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Title: Brittle-viscous deformation of vein quartz under fluid-rich low greenschist facies conditions

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Reviewed by Michel Bestmann

This research article deals with a coarse grained quartz vein, embedded in a phyllonitic matrix, deformed under low greenschist facies conditions in the Reppartfjord tectonic Window in northern Norway. The studied vein quartz acted as a relatively rigid body deformed under mainly coaxial strain. The authors analyses the microstructures of one specific deformed quartz aggregate by optical and SEM methods (SE, BSE and especially by EBSD). The quartz aggregate shows different deformation domains which are analysed and discussed in detail with respect to the dominant deformation mechanism. The different quartz domains are characterized by their different previous host crystallographic orientation and their internal deformation microfabric. The article focuses on the development of the intracrystalline localized deformation zones which show bands of recrystallized grains. Especially by means of EBSD data the authors discuss if the different deformation microstructures and, especially the new recrystallized grains, are developed by either crystal plastic processes (dislocation creep accommodated by subgrain rotation recrystallization) or are the result of microfracturing and subsequent fluid assisted sealing. The authors discuss the possible activation of slip systems for the different host orientations. The authors concluded that, for the quartz domain which is in a favorable orientation for easy slip by basal <a>, dislocation glide initially accommodated viscous deformation but during continuous deformation became inefficient and let to dislocation tangling, strain hardening and eventually micro cracking. The authors conclude that most of the analysed quartz domains are not in a favorable orientation for easy glide with respect to the local stress field (coaxial deformation). These crystals, misoriented for basal slip, underwent hardening and deformed pervasive by domainal fracturing. The authors concluded that, for all new recrystallized grains in the localized deformation zones, microcracking with a small amount of passive rotation (new grains show CPO and are scatter around the host orientation) and subsequent fluid assisted sealing resulted in the observed microfabric. Therefore the authors follow mainly the scenario of Vernooij et al. (2006) who carried out quartz deformation experiments that resulted in similar microfabrics.

The studied topic is quite interesting and up-to-date. Since a few years the “brittle-plastic transition” is a hot topic. The microstructural community still is far away to understand how initial stain localization takes place in nature. Therefore the research work of quartz deformation microstructure as the authors presented are essentially to learn more about this important topic.

I am aware that for such kind of microstructure it is not so easy to determine the deformation process: microfracturing versus crystal plasticity. Therefore it is essential to analyses the microstructure with various methods, including EBSD, TEM and CL as for example Vernooij et al., 2006 and Trepmann et al., 2007 and Bestmann et al., 2012 have demonstrated in a rigorous way.

Having said that I want to point out some main problems with the paper

1. The discussion about crystal plasticity is based on the favorable or non-favorable orientation of the host orientation domain for easy slip of know slip systems in quartz. But this only can be archived if the orientation of the main stress axis $\sigma1$ of the strain regime is known. There is no detailed description and discussion to constrain the orientation of $\sigma1$. I have the impression that the authors sometimes interpreted the crystallographic orientation of the host domains with respect to simple shear and sometimes with respect to pure shear. Therefore the authors should clarify this point to support their conclusions on the possible slip systems. In addition, the ms does not contain plots of misorientation axis of low angle boundaries in order to constrain the active slip systems in quartz (see Neumann, 2000). Further the presented misorientation profiles are not appropriate to show if crystal plasticity occurred or not (see Bestmann and Prior, 2003; Vernooij et al., 2006).
2. I believe the authors made a mistake when plotting the trace of the prism plane into the <a> axis pole figures (the trace of one of the prisms-plane can never be perpendicular to one of the <a> axis in quartz). As a consequence, the entire discussion and interpretation with respect to that aspect is influenced and, in the present version, is not correct.
3. I find that the discussion of the manuscript on possible deformation processes (crystal plasticity vs. microfracturing) is not providing a conclusion in an unbiased way. The discussion appears to be constructed in a way that final conclusion is following: the deformation is related to strain hardening by dislocation tangling, fracturing and subsequent fluid assisted sealing. This conclusion remains highly speculative without TEM and CL analysis. I am aware that a paper cannot deal with all aspects of analytic methods. However since the entire discussion and interpretation is based on the assumption that intracrystalline stain hardening resulted in micro fracturing and subsequent fluid assisted sealing “hard” data have to be shown and discussed in order to support the conclusions. Therefore TEM and CL analysis are essential - see Vernooij et al., 2006; Trepmann et al., 2007 and Bestmann et al., 2012.

