Dear Editor,

We appreciate the review by Dr. Michel Bestmann. He brings up important aspects of the study, which when taken into account in a revised version of the manuscript, will no doubt contribute to a greatly improved text.

We first address Dr. Bestmann´s three main comments separately here below before dealing with his detailed individual comments:

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.*

Answer: The paper clearly states in the geological setting that the NFZ is a thrust. Therefore, the greatest compressive stress is inferred to be sub-horizontal in the geographic reference system. We can thus reasonably assume that sigma1 makes an angle of 30-45 degrees to the foliation However, it is notoriously difficult to constrain stress directions in nature (these are not well-constrained experimental samples), and it is well known that we see strain and not stress in the deformed samples. Due to the competence contrast between the relatively strong quartz clast embedded in the weaker phyllonite matrix, stress trajectories may locally deviate from the theoretical pattern causing flow disturbances leading to, for example, antithetic structures such as shear bands displaying opposite sense of shear (e.g., Lister and Williams 1983, Bell 1985, Goodwin and Tikoff, 2002). This strain partitioning makes it very difficult to assess the real direction of sigma 1 at the millimeter scale.

We discuss the suitability of specific crystallographic planes to slip (and/or to nucleate cracks) considering the microstructural position of the different domains. For example, domain 2 is located at the tip of the vein, where a C' band is localized. We argue that the local sigma 1 for domain 2 is close to the bulk sigma 1 at the thin section scale (e.g. at an angle of 30-45 degrees to the foliation plane), so that domain 2 appears suitably oriented for activation of the basal plane. Indeed, domain 2 does not show the conjugate set of intracrystalline bands of recrystallized grains. On the contrary, domains 4 and 1 are farther away from the C' band and are likely to experience a more coaxial component of deformation, which is recorded by the conjugate set of bands. We emphasize that, as a matter of fact, the intracrystalline bands of recrystallized grains ARE parallel to the trace of specific crystallographic planes. Starting from this observation we elaborate a model, and we feel that it is hard to do better when trying to constrain stress directions in natural polymineralic samples.

To clarify this aspect, in the revised version of the manuscript we will expand the text accordingly, and a few relevant references where appropriate.

In addition, Dr. Bestmann expresses his skepticism regarding our interpretation that sub grain rotation recrystallization and thus dislocation climb were not active mechanisms in the development of the observed “tiger-striped” microstructure. We do not wish to exclude the possibility of dislocation climb being active within the parent grain, as the old grains do indeed contain a significant amount of sub grain boundaries as Dr. Bestmann points out. Our interpretation that the bands of new grains are preferentially formed due to fracturing and subsequent sealing due to precipitation from a fluid and not by sub grain rotation recrystallization is based on four main points: 1) High density of very large (10-40 µm) fluid inclusions between the new grains in the bands, whereas the host only contains significantly smaller fluid inclusions, 2) Fine grained mica crystals and coarser grained calcite crystals are found within the bands of new grains, 3) New grains show what appears to be well-developed crystal faces bordering the pores, thus indicating growth within a fluid filled open space, and 4) Misorientation profiles do not show a progressive misorientation from the interior of the host grains towards the bands of nucleated grains.

This can nonetheless be better documented in the manuscript and in the revised version we will take this into account and will also expand on the role of crystal plasticity within the quartz crystals (as also, Dr. Moralez has asked us to clarify this point).

2. (…) *the authors made a mistake when plotting the trace of the prism plane into the axis pole figures* (…).

Answer: Looking into the reviewer´s comment, we have realized that we had mislabeled most of our pole figures.

First of all, the notation will be modified so as to correctly describe with appropriate brackets an individual specific axis ([]), crystal faces ({}) and sets of crystallographically related axes (<>). In addition, pole figures that in the initially submitted version were used to plot <a>, are actually pole figure of the {m}. This is important, in that there is no need to revise our model meaning that the conclusions as to what crystallographic features are likely to be activated still hold true. In order to further improve the manuscript, though, we will modify figures 6 to 9 by also adding pole figures of the <a> axes (see figure below). This shall provide the reader with a complete picture of the crystallographic features of quartz within each of the investigated domains.

