed, and how the onset of localization can be explained.

**RESPONSE:** The sketch shows the particles which have properties calculated off the mesh, so the geometry is not perturbed (now clarified in figure 3 caption). However, we do have a numerical perturbation – see comment above for clarification.

(iv) Based on (iii), I am wondering why the resulting pinch-and-swell structure is rather asymmetric in terms of boudin spacing and the geometry of single swells and pinches. A higher level of softening explains unevenly spread sites of localization, whereas a lower level of softening results in a symmetric structure, respectively (1535, 7-9; 1537, 1-3). As the localization process remains hidden in the numerical scheme, please provide a better discussion of how the calculated asymmetries arise.

RESPONSE: Due to limitations of the model (see (ii) above regarding some sensitivity to numerical perturbation algorithm), we don't make any conclusions about the numbers of swells formed and their spacing (see the mesh test). We have found the asymmetry of the geometry of the single swells (i.e. the rotation of the swells) is a result of localisation in a single direction instead of a "conjugate" pair in the necks at either end of a swell. This is now stated explicitly in Section 5.2 page 14, 22-27; Section 6.2 page 18, 25-29 and page 19, 1-4.

If the layer surface is geometrically perturbed (iii), or healed fractures were assumed (ii), the site where localization occurs and the direction from where it propagates are predefined. Consequently, the initiation process itself is reconditioned and therefore cannot be studied.

RESPONSE: The surface is not geometrically perturbed and the fractures are not healed in the model we use. This is now clarified explicitly in Section 4.1 page 9, 23 and Fig. 3 caption.

(iv) Concentrations of strain can be found at the layer-matrix interface (e.g. Fig. 6f). However, the authors do not discuss these matrix effects.

RESPONSE: True, in Figure 6f there is some minor concentration of strain rate at the rheological boundary. However, the strain rate range is very narrow (0.35 to 0.51), suggesting a generally relatively homogenous strain rate across the whole model. It is beyond the scope of this contribution to discuss and explore in detail the local effects on the surrounding layers and vice versa. This would need to be part of an additional contribution.

Next, I miss a mesh sensitivity study and the scale of the finite element simulation. The suggested fracture is at the  $\mu\text{m}$ -scale. What is the finite element mesh size for small-scale and tectonic-scale simulations? Please provide a mesh sensitivity study (e.g. in the supplement).

RESPONSE: Hobbs also recommended a mesh sensitivity study and this has been included in the supplementary material Figure 3. Please refer to more detailed comment under Hobbs comments.

(v) The role of elastically stored energy was reported in various numerical studies. However, the authors refer to the work of Ranalli (1997) and assume that for "high strain" structures, the effects of transient deformation can be neglected. This is an outdated concept. This problem becomes evident as follows. In Figure 3b (data for black star), the material is intrinsically unstable, i.e. failure is obtained instantaneously. Data from within the swell (red star) indicate that the yield stress is reached just above a "stretch" of 1. Next, the sequence of boudinage formation (Fig. 4) indicates that between a stretch of 1.0 to 1.2, localization has already occurred.

RESPONSE: It is true that we have not included any discussion or modelling of elastically stored energy in the manuscript. The reason for this "apparent" omission is that our more detailed analysis of the pinch and swell structures starts when the competent material yields. We have used (as both reviewers point out) an inherently unstable material which in the model yields (fails) immediately the model is started. This is now more explicitly stated in the text in Section 4.1 page 8, 25-26. We focus mainly on the post yielding impact of the material parameters ( $R_v$ , softening and flow regime) on the formation character of the pinch and swell structures. The localisation is established (or not) over the first few iterations of the

model using the method discussed in Moresi and Mühlhaus (2006). The pre-yielding processes, including the effects of elastic energy are, of course, very important, but are not part of this study. This is now stated and explained explicitly in Section 4.1 page 8, 7-13.

I do not see how these findings are in agreement with the definition of "high strain". Furthermore, pinch-and-swell structures are interpreted to indicate low-strain deformation. This becomes evident in studies of crystallographic preferred orientations of dynamically recrystallized grains within necking areas (e.g. Schmalholz & Maeder, JSG 2012) or around coarse-grained clasts (e.g. Bestmann et al., Tecto 2006). These grains reveal a weak preferred orientation and deformation mechanisms indicative of low strain conditions.

**RESPONSE:** We generally agree with the reviewer. We have adjusted our terminology which has misled the reader, so we have changed this to 'higher' as we look at relative strain rather than absolute values. The strain rates are only relative within and between the models. Figure 3 caption and Section 5.1 page 13, 16-20 have been changed accordingly.

### **3) Linking microstructure and numerical model**

In a study that relates observations made from naturally deformed rocks to numerical simulations, there are some insight into e.g. the deformation mechanisms, material properties and flow conditions, at least to some extent. This might be the greatest advantage of such an interdisciplinary study. Be that as it may, the study of real rocks and their microstructures defines a framework for the initial and boundary conditions of a numerical experiment. Thus:

(i) The authors test the sensitivity of viscous creep, post failure, by varying the power-law exponent between 1 and 3. The stress exponent of the dislocation creep flow law was chosen for a certain typical range for grain size insensitive creep (see Tab. S1). As stressed further above, evidence pointing to dislocation creep processes being active during layer-parallel extension could underlie this post-fracture deformation mode. However, I am wondering why a stress exponent of  $n = 1$  was considered at all (p. 1521). Basically, the stress exponent should be in agreement with microstructural observations, i.e.  $n > 1$ . Analysis III (1528, 17-27) should be based on microstructural criteria, so: how realistic is scenario (1)? It has been shown before that only non-linear rheologies reveal a necking instability (Smith, GSABull 1977; Schmalholz et al., JSG 2008). Are there hints towards diffusion creep dominated deformation (grain boundary sliding) in the surrounding matrix or within the mafic layers?

**RESPONSE:** As this reviewer pointed out in 2(i) above, the common assumption has been that pinch and swell structures occur only in viscous materials with  $n > 1$ , as a high  $n$  was needed to allow strain localization. We decided to show results both for  $n=1$  and  $n=3$  to emphasise that it is possible to produce pinch and swell structure in  $n=1$  (in contrast to "common" belief) if brittle failure and then viscous flow is allowed. We now added one sentence to clarify this issue Section 3 page 7, 20-23). For this reason, we believe the inclusion of scenario 1 (Newtonian flow) in the analysis is justified. At the same time it is correct that the microstructures in the matrix around the St Anne Point pinch and swell chains do indicate non-Newtonian flow characteristics (see supplementary Figure 1). As Newtonian flow, that is, grain size sensitive flow is commonly seen in natural rocks we decided to explore the flow regime parameters in the model. We have found that if the assumption of a viscous material, (and thereby the requirement for non-Newtonian flow) is removed, then pinch and swell structures can occur in both Newtonian and non-Newtonian flow regimes where the competent layer can successfully localise strain.