All in all I cannot recommend the manuscript for publication in Solid Earth in this state. If the authors agree to follow my main suggestions than a new submitted paper can have a valuable impact with respect to the hot topic of strain location at the brittle-viscous transition.

Michel Bestmann

Main comments:

1. Analysis and interpretation of the EBSD data:

1a. The authors discussed the EBSD data in the light of easy slip orientation of the different domains with their different initial host orientation with respect to the stress field. The authors inferred a coaxial strain for the more rigid quartz domains embedded within the softer phyllonitic matrix:

p. 9, Line 1-2. *The small-scale shear bands locally indicate both dextral and sinistral sense of shear, suggesting a component of flattening.*

p. 21, Line 14-19. *The progressively increasing competence contrast between the phyllonitic matrix and the quartz vein generated significant strain partitioning, wherein coaxial deformation was accommodated by the quartz vein (e.g. the described conjugate shear bands) and non-coaxial deformation by the matrix, as shown by, for example, rotated porphyroclasts and asymmetric shear bands)*.

In order to constrain the slip systems in quartz, the orientation of the main shortening direction ($\sigma1$) has to be constrained. From the two sentence above I am not sure if $\sigma1$ is orthogonal to the main foliation orientation and the cut of the thin section and therefore the EBSD maps) (*wherein coaxial deformation was accommodated by the quartz vein (e.g. the described conjugate shear bands)* or is slightly oblique to the foliation (*The small-scale shear bands locally indicate both dextral and sinistral sense of shear, suggesting* ***a component of flattening***)

The orientation of $\sigma1$ actually should be discussed first before starting the discussion about possible slip systems which are in a favorable position for easy slip.

In the following I want show this problematic for the different domain (1 and 4)

Domain 1:
p. 15, Line 18-19: Domain 1, for example, would have been optimally oriented for viscous slip to be accommodated along either the prism <a> or prism <c> (Fig. 6b).
-> If we assume sigma-1 is vertical than I do not see that any of the prism-planes are in favorable position (45° to sigma-1) for easy slip.

ATTENTION: The trace of an m-plane is not correctly oriented: the trace of a {m} plane can never be oriented perpendicular to an <a> axis in the crystallographic system of quartz. The pole of the {m} plane with the a-axis makes an angel of 30°. As an addition comment: please also present the {m} planes as pole figures, than you will see it.

p. 15, Line 22-24: Synkinematic chlorite thermometry from similar faults in the area has established a peak temperature of < 300 °C, ideal for activation of basal <a> slip. The crystal was, however, misoriented for basal <a> slip.
-> If we assume sigma-1 is vertical than indeed the basal plane is misoriented for easy slip

Domain 3:

p. 16, Line 28-30 and p. 17, Line 1: *In contrast to Domain 1, Domain 3 was suitably oriented for effective accommodation of viscous deformation, with the basal plane oriented sub-parallel to the C' shear band, that is, optimally oriented for slip along the basal <a>*

-> If we assume sigma-1 is vertical than I do not see that the planes is in a favorable position (45° to sigma-1) for easy slip. Instead the basal plane is more or less sub-perpendicular to the vertical. The basal plane would be in a position of easy slip if we suppose that we have a dominant simple shear strain regime where sigma-1 is oriented 45° to the main foliation.

In general I suggest applying Schmid Factor analysis to the EBSD data in order to discuss easy slip orientations and the probability of slip systems (see for example Fig. 12 in Bestmann and Prior, 2003)

Domain 2:

p. 18, Line 3-5: *Domain 2 is remarkably different from all other sites. In it, the orientation of the now almost totally obliterated old grains is similar to that of Domain 3, that is, optimally oriented for glide-accommodated creep with slip along the basal <a>.*

-> Again if we assume sigma-1 is vertical than I do not see a favorable position *for glide-accommodated creep with slip along the basal <a>*. Instead the basal plane is more or less sub-perpendicular to the vertical. The basal plane would be in a position of easy slip if we suppose that we have a dominant simple shear strain regime where sigma-1 is oriented 45° to the main foliation.

