Solid Earth Discussions

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Comment on se-2022-2

Anonymous Referee #2

Referee comment on "Strain localized deformation variation of a small-scale ductile shear zone" by Lefan Zhan et al., Solid Earth Discuss., https://doi.org/10.5194/se-2022-2-RC2, 2022

The manuscript of Zhan et al. presents a microstructural study of strain localisation in small scale shear zones associated with a major continental strike-slip shear zone. The work includes a systematic attempt at microstructural characterisation and some lovely optical micrographs which indeed show some interesting changes across the shear zone. The authors interpret these to indicate changes in strain and deformation mechanisms. Whilst these highly localised features seem worthy of study in the context of strain localisation processes, I cannot recommend that the manuscript be accepted in its present form. The most significant problems include use of analyses that are not always valid (or not enough information is available to show that they are justified), insufficient results provided to support some of the key interpretations, and generally a lack of clarity in how this work and the results add to existing ideas about strain localisation in shear zones. I've listed some detail on these major issues (and others) in the general comments below in the hope that this proves useful.

General comments

- There are several occurrences where the choice of words and grammar needs to be improved in order to increase clarity - there are some apparently key points particularly in the intro and discussion where this causes problems. In general the overall structure of the manuscript is logical.
- There needs to be better description of the context of the studied shear zones used for microstructural analysis. Are there field photos of these shear zones, orientation data, and field maps showing their relationship to nearby structures? How far are they from more major shear zones? It is not at all clear whether all the analysed shear zones (including those in micrographs, EBSD and thermobarometry samples) come from different localities/samples – the map in Fig 1 suggests 3 but locations and setting for each should be described. It is not clear how the samples analysed for thermobarometry relate to the shear zones. Ideally locations of EBSD maps should be shown on optical micrographs, and field photos of the sampled shear zones included.

- Some of the key figures are not presented well enough to allow robust comparisons to support the authors interpretations – particularly EBSD pole figures and grain size histograms, suggested modifications are listed against relevant figure number in specific comments below.
- Methods needs to include more detail about the amphibole-plagioclase thermobarometry. E.g. how many pairs were used, how were they selected, what samples did they come from? Where is the raw compositional data? In the results, the reported P and T ranges do not exactly correlate with the range of points plotted in Fig. 10.
- For the piezometry, the authors need to indicate locations of the areas used for the grain size measurements in order to support the assumption that the quartz grain size is only controlled by differential stress during dynamic recrystallisation, and also need to describe the process used to distinguish recrystallised grains. In all the images of zone C, quartz is distributed in polyphase assemblages which makes pinning of the grain size by secondary phases very likely – this will invalidate the piezometry results. If subgrains can be observed in quartz, the authors may wish to consider a subgrain-based piezometer which can be used in polyphase assemblages (e.g. Goddard et al. 2020) although they should note that EBSD grain size analysis would be necessary here.
- Strain rate calculations do not seem completely valid. Be careful with relying on CPO patterns/microstructures for T estimates, as they are also influenced by strain rate as well. I recommend including a figure of the calculated strain rate curves, which allow clearer consideration of uncertainty in stress and temperature. The authors need to state the deformation mechanisms identified to justify choice of the flow laws – which appear to be for dislocation creep despite the fact that later the authors interpret diffusion creep for zone c, so these flow laws will not be applicable there. The choice of published flow laws also needs to be better justified – a more recent study by Tokle et al. (2019) which reviews previous work could likely be a better choice than the earlier Luan & Paterson study, unless the authors have reasons for their preference. Although the authors state that water fugacity was considered, this consideration needs to be outlined more fully. Make sure that the strain rates are shear strain rates – may need to be converted from axial experimental strain rates with a multiplication by $\sqrt{3}$ (Paterson & Olgaard, 2000).
- Hydrous retrogression of hornblende to mica seems to be a key interpretation – it is mentioned in the abstract and in the discussion – but I've not seen evidence that this is certainly recorded here, especially since biotite and hornblende are both shown to be magmatic phases. The authors reference Fig. 4c but I can't see any particularly clear relationships between hornblende and biotite there. The lack of hornblende in zone C is indeed interesting and I would encourage the authors to explore this but need to rule out alternative explanations e.g. preferential localisation over a hornblende-free layer in the granite. The authors need to present better thin-section evidence of this reaction occurring, and state the full reaction. Ideally a pseudosection could be created for this rock composition. This could also facilitate calculation of the fluid content, if no other constraints are available – this is also a major but unsupported part of the discussion. Castellan et al. (2021) is a good example of a study noting a potentially similar reaction in granitic shear zones, presenting the types of evidence suggested here.
- The interpretation of thermal heterogeneity localising strain (discussion section 8.4) is not clearly supported, in my opinion. In the introduction it states (L196) 'They are invariably localized on approximately planar structural and compositional heterogeneities within the protolith'. But in the discussion the authors state that their observations cannot be explained by precursors and I am not sure why – this seems contradictory and more explanation is needed here. Is it related to the sudden changes in apparent strain between the zones, which is an interesting feature here? How does this then lead the authors to their preferred model - 'hot-to-cold contacts ascribed to thermally enhanced rheological weakening' (L675) needs more explanation as to the processes involved and the evidence from these samples.