(ii) Studying the literature about numerical simulations of folding and boudinage, the competence contrast, i.e. the difference in effective viscosity between layer and matrix (Hobbs et al., JSG 2011 and references therein), was debated. This in mind, how realistic is a contrast in viscosity of about 125 (1531, 20-22)? Please provide a discussion.

**RESPONSE:** Two orders of magnitude for the viscosity ratios have been used by many researchers in their modelling (for example, Llorens et al., 2013; Passchier et al., 2005; Schmalholz et al., 2008; Takeda and Griera, 2006). For a discussion see Hobbs et al. (2008). We have now added this when discussing the appropriate parameter space for the presented models in Section 4.3 page 11, 26-27 and page 12, 1.

(iii) In case of ductile fractures (1534, 3), a certain amount of plastic deformation is accommodated before brittle failure, by e.g. mode-2 fracturing. Where is evidence of plastic deformation recorded in the central layer? And, where is evidence of massive plastic deformation in the simulated stress-strain curves (Figs. 3; 6)?

**RESPONSE:** When put under stress, a material initially undergoes elastic then plastic deformation prior to failure at the yield stress. As discussed in 2(v) above, we have investigated in detail the characteristics of the post yielding/fracture/localisation behaviour so plastic deformation will not be seen in the stress-strain curves from our numerical model. This simplification and reasoning behind it are now explicitly noted in Section 4.1 page 8, 7-13.

(iv) Regarding the comparison with the work of Mancktelow (Geology 2006), brittle deformation is considered the necessary mode in order to localize ductile deformation (e.g. 1533, 22). A discussion of the dynamic class of localization for viscous materials is entirely missing. This discussion would shed light on the fact that brittle failure is not a necessary condition for proceeding ductile deformation, but one possibility. For this reason, the statement that the initiation of pinch-and-swell structures is of a brittle nature cannot be supported in general.

**RESPONSE:** Yes, this is the same point made by Hobbs, so we have changed this to indicate that pinch and swell structures require localisation and that brittle deformation, such as that seen in the St Anne Pt samples (and Wongwibinda) is one possibility. We have modified the text in the introduction Section 1 page 3, 20-27 and discussion Section 6.2 page 18, 1 as well as conclusion Section 7 page 20, 30-31 accordingly.

Next, the provided microstructural criteria for shear failure of the layers are not convincing. Please consider restricting your conclusions to the limitations of your numerical scheme and the actual microstructural observations, i.e. boudinaged layers deform by different modes, given the geological boundary conditions.

**RESPONSE:** See Microstructures (ii) above. We believe our more detailed microstructural observations provided in improved Figure 1 (now Fig. 2) and new Figure 1 as well as in the new supplementary figure 1, do suggest brittle failure can initiate the localisation required to form pinch and swell structures, and our numerical model confirms this. We have added several lines in the description to Section 2 page 6, 19-29 as well as in the discussion of microstructures in Section 6.1 page 17, 16-18 to clarify this issue.

(v) Unfortunately, the application of the studied parameter range and boudinage geometries is not well explained (e.g. 1538, 24-27). A fluid-like behavior for the development of pinch-and-swell structures is suggested, once the layer is fractured. This implies that the structure is amplified by viscous creep ( $n > 1$ ) after initial fracturing. Based on this sequence and the role of power-law creep of the layer, how important is the factor RCO then? To me it is not clear how the authors attempt to estimate rheological parameters from boudinage geometries, obtained in the field.

RESPONSE: We have now expanded the application of the model to the field Section 6.3 where we discuss estimating the rheological properties of the layers. A future study will concentrate on this issue in more detail as it is beyond the scope of this contribution. Note that we discuss the effect of material softening,  $R_{Co}$  in some detail as we have one sensitivity test dedicated to this effect – Analysis IV Section 5.5 and 6.3 (Fig. 7).

## Minor comments

### Abstract:

1518, 2) Why does the second sentence begin with "However, ..."? This implies that some contrary thoughts, in contrast to the proceeding sentence, are following. Indeed, the flow properties of the lower and middle crust are in general described by viscous creep or more complex (elasto-visco-plastic) rheologies. I do not see how this common concept will change, even though the authors provide insight into a micro-scale structure. Introducing brittle failure, as in Moresi & Mühlhaus (PhilMag 2006), aims at the description of near the transition of brittle-ductile rheology. The typical rheological stratification of the crust, as illustrated in many textbooks (e.g. Passchier & Trouw, 2005, p. 114), is an extreme oversimplification and not a state-of-the-art concept of the rheology of the entire crustal section. For further studies, considering more appropriate rheologies, I have pointed out useful literature further above.

RESPONSE: 'However' has been removed.

1518, 6) The term "flexible" should be avoided. Please refer to your numeric scheme as e.g. "Mohr-Coulomb failure and post-yielding viscous creep".

RESPONSE: "Flexible" has been removed, and the abstract reworded.

1518, 6-10) See the major comment further above.

RESPONSE: See comment and reply above.

1518, 12) What is the condition that limits further strain localization? What process or mechanism is responsible for the arrestment of material softening? Please specify.

RESPONSE: It should be noted that this is the abstract, so an in depth discussion is not possible. However, we take the point that there is an interesting issue regarding the limitation of strain localization. From our models we can conclude that if the softening is very effective, no more strain localization is necessary and a wide zone of weak material governs the material behaviour and shape development. This is now included in Section 6.2 page 18, 28-

30 as well as in the conclusion Section 7 page 21, 7-8 and with one sentence in the abstract Page 1, 19-22.

## **1 Introduction:**

1519, 1-18) I miss references of studies of the frictional-viscous transition (e.g. Brantut et al., JSG 2013; Bürgmann & Dresen, *Annu. Rev. Earth Planet. Sci.* 2008; Karrech et al., JGR 2011; Regenauer-Lieb & Yuen, PAG 2008). Taking those into account, the "flawed" assumptions could better be placed in a geological (and modeling) context.

**RESPONSE:** We have now rewritten the introduction adding the relevant references as suggested Section 1 page 3, 15-17.

