Review of

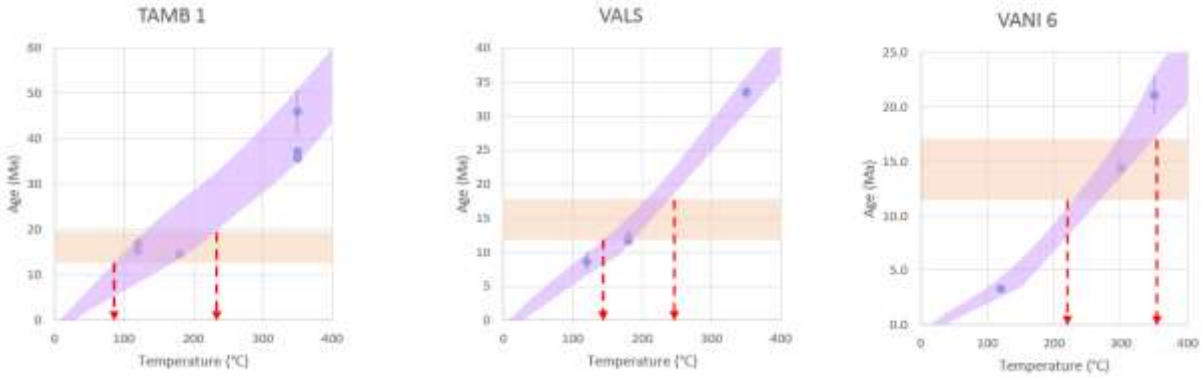
Dating exhumation and fault activity of the Lepontine Dome and Rhone-Simplon Fault regions through hydrothermal monazite-(Ce)

Christian A. Bergemann, Edwin Gnos, Alfons Berger, Emilie Janots, Martin J. Whitehouse

by Meinert Rahn

This review is an update of a previous version of the manuscript. The study of Bergemann and co-workers presents 480 single spot ages and 33 weighted mean ages from 19 locations and their cleft monazites within the northern Lepontine Dome (and areas adjacent to the dome). Here, I first pass through the major issues of criticism from the previous review to check what the authors have done with it.

1. Title of the manuscript. No doubt, the title has been changed, but the content has also been changed. I feel that the title still is very ambiguous with respect to the data which cannot (like many methods with a well-defined closure temperature) be linked to a specific moment of cooling in the exhumation history. I below show that the temperature range the authors argue with can at least be questioned. I would give this point a “partly fulfilled”.
2. Mess with figure numbers: No doubt, this has been sorted out, and this issue can be closed.
3. Short chapter on “Results”: The chapter on the result has been extended, but many issues still remain vague and are not clear. With respect to shortness, the issue is solved, with respect to clarity, the issue remains open.
4. Methodical descriptions: The authors have intensified their description on how, the data have been produced, but I feel that aspects have been blown up that do not play a role in your final data interpretation. Together with figure 5, the authors develop a distinction into several sample groups, based on the internal structures, which I am unable to follow and which in the end have no relevance for the age data:
    a. “Monazite stability field”: The authors claim at several places a range of 200-350 °C to be the temperature range of hydrothermal monazite formation (which is clearly different from its formation as rock forming mineral with a U/Pb closure temperature above 400°C). However, in figure 6, it becomes evident that the range given above does not fit, if the monazite ages are compared to the ages of thermochronometers with well-defined closure temperatures. Find a figure on that issue below, where I have compared three localities (TAMB 1, VALS and VANI 6)