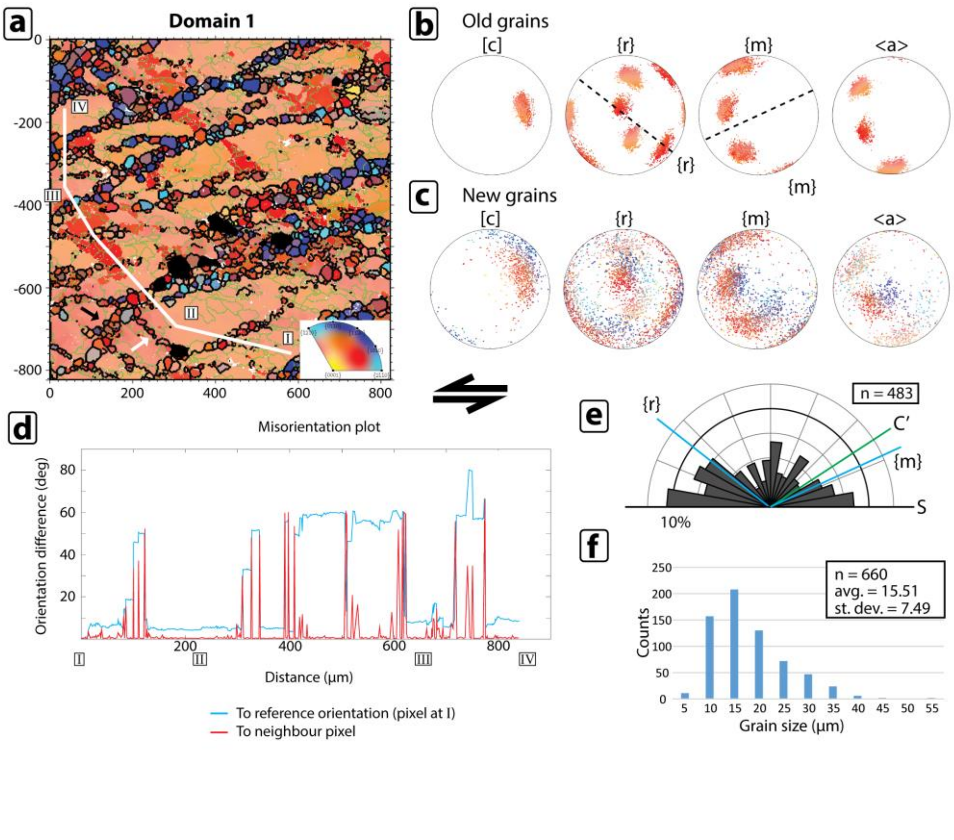


Figure 6 Kjøll et al

Revised figure. More misorientation profiles will be added.

In addition, although this modification to the manuscript is explained in more detail below, we would like to point out that the new version of the figures contains new misorientation profiles, calculated and drawn at high angle to the trace of the direction of the inferred sealed fractures, now healed by the newly nucleated grains. These new sections, when interpreted together within the lack of core and mantle structures, confirm our initial view that there is no sign of progressive lattice rotation within the host toward the nucleated bands of domains 1 and 4, thus strengthening our preferred model of no dominant SGRR therein.

