Response to referee comments RC2 Solid Earth se-2018-81

1) "Rheologically dominant processes" and strain The author set the scope of the manuscript to identify the rheologically governing processes (eg. p1,l3; p3,l13, p11,l29ff). It is noted that monophase layers define the "mylonitic microstructure and clearly correlate with strain". However, it remains unclear 1) how strain is determined (overall in the manuscript when reference is taken to "high strain" or "low strain") and b) which is the rheologically governing process. Dissolution-precipitation creep is a deformation mechanism, granular flow can be a mechanism or a process, quartz layers deforming by dislocation creep are another ingredient to bulk rheology of a rock. Which one out of all of these mechanisms in now dominant, is from my point of view still very open - and for a given mechanism, which process dominates the rheology is also not accessed. For example, does "dissolution-precipitation creep with granular flow" mean - so any interpretation based on those relations are rather speculations or somewhat vague? Similarly, (e.g. p11, l23): "growth parallel to the stretching lineation" , the stretching lineation is finite strain, why would a grain grow towards this direction?

We agree that with our "post-mortem" approach it is difficult to quantitatively judge the role of several fundamental deformation mechanisms and associated processes. The goal of this study is to correlate specific microstructures to different processes to evaluate how the microstructure evolved. From our findings, we discuss some aspects on the rheological behaviour through the deformation history. We will phrase this more carefully when revising the manuscript; specifically we will change the heading of Chapter 5.6.

2) Similarly "sites of shortening" appears multiple times in the text should refer rather to to e.g. contractional quadrant (in relation to prophyroclasts), surfaces at a high angle with respect to the inferred principal shortening direction or similar, but I'd argue a site of shortening is something like a point, and hence it does not make sense to refer to shortening of a point.

We will rephrase "sites of shortening". We used "site" rather in the sense of a volume and were not referring to a point. We usually refer to a crystalline volume close to boundaries of porphyroclasts at high angle to the shortening direction indicated by the plane normal to the foliation, *z*, of the finite strain ellipsoid. We will explain and phrase this now more carefully when revising the manuscript.

*3) Dislocation glide in albite: Bending of kfs is suggested to be mainly due to microfracturing while bending of albite porphyroclasts should primarily relate to dislocation glide. While microfracturing in Kfs might have been identified in the SEM or thinsection (?*both, see Fig. 5*), I do not see on which data, the absence of microfracturing in favour of dislocation glide in albite is based on? How was microfracturing in ab excluded?*

We did not exclude microfracturing of albite. In chapters 4.3 and 5.2 we describe albite porphyroclasts that show characteristically a mixture of fragments (twinned, bent, see Fig. 7 and 8) and strain-free new grains along boundaries perpendicular to the shortening direction of the finite strain ellipsoid, resembling "micro-crush zones" described in Tullis and Yund (1987). Microcracking can produce dislocations but also dislocation glide can cause micro fracturing. Pile up of dislocations during dislocation glide with ineffective dislocation climb (thus ineffective recovery) can cause strain hardening finally leading to brittle fracturing. The relative role of microcracking versus dislocation glide is clearly difficult to assess from natural microstructures. Yet, qualitatively, bent and twinned albite porphyroclasts without any evidence of microcracks on the light-optical and SEM-scales together with the albite replacement in "micro-crush zones" at boundaries at high angle to the shortening direction would indicate that dislocation glide plays a more important role for their formation in comparison to the formation of healed and sealed intragranular microcracks at low angle to the shortening direction of the finite strain ellipsoid visible on both light-optical and SEM scales (as characteristically observed here for K-feldspar, Fig. 5). We will strengthen this point throughout the manuscript. See also comments to referee #1 (points 31 and 34) and point 38 below.