1519, 12-14) Recent localization theories that encompass the effects of energy and thermo-mechanical feedbacks and references pointing to them are entirely underrepresented in the manuscript. The concepts of viscous shear heating and the role of elastically stored energy (e.g. Regenauer-Lieb & Yuen, GRL 1998; Regenauer-Lieb & Yuen, PEPI 2000; 2004; Regenauer-Lieb et al., *Nature* 2006; Regenauer-Lieb et al., JGeoDyn 2012) are of great relevance for the introduction and the discussion of softening mechanisms.

**RESPONSE:** See Major comment 2(v) above. As elasticity is not part of our study these references have not been included. However, reference to shear heating, metamorphic reactions and grain size reduction viscosity weakening have been added to Section 1 page 3, 31 to page 4-18.

1520, 13-23) The majority of numerical modeling studies is based on the idea of a growing instability (in terms of an unstable material or introduced geometric imperfection), covered by the linear stability analysis provided by e.g. Fletcher (1974). This concept has been applied in a whole range of numerical simulations. I suggest incorporating the linear stability analysis into the first paragraph Theoretical Analysis, and to outline its application in numerical models (e.g. Schmid et al., JSG 2004; Schmalholz et al., JSG 2008), which should both be cited as well.

**RESPONSE:** Schmid et al. JGR 2004 discusses folding of a finite length layer and the impact of the aspect ratio of the embedded layer. This is also discussed in the Fletcher, AJS 1974 paper. Both papers are referred to by Schmalholz et al. (2008) who have applied the concepts to the formation of pinch and swell structures. . All three references have been added.

1521, 23-26; 1537, 14-17) How does the term "heterogeneity" relate to the rheological stratification of the crust and the occurrence of pinch-and-swell structures in general? Please explain. The consideration at "all scales" rather sounds too far-reaching (see the discussion of softening in e.g. Montési & Zuber, JGR 2002).

**RESPONSE:** We refer to heterogeneity in terms of different rock units with contrasting rheological behaviour this can range from mm, thin section scale to map scale. This is now clarified in Section 1 page 5, 23; Section 5.6 page 19, 15; Section 6.4 page 24, 15.

## **2. Pinch and swell structures: field and thin section observations and initial interpretation**

1522, 10) The term "edge" is not useful. Please refer to the necking area, in general, or e.g. the rim of a swell.

RESPONSE: Sorry for this confusion. We have left the term "edge" where it refers to the interface between the competent layer and the matrix, and indicated at Section 1 page 3, 22 how the term is used in this paper. We have changed the references where we used "edge" to refer to the area of a swell structure close to the neck (in Section 2) to swell neck.

1522, 11-14) The fracture angles vary between 30-40°. How does this correlate with the classical Mohr-Coulomb fracture model? I think more explanations of the model of Moresi & Mühlhaus (PhilMag 2006) are inevitable here.

RESPONSE: Note, this comment refers to the fracture angles in the St Anne Point examples. We have now added an extra figure (new Fig. 1) to show the variations in angle seen. In the model we see a similar variation in angles where areas of strain localisation gradually rotate towards the horizontal (see Fig. 6b dashed line) as the layers are extended. In a similar manner, the St Anne Point shear bands, and the initiating fractures, have also likely rotated away from the direction of principal stress as the area undergoes pure shear. This has been added to the text in Section 6.3 page 23, 30 to page 24, 2.

1522, 22-23) Please provide evidence of an increasing grade of "fracturing" towards the necking area by means of e.g. image analysis. Can single, intragranularly fractured amphibole clasts really be related across the suggested "shear band" (dashed line)?

RESPONSE: Additional images and examples have been included, see Major comment 1(iii), (iv) & (v) above. Evidence of the increasing grade of fracturing is now provided in Figure 2d and e.

1522, 23-24) Please provide more micrographs.

RESPONSE: Provided (new Fig. 1, supplementary Figure 1).

1522, 27-28) Could the finer grain sizes observed in the necks be also due to some viscous processes?

RESPONSE: Yes, we believe the brittle failure is followed by fluid influx and viscous flow. This is now clarified in the text in Section 2 discussion.

1522, 27-28; 1530, 23-24; 1534, 3) Please stick to the common nomenclature. A shear band is commonly referred to as a ductile feature, i.e. a narrow intensely sheared region, in which plastic flow dominates (e.g. Fressengeas & Molinari, JMPS 1987), whereas as shear fracture is evidently brittle (sliding mode). If you want to use shear band for both ductile and brittle features, please add "brittle" / "ductile" in front of the term. In the current version of the manuscript, I find the nomenclature inconsistent.

RESPONSE: We have now gone through the text to ensure that we are using the term "shear band" when referring to a ductile feature only.

1522, 27) Does this mean that the discontinuity, suggested at the outermost swell section, referred to as "edge", formed by shear bands? This would be a ductile feature and contradict the before made assumptions. See comment above.

RESPONSE: It was initiated as a brittle failure then underwent viscous flow. It seems the confusion was due to our misleading use of edge instead of neck (sorry).

### 3. Conceptual model based on field analysis

1523, 18-21) Please provide microstructural evidence for dislocation creep processes being active (e.g. cross-polarized light micrograph / EBSD maps etc., showing subgrain formation / rotation recrystallization etc.). As dislocation creep is considered the main deformation mode after brittle fracturing, thus regarded as responsible for the symmetric necking areas, more emphasis should be laid on this microstructural feature.

RESPONSE: New Figure 1 and Figure 2d and e (with XPL now included) and Supplementary Figure 1 show clear examples of undulose extinction, subgrain formation and grain boundary migration.

1523, 23-25) This is an important statement and should be introduced or discussed in the proceeding introduction (please refer to my comment further above).

RESPONSE: We have added emphasis in the introduction Section 1 page 3, 8-10 and 19-25) that previous numerical modelling has used initial defined irregularities on/in the competent layer. We have added a comment in Section 4.1 page 10, 12-13 that our model uses a numerical perturbation rather than manually defining the perturbation.

1524, 12-15) Does this mean that viscosity is recalculated after yielding? The softening process is not clear to me.

RESPONSE: This comment refers to Section 3 which discusses the requirements of a conceptual model, not the numerical model. We have highlighted the need for material softening as the St Anne Point microstructures indicate a higher modal percentage of softer minerals (quartz and biotite) in the edges of the swells compared with the centres. How this is modelled is discussed in the Section 4 on the numerical implementation. Section 4 has now been updated per the comments from Hobbs (see below). Viscosity is indeed recalculated on each step of the numerical simulation, including after yielding.

### 5.2 Results: Analysis I: effect of mode II failure and stress exponent

1530, 19-21) How do you explain that there is no change in layer width, although the numerical box is extended?

