

Dear Mr. Massonne!

Thank you very much for the encouraging review. Your critical comments contribute to the quality and clarity of the paper, and are acknowledged accordingly.

In the following pages you will find your comments given in black color. My response to every comment is given in blue color. The text of the paper is given in two columns, italic typeface and blue color. The original text is on the left column, the modified one on the right.

I hope, I have responded satisfactorily to your critical comments.

With best regards

K. Petrakakis

Review of the manuscript "Ca-rich garnets and associated symplectites in mafic peraluminous granulites from the Gföhl Nappe System, Austria" by K. Petrakakis et al., submitted to Solid Earth

General comments: The authors present a petrological study on a mafic granulite from the southeastern Bohemian Massif. The study of this rock is very detailed as the authors considered also, for example, the various symplectites in the granulite and the application of modern thermodynamic modeling techniques. Because of that the derived exhumation path in terms of pressure-temperature conditions is well documented. Thus, previous works proposing high peak-temperatures in the range around 1000°C for the granulites of the Gföhl unit seem to have overestimated these temperatures.

In summary, I would like to see the manuscript published soon after minor revisions.

Specific comments:

page 3, line 12: batholith?

No, it is wrong; replaced with "Pluton", thank you.

page 5, lines 6-7: "but at much lower pressures of >4.5 kbar" - I am not sure which pressure range is addressed by the authors. My suggestion: "but at pressures between 4.5 and 6.5 kbar".

The original formulation is indeed not clear, thank you. It is replaced as shown below.

The Ostrong Nappe System differs distinctly from the other Moldanubian nappe systems. It shows similar anatexis temperatures of c. 720 °C, but at much lower pressures of >4.5 kbar. Based on rare relics of kyanite and staurolite, the rocks had attained a prograde maximum at <600 °C and c. 6 kbar before anatexis (Linner, 1996).

The Ostrong Nappe System is similarly anatexis, but differs distinctly in terms of pressure from the other Moldanubian nappe systems (Linner, 1996). Based on rare relics of kyanite and staurolite, the rocks had attained a prograde maximum at temperatures <600 °C and pressures of ~6 kbar. Subsequent anatexis took place at ~720°C and pressures not less than 4.5 kbar.

page 13, line 10: kelyphitized?

My wrong formulation! Sentence replaced as shown below.

It is noteworthy that the whole garnet is kelyphitized along the rim (cf. Fig. 2g) and,

It is noteworthy that the garnet is replaced by kelyphite along its rim (cf. Fig. 2g) and,

page 15, line 20: "GRT-type extends" - which garnet type?

My sloppiness! Reformulated and (hopefully) better explained as shown below.

Modal analysis of the phases in crack-symplectites (Fig. 4d, Fig. 5e) combined with their chemical composition showed that the bulk symplectite composition is almost isochemical with the GRT-type C. GRT-type extends beyond the diffusion profile shown in Fig. 5e acquired over a crack-symplectite. An analysis practically identical with GRT-type C and lying on this profile is #974 (Supplement, Table S3).

Garnet profiles acquired over crack-symplectites (e.g. Fig. 5e) show that GRT-type C extends beyond the diffusion-affected part of the garnet. An analysis practically identical with GRT-type C and lying on this profile is #974 (Supplement, Table S3). Modal analysis of the phases in crack-symplectites (Fig. 4d, Fig. 5e) combined with their chemical composition obtained by microprobe analysis showed that the bulk symplectite composition is almost isochemical with the GRT-type C.