4) Absence of an orientation relation between ab and kfs (p6,l21): The authors present polefigures for three crystal directions (a partial representation of the full crystal orientation e.g.Fig 6e) to discard an orientation relationship between e.g. kfs and albite. However, pole figures are not the suitable object to explore such relationships. Most easily, orientation relations are explored in misorientation space (for example see Krakow et al, 2017). Additionally, as far as I can tell from Fig. 6e, there is quite a lot of coincidence between kfs and ab directions in the pole figures already, so how comes that such a conclusion is drawn?

We agree that full misorientation space would probably be most telling. For triclinic minerals, this is unfortunately not easy to visualize. In the revised Fig. 5 (which the referee is probably referring to), we will present angle and axis distribution separately and also color-code the phase boundaries according to their misorientation angles. Independent on the way to visualize the EBSD data, a systematic crystallographic relationship between the new grains (green) and the original K-feldspar clast (yellow) is not evident.

5) Kfs replacement is independent on specific direction and hence not directly related to strain (p8,l23): How is the rotation of porphyroclasts excluded? I do not see a strong argument here, also no quantitative data to support or reject this claim.

We do not exclude rotation of porphyroclasts, which to some extent appears likely in mylonites. However, we argue that the replacement is not influenced by the orientation of the porphyroclast in the strain field, as the cuspate-phase boundaries to albite occur symmetrically at the boundary of the porphyroclast. If replacement would be significantly influenced by a specific orientation to the strain field, such a symmetric pattern would be difficult to explain by rotation of the porphyroclast with respect to the foliation. Furthermore, the replacements are cut-off by microcracks that occur exclusively at low angle to the shortening direction of the finite strain ellipsoid (Figs. 5; 6). In addition, the elongate shape of the K-feldspar porphyroclasts with the long axis being parallel to the stretching lineation of the finite strain ellipsoid, excludes major rotation of the porphyroclast independently to its surrounding after the formation of these microstructures.

6) Interface-coupled dissolution-precipitation: Conceptually, it has been demonstrated in mostly static environments (see: references in (2) p8,l13ff refer to static features mostly without any deformation involved) and one could argue, that it might be unrelated to strain. However, the opposite argument - because it is apparently independent on (the last state of) strain, it should be icdpc is not tested (see comment on rotating prophyroclasts).

The observed cuspate phase boundary of the K-feldspar to albite indicates replacement by dissolution-precipitation processes at the specific phase boundary, i.e. interface-coupled dissolution-precipitation.

7) "end-member matrix microstructures which correlate with strain" (p1, l18; p7, l13): While the microstructural differences are clearly present, I do not understand where the relation to strain could be established. How was strain measured? How could it be said that one is more strained than the other? Also, do those occur only different samples from different locations - as far as it seems in the way presented here - or could both also be found within the same sample?

The correlation is, that albite layers with elongated grains and SPO occur in samples, where monophase quartz layers are narrow (a few tens of µm) and several mm long, where the quartz aggregate is fine-grained (several µm in diameter). As opposed to the fine-grained (several µm in diameter) monophase albite layers with isometric grains that occur to coarse-grained (several tens of µm in diameter) quartz layers that have width of hundreds of µm. These microstructures are sample specific. The variations in the width of alternating monophase albite and quartz layers in thin section, correlate with mesoscopic observation of hand specimen and in the field by the width and spacing of the foliation planes defined by elongate mineral assemblages and is interpreted to reflect strain. We rephrase this correlation to be interpreted to reflect strain. The distribution of the endmember microstructures will be shown in Fig. 1 and listed in an additional (supplementary?) table (see comments to points 9, 17).

8) A few missing explanations in methods and or /figure captions: - how was grain size established - why was frequency distribution and frequency mean chosen over area weighted mean?