- The conclusions are a bit vague. More generally the authors should work on expanding the context and key points of their work. If strain localisation is the key theme, what are the underlying processes and controls which have caused this to be observed? Are small scale shear zones being used to explore (early?) localisation processes in the development of shear zones generally? If so how representative are they. Is there a reason why these shear zones are small and have remained so highly localised? And regionally what is their significance – why are there differences in the deformation between the foliated and non-foliated granites, what is their role in the wider GLG-SZ system? This needs addressing in the intro/discussion but a more detailed geological context for the samples and the study area is also required.

Specific comments

L153-175 (Methods section)

- How were samples selected and cut – can locations be provided? Do any of the field photos show the sampled shear zones? Were samples cut in the X-Z plane?
- Report the step size for EBSD acquisition. Was any post-acquisition filtering done on the EBSD maps?
- Were twin boundaries removed from rotation axes analyses of quartz and feldspar? Do some plots show 1 point per grain or not? These details need including either here or e.g. in figure captions.

L233-240 - The use of 'high' for zone B and 'strong' for zone C need additional descriptors to give them some sense – maybe this is strain? In any case I would avoid all interpretation of high/low strain in this section – stick to observed properties that underly these interpretations.

L247 – Fig. 3c needs to be shown at higher magnification to convincingly show GBM, it is difficult to see without zooming in and losing resolution.

L265 I can't clearly see the proposed core-mantle structure in K-feldspar in this image. An optical image may show this more clearly, or annotate the BSE image. Need to indicate orientation of the shear zone to support next sentence about K-spar long axes. See also L625 where this is mentioned again.

L380 – there are a few circumstances where I think 'corrected'/'uncorrected' have possibly been used where it should say 'correlated'/'uncorrelated' - or if used deliberately, the correction needs to be explained.

L621 – the CL images have not been described in the context of their CL properties – this needs to be included earlier in the results before discussing here.

L728 - 'deep seated crust' – are these SZs very deep? Results seemed to estimate 10 km, but deformation temperatures suggest higher?

Fig 1 –

- Part a contains some abbreviated fault labels that are not explained in the caption. If not needed (in the Himalaya) these could be removed.
- It would be helpful to have a higher-scale map of the studied location in addition to this regional one. Could the distribution of foliated vs unfoliated granites be shown on such a map? I am finding it hard to relate the location/context of the studies shear zones to deformed granites and the GLG-SZ.

Fig 2 –

- Parts a and b – this may be a resolution problem but the shear sense indicators labelled are not entirely convincing – a better resolution image and/or a more zoomed-in image would help here. Make sure that the label 'Tur' (part a) is defined in the caption
- in caption, part E – explain what is meant by 'are like joints' e.g. is there no shear, are there brittle features?

Fig 3 – Are parts e – g flipped or rotated relative to the main image in part d? The foliation looks a different angle. Part d is a fantastic image!

Fig. 4 – I don't think these images have a consistent reference orientation, can the shear zone orientation be annotated on each part to make this clear (or X-Z arrows, presuming that is the cut surface)

Fig. 5 – the bins should be the same size and interval for all the histograms in this figure, otherwise it is impossible to compare different subsets properly.

Fig. 6 –

- the M.U.D scales need to be consistent in order to be comparable i.e. the range should be the same across all pole figures. This will help avoid over-interpreting clustering when in fact the max M.U.D is very low. The scales need more values labelled – sometimes it is not clear whether the M.U.D should be increasing or decreasing when only one value is listed, some scales have no values! Is it possible to use a colour scale other than the rainbow one used here?
- Can the locations of the EBSD maps be shown on earlier figures?

Fig. 8 – is the X direction vertical in this map? Add annotation to show the reference frame on all EBSD maps.