RESPONSE: The wording has been improved to: Where the competent layer had no strain localising behaviour (Fig. 6a black dotted line), the competent layer was evenly stretched and thinned and no pinch and swell structures were formed;  $R_w$  remained at 1.

1531, 10-11; Fig. 4 first row) Why does the plot of the strain rate invariant reveal localization bands, whereas the plot of the 2D structure is continuous in terms of deformation? At which

time steps were both plots obtained? You could either add the time steps (as e.g. a number) or plot both for the same time step, which is more convenient.

RESPONSE: In this numerical model the competent layer yields immediately (hence the variability in the strain rate invariant plot at step 1). The shear bands develop after a few iterations of the model (this has now been clarified in the Section 4.1, see Hobbs comments below). The stretch value (left hand column in Fig. 5) was determined for the time steps depicted in the figures, so the material particles and strain rate invariant plots have been taken at the same time steps for both Newtonian (a) and non-Newtonian (b) examples in Figure 5. The Figure 5 caption has been updated.

### **5.3 Results: Analysis II: effect of relative initial viscosity ratio ( $R_v$ )**

1531, 19; 1532, 9; 1537, 8; 1552; 1553) Terms like "good" or "better" should be avoided throughout the manuscript. They imply that parameters were fine-tuned in order to fit the natural geometry.

RESPONSE: We apologise for this lack of precise wording we have now defined explicitly what we mean by "good" ( $R_w$  value range) and use this in the text.

### **5.5 Results: Analysis IV: effect of cohesion and material softening**

1532, 12) Please explain the term "complexity".

RESPONSE: At low levels of softening we see localisation occurring as a simple pair of shear bands, both with approximately the same strain rate. This causes simple, symmetric swells to form only with low levels of rotation. At higher softening levels we see localisation occurring with one of the shear band pairs having a relatively higher strain rate. This causes the swells that form to have a more asymmetric shape and to have more rotation, hence the increased 'complexity' of the pinch and swell structure formed. This is discussed in Section 5.2 at page 17, 4-9.

1532, 17-18) Is the "variability" in differential stress after yielding really related to the findings of Griggs and Handin (GeolSocAmMem 1960), or are these numerical oscillations? I suspect, the latter are responsible for the documented variabilities. In any case, there should be a discussion of mesh sensitivity be incorporated in the results section.

RESPONSE: Figure 6g shows differential stress reduces until it stabilises (for example, at approximate stretch 1.8 for  $R_{Co} = 20$  and 40). The variability that can be seen after this is likely to be due to numerical oscillation. We have modified page 16, 10-12. A mesh test discussion has been added to Section 4.1, page 10, 15-19 with figure included in the supplementary data.

### **6.2: Discussion: Effect of stress exponent, brittle behaviour, $R_v$ and $R_{Co}$**

1534, 16-25) In the works of Schmalholz and co-workers (Schmalholz & Maeder, JSG 2012; Schmalholz et al., JSG 2008), a linear instability with an infinitesimal small amplitude is growing, which ultimately leads to localization. This kind of inherited localization phenomenon is covered by the hydrodynamic theory of Fletcher (AmJSci, 1974) and co-workers, which is not cited, unfortunately. Studying these works, it becomes obvious that the

comparison with the aforementioned numerical models of viscous necking is not helpful with respect to the findings.

This immediately raises the question, of how localization is obtained at the onset of boudinage (please refer to the major comment further above). Here, a more detailed comparison with work on brittle boudinage (e.g. Abe & Urai, JGR 2012) would be more useful.

**RESPONSE:** It has already been pointed out in the discussion at 6.2 page 18, 13-18 that the linear instability (i.e. purely viscous) models cannot be directly compared, and a discussion of the brittle models was also included. We also now discuss the (Abe and Urai, 2012) and (Komoróczy et al., 2013) brittle boudinage in the introduction (page 3, 14-19).

1535, 3-5) This comment relates to the latter one, made above. The softening mechanism in Hobbs et al. (2009) is triggered at a critical stress-strain-strain rate condition, termed dissipative work, which was uncovered by a study of the critical strain needed to trigger a thermal runaway (e.g. Hobbs et al., JSG 2011). In contrast, Neurath & Smith (JSG 1982), repeated again in Montési & Zuber (JGR 2002) or Schmalholz et al. (JSG, 2008), introduce a negative power-law exponent, inducing softening of a power-law material. As both concepts are encompassed by a viscous strain localization criterion, I do not understand how this relates to the initiation of boudinage by brittle failure? Is the material intrinsically unstable, therefore always softening? The classical mechanics literature provides the criterion for brittle failure to occur, but it is not referred to (e.g. Rudnicki & Rice, JMPS 1975). Please modify your comparisons accordingly.

**RESPONSE:** we agree the material is intrinsically unstable, this is now stated explicitly in Section 4.1 page 8, 15-18.

### **6.3: Discussion: Application of the numerical model to field interpretation**

1536, 25-26) I find this passage a more appropriate way to deal with the localization problem observed at the St. Anna point rocks. Please consider revising your fundamental conclusions elsewhere.

**RESPONSE:** Our conclusions have been modified to indicate that brittle failure is an additional method of pinch and swell initiation. See Hobbs comments below.

### **6.4: Discussion: Implication of the presence of brittle-viscous behaviour in the middle to lower continental crust**

1537, 14-17) As criticized before, I do not agree that brittle fracturing has to be a necessary precondition for ductile flow, because there exists a criterion for strain localization in ductile rocks indeed (e.g. Hobbs et al., JSG 2011). Next, please add studies of viscous necking phenomena (using low-temperature plasticity;  $n = 10$ ) under greenschist facies metamorphic conditions (e.g. Schmalholz & Maeder, JSG 2012) here.

**RESPONSE:** Our conclusions have been modified to indicate that brittle failure is an additional method of pinch and swell initiation. See Hobbs comments below

## **Tables**

1546, Tab. 1) As the viscosity ratio (RCO) was increased up to 125 and tested for its sensitivity in the manuscript, please modify the second row for the values of RCO accordingly.

**RESPONSE:** Table 1 label has been modified to clarify the values in the different analyses.

1547, Tab. 2) The last row with numerical experiments No. (iv) shows that there is no development of pinch-and-swell structures. This is due to the fact that there is no softening mechanism implicitly accounted for in the viscous creep description in the modeling scheme. Please refer to my major comment further above.

**RESPONSE:** Per comment in 2 (i) above. In our model of the exclusively viscous creep rheology we don't get any swells as we don't have any geometric perturbation and our numerical perturbation only occurs in the M-CB material. Hence, no localisation and no pinch and swells formed. Now clarified in Section 5.2 page 15, 3-6.