We also checked area weighted, which is now changed in all grain diameter histograms

how was twinning dealt with in ebsd data wrt grain size or other grain related measures

For grain reconstruction a thresholding value of 10° was used. For grain reconstruction, Dauphiné twin boundaries in quartz are neglected. Evaluating albite grain boundaries in full misorientation space (Krakow et al., 2017) revealed that almost all twins correspond to the albite law and some to the pericline law. Also for grain reconstruction of albite, these twins are neglected by merging along twin boundaries. To evaluate also mean grain orientations, requires to use a higher symmetry, which contains the symmetry element responsible for twinning, which is the point group 121 for albite and 622 for quartz. The mean orientation of the "higher symmetry" grain is the modal orientation of the "lower symmetry" grain. Using the higher symmetry yields the same grain reconstruction result as merging along twin boundaries.

why are point plots chosen over properly contoured pole figures. In many cases point plots may not be very useful.

We will use contoured pole figures in the new figures

Misorientation angle profiles: Misor. angle to origin - please specify what is meant with the various occurrences of "relative misorientation (angle or map)", " internal misorientation (angle)"

We mean the angle to a reference point or to mean orientation, respectively, as will be described in the caption

the authors note that orientation contrasts camouflages subtle compositional differences in the BSE images, - just adding that the latter then should be, what is seen in CL - so why are then EBSD polished section used for BSE analysis to begin with, if this is a known problem?

We checked the grey-scale contrast whether it is derived by compositional or orientation contrasts using EDS and EBSD measurements. BSE orientation contrast is present when there is a difference in orientation, irrespective whether Syton-polished or not.

How were apatite needles identified? P signal in EDX? Yes.

We will add the information in the methods-chapter in the revised manuscript.

9) Notes on Figures: Fig. 1: Great to see where samples come from, however out of all of these, only 4 appear in the text. Were the other not suitable or were the selected samples the ones that fit the observation?

All samples were carefully analysed and the systematic and characteristic observations are described. We do not want to present the same characteristic and systematic observations from all analysed samples. We will add a table (see comments to points 7, 17), which gives some more overview on our comprehensive data.

10) Fig. 2: Fractures oriented at small angle to shortening direction - where should that be ? (please indicate shear sense); abbreviation Pl not in the image - see also comments above on fracture orientation

We will add in the figure caption that the fracture is indicated by the white arrow in Fig. 2 b and also changed Pl (plagioclase) to Ab (albite).

11) Fig. 3: Unclear what this figure adds to the overall story of the manuscript. Is it needed?

The figure is intended to give the reader some context on the Alpine metamorphic mineral assemblages. We feel that this information is important even though it does not directly relate to the investigation of feldspar deformation.

12) Fig. 4: Pole plots (d,e,g) cannot should be properly contoured. If the message should be, that they are all different, not to distinguish from uniform etc… a proper contouring is needed as pole plots are hard/not to interpret for this purpose. Why are only pole figures plotted for poles to planes and not for directions? Maybe plotting IPDFs for a reasonable reference direction might be even more telling. "relative misorientation map" -> misorientation angle; also relative to what? An arbitrary reference orientation? Grain size histograms: Why are bins chosen to be so narrow that many of them have populations of just one or two grains? Also, please indicate total number. What is the reasoning for the choice of frequency distribution instead if area fraction?

We will present contoured plots, calculated from the ODF, with texture index and pole figure strength as measured. We also give contour intervals now. ODFs were calculated for the mean orientation of grains. Relative misorientation in the map is towards mean orientation, we mention this now in the text. We revised the histograms and now show area fractions.

13) Fig. 5: (e) Pole figures are not very suitable to establish/discard any orientation relationship between the two phases. Maybe colorcoding the misorientation angles might be more telling - or better, colorcoding either for the full misorientation or e.g. misorientation axis might be more telling.

See point 4): We agree that full misorientation space would probably be most telling. For triclinic minerals this is unfortunately not easy to visualize, so we chose to use to present angle and axis distribution separately and also color-coded the phase boundaries according to their misorientation angles.

14) Fig. 5/6: Could it be that the albite growing into kfs is larger than the matrix albite?

From our observation, both populations have a similar size distribution. However, we feel that there are too few albite grains replacing K-feldspar to make a meaningful analysis.