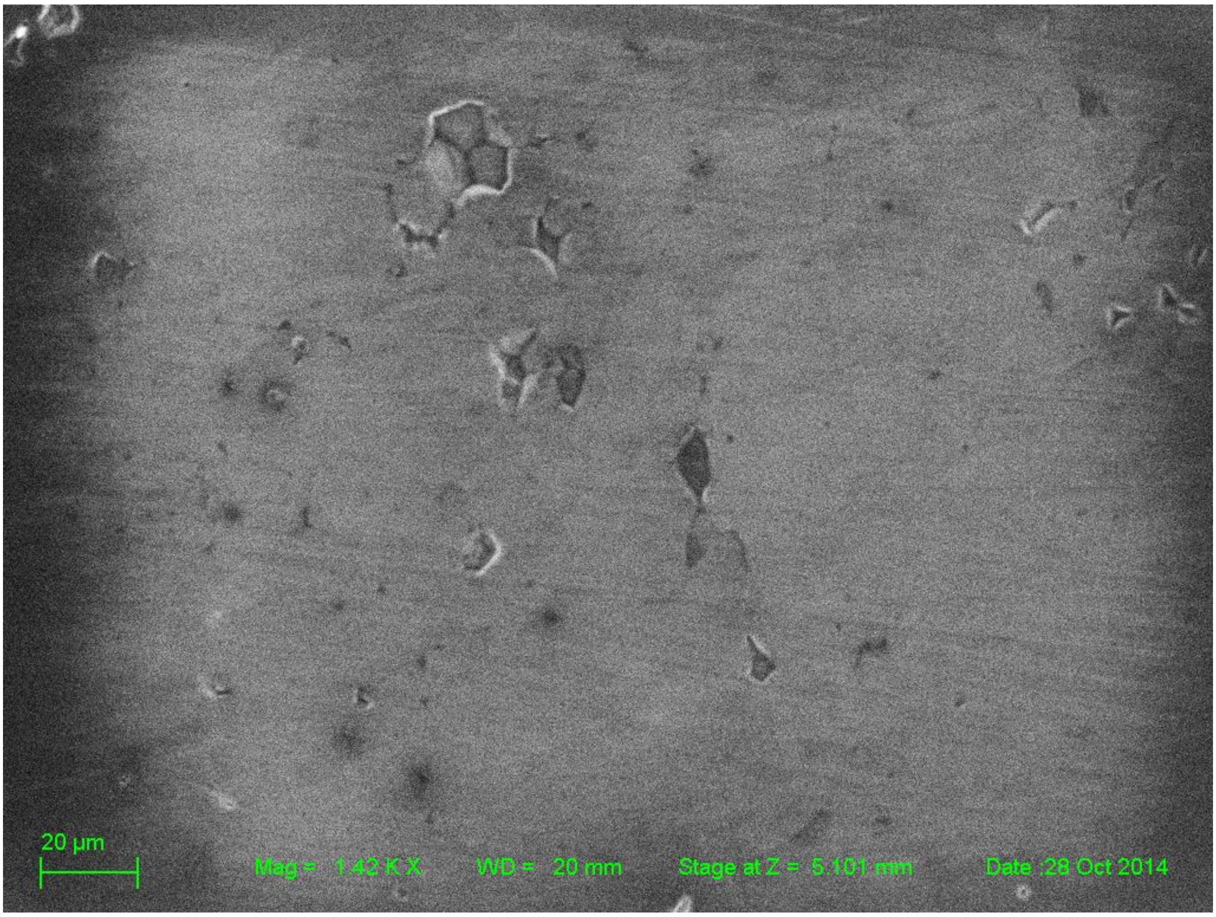
3. (…) *conclusion remains highly speculative without TEM and CL analysis.*

Answer: In order to get the hard evidence Dr. Bestmann is mentioning in his review, TEM is a method we would have liked to use. With that said, the evidence provided by microstructures visible by optical microscopy should not be downplayed or trivialized. Optical microscopy (especially when integrated by solid EBSD analyses and careful observations) and the conclusions and models drawn from it, still hold a potential to help us unravel complex processes and phenomena. While fully aware of the limitations of our study, we remain convinced of the solidity of our observations and of the validity of the model (!!) that we propose. Anyone is free to work further on the subject and even on the rocks we have investigated and prove us right or wrong. What we propose in our study reflects the very best we can produce at the moment in terms of access to instrumentation. To reject the input and contributions of our study and to label them as purely speculative because not validated by TEM analysis is probably excessive and, we may add, offensive to our sincere efforts toward a better understanding of the processes we believe have deformed the studied rocks. We do not pretend to provide the final answer to the highly debated issue of nucleation and growth of recrystallized grains at the brittle/viscous transition here, but we firmly state that our dataset is sufficiently accurate and robust for the current scientific standards. The reviewer knows that a TEM is not a readily accessible instrument, but we are more than happy if the reviewer or other readers wish to perform TEM on the samples to prove/disprove our model.