The orange age range is given by the monazite, while the purple envelope covers the existing thermochronology data I know from these localities. For the locality VANI 6, the resulting formation range (indicated by the dashed red vertical arrows) corresponds well to the quoted temperature range of 200-350°C, however for the two others, the range is at distinctly lower temperatures. In the end, the range does not really matter, as the range, if I understand the authors correctly, is not comparable to a closure temperature. But it illustrates that fissure monazites can form over wide temperature range. Since monazite formation in fissures is controlled by the opening of fissures and the evolution of the fluid, the temperature within the fissure should not be taken as a measure for the temperature of the surrounding rock. One part of disequilibrium with fissures is that there is a thermal gradient from fissure to rock, which leads to fast cooling and precipitation of minerals in the fissure. This brings me

to another unsolved issue:
"Disequilibrium": I do not find the discussion about chemical equilibrium and disequilibrium very fruitful. Can we agree on this: Mineral precipitation within fissures is the result of disequilibrium and the system's approach to reach equilibrium? It does not matter whether this mean chemical or physical disequilibrium as they commonly develop simultaneously. Accordingly, monazites in fissures are not a sign of equilibrium, but a disturbed system that reacted by precipitating this mineral. The current discussion on equilibrium and disequilibrium is at odds with such a description.

b. BSE images: Yes, figure 5 has now a key function, and I like it very much to gain an overview. However, it is also this figure that causes most of the methodical trouble: I do not understand your grouping of the samples (not the regional one, but the one on internal structures). I can only follow some of your descriptions, as they are not clear and they are not transparent to the reader. You are using several terms that are not clear (or at least not clear to me): zonation, sector zonation, sector-like zonation, oscillatory zonation, oscillatory-complex zonation, ring zonation, complex zonation, alteration, alteration features, diffuse pattern. Even with the help of figures 3 and 5, for most of these features I cannot follow your description. It does not help to speak about "signs of alteration", if the reader does not know, what "signs" you are talking about. I understand that the authors tried to date areas of different colours within the BSE images. But I do not understand, why you have then placed analytical spots into oscillatory zoning patterns. As stated in the previous review, the division into different groups (separated by colour) of the analytical spots is not clear at all.
c. Avoiding measurements next to cracks and holes: This issue has not been solved. I would expect that the authors have one of several arguments, based on which they know whether an analysis spot is a good spot or not (like in microprobe analysis, e.g. if the sum strongly differs from 100%). But they do not tell. It is evident from figure 5 (e.g. VANI 4, VALS) that the authors have discarded some analyses, but the arguments on which they were discarded, is not mentioned. Since the BSE images are only 2D, but some analysis spots could be bad due to 3D effects (e.g. holes not visible from the surface), discarding single spots should be a normal procedure, but based on arguments that are applied when processing the data (and not afterwards).
d. Choice of weighted mean ages: This issue has not been addressed at all. If you take some investigated crystals such as e.g. KLEM 3 or LUCO 1, then the choice of which analytical spot has what colour and which of the spots are combined into a weighted mean age is not clear to me. Since this is the basis for the age interpretation afterwards (in particular figure 7), I do not know, whether I can trust the authors' choice. Without any further information, the calculation of these mean ages is cryptic to me. Issue not solved.
e. Age groups: This issue is not solved either: On p. 12 (lines 12/13), you are stating that weighted mean ages are "precise ages", while "spot ages" are "approximate ages". I would agree with this classification. But the authors do not stick to it. In the Discussion chapter you mainly use the term "age", sometimes "spot age", sometimes "mean age", but in many cases, it is not clear whether we now talk about a "precise" or an "approximate" age information. The same is true for figure 4, in which a two colour classification is introduced, but seemingly without any consequence for the later interpretation.
f. Figure 8 is now figure 4 and has not been changed. We recommended to give clear definitions for the boundaries from white to faint and faint to strong colour. The authors only provide a vague definition by stating that strong colour means peaks and plateaus, but for the blue curve I see a plateau from 7 to 10 Ma, while the authors do not. Issue not solved for me.
g. Figure 8 and previous literature data: This point has not been taken up as far as I can see.
h. Regional groups: There is no new information in this issue, therefore the issue cannot be closed. Furthermore, the distinction into different patterns in the description of the samples along with figure 5, does cause additional confusion. Why have the authors not stated that their division has been e.g. made initially and that this division seems to work well? It is never clear whether the division is an initial feature or a result of the study.
i. Comparison with other thermochronometers. I definitely like figure 6. But as shown in my figure above, I would draw other conclusions from it. I consider the fact that monazites are formed over a wide temperature range as a methodical advantage, not a disadvantage. Let us backtrack for a second: Monazites in fissures have no closure temperature, the ages are formation ages, we do not date cooling or exhumation, but we date fluid activity (and in most cases then tectonic activity). As such, figure 6 has an important methodical impact, but to me not the same as for the authors.