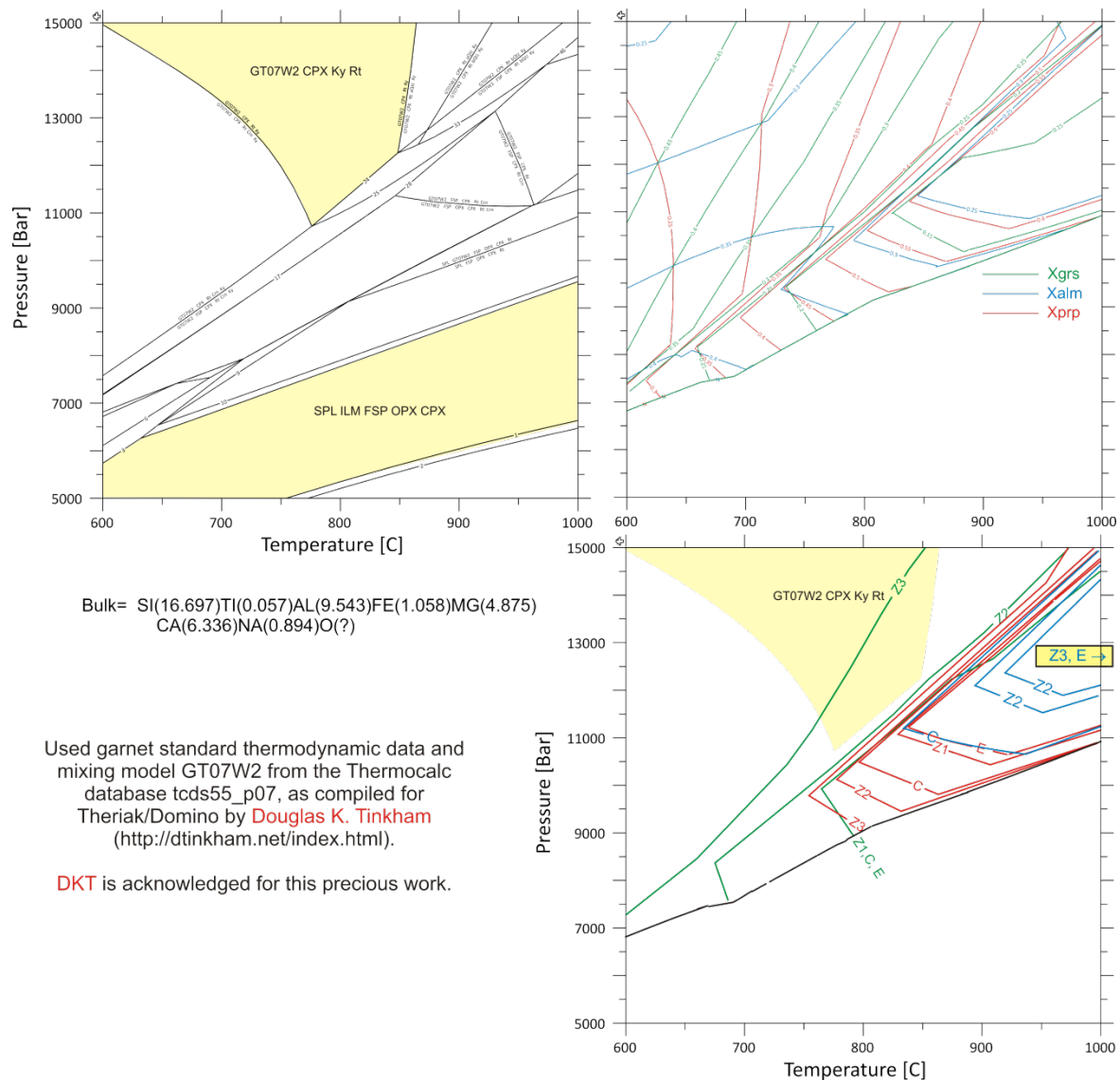
page 18, lines 13-14: "does a common intersection point ... exists, indication that none of the GRT-types corresponds to a preserved equilibrium state" - I agree that this statement is likely, but not compelling. But other reasons are also possible and should be mentioned here. Perhaps the selected solid-solution models (here especially garnet) could be not fully adequate. The authors did not test the selected models and did not achieve their pseudosection calculations with alternative solid-solution models. A further reason for the missing fit of the isopleths could be the used bulk-rock composition although I also think that it is very likely that this composition has not changed during metamorphism.

I fully agree with you that the absence of common intersection points of the garnet isopleths may be an artefact of the standard thermodynamic data and / or mixing models used. Let me follow your suggestion and focus on garnet.

The following figure is a re-calculation of the paper diagrams (Fig. 10) based on the widely used garnet data in the Thermocalc dataset `tcds55_p07`.

The two target assemblages GRT+CPX+Ky (primary assemblage) and CPX+OXP+SPL+PL (in crack-symplectites) are well reproduced (cf. Fig. 10). Expectedly, the calculated isopleths are somehow different. Nevertheless, the measured isopleths lack again any common intersection points for all GRT-types.

Now, it can be argued that also this garnet model is not fully adequate. This might be the case. I think that every model has its own problems. But, on the other hand, the usefulness and reliability of a model can be tested by its ability to reproduce the observed assemblages and microstructures, and the measured compositions of the minerals (not necessarily by calculated P and T that may be suitable or non-suitable to personal prepossessions). With the applied thermodynamic, the outcomes of the submitted paper model (cf. Fig. 10) are based not on thermo-barometric determinations of P and T, which imply equilibrium compositions of the involved phases, but on the reproduction of the target assemblages and, in case of the crack-symplectites, mineral compositions (Fig. 11 and Table 3). This is discussed also on page 20, lines 29- 34. The composition of CPX in the primary assemblage cannot be reproduced, because it has been reset. Such a resetting of CPX is frequently observed. Similarly, the garnet composition cannot be fully reproduced, because it has been modified, presumably by intra-crystalline diffusion and metasomatic alteration. Only GRT-type C shows a composition closest to an equilibrium composition pertaining to the low-pressure part of the stability field of the primary assemblage.



So, is the formulation in submitted paper justified? Taking account of your critical comment, I changed the formulation as follows.

Based on this figure [Fig. 10b] , Fig. 10c shows the isopleths distribution for the observed GRT-types. For none of the GRT-types does a common intersection point of the isopleths for the measured Xalm, Xprp and Xgrs exists, indicating that none of the GRT-types corresponds to a preserved equilibrium state.