Therefore the entire discussion about possible slip system has to be rewritten

1b. Further I suggest that the data of the misorientation axis of low angle boundaries (plotted in the crystallographic reference system) should be presented in order to discuss the activity of possible slip systems (see for example Bestmann & Prior, 2003 for calcite and Neumann, 2000, Vernooij et al., 2006 for quartz)

1c. sub grain rotation recrystallization can be excluded for the new grains

The authors stated at several places, for example p. 19, Line 28-29 and p. 20, Line : *we conclude that dynamic recrystallization by sub grain rotation recrystallization can be excluded for the new grains because:*

(1) no progressive rotation of the lattice towards the bands of new grains is observed (Figs. 6d–9d)

-> In order to prove your conclusion you have to show more detailed misorientation profiles (see Bestmann & Prior, 2003 or Vernooij et al., 2006). The locations of the misorientation profiles you show are not appropriate to prove if there is any progressive rotation of the lattice.

(1) *the new grains show large misorientations to the host*:

➔ Bestmann & Prior (2003) showed that dynamic recrystallization by subgrain rotation can produce large misorientation angles between host and new recrystallized grains due to subsequent grain boundary sliding after subgrains are surrounded totally by high angle boundaries.

- ➔ In order to tell something about the misorientation between new grains and their host grains the misorientation has to be measured statistically – see also Fig. 10 in Bestmann & Prior (2003)

1d. When I look to your EBSD maps I see in all maps a more or less high content of subgrain boundaries where subgrains have the same size as new grains, which would support the fact that new grains are related to subgrain rotation recrystallization. This is also supported by the CPO of new grains which scatter around the host orientation.

In many cases (for example for domain 1) you did not describe the subgrain boundary microstructure. Further the locations of the misorientation profiles are not representative/appropriate in order to show if lattice distortion in the host grain is evident. This information is important for a detailed unbiased discussion about the deformation mechanisms.

The EBSD maps should be larger and presented with a higher resolution in order to be able to regard the subgrain boundary microstructure in detail.

Page 2, Line 15-16: *dislocation glide-accommodated deformation of quartz resulted inefficient and led to dislocation tangling and strain hardening of the vein*

- ➔ Since there are all over the EBSD maps subgrain boundaries and at least in Domain 2 and Domain 3 (I presume also in all other domains) also continuous lattice deflection is evident I would conclude that also dislocation creep was active

1e: I suggest applying a texture component to the EBSD maps (see for example Bestmann & Prior 2003) in order to highlight continuous lattice rotation in the deformed host

2. Nucleation of band with new grains along localized shear zones sub-parallel to prism planes (e.g. as stated for Domain 1 and Domain 4)
   - ➔ As already pointed out above there is a systematic error in presenting the trace of the prism plane {m} with respect to the host crystal and therefore with respect to the reference frame. An {m} plane cannot be oriented perpendicular to an <a> axis as you have drawn in Fig. 6b and Fig. 9b.

3. *dislocation glide-accommodated deformation of quartz resulted inefficient and led to dislocation tangling and strain hardening of the vein (p. 2, Line 15-16)… quartz crystals began to deform frictionally along specific, optimally oriented lattice planes, creating microgouges along microfractures (p. 2, Line 18-20)... Instead, the crystals misoriented for basal slip hardened and deformed by pervasive domainal fracturing (p. 2, Line 26-27)*
   - ➔ The entire discussion and interpretation follows mainly the work and conclusion of Veroniij et al (2006) and Trepmann et al (2007).
     I want to point out some points where to pay attention to take over 1 to 1 the conclusions from Veroniij et al (2006) and Trepmann et al (2007).

In both papers the quartz microstructure are produced experimentally under high temperatures (Veronij et al, 2006: 800°C; Trepmann et al., 2007: deformation at 400°C and annealing at 800-1000°C). Therefore extrapolation to natural samples with lower temperatures should be handled with care and should be discussed in detail. Further the interpretation of the microstructure of Vernooij et al. (2006) and Trepmann et al (2007) is based on a combination of EBSD and TEM work.

First lets start with the EBSD data: For both Vernooij et al. and Trepmann et al. the low angle boundaries are located near the band with recrystallized grains. Between the recrystallization bands not many subgrain boundaries are evident. In your microstructures there are more or less in all maps subgrain boundaries all over the place. Vernooij et al. showed that many of the subgrain boundaries are parallel to rhomb planes and are healed microcracks. You should check if this is also the case for your microstructures.

In contrast to you work Vernooij et al. and Trepmann et al. showed by TEM analysis that in deed tangled dislocation are evident and that deformation hardening and microfracturing occurred (also evident by TEM).

Without TEM analysis your entire discussion about dislocation tangling and microfracturing is highly speculative.