    j. Chapter on hydrothermal monazite “crystallisation : This information has now been shifted to the intro part of the paper. Issue closed.
5. New information about exhumation and tectonics within the Lepontine Dome: To me this issue is only partly solved. First of all, the authors do not consider the possibility that fluid pathways do not need to be changed to precipitate monazite. Imagine e.g. a dehydrating mineral reaction in depth that releases a lot of hot fluid, which then migrates upwards along existing pathways and leads to monazite formation. No tectonic “event” is needed, but only progressive burial. In the view of the authors, all monazite formation must ultimately be linked to tectonic activity (which in many, but not necessarily all cases may be true). But: In the end (and this is the important message!), the authors present some interesting age pattern across the Lepontine dome (even if we neglect any weighted mean ages and only stick to the range of the spot ages). This is what remains to me if I subtract all doubtful clustering of the ages. And it probably results in a much more vague interpretation of the ages. Which would be ok with me, as such an interpretation would be based on sound data that nobody can criticise as I do above.

I conclude, that many issues have not been solved and the paper still lacks convincing straightforwardness. Since above, we have mainly looked backwards, I now bring up my concerns with respect to the in a shortened form. All issues rely on the many detailed comments as listed below.

As stated within my previous review, there is no doubt to me that the provided data are an interesting data set. The authors have failed though to convincingly explain on how the single spot analyses can be used to derive mean ages to derive age groups and to derive conclusions for the structural evolution of the Lepontine dome.

My major concerns on the revised version are the following:

1. Methodical meaning of the ages. The monazite ages are formation ages, if we assume in particular that one spot is placed on one monazite generation and there were no mixing processes afterwards. Formation is due to fluid disequilibrium, caused by PT changes, fluid changes, and fluid composition changes. Part of the changes may be caused by tectonic activity. This is my general frame. It seems that the authors differ from it, but I do not understand why. I find the methodical description (e.g. chapter 3.1) not yet very clear. In addition, the paper strongly promotes that monazite formation means tectonic activity, and such a simplification is not exclusively true. The authors miss to express their caution about this. The authors could also discuss the effect of alteration on the ages, if there is a systematic effect; but they do not.
2. Grain-internal “features”: The authors use a large range of expressions to describe the internal patterns they see. In chapter 4.1, they present a division into 5 categories that is based on the internal structures, but this categories seem to have no influence afterwards and I do not understand your applied terminology. Methodically they present figure 3 to explain, however, with many descriptions to the BSE images in figure 5 I disagree or, at least, am not able to see the same. In the end, I wonder what the purpose of the classification into 5 feature categories is. The authors somehow have used these patterns to place their analysis spots. But this is far from being transparent to the reader.
3. Group formation and weighted mean age calculation: I do not understand how the different analyses were grouped (colour groups in figure 5), and how you decided on which spots are grouped forming a weighted mean age (see many questions below). To me the logical consequence is that I can only trust your single spot ages and the resulting spot age range (see statement on p. 12, lines 10-12), while the “weighted mean ages” in figure 7 are not explained in a transparent way.
4. Temperature range of fissure monazite formation: As shown in the figure above, I have doubts about the temperature range of 200-350°C. If we assume that fluid temperatures in fissures can substantially differ from the temperatures of the surrounding rock, then these two parameters become decoupled and the comparison with thermochronology data is relatively useless.
5. Estimation of exhumation rate: If the temperature range, in which the fissure monazites are formed is very large (see my figure above), any comparison with thermochronological ages to estimate exhumation rates is difficult. In your chapter 5 you refer to some examples from areas of slow exhumation, but you do not show any example for fast exhumation to show any difference. In addition, one of the assumptions in the estimation is that the temperature of the fluid in the fissures is identical or close to the temperature of the host rock. I am sure that this is not always the case (monazite formation out of disequilibrium (e.g. causing fast cooling of the fluid). Thus I have serious doubts about the conclusions of chapter 5.
6. Repetitive structure in Discussion and Summary chapter. Chapter 6 and 7 seem to be repetitive and I do not know why the authors have chosen such a repetitive structure.