Based on this figure [Fig. 10b] , Fig. 10c shows the isopleths distribution for the observed GRT-types. According to the applied thermodynamic model, for none of the GRT-types does a common intersection point of the isopleths for the measured Xalm, Xprp and Xgrs exist. We consider this as an indication of perturbation of garnet composition from equilibrium.

page 18, line 19: "some prograde" - better: "a prograde"

Yes, it is better. Replaced as suggested.

page 20, line 9: "+PL, seed more" - probably: "+PL shed more"

Yes, it is better, thank you. Replaced as suggested.

page 22, line 5: "of reaction" - rather "of reactions"

Yes, it is better, thank you. Replaced as suggested.

page 22, line 16: something is missing at the end of the line - my suggestion "at hand, these conditions are characterized by"

Thank you. Replaced with minor modification of your suggestion as shown below.

Symplectite formation is initiated by a change in the environmental conditions of the rock. In case of the sample at hand, by decompression at a more or less constant high temperature.

Symplectite formation is initiated by a change in the environmental conditions of the rock. In case of the sample at hand, this change is characterized by decompression at a more or less constant high temperature.

page 23, line 4: "they allow for the calculation" - better: "they allow us to calculate"

Replaced as suggested.

page 23, line 6: "It is often observed" - better: "It is frequently suggested"

Replaced with minor modification of your suggestion as shown.

It is often observed that the characteristic inter-granular spacing (and size) of the symplectite phases decreases with decreasing temperature of symplectite formation. Remmert et al. (2018) presented experimental evidence corroborating this view and argued that the characteristic spacing of symplectite phases is small at low temperatures,

It is suggested that the frequently observed change of characteristic inter-granular spacing (and size) of the symplectite phases correlates positively with the temperature of symplectite formation. Remmert et al. (2018) presented experimental evidence corroborating this view and argued that the characteristic spacing of symplectite phases is small at low temperatures,

page 23, line 12: Replace "earlier" by "above"

Replaced as suggested.

page 23, line 15: "in ultramafic rocks have" - "in ultramafic rocks that have"

Missing word inserted as suggested.

page 23, line 23: replace "composition" by "compositional"

Replaced as suggested.

page 23, line 27: "the Mn" - "the entire Mn"

Word "entire" inserted as suggested.

page 24, line 4: "A limited fluid availability" - "A limited availability"

Word "fluid" deleted as suggested.

page 24, line 6: "zonation" might be better than "structure"

Well, it is a matter of taste. I think, for most of the people, "zoning" or "zonation" is intuitively associated with "co-centric" element distribution patterns, for example, as those related to growth zoning. As this is not the case in the measured garnets and in order to avoid possible misunderstandings, I have chosen the phrase: "The complex compositional structure of the garnet".

page 24, line 9: "conditions." - "conditions using pseudosections" or do the authors mean "thermo-barometric estimate"? If yes, I would not agree with as the size of the lamellae delivered (rough) temperature constraints.

Well, I don't quite understand the point here, especially in respect to "as the size of the lamellae delivered (rough) temperature constraints". I may guess, your comment may be related with the discussion above. Therefore, the formulation (page 24, lines 7-11) is slightly changed as follows.

Additionally and as shown by the measured isopleths, the composition of all garnet types deviates from equilibrium, precluding thus any reliable thermo-barometric estimate of their formation conditions. The "older" GRT-type C occupies the large interior part of the garnet; its composition shows the least deviation from equilibrium, corresponding closest to the stability field of the primary assemblage GRT+CPX+Ky

Additionally and as shown by the measured isopleths, the composition of all garnet types seems to deviate from equilibrium. The "older" GRT-type C occupies the large interior part of the garnet; its composition shows the least deviation from equilibrium, corresponding closest to PT conditions pertaining to the stability field of the primary assemblage GRT+CPX+Ky.

page 24, line 10: "stability field" - better "P-T field" as the stability of the mentioned assemblage is more extended.

Right, thank you! The necessary change in line 10

..., corresponding closest to PT-conditions pertaining to the stability field of the primary assemblage GRT+CPX+Ky.

is already included in the previous reformulation.

page 24, line 12: "It is developed" - better: "It formed"

Replaced as suggested.

page 24, lines 18-20: Seems to me somewhat speculative. This should be mentioned.

We are trying here to give an answer to the question, when did the metasomatic alteration take place along the PT-path of the rock. We argue as following.

The "younger" GRT-type Z3 (cf. Ca-distribution map, Fig. 2g) might have been evolved from the "older" GRT-type C by removal of Fe+Mg. This process should have taken place before symplectite formation, because diffusion profiles have overprinted this GRT-type (Fig. 5). Additionally, the "youngest" GRT-type Z1 (replacing GRT-type Z3, see the Ca-distribution map, Fig. 2g) shows intracrystalline deformation that, as explained further below in the text, is related to the tectonically induced decompression. This argument is added in the new formulation below. So, the answer to the question may be the following: The metasomatic modification should have taken place before decompression, i.e. under conditions most probably close to those of the primary assemblage. All these features comprise a line of argumentation or suggestion (or speculation?) with emphasis given by the used formulations "may evolve", "we may conclude", "may have taken place". Taking account of your critical comment, the new formulation (of course without underlining) is as follows.

