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Interactive comment on "Microscale strain partitioning? Differential quartz lattice preferred orientation development in micaceous phyllite, Hindu Kush, northwestern Pakistan" *by* K. P. Larson et al.

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Response to authors reply to Manuscript Se-214-85 By: K. P. Larson, J.L. Lamming & S. Faisal

It is a great pity that I can't see the revised version with modifications that the authors claim they have undertaken to text and figures. I have to disagree with their response on a number of points and make some additional suggestions.

They have in part clarified my major concerns about which version of Fabric Analyser

they are using; but this has to be clarified in revised text. However, they appear to fail to abide by my recommendation that they delete Fig 4a (bulk analyses) and play down the significance of their bulk analyses. In light of my reservations about this data I would highly modify any statements about [c]-slip. Further additional comments are listed below:

Abstract – This should be as brief as possible and I again make the suggestion. The first sentence can be rewritten and last sentence can be deleted.

Lines 28-32: I still maintain that the authors should delete all the references to modeling the development of crystallographic fabrics (e.g. Listers papers etc) as these have no relevance to this investigation and are not discussed later in the paper. Probably the most important initial studies on CPO development were Sander's pioneering investigations; why weren't these mentioned? Only cite papers relevant to current work.

Lister's work was certainly pioneering at the time, but not relevant to the current paper. It is modeling based on the Taylor-Bishop-Hill model, often used to predict crystal orientations based on free lattice spinning of the crystals in a homogeneously deforming polycrystal and does not require accommodation processes such as dynamic recrystallization. This same situation also applies to a number of other simulation models, that the authors fail to cite; they also do not involve combined crystallographic slip and dynamic recrystallization. The latter processes are what the author is trying to highlight in this paper and the initial modeling statements are a 'red herring' and should be deleted.

Line 34: Could also cite reference for U-stage techniques (e.g. Kile 2009)

Methods – I stand corrected here, as I was informed Larson had been given the last remaining G50 as a stand-in machine before he got his G60. In their revised text the authors should be quite specific that this instrument is using the monochrome CCD sensor etc etc. However, the authors should be very aware that nearly every G50 and earlier versions of the FA, are slightly different and many of the earlier glaciology

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instruments do present problems when used to analyse geological materials; I have seen this in other papers that I have had to review, and particularly when students use the fabric analyser they are not aware of how important geometrical quality is for producing correct data sets.

This raises the very important question of calibration and interlab comparisons. I have used a collection of precisely cut thin sections from single quartz crystals to verify my instrument and earlier versions of FA together with X-ray and EBSD data to verify orientation data. I have also given Russell-Head some of these sections and my quartz wedge for instrument calibration. On numerous occasions we have discussed that each new purchaser should be given a reference thin section, however because of instrument delays and lack of funds we never got around to producing reference sections. This is even though I had a sample of Heavitree Quartzite (from Alice Springs) that was going to be used for this purpose. It is the same material Jan Tullis experimentally deformed at Brown University. I may suggest that sometime in the future an interlab comparison could be made as part of the G60 delivery.

Lines 147-148: I am still concerned about the presentation of the bulk data. The strong single maxima here are biased to larger grains (more pixels per area). This is giving you a bias that is not a true representation of the orientation of the true grain population. Such bulk analyses should only be presented if you have a uniform grain size and in a pure quartzite, otherwise they can be quite misleading. The main advantage of a bulk analysis, is that they are a quick and easy method of establishing the general orientation pattern; but should not be used here. Therefore I would delete Fig. 4a.

To alleviate my concerns I would definitely recommend that you delete Fig 4a (bulk analyses) and play down the significance of your bulk analyses. This includes modifying figure caption.

Lines 167-169: I still maintain that this statement seems far-fetched, the sample is 3 km from fault; are similar microstructures and fabrics observed in the fault and intervening

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area? In the geological setting the reader is given no idea of strain regime across region – is this going to be rectified?

Lines 170-186: The authors need to delete any reference to bulk analyses. These two paragraphs are confused, and could very well be rewritten more concisely to highlight strain portioning. Any statements about [c]-slip being interpreted as higher temperature should come at end and not in the beginning. Alternatively the statement regarding temperature is more appropriate in next section.

Lines 194-204: Contrary to the authors response much of this section could be shortened. Why not combine with next section? Will need a new sub-heading.

Lines 215-244 and Fig. 5: Again I reiterate this section needs to be modified and shortened in light of foregoing comments. It could be combined with lines 205-213 and needs to be changed in light of the comments below.

Many of the arguments used by the authors in this section are based on Stipp & Tullis (2003), Stipp et al. (2010), Hirth etc, whose studies involved pure quartz rich rocks, especially mylonites. The authors are not describing a quartzite or mylonite but instead a phyllite where there is obvious strain localization and strain partitioning. As Stipp et al. (2010) point out, distinct piezometer calibrations for different recrystallization mechanisms is a minimum and varies in pure quartz rich samples. Whereas, we don't have any good comparisons for the rheology of quartz-mica rich samples [except for the work of Mista et al. 2013 (Tectonophysics) at very much higher temperatures where there is partial melting] and again this is going to depend on strain and strain rate. Therefore I would like to see this section reorganised, incorporating material from lines 193-213 and with scaling back discussion between lines 230-244.

Discussion This definitely needs to be shortened and rewritten in light of above comments, as there is obvious repetition. Lines 272-275 seem out of place and overlap with lines 247-253. Between lines 272-285 there is too much speculation "..may reflect. . .", "..also possible. . ." ". . .may have. . ." and again an inappropriate reference to Lister &

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Paterson (1979). Why not combine it with conclusions?

Reference Kile, D.E 2009. The universal stage: The past, present and future of a mineralogical research instrument. Geochemical News v. 40 (not sure of page numbers). (July 2009).

Chris Wilson 07/10/2014

Interactive comment on Solid Earth Discuss., 6, 2735, 2014.