Interpretation of Domain 2 (p. 18, Line 5-12 : *Despite the similarity in orientation, however, the microstructures observed are remarkably different, with Domain 2 displaying a significant volume of newly nucleated grains, with only very few remnants of the old grains (Figs. 3 and 7). We ascribe the pervasive nucleation of Domain 2 to its location. The domain is located in a very high strain zone, confined between the C' shear band and the foliation, which likely caused substantial fracturing during incipient deformation and subsequent pervasive nucleation and sealing.*

I cannot follow the argumentation. How can you preserve a CPO by fracturing and passive rotation of fragments in a very high strain location? During ongoing deformation/strain the fragments should lose any kind of orientation relation with respect to the host orientation. Only very small amount of strain and little passive rotation of subgrain created by microcracking should result in CPO characterized by a scattering of new grains around the host orientation.

How can you produce a relatively homogeneous uniform grain size by fracturing and subsequent annealing? Fracturing should result in a very heterogeneous grain size distribution and I cannot imagine how subsequent sealing by precipitation from a fluid should homogenize the grain size distribution.

4. *…quartz crystals began to deform frictionally along specific, optimally oriented lattice planes, creating microgouges along microfractures. These were, however, rapidly sealed by nucleation of new grains as transiently over pressured fluids penetrated the deforming system. The new nucleated grains grew initially by solution-precipitation and later by grain boundary migration (p. 1, Line* 20-22)

- ➔ I suggest applying SEM cathodoluminescence analysis. This could support the idea of microcracking and healing (see for example, Bestmann et al., 2012). The SEM images of micropores by itself are not 100% convincing that microfracturing and healing/sealing occurred.

5. *The new nucleated grains* ***grew*** *initially by solution-precipitation and later by* ***grain boundary migration*** *(p. 1, Line* 20-22)…. ***New grains*** *started* ***to grow*** *by a solution-precipitation mechanism promoted by the ion-rich fluids (p. 18, Line 20-21)*

   - ➔ When I look to your EBSD maps I have the impression that the subgrains have the same grain size range as the new grains. You state that along the fracture bands fragments of the host grain are ripped out from the host and these acts as nucleus and grow by solution-precipitation. I do not find evidence for a subsequent growth of initial smaller nucleus in your data set. Again if a iron rich fluid was involved I would presume that you see some evidence for that process in CL images.

6. I am not sure if the reference system of your pole figures is the same as for your rose diagram. In general horizontal line in the pole figures should be the main foliation and therefore relate to S in your rose diagram. But if I look to Figures 7 and 8 than the orientation of the trace of the basal plane is different with the one in the rose diagram.

7. Mechanical implication for the PIS development

Page 22, Line: *Although the weak phyllonitic core of the NFZ could suggest that the fault deformed mainly by aseismic creep, the current fault architecture has to be projected against its temporal dynamic evolution. Initial strain accommodation occurred in fact under different rheological conditions, with transient embrittlement episodes and a possible* ***seismic stick-slip*** *behavior as also suggested from other localities within the PIS*

- ➔ based on the microstructural analysis of the quartz aggregate a suggestion about the occurrence of seismic stick slip behavior is highly speculative

Step by step comment:

Abstract:

in general: in the abstract also the data should be mentioned

Page 2:

Line 2: *A coarse grained* -> give gain size

Line 13-14: *Viscous deformation was initially accommodated by* ***basal <a> slip*** *of quartz*

- ➔ See general comment 1a; refer to data (e.g. misorientation axis of low angle boundaries)

Line 15-16: *dislocation glide-accommodated deformation of quartz resulted inefficient*

- ➔ Since there are all over the EBSD maps subgrain boundaries and at least in Domain 2 (I presume also in all other domains) also continuous lattice deflection is evident I would conclude that also dislocation creep was active – see main comment 1d

Line 15-16… *and led to dislocation tangling and strain hardening of the vein*

- ➔ Without TEM images this interpretation is purely speculation – see main comment 3

Line 20-22:… *These were, however, rapidly sealed by nucleation of new grains as transiently over pressured fluids penetrated the deforming system. The new nucleated grains grew initially by solution-precipitation*

- ➔ Should be checked by CL analyses

Line 22: *… and later by grain boundary migration*

- ➔ See main comment 5.