15) Fi. 7: "bent and kinked" Where do I see the difference?

Indeed, in this image the change in orientation is rather continuous (i.e., "bent"), however, rarely more abrupt changes in orientation occur, which rather resembles "kinking". Yet, because bending is much more common, we will no longer refer to "kinking".

(f) What is the bright phase ? Apatite? Some other Ca-phase? It seems that it grows over the clast-new grains boundary (vertical one at the left side).

The bright phase is zoisite, which grows in the rare cases of plagioclase with An-contents up to 14 %. This information will be added in the caption. The zoisite grain is actually fractured at the boundary between clast and new grain. From the positions of other zoisite grains, it is very likely that the zoisite formed before the new grains.

16) Fig. 8: (a) relative misorientation -> angle ; also relative to what?

Sorry this was mistake, it relative to the mean orientation, this information will be added.

As noted in the text, I do not see the necessity that the core-rim orientation gradient in the fragmented clast should relate to crystal plasticity.

(see comments to point 3 and comments to referee #1, points 31 and 34).

(a)-(b) Why is the choice of grains different. Also, if in (a) only the central big grain is displayed, why does it seem that in (b) several grains occupy the same area?

This is a misunderstanding, the choice of grains is not different. In (a) not only the central big grain is colorized, but every albite grain within a maximum misorientation of 30°. We did this exactly because the porphyroclast was fragmented into several grains. (Although, there is still one central grain, which we also used for the pole plot in (c). We will clarify this in the revised manuscript.

Red lines being low angle boundaries: In (a), they are barely visible, in (b) it looks like they follow direction which could be consistent with the trend of albite twin boundaries - see also the misorientation profile. Also, comparing (a) and (b), again the segmentation seems to be different i.e. in (a) some of the "low angle boundaries" seem to be actually grain boundaries. So maybe something n the segmentation/ handling of twin boundaries went wrong? Please clarify.

Sorry, yes, we made a mistake in labelling, will correct it and will more clearly display the LAGBs (see comment to point 30). The segmentation is now corrected. The general information is not affected by this.

(c) what is the colorcoding of points in the pole plot?

The color-coding is the same as in (a)

(d) a proper contouring might be nicer.

As we only want to show the orientation of the porphyroclast, we do not think contouring would add any information.

(e) Grain size histogram -> see comment on Fig. 4 (g,h)

We revised the histograms and now show area fractions.

please indicate that this is most likely misorientation angle to origin.

We will indicate this.

17) Fig. 9: Do both matrix types also occur in one and the same sample? Here it's FH5 and CT599 which come from different locations. Any systematics about their occurrences?

The microstructure type B is more common than the microstructure type A. The distribution will be shown in Fig. 1 and in an additional table, we will state this information more clearly in the revised text (see comments to points 7, 9).

18) Fig. 10: (d) please provide number of grains, what is contoured (1 point per grain or all points) While contouring is much better than the point plots in Fig.9d, it looks like a broader kernel might be more appropriate. (e) Grain size histogram -> see comment on Fig. 4

We will provide the number of grains and we recalculated the contouring with a more appropriate (and broader) kernel, determined by the cross-validation approach provided by mtex.

19) Fig. 11: (c) So orientation contrast camouflages compositional contrast, so what should be learn from the image? That we can see something in the CL (d) what we might have seen in the BSE if the sample wouldn't have had EBSD-quality polishing?

The BSE signal is showing both, orientation and chemical contrast, independent of Syton-polishing (see also comment to point 7). Orientation contrast does not camouflage compositional contrast inside single grains in this case, but leads to an additional contrast between grains. CL images shows internal structures, which do not cause a strong enough contrast in BSE-images.

20) Fig. 12: Where do color artefacts (center lower part and lower left) in (b) come from?

We think the color artefacts come from a high density of unusual twins (not albite/pericline), which are present in these grains. We could not find these twins at any other occasion.