All these features provide convincing evidence that the garnet shown in Fig. 2g has undergone diffusion-aided metasomatic modification during the late stages of its evolution represented best by the GRT-types Z3 and Z1. Accordingly, the "younger" GRT-type Z3 may evolve from the "older" GRT-type C by removal of a total amount of ~12 mol-% of Fe + Mg (Table 1; Fig. 5a). Recalling that GRT-type Z3 also predates symplectite formation, we may conclude that the metasomatic alteration that formed GRT-type Z3 along the lower garnet margins in Fig. 2g took place under PT conditions not substantially different from those of the primary assemblage.

All these features provide convincing evidence that the garnet shown in Fig. 2g has undergone diffusion-aided metasomatic modification during the late stages of its evolution represented best by GRT-types Z3 and Z1. Accordingly, the "younger" GRT-type Z3 might have been evolved from the "older" GRT-type C by removal of a total amount of ~12 mol-% of Fe + Mg (Table 1; Fig. 5a). Recalling that GRT-type Z3 predates symplectite formation and that the GRT-type Z1 shows intracrystalline deformation (Fig. 7) due to tectonic displacement (see below), we may conclude that the metasomatic alteration that formed GRT-type Z3 and Z1 may have taken place under PT conditions close to the formation conditions of the primary assemblage.

page 24, line 26: "is more than questionable" - "is questionable"

Changed as suggested.

page 25, line 9: "pre-date formation" - "pre-date the formation"

Changed as suggested.

page 25, line 10: "to activity" - "to the activity"

Changed as suggested.

page 25, line 13: "in depth" - "in detail"

Changed as suggested.

page 25, lines 15 and 31: "related with" - "related to"

Both changed as suggested.

page 26, line 16: "rather, than with increasingly" - "rather than increasingly"

Changed as suggested.

Hans-Joachim Massonne (Universitaet Stuttgart)

Dear Mr. Racek!

It is an honor for me and all co-authors that you, a “Moldanubian specialist”, have evaluated positively the submitted paper. Of special value are your critical comments, the exact reading and inspection of the figures. You have contributed to the clarity and value of the paper and this is acknowledged by us accordingly.

In the following pages you will find your comments given in black color. My response to every comment is given in blue color. The text of the paper is given in two columns, italic typeface and blue color. The original text is on the left column, the modified one on the right.

I hope, I have responded satisfactorily to your critical comments.

With best regards

K. Petrakakis

The referee comment to the manuscript "Ca-rich garnets and associated symplectites in mafic peraluminous granulites from the Gföhl Nappe System, Austria" by authors Konstantin Petrakakis et al.

General Comments:

The reviewed manuscript "Ca-rich garnets and associated symplectites in mafic peraluminous granulites from the Gföhl Nappe System, Austria" by authors Konstantin Petrakakis et al. represents a very detailed and focussed study of processes recorded in mafic granulites from Bohemian Massif mainly as a complex zoning of garnets.

The work is of a particular interest from both the more regional point of view, but mainly for a broader audience due to its quite unconventional approach, which seems to be quite appropriate for a study of such peculiar lithologies. The manuscript fits well within the scope of SE, it contains large set of new data and includes novel approach for estimates on metamorphic history of high-grade rocks. The conclusions are reached by relevant and clearly outlined methods and are fully justified. The methods are described and explained in detail allowing anybody to reproduce them. The substantial part of the manuscript is new authors contribution, while any references to previous works of other authors are properly cited. The title is relevant to the manuscript content, abstract summarizes the most important information reached by the work. The presentation is generally well structured, except of some minor flaws in the descriptive part making it a bit hard to follow (see specific comments). The language is fluent without any obvious mistakes (as far as I can recognize not being native speaker). Symbols and abbreviations are properly defined and used, references and supplementary material are appropriate. There are only several rather minor issues mostly of formal character, where I would recommend to make some changes in structure of some figures and text - mainly descriptive part. Also, I have few comments regarding the section about thermodynamical modelling - see specific comments.

Overall, it can be summarized as follows - scientific significance - excellent, scientific quality - excellent, presentation quality - good to excellent.

In conclusion, I recommend the manuscript to be accepted after rather minor revisions.

Specific Comments

Page 3 - lines 4 - 5: it is stated that "three lithotectonic nappe systems are generally dipping to the east" - however I have impression that the general structure of the eastern margin of the Moldanubian zone is gently west-dipping.

Considering Fig. 1 in your excellent work on the Drosendoerf Window (Racek et al. 2006), I fully understand this remark. However, "Generally dipping to the east" is meant as follows. In the Austrian part of Moldanubia the "traditional" succession of lithotectonic units from west to east is the following (works of Fuchs, Matura etc.). Ostrong Unit (tectonically lowest) / Drosendorf Unit (tectonically intermediate) / Gföhl Unit (tectonically highest). This implies that the general / regional dip is to the east (according to these authors related to the so called "Intramoldanubischer Deckenbau"). This general relation may have been modified on a local scale during the multi-stage tectonic evolution of

Moldanubia, especially during the lateral spreading of the units. As stated in Racek et al. (2006), (p. 228 and caption Fig. 9) the westward dip in the Drosendorf Window is the result of reworking of the earlier subvertical S2 during the latest deformation D3. D3 has been induced by the indentation of the Brunia basement.