Line 23: *… to the random initial orientation of the vein crystals*

- ➔ I cannot find any data in your manuscript about the initial orientation of the vein crystals

Line 25-26: … *Crystals oriented optimally for basal slip accommodated strain mainly viscously and experienced only minor fracturing*

- ➔ See main comment 1

2. Geological setting:

Since the paper is not so strongly related to the regional setting I would suggest shortening both the Regional setting (section 2.1) and Nussirjavrri Fault Zone (section 2.2)

3. Analytic method

Was the thin section coated?

What acquisition software did you use?

Page 8,

Line 2: ***To enhance the diffraction signal****, thin sections were polished using colloidal silica*

- ➔ Instead *"To enhance the diffraction signal"* – To remove surface damage caused by previous mechanical polishing

Line 25 and Page 9, Line 1: *… exhibit partial recrystallization localized in the neck zones of the boudins (Fig. 4a).*

- ➔ This is not obvious from the photo – too low magnification and resolution.

Page 9:

Line 2: *dextral and sinistral sense of shear, **suggesting a component of flattening**.*

- ➔ This is an interpretation and should go into the discussion part of the ms

Line 11-12: *Quartz crystals within the vein range in size from < 0.5 to > 6 mm, do not show a crystal preferred orientation (CPO; Figs. 6–9b)*

- ➔ Come on, please don't tell me that you use 4 grain orientations to convince the reader that statistically the quartz crystals in the vein do not show a crystal preferred orientation (CPO). In order to prove statistical a random distribution you need to measure a minimum of 250-300 grain orientations.

Line Line 12-15: *In general, the boundaries between the individual grains are slightly irregular but straight, which gives the individual quartz crystals a blocky appearance.*

- ➔ Do you have a figure to show that?

4.2 Microstructural and EBSD analysis

Line 17. *Because of the marked textural differences*

- ➔ Avoid the term "textures"

Page 10

4.2.1 Domain 1

Line 13: *WEB's have an antithetic geometry to the large shear band that cuts through*

- ➔ This is not obvious for me

Line 17-18: *EBSD analysis of the sub parallel bands of new grains shows that they range in size between ca. 5 and 50 µm (Fig. 6f and Table 1), and have a different crystallographic orientation from the host (Fig. 6d).*

- ➔ What do you mean by "*have a different crystallographic orientation from the host"?*

For me the new grains have a CPO and scatter around the host orientation and are slightly rotated anti-clockwise around the Y-axis of the sample reference system.

Line 24-25: *contains bands that are sub-parallel to the prism plane of the host old grain (dashed* *line in right stereonet Fig. 6b)*

➔ The trace of an m-plane is not correctly oriented: the trace of a {m} plane can never be oriented perpendicular to an <a> axis. The pole of the {m} plane with the a-axis makes an angel of 30°. As an addition comment: please also present the {m} planes as pole figures, than you will see it - see also main comment 1a.

Page 11

Line 3-6: *The new grains have a more scattered c axis distribution (Fig. 6c), possibly reflecting a clockwise rotation around a sub-vertical axis causing their distribution to fade towards the periphery and the "north" of the pole figure.*

➔ What do you mean by "*rotation around a sub-vertical axis"* – for me the new grain scatter around the host orientation with a tendency of an anti-clockwise rotation around the Y-axis of the sample reference system (center of the pole figure).

Line 6-7: *They display large misorientations (locally > 40°) to each other and to the host*

➔ In order to tell something about the misorientation between new grains and their host grains the misorientation has to be measured statistically – see also Fig. 10 in Bestmann & Prior (2003)

Line 7-8: *There is no progressive rotation of the lattice when approaching the bands from the host, i.e. the change in misorientation is abrupt…*

➔ In order to prove that you have to show more detailed misorientation profiles (see Bestmann & Prior, 2003, or Vernooij et al., 2006). The location of the misorientation profiles you show are not appropriate to prove if there is any progressive rotation of the lattice – see also main comment 1c

Line 8-9: *thus excluding recovery and recrystallization by subgrain rotation (Fig. 6d).*

➔ This is an interpretation and should go into the discussion part – Bestmann & Prior did show that dynamic recrystallization by subgrain rotation can produce large misorientations between host and new recrystallized grains due to subsequent grain boundary sliding after subgrains are surrounded totally by high angel boundaries – see also main comment 1c

➔ I suggest applying a texture component to the EBSD maps (see for example Bestmann & Prior 2003) in order to highlight continues lattice rotation in the deformed host – see also main comment 1e.

