

Interactive comment on “Deformation of feldspar at greenschist facies conditions – the record of mylonitic pegmatites from the Pfunderer Mountains, Eastern Alps” by Felix Hentschel et al.

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Review Solid Earth se-2018-81 / Hentschel et al.

The manuscript by Hentschel and co-workers reports on microstructures in deformed pegmatites in the Eastern Alps. The intention is to find microstructures which can be related to the active deformation processes and mechanisms in those feldspar dominated rocks. Since these rocks seem to be deformed under greenschist facies conditions, crystal plastic processes (dislocation glide and climb) are expected to be of minor importance and fracturing and dissolution-precipitation and reaction-related processes are those which are usually to be encountered. Since feldspar is an important

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mineral but with regard to many processes still quite poorly understood, it is a useful study to gain more insights on processes involved in the deformation behaviour of this group of minerals. The data of the manuscript is overall good, the manuscript is generally and in most parts understandably written and in many parts sufficiently concise. The argumentation and discussion is mostly free of contradictions but in certain parts, clarifications and corrections are required. Comparing the presented data with the interpretations done by the authors, there is room for improvement as some - which will be outline further below - interpretations are rather of speculative nature (although, most likely what one would expect in such a rock and hence in general not surprising). A few concepts presented need to be clarified as in the current way, they might/can not be correct. A few parts seem somewhat lengthy and could benefit from streamlining while e.g. the methods section clearly requires an extended explanation with respect to some of the data shown. Also, the discussion and associated interpretations might benefit from a clearer separation of interpretations based on data and speculations (in part lifted from the literature).

I suggest this manuscript is worth publishing but would strongly benefit from a number of (smaller) enhancements and corrections, which in total suggest major revisions.

1) “Rheologically dominant processes” and strain The author set the scope of the manuscript to identify the rheologically governing processes (eg. p1,13; p3,113, p11,129ff). 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 is 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

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it is grain boundary sliding in the sense of Rachinger sliding accommodated by dpc, or dpc accommodated by Lifshitz sliding - and in any of those mechanisms, which is the rate limiting, and hence rheology controlling process? Any answer to this sort of question is what one would expect under a section "Rheologically dominant processes". Maybe the authors meant to actually focus their interpretation on which mechanisms are most likely dominating the formation of a dynamic microstructure? How this relates to bulk rheology within a polyphase rock, evolving microstructurally and mineralogically with strain is then a somewhat different question.

An outcome of the authors interpretation - monophase layers apparently appear most highly deformed - is really interesting and might deserve some discussion. Usually it can be observed that phase mixing of materials undergoing any sort of interface-involved deformation process results in weakening or strain localisation. Why does this rock behave differently?

2) Please clarify the concept of strain (and related terms) in relation to microstructural observations. "Fractures formed at a low angle to the shortening direction" (p12,115) and "main shortening direction indicated by the foliation" (p1,6) need clarification. If bulk shortening direction refers to finite strain - as suggested by the reference to the foliation -, it needs an explanation, since this is not what one would expect. Why would fractures form or why would clasts (prone to rotation) containing fractures formed in relation to finite strain? Fractures usually form with some relation to the stress field, i.e. relate to instantaneous strain. If bulk shortening direction refers to the instantaneous shortening direction - which would be physically reasonable (and fractured grains might still be prone to later rotation) - they'd be unrelated to foliation, which seems to be a finite strain feature. Unless the rocks deformed by coaxial progressive deformation (which seems not the case) - a foliation defined by the xy-plane of finite strain is unrelated to the ISA and hence principal stress directions. So, a) the relation of fractures to fabric elements and those relations to finite strain or kinematic directions need clarification and b) I did not find any data actually on the distribution on the trends of fractures

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- 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?

Similarly "sites of shortening" appears multiple times in the text should refer rather to e.g. contractional quadrant (in relation to porphyroclasts), 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.

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 (?), 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?

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?

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.

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 unre-

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lated 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).

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?

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? - how was twinning dealt with in ebsd data wrt grain size or other grain related measures - why are point plots chosen over properly contoured pole figures. In many cases point plots may not be very useful. - 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)” - 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? - How were apatite needles identified? P signal in EDX?

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?

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

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

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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?

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.

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

Fi. 7: “bent and kinked” Where do I see the difference? (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).

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

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colorcoding of points in the pole plot? (d) a proper contouring might be nicer. (e) Grain size histogram -> see comment on Fig. 4 (g,h) please indicate that this is most likely misorientation angle to origin 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?

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

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?

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

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 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.

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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.

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

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

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?

p5, l7: Was ebsd data cleaned of orientation noise? That's usually a good idea before doing KAM/ gKAM

p5, l17: sentence

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)

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.

p7, l2: LAB parallel to shortening direction: anything quantitative on that? Also, where is the shortening direction?

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

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

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

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.

p9. l21: influence of water on diffusion e.g. R&D2004: this most likely relates to gb-transport 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

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.

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

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

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

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

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

p11, l15: grain boundary sliding: while one can see a few straight boundaries in Fig.

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13, a) why should they indicate gbs b) how frequent are those compared to others ?
Anything more convincing on gbs?

p11, l19: Hildyard needs year

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

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

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 to the maximum grain elongation direction?

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

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

p12, l7: why probably?

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.?

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

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

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 es-

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timates while for others, we are simply guessing, i.e. dislocation density and elastic energy added by dislocations during deformation etc.

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Please also note the supplement to this comment:

<https://www.solid-earth-discuss.net/se-2018-81/se-2018-81-RC2-supplement.pdf>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2018-81>, 2018.

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