(c) please use a proper kernel for contouring (e) misorientation angle distribution of "albite" Pole figures of pixels or 1 point-per grain? How many data points? It looks like both, ab and kfs is colorcoded in the ipf map: Is that useful? How should one distinguish both there? "maximum mud …": Maxima of pole figures are often relatively meaningless, especially if a relatively arbitrary kernel seems to be chosen or multiple maxima exist. The 2-norm of the pole figure (sometimes called pfJ) or any other measure that suits the symmetry and application might be better, or any of proper measures for orientation distribution functions.

We will present contoured plots, calculated from the ODF, with texture index and pole figure strength as measured. We also will give contour intervals. ODFs were calculated for the mean orientation of grains. The one Kfs grain will not be colored in IPF-colors any more.

21) Fig. 13: "Preferred growth parallel to stretching lineation" Why would it grow parallel to the finite stretching direction - unless the pure shear p.d. contribution is very large shouldn't it grow parallel to the extending ISA and eventually rotate? All figures, where a shear sense is available but not provided, should have nice arrows indicating the shear sense.

We rephrase that the grains grow parallel to the extensional (or dilatational) direction during deformation, in the finite strain state represented by the stretching lineation (x) (see also comments to points 28, 46). Where a shear sense indicator is present, the shear sense is presented by arrows.

22) General notes on figures: Please make sure the reproduced quality will be better than in the manuscript. I assume that the authors submitted high quality figures - and I am aware of the eagerness of file size reduction at the cost of quality at the side of the Copernicus graphics office/ layout people - so please double check later, that the quality of figures remains very good.

Thank you, we will take care of that.

23) A few more notes: p1, l12: Doesn't kinking and twinnign indicate that glide can't be too effective in accommodating deformation?

Twinning involves glide of dislocations (e.g., Groshong 1988).

24) p1. l21: layers ... parallel to the foliation rather than lineation

We will correct this.

25) p3,l10ff: The last paragraph of the introduction reads like a conclusion, or at least mentions the processes which are later interpreted based on specific microstructures. Is that intentional?

We will rewrite this part.

26) p5. l7: Was ebsd da cleaned of orientation noise? That's usually a good idea before doing KAM/ gKAm

Data was cleaned with a half-quadratic filter before gkam. This will be more comprehensively described in the methods section.

27) p5, l17: sentence

We will change that sentence to "The K-feldspar is Na-poor (<10%) and rarely shows perthitic exsolution." See also our comment to the first referee (point 11.).

28) p5, l30: dilation or extension? (also in other places, please clarify why you think it is dilation and not simply not sites of e.g. lower P)

During dissolution precipitation creep, “sites” at high angle to the extensional direction are “sites” where new material is precipitated for example in veins or strain shadows, causing dilation in this specific direction and represented in the finite strain state by the stretching lineation (e.g., Groshong, 1988; Passchier and Trouw, 2005, Wassmann and Stöckhert, 2013). In this specific sentence, we will refer to “strain shadows” instead of “areas of dilation”.

Groshong, H., 1988. Low-temperature deformation mechanisms and their interpretation. Geol. Soc. Am. Bull. 100, 1329–1360.

Passchier, C.W., Trouw, R.A.J., 2005. Micro-Tectonics. Springer, Heidelberg 159–187.

Wassmann, S., Stöckhert, B., 2013. Rheology of the plate interfaced - dissolution precipitation creep in high pressure metamorphic rocks. Tectonophysics 608, 1-29.

29) p6, l33, p7 1ff: Quantifying lattice bending using a misorientation angle wrt origin as a function of distance is not very satisfying since this may only make sense if it can be reasonably assumed that all misorientation is realised around the same axis and rotations remain so low (or at a given symmetry element) that crystal symmetry does not yet matter.

We give the misorientation angle along a distance in addition to the gKam value, as we find this information more intuitively and indeed it refers to the continuous bending of a crystal that is already visible in polarized light micrograph.