### **Figures**

1548, Fig. 1) The labels of minerals (Fig. 1a) are not visible in the sketch. Please add a label (Fig. 1e?) to the last micrograph. Asterisks are not included in the graph (?).

**RESPONSE:** This has been rectified.

(c) How do you explain the strong asymmetry of the studied necks? The boudin spacings and aspect ratios are highly heterogeneous, which suggests that the transient response was somehow altered. This might be due to the imposed geometric imperfections at the layer-matrix interfaces (see also my major comment further above)?

**RESPONSE:** This comment refers to the St Anne Point outcrop scale images. We believe the heterogeneous nature of the spacing and aspect ratios of the pinch and swell structures is a result of uneven distribution of strain localisation across the rock platform and the impact of fluid inflow causing material softening. Now clarified in Section 6.3 page 19, 26-29.

(d) What does "Modes" refer to (e)? Is this  $\mu\text{m}$ -wide "shear band" really seen at outcrop scale? On the outcrop photo, no trace of this feature can be found. Within the so-called "shear band", fractured clasts of amphibole should, at least, lie within the suggested shear plane (dashed line) and should easily be matched (size, orientation). An additional micrograph, showing a necking area under cross-polarized light, could help to provide further insight into the dislocation creep processes.

**RESPONSE:** Modal percentages of the minerals – changed in (now) Fig. 2 label and supplementary data. See Major Comment 1(ii) above for discussion of the shear bands at outcrop scale.

1549, Fig. 2) (b,d) The layer-matrix surface is not a straight line, thus geometrically perturbed. The perturbation technique that explains location and direction of localized deformation should be mentioned in the text.

**RESPONSE:** See major comment 2(iii) above. A numerical perturbation, not geometric, is used.

(c) The stress-strain curve reveals a complex transient deformation stage (linear elasticity and strain hardening). In agreement with the numerical modeling scheme, there should be only linear elasticity illustrated (linear increase of stress with loading).

RESPONSE: This is a theoretical diagram only and does not relate to the numerical model. As such the label is misleading. The numerical model starts where the layer yields and only models viscous flow. We have changed the figure 3 caption and labelling accordingly.

1550, Fig. 3) (c) Why is the plot of the stress invariant rather blurry than localized?

RESPONSE: This is due to the relationship between stress and strain rate (equation 1), a material with a stress exponent unequal to 1 (in this example  $n = 3$ ) there is a smaller variation of stress compared to strain rate and viscosity (see the y axis values or the ranges shown on Fig. 3b and c). Now explained in Section 5.1 page 15, 11-12 and Figure 4 caption.

1551, Fig. 4, first row) Why does the plot of the strain rate invariant reveal localization bands, whereas the plot of the 2D structure is still homogeneous?

RESPONSE: Same comment as above: see response to 1531, 10-11 above.

1553, Fig. 6) Please indicate from where (which model setup and location within the boudinaged layer) the stretch-differential stress data (g) are coming from.

RESPONSE: The 3 principal stresses were collected on a series of 45 particles (5 rows of 9 points) across each model in Analysis IV. Differential stress was calculated and graphed. The provided graph is an example of these graphs of the point in the competent layer at coordinates 0.4,0.5. This information has been added to the Figure 7 label and Section 5.5. page 16, 7-9.

(f) Why do the rheological data for  $RCO = 1$  suggest continuous softening, although there is no softening mechanism implemented in the viscous part (and thus not detectable in graph -f-)? Is the layer thinned to a certain high degree (with elevated strain rates) and therefore apparently softening? In graph (f), I observe localization at the layer-matrix interface. If the material is homogeneous and the geometry unperturbed, how do you explain such localization patterns?

RESPONSE: Same comment as above: see 2(iv) above.

(g) The rheological data for  $1 < RCO < 10$  reveal continuous softening, whereas the data  $RCO = 20$  are rather bumpy. Only the data for  $RCO = 100$ , i.e. the highest contrast in cohesion before and after yielding, are in steady state. This finding limits the potential application as a deformation (rate) gauge for natural viscous rocks, because constraints on matrix flow can only be obtained from steady-state creep within the necking areas. I suggest including this to the discussion and limiting the application to the boudinage geometries, due to brittle fracturing. For these reasons, I am wondering about how much pinch-and-swell structures are actually being addressed with this respect? For  $RCO > 100$ , I suspect that the data are more stable. Be that as it may, does it make sense (from a microstructural or material properties perspective) to apply contrasts in cohesion of larger than 2 orders of magnitude?

RESPONSE: We hadn't considered using the model as a deformation (rate) gauge for natural viscous rocks. The graph in Fig. 6g shows only the data to stretch 2.3. We have the data to stretch 3.2 and this shows only models  $R_{Co}$  1 and 2 are still softening, models  $R_{Co}$  4 and 10 have stabilised by stretch 2.5.

Per Hobbs discussion below we have removed  $R_{Co}$  100 and included  $R_{Co}$  40 instead.

## Reviewer 2 (Hobbs)

### Major comments

My comments involve four main points:

- (i) The paper needs to make clear to the reader what Mohr- Coulomb constitutive behaviour actually means. In its present form the paper mixes up the concepts of Coulomb-Navier-Mohr fracture criterion (a very old concept, 18th-19<sup>th</sup> centuries; see Jaeger 1969) and Mohr-Coulomb constitutive behaviour (a relatively recent concept; see Vermeer and de Borste, 1984).
- (ii) The paper needs to clarify why Mohr-Coulomb behaviour is relevant.
- (iii) Mesh sensitivity needs to be explored or commented upon.
- (iv) What is the influence of gravity in the crustal scale model?

RESPONSE: We address each of these issues below.