In summary, I note that still fundamental issues have not been solved. I simply realise that the list of detailed comments below is very long again (nearly 11 pages…). The issues mainly cover methodical aspects, it is not due to a bad data set. Any interpretation of the data depends on the data treatment. For the time being I feel unable to understand the procedure the authors have applied. I have less concerns about the general interpretation of the data pattern and its evolution in time (in particular Fig. 7). But the data presentation would need a fundamental revision to clarify the procedure and by this provide a transparent base for later data interpretation. This would still require again major revisions of the manuscript. This must be disappointing for the authors who have already done a strong revision, but evidently not sufficiently focussed on the comments of this reviewer.

Detailed comments:

The following comments are sorted according to page numbers and text lines (in the pdf provided). They were continuously gathered while reading. Many of them have later been clustered to major comments (see above). With respect to text quality, I have found very few typos, thus the text seems to be in an advanced stage. Many text comments should be seen as suggestions (as indicated), e.g. to clarify a statement.

Page 1

Line 2	Suggestion: "… in the Central Alps and the timing of deformation …"
Line 3	Suggestion: Start sentence with "Hydrothermal monazites-(Ce) …"
Line 3	"physicochemical"
Line 4	"through their crystallization ages"
Lines 4/5	Delete "including …type locality," this information is not as important as to be mentioned in the abstract.
Line 6	Suggestion: "geometric"
Line 6	It is not clear what you mean with "spatial distribution". Do you mean the spatial distribution of the analysis spots on the crystals or the spatial distribution of the samples within the Lepontine dome?
Line 8	The "followed" is a fuzzy term, as time wise "15-10 Ma" do not follow "16.5-10.5 Ma", but have a large overlap.
Line 9	Suggestion: "along the western limit"
Line 9	Suggestion: "A youngest age group"
Line 10	Suggestion: delete "large", not necessary, perhaps in 20 years with automated analytical machines, this number will be small…
Line 11	Suggestion: delete "spread over significant metamorphic structures". The paper does not discuss metamorphism in the Lepontine dome (or steps in metamorphic overprint e.g. across the RSL).
Lines 11/12	I do not think, the formation of monazite has got to do with stability (do you mean thermodynamic stability?). At these relatively low temperatures, probably, the fluid composition and kinetics are much more relevant. Thus, I would avoid the term "stability".
Lines 12-14	I do not believe in this estimation and I would suggest to complete delete this last sentence.
Line 16	Suggestion: Use "multi-phase" instead of "complex"
Lines 18/19	If "much of the retrograde evolution of the area is well known", you should be able to cite some of the relevant references.
Line 19	In this early stage of the paper, it may be more cautions and state "monazite-(Ce) ages may be able to complement …". If this finally is true you have to prove.

Page 2

Line 1 I am not sure, whether “reactive” is the correct word. In some cases, dissolution of existing monazite may occur, which, from a monazite point of view, is not an “active” process at all...

Lines 10/11 It would be interesting to add, why the monazite system is different and does not have such methodical flaws. Why do monazite do a better job?

Line 18 Suggestion: “where newly formed monazite-(Ce) …”

Line 27 I do not think that the statement “development of several metamorphic areas” (and the given examples) is correct. What is a “metamorphic area? The Tauern Window is a tectonic window, showing higher metamorphic grade than the surrounding and covering units, but this does not mean that the window has experience a different metamorphic history, but only a different exhumation/denudation history.