In general:

- Why do you not describe the subgrain boundary microstructure which is obvious in the EBSD maps. This information is important for a detailed unbiased discussion about the deformation mechanisms – see also main comment 1 d
- Do the new grains show subgrains or reveal internal distortion?

4.2.2 Domain 2

Line 14: *It is almost completely made up of nucleated new grains*

- ➔ Cut out "*nucleated*"

Line 26-27: *No progressive lattice rotation is visible in the old grains towards the new grains (Fig. 7d).*

- ➔ But the old grains obviously show subgrain boundaries – you should mention that.

Page 13

4.2.3 Domain 3

Line 17-18: *The c axis of the host old grain is subparallel to the **Z direction of finite strain**, and slightly inclined with the bulk sinistral sense of shear (Fig. 8b)*

- ➔ Do you mean the strain ellipsoid? How did you constrain the strain ellipsoid? I though Z is related to the sample reference system and is normal to the main foliation and therefore the top of your pole figures. That is the general way to present crystallographic orientation data in pole figures.

Page 13

Line 5: *The data seems to have been rotated clockwise around a sub-vertical rotation axis.*

- ➔ What do you mean by "sub-vertical rotation axis" – you should use the axes of the sample reference system (X, Y, Z) or a crystallographic axis when you describe the dispersion path of orientation data.
- ➔ For me the scattering of the new grains looks as if they were slightly rotated in an anti-clockwise sense around an axis sub-parallel oriented to Y-axis of the sample reference system (center of the pole figures)
- In general: The host shows an intense subgrain microstructure, the misorientation profile reveals evidence of more or less continuous lattice deflection. This information is important for a detailed unbiased discussion about the deformation mechanisms – see also main comment 1 d

4.2.4 Domian 4

Line 25: *the Z and Y axis of strain*

- ➔ See comment p. 13, Line 17-18

Line 26-27: *Undulose extinction is seen as different shades of* ***orange****.* ***Red*** *represents Dauphiné twins.*

- ➔ You should refer that orange and red are the colour shades with respect to the euler angles.

Line 27: *The two identified sets of bands are parallel to the prism*

- ➔ the trace of an m-plane is not correctly oriented: the trace of a {m} plane can never be oriented perpendicular to an <a> axis. The pole of the {m} plane with the a-axis makes an angel of 30°. As an addition comment: please also present the {m} planes as pole figures, than you will see it - see also main comment 1a

- ➔ In general: The host shows an intense subgrain microstructure, the misorientation profile reveals evidence of more or less continuous lattice deflection. This information is important for a detailed unbiased discussion about the deformation mechanisms – see also main comment 1 d

5 Discussion

7.1 Strain accommodation history within the different domains

Page 15

Line 2-3: *Environmental conditions were such that quartz began to deform by low-grade crystal plastic deformation by dislocation glide (Fig. 10b).*

- ➔ Why dislocation glide and not dislocation creep? The EBSD reveal an intense subgrain boundary microstructure. Also Fig. 6b reveals a dispersion of the host orientation. Together with the subgrain boundaries this points to recovery by dislocation creep - see also main comment 2b

Line 18-19: Domain 1, for example, would have been optimally oriented for viscous slip to be accommodated along either the prism <a> or prism <c> (Fig. 6b).

- ➔ If we assume sigma-1 is vertical than I do not see that any of the prism-planes are in favorable position (45° to sigma-1) for easy slip.

  The author make a serious error, the trace of an m-plane is not correctly oriented: the trace of a {m} plane can never be oriented perpendicular to an <a> axis. The pole of the {m} plane with the a-axis makes an angel of 30°. As an addition comment: please also present the {m} planes as pole figures, than you will see it.

  See main comment 1a

Line 22-24: Synkinematic chlorite thermometry from similar faults in the area has established a peak temperature of < 300 °C, ideal for activation of basal <a> slip. The crystal was, however, misoriented for basal <a> slip.