**(i) Mohr-Coulomb constitutive behaviour.** The authors apparently do not understand what Mohr-Coulomb behaviour is. The behaviour reported in Figure 2(a) of the paper is the classical Coulomb-Navier-Mohr criterion for fracture best discussed by Jaeger (1969) where the normal to the plane of fracture makes an angle with the direction of  $\bar{\sigma}_1$ . Notice that the paper claims (erroneously) this relation to be for the angle between the plane and  $\bar{\sigma}_1$  so that equation 2 needs to be corrected. In doing so Figure 2(a) needs to be re-drawn so that the "Mohr envelopes" are straight lines reflecting the constant friction angle with no tension cut-off as assumed in most of this paper. The Coulomb-Navier-Mohr concept is a criterion for fracture and is the one upon which classical Andersonian fault mechanics is based. By contrast the Mohr-Coulomb relation (see Vermeer and de Borste, 1984 and Hobbs and Ord, 2015, pp 168-173, for details) is not a criterion for fracture; it is a constitutive relation that describes how pressure sensitive flow occurs with and without localisation. An important part of the constitutive framework is the presence of a flow rule (equations 16 and 18 of Moresi and Muhlhaus, 2006). This distinguishes the behaviour from classical Coulomb-Navier-Mohr behaviour. Mohr-Coulomb behaviour involves a criterion for localisation but that zone of localisation may not necessarily be a single fracture; it is a localised zone of brittle deformation with no comment on the detailed microstructure of the zone. It might for example not be a discrete fracture but a zone of crushed grains. The zones of localisation predicted by Mohr-Coulomb constitutive behaviour are not mode II fractures as implied by this paper. They are shear zones where compatibility of deformation is matched across the boundary between the localised and adjacent non-localised material. It is this compatibility requirement that controls the angle between the shear zone normal and  $\bar{\sigma}_1$  (Rudnicki and Rice, 1975). In other words they are not faults with discontinuities on their boundaries. The paper has to be reworded to remove this connotation.

RESPONSE: The reviewer is correct in his discussion above of the Mohr-Coulomb constitutive behaviour used in the Underworld model, and the issues he raises have been rectified:

a) The criterion for failure in equation 2 has been removed from the conceptual model discussion (1524 1-5) and the Moresi and Mühlhaus (2006) equation with a discussion has been added to Section 4.1 page 10, 6-14.

b) The Mohr envelopes used in the model have been added to Fig. 2a as straight lines and the figure label reworded to clarify the Mohr-Coulomb theory vs. the numerical model implementation.

c) As Hobbs points out, the Mohr-Coulomb behaviour used in the model is a criterion for localisation, which could be but is not necessarily a brittle fracture. The text has been modified to specify the Mohr-Coulomb strain localising behaviour instead of Mohr-Coulomb brittle behaviour when discussing the numerical model. References to brittle deformation have been retained where the St Anne Point and Wongwibinda outcrops are discussed.

We do take the point, that our general conclusions should be modified to indicate that the localisation forming the structures can be due to causes other than brittle fractures.

In this particular implementation of Mohr-Coulomb no elasticity is included so that the behaviour is unrealistic) rigid-plastic behaviour. This places severe constraints on compatibility between the localised and non-localised material so that the boundary is a plane of zero strain. This probably means that in these models the angles predicted by equation 14 of Moresi and Mühlhaus do not occur. Some comment based on observations would be useful.

RESPONSE: The reviewer is correct in his assumption. To test this we conducted a number of runs on the base model varying only the friction coefficient. On these models, we measured the initial shear band angles. Comparison showed that there is a range of initial angles that are only to a minor extent, influenced by the friction coefficient (for reference an extra figure is provided for the editor (at the end of this document). Accordingly, we specify in the text this issue and report the range of angles observed (Section 4.1 page 10, 14-16).

As indicated above, the initial angle between the normal to the plane of localisation and  $\bar{\alpha}_{c,1}$  is given by equation (14) of Moresi and Mühlhaus : where  $\bar{\alpha}_c$  is the friction angle ( $= \tan^{-1} \mu$ ). This angle is, in general, different to that predicted by the Coulomb-Navier- Mohr criterion. Moresi and Mühlhaus discuss the way in which this angle changes with strain. It would add to the paper if some discussion was included regarding the initial orientation of the shear bands and how this changes with strain. I can see no systematic variation but it is difficult to analyse this with the figures presented. At the very least the paper should include a comparison between predicted and observed orientations.

RESPONSE: We have now plotted the shear band rotation vs. stretch and added it to Fig 6b. Discussion has been added to Section 5.2 page 14, 19-23.

It should also be noted that the behaviour of Mohr-Coulomb materials is intrinsically unstable because of the corners on the yield surface. Thus localisation is an intrinsic part of the behaviour of Mohr-Coulomb materials. The material used in this paper is also unstable because it has non-associative constitutive behaviour (the dilation angle is presumed to be zero, although this is never stated, so that the dilation angle and the friction angle are not equal). This means that the yield and potential surfaces (as discussed by Moresi and

Muhlhaus) are not coincident. These are important points that describe why localisation occurs in these simulations and they should be emphasised.

RESPONSE: Yes, this is correct. The model allows no dilatancy, no elasticity and the material is incompressible. The material with Mohr-Coulomb behaviour fails immediately causing localisation to occur over the first few iterations of the model. The site of the localisation is determined numerically by the Moresi-Mühlhaus code. As such, it models only a subset of the Mohr-Coulomb constituent behaviour discussed in Vermeer and de Borste, 1984. We have given this more emphasis in the model description in Section 4.1 page 8, 14-18.

In principle Mohr-Coulomb materials do not need to feature softening behaviour in order to localise, they are intrinsically unstable. In fact, Rudnicki and Rice (1975) show that these materials can localise in the hardening regime. Thus a lot of the discussion in this paper justifying weakening behaviour misses the mark. Weakening is sufficient but it is not necessary to produce the modelled behaviour in non-associated pressure sensitive materials.

RESPONSE: As Hobbs points out, weakening may not be necessary for the localisation to occur, but in the pinch and swells structures we see at St Anne Point (and other sites) weakening is visible. Our purpose here is to explore the impact of varying the amount of softening to see what impact it has on the structures formed. Our conclusions are that at high levels of softening, strain localisation is not as effective (i.e. through-going localisation occurs on many different locally adjacent planes concurrently thereby inhibiting the formation of simple symmetric pinch and swell structures. This is now added in the Section 1 page 3, line 22, Section 5.5 page 16, 12-14, and Section 6.2 page 18, 25-30.

One should also note that although the authors go to great pains to insist that no initial irregularities or perturbations are in their models, one needs a perturbation of some kind to set the instability off. In their case the perturbation comes from rounding errors in the computations.

RESPONSE: Yes, the model includes a numerical perturbation, so this has been modified in the text in Section 4.1 page 10, 12-13.

Another point concerns references to the values of  $c_1$  and  $c_s$  on page 1527. The authors claim these are dimensionless. This cannot be true and still remain consistent with the formulation of Moresi and Muhlhaus. They must have the units of stress. Their ratio of course is dimensionless. However one should note the implications of a ratio  $c_1/c_s = 100$ . If  $c_1 = 50$  MPa (a reasonable value) then  $c_s = 0.5$  MPa, a ridiculously small value for a cohesion. This is approaching the behaviour of a cohesionless Mohr-Coulomb material which is thermodynamically inadmissible (Hobbs and Ord, 2015, p 170).