Page 6 - line 11 (and elsewhere) - here stated "as will be discussed later, its origin is secondary". In general, I would prefer that the description and interpretations of the features would be more separated - first the description, interpretation later. This is not the case of this manuscript and in some cases (as this), the interpretative statements are incorporated in the descriptive text without some supportive argument.

I see the point and I have reduced such "occurrences" in case this reduction was appropriate. In some cases, it was not possible. For example, in the section Mineral Chemistry. There, the relative age relations between the garnets types are derived first. What follows in this section is about the pronounced diffusion profiles that overprint the garnet types. Implicit with this overprinting is the relative age of symplectite formation, if and only if the link between diffusion profile and symplectite formation is established. Let's see the text there, page 12, line 11

Such compositional zoning profiles within the reactive and retreating garnet edge are imposed over pre-existing garnet compositional patterns and are, therefore, secondary. As will be discussed later, their evolution is linked to the formation of the symplectites.

Further below on page 13, line 7

As such overprinting relations have not been observed in case of the younger GRT-type Z1, we conclude that symplectite formation is at earliest coeval to this garnet type and consequently younger than the internal compositional structure of the garnet related to GRT-types Z2, Z3 and C.

Without the necessary link, a large part of this section should be moved to section "Discussion". Similarly any other occurrence of such diffusion profiles and the resulting age relations, for example the garnets with subgrains and diffusion profiles, or the poikiloblastic garnet with diffusion profiles etc. should be moved accordingly. In my opinion, this would be inappropriate.

Page 7 - Figure 2: The Figure 2G seems not to be on appropriate place. It is not cited between Fig 2F and Fig 3. Since it is a figure showing already features of mineral chemistry, the reader does not have information to understand all the indicated garnet types etc. that are shown in this figure. I think it should be somehow involved in Fig 5.

I fully understand this remark. As the number of figures / templates is rather large, it took me a lot of time to arrange them in a way that saves space. But, as you say, the result is not the best one. I think, as Fig. 5 contains too many profiles, Fig. 2g cannot be incorporated there. I changed the arrangement of the figure and the numbering by subtracting Fig 2g from Fig. 2, "making" it Fig. 6 and re-numbering the subsequent figures. It sounds easy, but resizing Fig. 2 and adding Fig. 6 resulted in a new pagination of the paper by LaTeX. Therefore, a certain text part originally on page X, line y may now be displaced to

page A, line b. Therefore, your suggested text changes are discussed below with reference to the old and the new pagination.

Page 11 - Figure 5C: The diffusion profile X-Y is asymmetrical. While the left (X) part has features described in the text (decrease of Ca etc.), the right side (Y) is missing them and in fact, the trends of the zoning resemble those observed by the profiles E-F, Q-S, and T-U (although the zoning is much less pronounced). Maybe this could be discussed in the text?

Strictly geometrically, yes, the right part (Y) is not a mirror image of the left part (X), because 3 analyses at the beginning of part Y have been rejected. The reason was that their quality was not OK due to bad local polishing at the garnet/symplectite interface. Should I have discussed this? Nevertheless, I think, the key point is that the component trends are “symmetrical” and, additionally, similar with those in profile P1-P2. Profiles X-Y and P1-P2 are crossing inclusion-related symplectites. I don’t agree that they resemble profiles E-F, Q-S, T-U. Therefore, all these different profiles are put in Fig. 5, else they would be redundant. The differences between these profiles were described on page 13, lines 23 – 30. As I have used the formulations “the former” and the “latter” by referring to two figures too often, I changed the text accordingly to make it (hopefully) clearer. Please, see the changes in the original text marked in yellow color below.

Page 13, lines 23 - 30

The garnet profiles shown in Figs. 5c,e are acquired over domains of inclusion-related and crack-symplectites, respectively. The former shows a diffusion profile within about 10 μm towards the garnet–symplectite interface characterized by increasing X_{prp} and X_{alm} , and sharply decreasing X_{grs} . The latter diffusion profile is characterized by unchanged X_{grs} , increasing X_{alm} and decreasing X_{prp} . This striking difference reflects primarily the influence of the local environment on their formation mechanism. In the former case, the environment is defined by GRT-type Z2 reacting with its kyanite inclusions. In the latter case, it is defined by the instability of GRT-type C alone. Diffusion profiles at rim- symplectites of GRT-type C as the one in Fig. 5d are similar to those of crack-symplectites. Occasionally and as shown in Fig. 5d, the diffusion curves for Mg, Fe, Mn adjacent to rim symplectites show an inflection point at some short distance before the retreating garnet edge.

Page 14, line 1 –9

The garnet profiles shown in Fig. 5c and Fig. 5e are acquired over domains of inclusion-related and crack-symplectites, respectively. Fig. 5c shows a diffusion profile within about 10 μm towards the garnet–symplectite interface characterized by increasing X_{prp} and X_{alm} , and sharply decreasing X_{grs} . In Fig. 5e, the diffusion profile is characterized by unchanged X_{grs} , increasing X_{alm} and decreasing X_{prp} . This striking difference reflects primarily the influence of the local environment on their formation mechanism. In case of Fig. 5c, the environment is defined by GRT-type Z2 reacting with its kyanite inclusions. In case of Fig. 5e, it is defined solely by the instability of GRT-type C. Diffusion profiles at garnet-rim symplectites are similar to those at crack-symplectites. Occasionally, and as shown in Fig. 5d, the diffusion curves for Mg, Fe, Mn adjacent to rim symplectites show an inflection point at some short distance before the retreating garnet edge.