In addition, we wish to point out that there is an enormous amount of well-respected literature covering similar scientific issues that did not rely on TEM imaging. There is no need to list them all here, but all of them have substantially contributed to shed light on the processes of intracrystalline deformation and recrystallization. In summary, we think that our interpretations are safely anchored in the existing literature and would like to believe that they even represent a nice step forward toward the refinement of our understanding of fundamental deformation processes.

Having said this, we will add a paragraph reminding the reader of the inherent limitations of our model due to the lack of TEM imaging.

Dr. Bestmann also mentions that CL would help substantiate our conclusions. We agree and we did indeed attempt to study our samples by CL. Unfortunately, the quartz in question is very low temperature and is not particularly luminescent; therefore, our CL detector could not obtain a clear enough CL image. We thus decided not to proceed with more detailed CL investigations. We can mention this in the revised version of the text.



CL image showing and example of the extremely poorly luminescent quartz in domain 4.

Answers to Dr. Bestmann's detailed comments (his comments are in blue, our answer in black):

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

Page 2:

Line 2: give gain size:

Grain size added in the revised version.

Line 13-14: Viscous deformation was initially accommodated by basal 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.
Yes, dislocation creep might also have been active to a certain extent and is visible in the old grain.

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
See comment above.

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 solutionprecipitation
-> Should be checked by CL analyses
Unfortunately the investigated quartz has very low luminescence and nothing conclusive can be taken from a CL investigation.

Line 22: … and later by grain boundary migration
-> See main comment 5.
As stated above, we believe that this is a fair statement based on all the arguments presented, such as the mica grains and the large fluid inclusions displaying the crystal faces of the new grain.

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
The initial orientation of the vein crystals are reflected in the old grain pole figures in fig. 6, 7, 8 and 9. However, no large data set (hundreds of orientations measured) has been generated to evaluate if crystal orientation is truly random, and therefore we will downplay this aspect in the revised version of the manuscript. We wish to point out, however, that there is a limited amount of vein material that we could sample.

Line 25-26: … Crystals oriented optimally for basal slip accommodated strain mainly viscously and experienced only minor fracturing
-> See main comment 1.
This has been dealt with in the revised version of the manuscript.

1. 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)
   We believe it is important to set the scene for the rest of the paper and the regional implications. However, in order to comply with the reviewer´s opinion and to make the paper slimmer and more to the point, we will shorten the introductory sections.

2. 3. Analytic method
   Was the thin section coated? Added to text.
   What acquisition software did you use? Added to text.

   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
   Text changed.

   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.
   A higher resolution photomicrograph has been added in the revised version.

   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
Okey.

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.
See comment above. The text will be modified.

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?
See figure 3b, which will be larger in the final version of the manuscript.

4.2 Microstructural and EBSD analysis Line 17. Because of the marked textural differences
-> Avoid the term "textures"
For the sake of clarity we will keep using crystallographic preferred orientation throughout the text. The text will be modified and we will refrain from using the term texture in this section.

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
The text will be changed to make it clearer.

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 anticlockwise around the Y-axis of the sample reference system.
That is exactly what we mean, that the "new" CPO is more scattered and show counter-clockwise rotation around the Y. Thus, they have a different crystallographic orientation from the host. Text has been changed to make this even clearer.

Line 24-25: contains bands that are sub-parallel to the prism plane of the host old grain (dashed 10 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 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.
See comment above.

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).

The text has been changed and it now contains a reference to a counterclockwise rotation around the Y-axis.

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)
We will look into the possibility to produce such plots.

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
As mentioned above, new misorientation plots have been generated so as to approach the bands at very high angle. The results do not change.