- ➔ If we assume sigma-1 is vertical than indeed the basal plane is misoriented for easy slip - see main comment 1a

Further I suggest when the activity of possible slip systems are discussed also the data of the low angle misorientation axis plotted in the crystallographic reference system should be discussed (see for example Bestmann & Prior, 2003 for calcite and Neumann, 2000, Vernooij et al., 2006 for quartz) – see main comment 1b

Line 26-29: *dislocation glide resulted soon ineffective, leading in turn to dislocation tangling, strain hardening and localized embrittlement of the deforming quartz accommodated by diffuse fracturing*

- ➔ To prove that you need to do TEM and also CL otherwise your conclusions are purely speculative. Again I do not see the reasons why only dislocation glide and not dislocation creep occurred – see comments before

Page 16

Line 2: *Evidence for fluid-accompanied fracturing, such as fluid inclusion trails (Fig. 5b and c),*

- ➔ You should do CL imaging in order to prove the interpretation – see also main comment 4

Line 14-15: *The two sets of bands with nucleated new grains within Domain 1 are sub-parallel to the prism and the rhomb face of the crystal.*

- ➔ the trace of an m-plane is not correctly oriented: the trace of a {m} plane can never be oriented perpendicular to an <a> axis. The pole of the {m} plane with the a-axis makes an angel of 30°. As an addition comment: please also present the {m} planes as pole figures, than you will see it.

Line 22-24: *This suggests that a component of flattening characterized the NFZ deformation, as also indicated by numerous conjugate shear bands in the surrounding phyllonitic matrix.*

- ➔ See main comment 1a

Line 26-26: *In summary, the "striped tiger" microstructure observed within Domain 1 is best interpreted in terms of "sealing" of these earlier micro-fractures by nucleation of new grains*

- ➔ You should do CL imaging in order to prove your conclusion – see also main comment 4
- ➔ But why do the new grains show a CPO scattering around the host orientation? It also can be explained by subgrain rotation recrystallization – see main comments 1a-d

Line 28-29: *In contrast to Domain 1, Domain 3 was* ***suitably oriented*** *for effective accommodation of viscous deformation, with the basal plane oriented* ***sub-parallel to the C'*** *shear band,*

1. In Figure 8f, the trace of the basal plane is not correctly oriented when I compare the c-axis pole figure in Fig. 8b and the rose diagram in Fig. 8e. I hope your S (main foliation) in the rose diagram corresponds to the horizontal of your pole figure. When I measure the true angle between the basal plane and the C' than I get an angle of 35°, which I would NOT call "sub-parallel".
2. The angle between the vertical (I suppose that you consider this direction as your sigma-1, as you stated before: *this suggest a component of flattening*) and the basal plane has an angle

of 79° and therefore is far away from being “suitable” oriented to accommodate viscous deformation - see also comment 1a

Page 17

Line 1: *that is, optimally oriented for slip along the basal <a> (Schmid and Casey, 1986).*

- ➔ But only when you assume simple shear where sigma 1 is oriented 45° to the main foliation – see also main comments 1a.
- ➔ You should show the misorientation axis plots of low angle boundaries plotted in the crystallographic reference system – see main comment 1b

Line 4: *The pole figure of the old grains shows indeed a single crystal maximum in the location expected for quartz deformed under lower greenschist facies conditions*

- ➔ What do you mean? The information is unclear.

Line 7: *As in the case of Domain 1, we interpret these as sealed fractures, with one set oriented sub-parallel to the basal plane of the crystal and the second sub-parallel to the rhomb plane (Figs. 6b and 10c).*

- ➔ Your conclusion is not convincing. The host contains an intense subgrain microstructure where subgrains have the same grain size range as the new grains. Your misorientation profile reveals evidence of lattice deflection. Together with the CPO of the new grains scattering around the host orientation subgrain rotation recrystallization is very likely to be the main deformation process – see also main comment 1d

Line 20-22: *We suggest that these fractures opened during an earlier embrittlement event, possibly under different physical boundary conditions, wherein the fractures healed by epitaxial growth on the fracture wall.*

- ➔ I cannot see any evidence in your microstructure for *fractures healed by epitaxial growth on the fracture wall* - neither in the microstructure nor in the CPO of the new grains

Line 25-26*: by early dislocation tangling resulting in undulose extinction and progressive strain hardening and domainal fracturing*

- ➔ Do you have any evidence for dislocation tangling? There are a lot subgrain boundaries which point to recovery by dislocation creep

Line 28: *but the prism-parallel bands are more continuous*

- ➔ the trace of an m-plane is not correctly oriented: the trace of a {m} plane can never be oriented perpendicular to an <a> axis. The pole of the {m} plane with the a-axis makes an angel of 30° - see also main comment 1a

Page 18:

Line 5: *optimally oriented for glide-accommodated creep with slip along the basal <a>*

- ➔ if we assume sigma-1 is vertical than I do not see a favorable position *for glide-accommodated creep with slip along the basal <a>*. Instead the basal plane is more or less sub-perpendicular to the vertical. The basal plane would be in a position of easy slip if we suppose that we have a dominant simple shear strain regime where sigma-1 is oriented 45° to the main foliation – see also main comment 1a