Page 12 - lines 6 - 7. You mention that the garnet C and Z1 have similar compositional characteristic, but you distinguish them based on the character of their occurrence. With respect to the Fig 2G - can you exclude that the C and Z1 garnets are really not the same type, taking into consideration possible effects of section of the garnet?

Let me start with the latter part of your question regarding possible cutting effects. The garnet in Fig 2g is the largest observed within the collection of all samples. Therefore, I believe, this section “goes” through the garnet “center”. Then I ask myself, how GRT-Type Z1, which

- a) is related to a garnet crack (Figs 2G and Fig. 2A),
- b) “intrudes” irregularly GRT-Type Z2 (Fig. 2G),
- c) is related by this mode of occurrence with some mobile phase,
- d) cross-cuts GRT-Type Z3 (Fig. 2G),
- e) occurs at garnet margins (Fig. 2G)

can be related to GRT-Type C, which is spatially restricted in the homogenous (not “intruding” or so) inner/central, part of the garnet?

My answer and suggestion is, they are not related. These matters are discussed already in page 13, lines 1 – 8 (now page 9, line 33 to page 12, line 6). Let me copy this text here.

GRT-type C occupies the large, inclusion-poor interior part of the garnet. GRT-type Z1 has evolved at a strongly Ca- depleted area along a garnet crack, which can be recognized in Fig. 2a,g. Therefrom, it “intrudes” irregularly the garnet interior and extends over a narrow zone along the lowest rim of the garnet (Fig. 5g). As can be recognized in Fig. 2g, GRT-type Z1 cross-cuts type Z3 over a narrow transitional zone and is therefore younger. This age relation is supported also by the typical middle-sized garnet in Fig. 5b. This garnet is of type Z3, but has evolved to GRT-type Z1 towards its margin. Compared with the other GRT-types shown in Fig. 2g, GRT-type Z1 is a late feature related most probably with the action of metasomatizing agents. GRT-types Z2 and C are seemingly older, but their temporal interrelation is not clear. Their transition towards GRT-type Z3 is smooth.

However, your remark has given me the opportunity to re-evaluate the way I have formulated this “similarity”. Thank you. It is probably better to emphasize that this similarity consists of sharing the same relation $X_{grs} < X_{alm} < X_{prp}$, which is not shared by the other GRT-Types, see Table 2. So, I changed the text as follows.

Page 12, line 6.

GRT-types Z1 and C show similar compositions characterized by $X_{grs} < X_{alm} < X_{prp}$. However, they differ distinctly in their mode of occurrence, see below.

Page 9, line 24

GRT-types Z1 and C share the same component relation characterized by $X_{grs} < X_{alm} < X_{prp}$.

Page 13, lines 8 – 9.

Despite the compositional similarities, we discriminate GRT-type C from GRT-type Z1 based on their different modes of occurrence.

Page 12, lines 6-7

Despite their similar component relation described earlier ($X_{grs} < X_{alm} < X_{prp}$), we discriminate GRT-type C from GRT-type Z1 based on their strikingly different modes of occurrence described above.

Page 13 - line 6 - 7. Sentence "GRT-type Z1 ... metasomatizing agents" is pure interpretation, however the reader does not yet have any background to understand it.

I fully agree, yes, it is an interpretation based on the criteria listed in the previous discussion and fulfilled by GRT-type Z1, see the copied original text above. I am asking myself,

‘What background a reader of Solid Earth might need in order to understand that this GRT-Type (Ca-depleted, crack-related, “intruding” the other garnet types, etc.) can be interpreted as a product of metasomatic modification’

and I can’t find an answer.

Page 14 - line 8: thin exsolution lamellae - of which phase?

Thank you for this remark. In fact, the lamellae are very thin and could not be resolved by the microprobe analysis. Therefore, the text is slightly changed as follows.

Page 14, line 8

As shown in Fig. 2c, the interiors of some larger matrix clinopyroxene crystals contain very thin exsolution lamellae.

Page 15, lines 2-3

As shown in Fig. 2c, the interiors of some larger matrix clinopyroxene crystals contain very thin, analytically unresolved exsolution lamellae.

Page 17 - line 9: statement "is isochemical to GRT-type C" should be rather "is almost isochemical". I think that this is not just a small detail but important point that although the local bulk rock chemistry did not deviate too much from the original garnet composition, the involvement of fluid (or melt) and the subsequent minor change of the bulk rock chemistry is crucial and the symplectites would probably not form without it.

Right, your formulation is better, thank you. It has already been changed. It is also appropriate in view of the fact that, indeed, the deviation from “pure isochemical” is already discussed a few lines above.

Page 17, line 9

The second one is the crack- symplectite assemblage CPX+OPX+SPL+PL that is isochemical to GRT-type C.