RESPONSE: Yes, of course they have the dimensions of stress, but we have not defined what these dimensions are, so this was the wrong terminology. Apologies. We have changed this at Section 4.1 page 9, 1. In our discussion and use of the model, we use only the ratio and as Hobbs points out, this is dimensionless. We have removed all references to  $R_{Co} = 100$  and included instead  $R_{Co} = 40$  and indicate this is a very high value (Section 4.3 page 12, 18; Section 5.5, 9-10, 12).

**(ii) The relevance of Mohr-Coulomb behaviour.** An important emphasis in this paper is the claim that Mohr-Coulomb behaviour is important throughout the crust. This is not the first time such a relation has been explored for crustal behaviour (see Ord, 1991) and that paper should be referred to not simply as an example of Mohr-Coulomb material on a crustal scale but also as a reason for using 0.6 as a value for the internal friction coefficient. The dominating effect of Mohr-Coulomb behaviour needs to be toned down.

RESPONSE: In the last decade, it has become more apparent that localization is commonly initiated through brittle failures followed by fluid influx causing the formation of shear bands/zones and followed by viscous flow (for e.g. Brander et al., 2012; Fousseis et al., 2006). We believe this is the case for the St Anne Point pinch and swell chains which formed at approximately 30km (Klepeis et al., 1999). The numerical model also suggests Mohr-Coulomb strain localising behaviour occurs at these depths. We have now added in Section 1 page 5, 17-20, Section 4.1 page 11, 17 the Ord (1991) reference. We have reworded the manuscript to indicate that the field examples we've chosen show brittle failure and subsequent material softening, but where we discuss the numerical model we stress that it is modelling strain localisation, rather the brittle failure *sensu stricto*. We have now made this clear throughout the manuscript. As such, the dominating effect of Mohr-Coulomb behaviour is toned down.

An important point is that the experimental evidence for Mohr-Coulomb constitutive behaviour is very weak and other forms of brittle crustal behaviour are to be preferred (See discussion in Hobbs and Ord, 2015, pp168-173). The only reason I can see for promoting Mohr-Coulomb behaviour in this paper is that it is available for use in Underworld. Even if fracturing is documented that does not necessarily indicate that the localisation leading to pinch and swell features is controlled by Mohr-Coulomb constitutive behaviour. The same behaviour could arise in a material that is deforming essentially by viscous flow and following a viscous constitutive law but where energy dissipated by local fracturing leads to viscosity weakening and hence localisation. This is the type of behaviour reported by Hobbs et al. (2008: viscosity weakening due to thermal feedback), Hobbs et al. (2010: viscosity weakening due to dissipation arising from chemical reactions) and by Peters et al. (2015: viscosity weakening due to dissipation from grain size reduction). Any process (including local fracturing) that dissipates energy will lead to localised structures of some kind simply from strain-rate (that is, viscosity) softening and need not specifically involve a brittle-type constitutive relation directly. In this regard, the discussion in the last paragraph of page 1535 is incomplete.

RESPONSE: Yes, these are good points that have been included in Section 1 page 3, 31 to page 4, 18.

Softening resulting in a decrease in stress is important (but not necessary for localisation) in rate insensitive materials such as Mohr-Coulomb but in rate sensitive materials (such as viscous materials) the important process is strain-rate (viscosity) weakening. This is because viscous materials (with  $n$  not equal to 1) are strain rate hardening (a positive perturbation in strain-rate leads to an increase in stress) and in order to weaken them one needs a coupled process that decreases the viscosity with an increase in strain-rate. The authors need to flesh this out and admit that the model they present is one way of producing what is observed and not push the line that their results unambiguously show that Mohr-Coulomb behaviour is present throughout the crust.

RESPONSE: Thank you for this explanation. We have modified the manuscript in a number of areas (Section 1, Section 5.6 and Section 7) to indicate that, more generally, strain localising behaviour, rather than specifically Mohr-Coulomb behaviour is present in the crust. We have also removed references to the lower crust as our models do not encompass depths past those normally considered middle crust.

Even if one accepts that brittle behaviour controls what we see in these structures, the authors also need to indicate why Mohr-Coulomb behaviour is likely rather than some other brittle constitutive relation such as Drucker-Prager. Drucker-Prager behaviour is more stable than Mohr-Coulomb because there are no corners on the yield surface. However in the absence of dilatancy such materials still localise and would produce very similar results to those reported in this paper.

RESPONSE: We have modified Section 4.1 page 8, 18-21 to discuss evaluating the applicability of the model relative to other possible models.

We believe the value of the Moresi-Muhlhaus (2006) UW model is that it couples an initial localisation followed by viscous flow. We believe that the use of Mohr-Coulomb rather than Drucker-Prager behaviour in the model does not impact the conclusions we have made (and Hobbs agrees that these would work similarly under our assumed conditions). In the case of the St Anne Point and Wongwibinda pinch and swell structures we believe the initial localisation is caused by brittle failure of the competent layer, and that this occurred at middle crustal levels for the St Anne Point sample.

**(iii) Mesh dependency.** Localisation in Mohr-Coulomb materials is well known to be mesh dependent because there is no intrinsic length scale in the constitutive relation and the only length scale in the model is the mesh size. This means that the spacing and thickness of shear zones depends on the mesh size. I have checked with Moresi and he confirms that mesh dependency exists for Mohr-Coulomb behaviour in Underworld. It would be nice to see two models run under identical conditions except for the mesh size to see the effect. Certainly if mesh dependency exists then nothing can be said about the details of pinch and swell shapes without a detailed analysis.

RESPONSE: As suggested the reference model used in Fig. 5a has been run at two additional mesh sizes (twice the size and half the size). The results are included in an additional supplementary figure 3. The results show that numbers and spacing of swells are mesh dependent, but the measurements we use,  $R_w$  and tortuosity, are generally mesh independent, so the conclusions we have made are not materially changed. This has been added to Section 4.1 page 10, 17-21.