Line 5-12 : *Despite the similarity in orientation, however, the microstructures observed are remarkably different, with Domain 2 displaying a significant volume of newly nucleated grains, with only very few remnants of the old grains (Figs. 3 and 7). We ascribe the pervasive nucleation of Domain 2 to its location. The domain is located in a very high strain zone, confined between the C' shear band and the foliation, which likely caused substantial fracturing during incipient deformation and subsequent pervasive nucleation and sealing.*

- ➔ I cannot follow the argumentation. How can you preserve a CPO by fracturing and passive rotation of fragments in a very high strain location? During ongoing deformation/strain the fragments should lose any kind of orientation relation with respect to the host orientation. Only very small amount of strain and little passive rotation of subgrain created by microcracking should result in CPO characterized by a scattering of new grains around the host orientation.

How can you produce a relatively homogeneous uniform grain size by fracturing and subsequent annealing? Fracturing should result in a very heterogeneous grain size distribution and I cannot imagine how subsequent sealing by precipitation from a fluid should homogenize the grain size distribution.

5.2 Sealing of the microstructure

Line 20-21: *New grains started to grow by a solution-precipitation mechanism promoted by the ion-rich fluids*

- ➔ why iron-rich fluids?
- ➔ I suggest applying SEM cathodoluminescence analysis to prove that
- ➔ When I look to your EBSD maps I have the impression that the subgrains have the same grain size range as the new grains. You state that along the fracture bands fragments of the host grain are ripped out from the host and these acts as nucleus and grow by solution-precipitation. I do not find evidence for a subsequent growth of initial smaller nucleus in your data set. Again if an iron rich fluid was involved I would presume that you see some evidence for that process in CL images – see also main comment 5.

Line 24-27: *fragments of the host grain were ripped loose generating micro gouges, especially in volumes with jogs and geometric irregularities along the fracture planes (Fig. 10c). This mechanism explains the rotation of the crystallographic axes as shown by the EBSD data*

- ➔ Only for very low strain

Line 27-28: *Subsequent growth of the new grains occurred either by precipitation from the actively infiltrating over pressured*

- ➔ See main comment 5.

Page 19:

Line 6-7: *where fracturing was minimal due to the favorable orientation of the crystal for basal <a> dislocation glide,*

- ➔ Only when you assume simple shear – see main comment 1a

Line 13-23: this is pure speculation – you have to show CL images to prove that.

Line 21-22: *As the new grains are largely strain free they grew at the expense of the old grains, which have a higher dislocation density*

- ➔ I see no evidence of grain growth- see main comment 5.

Line 29 – p. 20, Line 1-2: *no progressive rotation of the lattice towards the bands of new grains is observed (Figs. 6d–9d), (2) the new grains show large misorientations to the host*

- ➔ See main comment 1c

Line 13-14: …*also report partially deformed new grains that show flattening and hence elongation perpendicular to* σ1.

- ➔ You have not defined the orientation of sigma-1 – see man comment 1a

Page 21

Line 14-19: -> see main comment 1a

Line 2-25: *This contrast enhanced dislocation tangling within the vein because deformation mechanisms within the matrix (such as dissolution and grain boundary sliding) were much more effective than glide-accommodated dislocation creep.*

- ➔ This is pure speculation – you need to do TEM analyses; see main comment 3

Page 22

Line 9-10: *with transient embrittlement episodes and a possible seismic stick-slip behavior as also suggested from other localities within the PIS*

- ➔ How can you generate stick slip behavior in a rock where a phylonite takes up the strain? I cannot see how a high stress can be build up

Line 11-13: *Comparison of the microstructures described here with the results of Trepmann et al. (2007) thus corroborates the interpretation of the NFZ as a fault accommodating coseismic deformation.* Trepmann et al. microstructures are different, for example in her kicked and cooked experiment A+B the new grains show a random distribution. Further the temperatures are much higher – see also main comment 3

Figures: General comments

- you should insert in the figures the sense of shear

- The EBSD maps should be larger and presented with a higher resolution in order to be able to regard the subgrain boundary microstructure in detail.
- Schmid factor maps are to be considered
- Application of texture component for EBSD maps
- Pole figures should have the same reference system as the rose diagrams
- In figure caption of the optical images it is not necessary to write with which lens the photos were taken – the scale bar is sufficient