Page 17, lines 16-17

The second one is the crack- symplectite assemblage CPX+OPX+SPL+PL that is almost isochemical to GRT-type C.

Page 17 - line 17: It is worth to mention that the a-x model of cpx including the CaTs substitution was recently developed by Green et al. (2016 - Activity--composition relations for the calculation of partial melting equilibria in metabasic rocks. Journal of Metamorphic Geology). I can only speculate, what would be the impact of using this model (together with the recent dataset 6 by Holland and Powell 2011 and adequate a-x models of other solid solutions) on the calculated mineral chemistry. I know that this would need to be done by using either thermocalc or perple_x, but anyway, I think it is worth to test it.

Please appreciate that as a “very, very, very small” co-author of the Theriak/Domino paper (that is based on a huge, long-lasting work of Christian De Capitani), I rather prefer to operate with this. Your idea to test various datasets is interesting in a very general sense, but not the purpose of this paper. As you ascertain below, the used model “worked” good by reproducing the observed assemblages and the measured mineral compositions. In my opinion, this is the crucial point. Please, take also a notice of the ongoing discussion with Mr. Massonne (first reviewer) about a similar topic. So, I try to keep things as simple as possible as long as they reproduce observation.

Page 22 - Table 3: The table shows good agreement of the calculated and measured composition and modal proportion of phases. However I don't understand why the measured pyroxenes are not divided to CPX and OPX? (I would expect based on the presented images that the measured proportion of CPX would be considerably higher than the calculated one).

Thank you for the reliability confirmation of the applied model. The distinction between CPX and OPX during volumetric analysis was not possible, because both phases showed the same gray color in the used BSE-images. This information and in general the methods used are explained in the Supplement, page 2, line 35 to page 3, line 55. The reader is invited in the caption of Table 3 to get all relevant information therefrom.

Page 22 - lines 5 - 8. See the comment Page 17 - line 17. I recommend to reformulate the sentence. The symplectites are not completely isochemical, as it is illustrated by the Fig 9.

Right, it is reformulated as shown below. However, please note that this sentence is talking about a “presumption” implied by the observed microstructural features.

Page 22, lines 5 - 8

In case of the crack-symplectites, it is just the garnet instability that has led to its partial break-down, leading to the presumption that the crack- symplectites are isochemical to the garnet.

Page 22, lines 23-25

In case of the crack-symplectites, it is the fact that garnet was less stable and broke-down partially, leading thus to the presumption that the crack- symplectites are more or less isochemical to the garnet.

Page 24 - lines 15 - 16. It is not clear if authors suppose that the garnet zoning was developed during the garnet growth or by modification of already existing garnet by diffusion during the metasomatism.

Let me copy the original text here and underline the key-word.

“All these features provide convincing evidence that the garnet shown in Fig. 2g has undergone diffusion-aided metasomatic modification during the late stages of its evolution represented best by GRT-types Z3 and Z1.”

I think, it is clear that the current compositional structure is a metasomatically induced modification, not a product of growth or homogenization or both combined. This implies certainly a pre-existing garnet at the time of modification. How this pre-existing garnet did look like, I cannot say with certainty and without risking a huge degree of speculation. Two limiting cases are, however, possible. Typical growth zoning and typical homogenization by intracrystalline diffusion due to high T. The true state before modification might have been somewhere between these two limiting cases.

Please note that I avoid the formulation “zoning” or “zonation” for this garnet, as these words may connote to the reader “growth zoning”. Therefore, I prefer the formulation “compositional structure” to describe a pre-existing garnet (with growth zoning and/or homogenization and/or both) that has been asymmetrically modified (GRT-types not concentrically distributed, cross-cutting relations etc.) by metasomatic action. Please see also my response to Mr. Massonne about this topic.

Technical Corrections

Page 2 - line 6 - Jedlička (diacritics)

Done, thank you!

- line 7 cf. Table 1 (missing space)

Done, thank you!

- from line 28 - list of abbreviations - missing abbreviations for muscovite and prehnite that are used later

Thank you for this remark. Yes, the abbreviations Ms and Prh are used (e.g. in Fig 2), but unfortunately not explained. This is already corrected.

Page 7 - Figure 2 - B - labels of minerals would be helpful. C - lines C-D and A-B are not explained in figure caption, as well as the ellipse.

Thank you for the constructive suggestions. Labels in Fig. 2b inserted. Profile A-B in Fig. 2c is not used and, therefore, the line is deleted. Profile C-D is shown in Fig. 6B. So, the caption looks now as follows.