30) p7, l2: LAB parallel to shortening direction: anything quantitative on that?

We will display the LAGB in Fig. 8 (see comment point 16).

Also, where is the shortening direction?

We indicate in all Figures the shortening and stretching directions (z and x) of the finite strain ellipsoid inferred from normal to the foliation and the stretching lineation on sample scale.

31) p7, l22: What are (monophase) layers composed of aggregates.

Monophase means just one mineral phase, aggregate means it is composed of different grains (of the same phase, but this is already included in the term “grain”)

32) p7, l29: How were traces of planes related to real 3d boundary planes?

We will make clear, that these straight segments can be parallel to traces of (001) and (010) cleavage planes.

33) p8, l2: i.. not show an internal orientation contrast ...

Yes, internal, we agree.

34) p9,l11-15 (but also elsewhere): Observations and interpretations of the authors are mixed with references to the literature in a way making it hard to figure what information is claimed by the authors and what comes from literature. These sections can benefit from a more clear separation of citation and authors interpretation.

We will carefully revise the text accordingly.

35) p9. l21: influence of water on diffusion e.g. R&D2004: this most likely relates to gbtransport phenomena, at least it was never demonstrated that it is intracrystalline diffusion, hence it's a bit of a brave jump to speculate on climb enhanced by the presence of fluid - or the absence

This is a misunderstanding, we did not mean to speculate on the enhancement of climb by the presence of fluid. We referred to findings from the literature and will transfer a few of these aspects into the introduction (see point below).

36) p9,l15-26: this is a collection of citations in relation to the inability of dislocation climb and the sluggishness of diffusion in the absence of a hydrous fluid. However, this section might be better placed into the introduction.

We agree and will place a few of these aspects into the introduction.

37) p9,l30: "as opposed to solid state grains boundary migration": please explain/clarify; there needs to be transport across the boundary in each case

Our point was to stress the role of dissolution-precipitation as opposed to for example climb-involved subgrain rotation recrystallization (Drury and Urai, 1990; Schenk and Urai, 2005; Stipp and Kunze, 2008). We will clarify this in the revised manuscript.

38) p10. l1ff(and earlier): While all reasonable in very general terms and something one would expect for such a rock, here a few ingredients to the interpretation are somewhat speculative: a) glide and b) strain induced gbm are not demonstrated. While both may be likely, here it remains a speculation since it is not backed by any (semi) quantitative data

Here, we disagree, the indication of dislocation glide is not speculative (see comments to referee #1 (points 31 and 34 and point 3 above). Dislocation glide is demonstrated, e.g., by twinning (which involves glide of dislocations, e.g., Groshong 1988, see point 23) and the continuous bending of the crystal lattice. Even the formation of "micro-crush zones" sensu Tullis and Yund (1987) involves dislocation glide. It would be much more speculative to argue that continuous bending of a crystal is purely brittle, especially without any evidence of fracturing on SEM and polarized-light microscopic scales. We will discuss this in some detail in Chapter 5.2. Strain-induced grain boundary migration is likely by the presence of new grains that are basically strain-free and not represent fragments of the original clasts, we will further strengthen this important point when revising the manuscript. However, also overgrowth of grains by precipitation parallel to the extensional direction during deformation will be important in addition to strain-induced grain-boundary migration.

39) p10. l8: reaction of fracture to crystal directions: a) How was this investigated? and B) is there any data on that?

We will phrase this more carefully when revising the manuscript. For K-feldspar, we investigated this by comparing the orientation of microcracks to EBSD data. See, for example Fig. 5. Fractures in K-feldspar are clearly related to the axes of the finite strain ellipsoid and not to the crystallography of the crystal. For fragmentation of plagioclase in the "micro-crush" zones, however, cleavage fractures might indeed play a role.