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
Will be moved to discussion.
In the case of Bestmann and Prior (2003), the deformed rock is a calcite marble and the microstructures described belong to a monomineralic mylonite. We wonder therefore, how applicable this is to our case.
Furthermore, we find this continue referring to Bestmann and Prior (2003) unnecessary. The paper by Bestmann and Prior (2003) (which by the way is cited in our work) is certainly relevant and of high scientific standard. However, the interpretations provided there should not be considered as a dogma, and all the procedures adopted by Bestmann and Prior (2003) should not be regarded as the only rigorous EBSD and microstructural work available in the literature. There are many other papers of at least equal quality, which deserve the same consideration.

Having said that, the model of Bestmann and Prior (2003) of SGR followed by grain boundary sliding is an elegant model, which however does not provide clear evidence for the grain boundary sliding process itself (for which, admittedly, it is difficult to find diagnostic microstructures). We believe that the only unequivocal evidence to date of GBS following SGR is provided by Kilian et al. (2011) who clearly showed cavitation in monomineralic recrystallized quartz ribbons. It is worth noting that the model of Kilian et al. (2011) is based on accurate and rigorous SEM and light microscopy observations, no TEM involved… We do not dispute the validity of the Bestmann and Prior (2003) model, but we simply propose an alternative scenario where the high misorientations derive from nucleation and growth from fractured fragments, and we believe that our dataset CAN be interpreted in this way.

-> 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.
We will look into the possibility of adding this to the maps.

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?

A more thorough discussion regarding the sub grain boundaries has been added in the revised version of the manuscript.

No, the new grains are strain free.

4.2.2 Domain 2

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

-> Cut out “nucleated”

“Nucleated” fits well with the proposed model.

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.

The text has been modified in the revised version.

-> 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.

As a general approach, we believe that extrapolation of experimental work to nature has to be assessed by comparing experimental and natural microstructures. Thus, accurate documentation of natural microstructures is essential in order to validate (or not) experimental results. To our knowledge, intracrystalline thin bands of recrystallized grains in quartz (and other minerals) have not been reported from a huge number of natural samples yet and, therefore, documentation is important to lend further support to any experimental model.

Second, we assume that it is well known that experimental rock deformation is based on trading temperature for time. Temperature is increased in the lab to speed up processes. We do not take 1 to 1 the conclusions from Vernooij et al (2006) and Trepmann et al (2007), we rather draw from these papers (as well as from others) to elaborate a conceptual model.

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.

Yes, we do mean strain ellipsoid, as this is standard practice. The foliation is assumed to be the XY plane of the strain ellipsoid. We do not want to enter in a discussion about constraining the shape of strain ellipsoids here. “In stereograms, standard presentation of LPO patterns is with the Y-direction of finite strain vertical and the X- and Z-directions along the EW and NS axes (Fig. 4.41)” (Passchier and Trouw 2005, page 104).

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) –
This has been changed to the reference system of the pole figure.
Agreed, should be counter-clockwise and has been changed.

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
The possible role of crystal plasticity will be expanded upon in the revised version of the manuscript, as explained above.

4.2.4 Domian 4
Line 25: the Z and Y axis of strain
-> See comment p. 13, Line 17-18
This has been changed to the reference system of the pole figure.

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 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
See comment above.

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
The text was amended and we now refer to dislocation creep.

Line 18-19: Domain 1, for example, would have been optimally oriented for viscous slip to be accommodated along either the prism or prism (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 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

See our comment above on page 1.

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 slip. The crystal was, however, misoriented for basal slip.
-> If we assume sigma-1 is vertical than indeed the basal plane is misoriented for easy slip - see main comment 1a 13
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
As mentioned above sigma 1 is sub-horizontal in geographic coordinates, thus forming an angle of about 30-40 degrees to the foliation. We can thus exclude sigma 1 to be in a sub-vertical position.
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
We rely on the microstructures visible under the optical microscope together with our EBSD analysis.

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
CL proved inefficient on the low luminescent quartz.

Line 14-15: The two sets of bands with nucleated newgrains 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 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.
The pole figures were labeled incorrectly, this has been dealt with in the revised version of the manuscript.

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
See answer to main comment on page 1.