Page 7 - Figure 2 – B

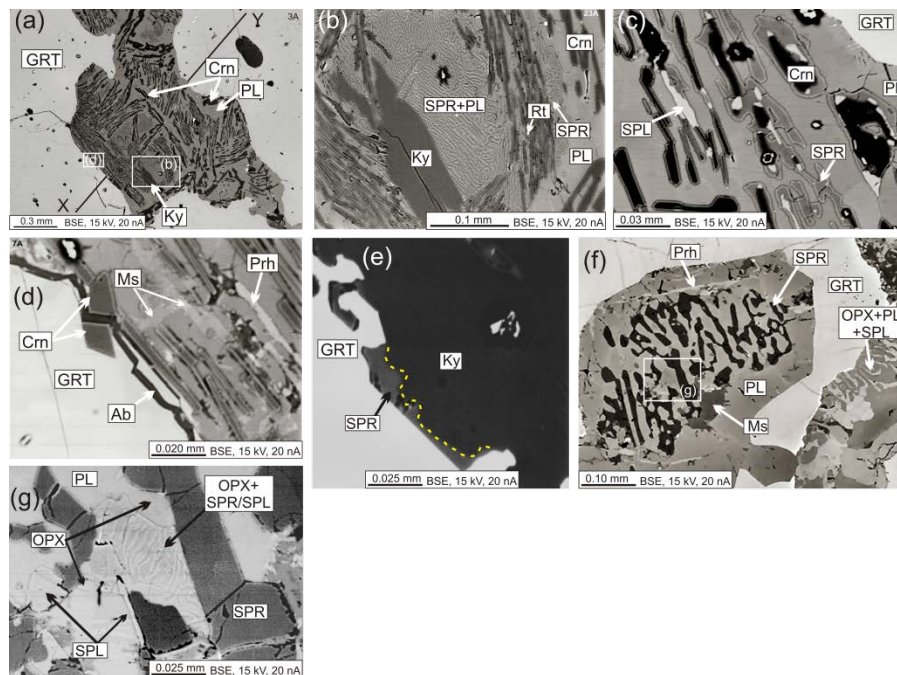
(c) BSE image of the rock matrix showing smooth interfaces and triple junctions among clinopyroxene, plagioclase and hornblende as well as thin exsolution lamellas in the clinopyroxene interior. The plagioclase rims are enriched with Ca.

Page 7 – Caption Figure 2b

(c) BSE image of the rock matrix showing smooth interfaces and triple junctions among clinopyroxene, plagioclase and hornblende as well as thin exsolution lamellas in the clinopyroxene interior designated with an ellipsis. The plagioclase rims are enriched with Ca. The profile C-D is shown in Fig. 7b.

Dear Mr. Racek!

You are a perfectionistic, exact observer. Thank you. To your three remarks below, I say nothing more, than, please, see Fig. 3 as an example of the re-edited figures according to your suggestions. It was a little bit hard, but it helped!



Pages 7, 9, and 10 - Figures 2, 3, and 4. The format of the figures is not unified. The scales and labels have often various fonts (see Fig 3C) and font size, sometimes are bold (see Fig 3B). The scales by BSE images sometimes involve information about voltage and current, sometimes not (see Figure 4). It would very good to unify the format of all the figures.

Page 9 - Figure 3: 3A - try to avoid intersection of line (arrow) with text. Check format of the arrow pointing from 3A to 3D. 3B - should be SPR and PL (instead of Spr and Pl). 3D - muscovite and prehnite are missing in the list of abbreviations. 3G - what is the strange bright rectangle in the centre of the image? Generally, there is not much visible in this figure.

Page 10 - Figure 4D + caption - profile T-Y is probably the profile T-U in the Fig 2G. Please check the Y/U throughout the text.

Page 11 - Figure 5: Maybe it would be helpful to mark the exact limits of described garnet types in the profiles by some vertical lines? 5D - I cannot find the E-F profile marked in any BSE or OM image.

I see the point, but, I think that Fig. 5 is overloaded with symbols, boxes and text. However, the caption in Table 2, where the average compositions with standard deviations of for the GRT-Types are given, invites the reader to see the Supplement. There, in Fig. S 2, those parts of the large profile Z1-Z2-Z3 (615 point analyses!) are re-plotted, which were incorporated into average calculation. The plots there are at higher resolution and are intended to show that the averaged analyses are several consecutive analyses of nearly constant composition along the profile Z1-Z2-Z3.

Page 13 - line 11: Fig 5A - should be Fig 2G? I can see no dotted line in Fig 5A.

You are a perfect reader! Yes, Fig. 5a is wrong. The text is now changed as follows.

Page 13 - line 11

It is noteworthy that the garnet is replaced by kelyphite along its rim (cf. Fig. 2 g) and, as shown by the dotted line at the lowest garnet rim (Fig. 5a), only the younger GRT-type Z1 may be formed as late as the rim- symplectite.

Page 12, lines 8-10.

It is noteworthy that the garnet is replaced by kelyphite along its rim (cf. Fig. 2 g) and, as shown by the dotted line at the lowest garnet rim, only the younger GRT-type Z1 may be formed as late as the rim- symplectite.

Page 19 - Figure 10B: Even after reading the figure caption, I am not sure what the yellow circle stands for. Should it just generally symbolize intersection of isopleths?

Thanks, it is to emphasize, that the recognition of a preserved equilibrium composition requires a common intersection point of the isopleths. It is (hopefully) better formulated now, as follows.

Page 19 - Figure 10B

The yellow circle emphasizes the necessary features of a preserved equilibrium composition.

Page 19 - Figure 10B

The yellow circle emphasizes the necessity of a common intersection point of the isopleths in case of a preserved equilibrium composition.

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