40) p10, l19: Dilation: Please explain, it this true dilation or low P sites or surfaces near orthogonal to extensional directions?

See comment to comment to point 28.

41) p11,l8: albite aggregates instead of albite taking up some deformation

The aggregates formed from strained albite porphyroclasts…

42) p11, l15: grain boundary sliding: while one can see a few straight boundaries in Fig. 13, a) why should they indicate gbs b) how frequent are those compared to others ? Anything more convincing on gbs?

We indeed do not have further microstructural evidence on granular flow except of the fine-grained albite layers deflected around porphyroclasts with minor straight boundaries but mostly lobate boundaries (Fig. 13). We will delete "sliding of grains relative to each other" in that sentence and leave it to "granular flow (e.g. Behrmann and Mainprice, 1987; Stünitz and Fitz Gerald, 1993; Jiang et al., 2000) probably has played a role in the formation of the fine-grained albite matrix."

43) p11, l19: Hildyard needs year

We will add it (Hildyard et al., 2011).

44) p11, l25: "Microstructure correlates with strain": again, where does strain come from? How does such a "correlation" manifest? Simply elongated vs more equiaxed grains?

Please see comment to point 7).

45) p11, l25: "The higher ..." Sentence

We will correct the sentence.

46) p11. l27: growth parallel to the stretching lineation: While this does not make a lot of sense for non-coaxial p.d. (see comment 2), why preferred growth? Preferred by what? Crystallography? Where should the "dilation" come from? Anything tested on that? What is the CPO of the most elongated grains, or which crystal direction is parallel to the maximum grain elongation direction?

The observation is: The long axes of grains is parallel to the stretching lineation of the finite strain ellipsoid. There is no preferred crystallographic orientation of grains with high aspect ratio (see also comments to points 21 and 28). We will rephrase the sentence on p11., l. 27 to: "Therefore, we suggest that the albite grains preferentially grew in the direction parallel to the extensional direction during deformation, resulting in a higher aspect ratio of grains with the long axis of grains parallel to the stretching lineation of the finite strain ellipsoid."

47) Entire section 5.6 does not allow me to understand which by now is the process that dominates rheology.

We will change the caption heading, see comment to point 1.

48) p12, l1: "granular flow" (here and elsewhere) please define your understanding of granular flow within the context of a mylonite. Or do you refer to grain boundary sliding in the sense of Rachinger sliding?

Please see comments to point 42).

49) p12, l7: why probably?

We agree and omit "probably", as this is indeed too speculative.

50) p12. l7: Please enlighten (probably not in the conclusion) why the lobate boundaries between newly grown albite and kfs should be chemical disequilibrium and not due to other driving forces, i.e. gb-width, porosity variations in kfs, defect densities etc.?

We agree that not only chemical disequilibrium but also other factors do play a role for the replacement and will discuss this more comprehensively in the section 5.1. However, as albite does replace K-feldspar, the chemical driving force is an important factor.

51) p12, l19: Why would glide drive gs-reduction in this combined mixture of mechanisms?

As discussed in section 5.2, dislocation glide in association with microfractures in the sense of low-T plasticity is causing a reduction in grain size similar to the "micro-crush" zone in Tullis and Yund, 1987 (see comments to points 3, 38).

52) p12,l20: "observed tendency of slightly enriched Na-content…" Any data on that?

We mention the range of compositions of both the porphyroclast and new grains (p5,18; p6,l29-30). They overlap with a slight tendency for new grains to be more Na-rich.

53) p12,l22: Why subordinate? The balance between chemical driving force vs. e.g. strain energy depends on a lot of variables. For some variables we might have good estimates while for others, we are simply guessing, i.e. dislocation density and elastic energy added by dislocations during deformation etc.

It is true that many variables are not known and we do not try to ignore this problem. Estimates on the influence of chemical driving forces, consider the chemical differences that we observe as too small to play a significant role (e.g., Stünitz, 1998). We discuss this in Chapter 5.1.