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
CL proved inefficient due to low luminescent quartz.
The scattered CPO of the new grains is interpreted as due to rotation of the fracture fragments during fracturing. We agree that SGRR may have been active within the old grain and especially in domain 2. This will be discussed more comprehensively in the text.

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 "subparallel".

This has been dealt with in the revised version of the text.

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

The overall sigma 1 is inferred to be sub-horizontal in the geographic reference system. Local, minor rotations due to strain partitioning may occur.

Page 17
Line 1: that is, optimally oriented for slip along the basal (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
We will look into the possibility of including this in the revised manuscript.

Line 7: As in the case of Domain 1, we interpret these as sealed fractures, with one set oriented subparallel 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
Se answer on similar comments above.

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
This is best illustrated with the optical microscope where there are a series of parallel fluid inclusion planes, but no, to very little new grains. The paragraph has been rewritten in the revised version of the manuscript to make this clear.

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
We rely on microstructures visible under the optical microscope and with EBSD on our interpreteation that strain hardening occurred as a consequence of for example dislocation tangling.

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 axis. The pole of the {m} plane with the a-axis makes an angel of 30° - see also main comment 1a

See answer to exactly the same comment above.

Page 18:

Line 5: optimallyoriented for glide-accommodated creep with slip along the basal
->if we assume sigma-1 is vertical than I do not see a favorable position for glide-accommodated creep with slip along the basal . Instead the basal plane is more or less sub-perpendicular to 15 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
See previous answer on exact same comment.

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.
“Relatively“ high strain zone would be more precise.

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.
Ion-rich, not iron-rich.
See previous comments regarding CL.
Fracturing and fragmentation must have occurred to in order to create the very dense distribution of fluid inclusions and secondary phases, such as calcite and muscovite between the newly formed grains as shown in fig. 5d.

Line 24-27: fragments of the host grain were ripped loose generatingmicro 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
Most strain was accommodated by the matrix, hence the strain partitioning.

Line 27-28: Subsequent growth of the new grains occurred either by precipitation from the actively infiltrating over pressured
-> See main comment 5.
See previous answer to comment.

Page 19:
Line 6-7: where fracturing was minimal due to the favorable orientation of the crystal for basal dislocation glide,
->Only when you assume simple shear – see main comment 1a
See previous answer to comment.

Line 13-23: this is pure speculation – you have to show CL images to prove that.
See previous comments regarding CL.
We will however look into the possibility to add more photomicrographs of microstructures to substantiate our interpretation.

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.
The new grains show perfect crystal faces towards the pores and rounded boundaries towards the old grain.

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
See previous answer to comment.

Line 13-14: …also report partially deformed new grains that show flattening and hence elongation perpendicular to $\sigma 1$.
->You have not defined the orientation of sigma-1 – see man comment 1a Page 21
See previous answer to comment.

Line 14-19: -> see main comment 1a
See previous answer to comment.

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 glideaccommodated dislocation creep.
-> This is pure speculation – you need to do TEM analyses; see main comment 3
No, this is not speculation! As shown by many references, an interconnected phyllonitic matrix is weaker than isolated quartz clasts and will therefore deform much more readily than the quartz clasts under the conditions constrained by other workers within the PIS.

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

The stick-slip behavior is inferred to have taken place during the early stages of fault formation, before phyllonitization and during deformation of a thick dolostone layer. The reviewer is referred to Torgersen and Viola (2014) should he be interested in finding out more on the details of the rheological behavior of thrust faults within the PIS. Having said that, this is a model and should be taken for what is it.

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

As you pointed out above the temperatures of their experiment is much higher due the fact that they are trying to reproduce natural conditions and strain rates in the laboratory. We describe naturally deformed rocks.

Figures: General comments

- you should insert in the figures the sense of shear 17: Done.
- 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.: We believe they will be in the printed version of the manuscript, but we'll look into it.
- Schmid factor maps are to be considered: It will be considered.
- Application of texture component for EBSD maps: Will also be considered.
- Pole figures should have the same reference system as the rose diagrams: They do. We have corrected the inaccurate drawing of a couple of traces in some of 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: Ok.