**(iv) Gravity and pressure.** The authors imply that it is not necessary to consider pressure in their models. They mistakenly quote equation (3) of Moresi and Muhlhaus to support this. This particular equation describes the coupling between the motion of deforming material and the effects of thermal expansion upon the density of material during mantle convection. It has absolutely nothing to do with the effect of pressure on the mechanical behaviour of Mohr-Coulomb materials and is true for any material. In fact the effect of pressure on the flow stress of Mohr-Coulomb materials is very large. Pressure can also have an influence on the cohesion and friction angle (and the dilation angle) of Mohr-Coulomb materials (see Ord, 1991). The point made here is particularly relevant with respect to the crustal scale models. As far as I can determine, gravity is not turned on in the crustal scale models reported here. If

one does this then for an average crustal density of 2700 kg m<sup>-3</sup> at a depth of say 20 km the normal stress on a plane of localisation would be of the order of 500 MPa. Using equation 9 of Moresi and Mühlhaus, a pressure independent value of the cohesion of 50 MPa and a pressure independent value of  $\tan \phi = 0.6$ , as assumed by the authors, one obtains a shear stress necessary to initiate failure of 350 MPa; at 40 km the failure stress is 700MPa. This is quite high and the issue is whether in Underworld, with realistic values of viscosity, failure of Mohr-Coulomb materials can occur at these depths. I doubt it. Hence, if the authors have already included gravity then they should say so and I am wrong. If they have not included gravity they should do so and see if I am correct. As the paper stands at present this part of the modelling needs clarification or needs to be redone.

**RESPONSE:** Yes, this is all correct. The erroneous pressure discussion has been deleted and gravity has been included in the landscape scale models Section 4.4 page 12, 30 to page 13.

Our initial tests of gravity in our models were flawed, and some invalid assumptions were made, so the model has been rerun with full scaling. We have included in the manuscript a 10 x 10 km, 20 x 20 km and a 40 x 40 km model. Section 4.4, Section 5.6, Section 6.4 and Section 7 have been rewritten.

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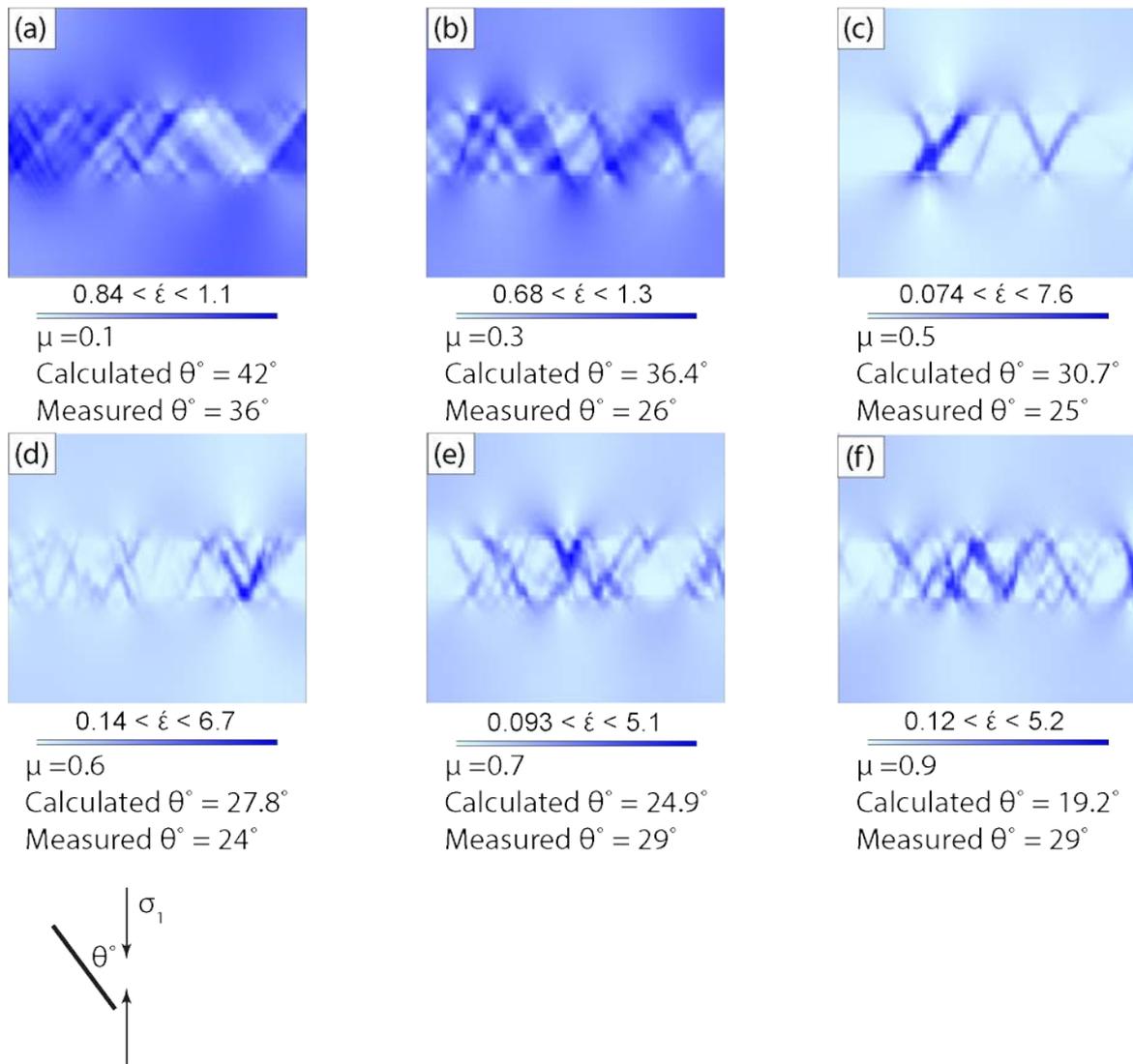


Figure 1. Testing of the effect of friction coefficient ( $\mu$ ) on the angle of shear band formation. The model in Figure 5a (b), with  $Rv = 20$  and Newtonian flow, has been used for this test. Strain rate plots are taken at 4% stretch (step 5) when shear bands have established for friction coefficients (a) 0.1, (b) 0.3, (c) 0.5, (d) 0.6, (e) 0.7, (f) 0.9.  $\theta^\circ$  angles were calculated using equation 5 and compared with the angles measured from the plots using ImageJ (Rasband, 2013). The average of a random selection of shear band angle measurements in each direction have been taken. Some models (for example, (b) and (c)) have shear bands at varying angles, suggesting some other mechanism other than the calculation in equation 5 is operating. Moresi and Mühlhaus (2006) suggest these anomalous angles may be due to an invalid assumption that all failures occur by pure frictional sliding at shallow depths (that is, low gravity/pressure impact). However, as pointed out by Hobbs in his review of this manuscript this could be due to there being no elasticity included in this numerical model. A friction coefficient of 0.6 was used throughout our modelling tests.