Lines 29/30 I do not know, what the statement “The Western and Central Alps … complex tectonic and metamorphic history.” should add to the content of the paper. The term “complex” is fuzzy. It simply means that there is not sufficient data to fully understand it. Is this of importance to this study?

Page 3

Figure 1 ok, no comments.

Line 2 Is the bracket “in excess of 650°C in some regions” important? The information is not very specific. It would help much more to state that the Lepontine Dome consists of medium-grade metamorphic rocks (meaning minimum Alpine metamorphic temperatures of 500°C, which of course is not true for the samples from the Gotthard massif, TAMB 1, VALS and VANI 6). One might even think about showing the 500°C metamorphic isotherm in figure 1.

Line 10 Suggestion: “margins” instead of “limits”. The border is not very sharp and not very obvious from figure 1.

Line 12 “contemporaneously”

Page 4

Figure 2 One wonders about a combination of figure 1 and 2, as there is a large overlap in their content. In figure 2, the corner top right and bottom centre of the map both show and area of light grey colour, which is not defined and in the first case is simply geologically wrong. Check with the Tectonic map of Switzerland. In the figure caption you are mentioning that the different profiles have different scales among each other and compared to the map. But for each of the profiles a km-scale should be added. ¨Profile (b) should have SW – NE instead of S-N at the endpoints.

Figure caption Line 2: Suggestion: “across” instead of “over”. In the figure caption you should add an explanation about A and A’, you should refer to figure 6, in which the line A-A’ is used.

Page 5

Table 1 For sample “DUTH 2”, the name of the lake is “Lago Scuro”. It is not my job to check this…

Page 6

Line 1 Change title to “2.2 Study area”

Line 2 Suggestion: “comprises the northern part of …”

Line 2 You define the study area as the part, “in which mineralized fissures/clefts are commonly found”. This seems to be a strange definition, as you do not explain, why there are no mineralized fissures further south and who has made this statement/observation beforehand. The study area may simply be defined by the collected samples. If however there is a southern border that is defined geologically, it would be important to know (“stability” or field of occurrence of fissure monazite).

Line 3 Suggestion: “across” instead of “over”

Line 5 The statement “were divided into four groups” needs an explanation. Has this grouping been based on major tectonic boundaries? If yes, why were the Gotthard samples not placed into a separate group?

Line 5 “to the east” instead of “to the west”. Could this be more specific? One may e.g. state “Adula nappe and further east”.

Line 6 The “Verzasca Line” is not visible in any figure, so the reader cannot assess this explanation.

Line 8 The statement “and adjacent south-western Gotthard nappe” comes as a surprise. All other groups are defined by major tectonic lines, but here, group swaps across a major tectonic boundary. Should this not be a result of your study, instead of a prerequisite? One result could be, there is no change in monazite formation ages from Lepontine Dome to Gotthard Massif.

Line 12 Suggestion: Start paragraph with “The formation of hydrothermal …”

Lines 15-24 I beg to disagree with this text. Fissures are areas of disequilibrium. If there is equilibrium, nothing happens. Due of this disequilibrium, however, minerals are precipitated. Thus, I see the formation of monazite not as a sign of equilibrium. The text here is misleading and would need rewriting. I also disagree that “chemical disequilibrium is generally triggered by tectonic activity (line 17). The disequilibrium could be reached by new fluid flushing through the system, and this does not “generally” be based on tectonic activity, but may be related to e.g. a dehydrating mineral reaction further down. Finally, what is so important to state that this is a “chemical” disequilibrium? The disequilibrium may mainly come from large temperature differences between fissure and rock fluid. In the abstract you are more general, referring to “physicochemical” conditions that change.

Line 26 Avoid the term “reactive (see comment p. 2, line 1).

Line 28 Suggestion: “may cause the crystal …”. It is not clear at all that precipitation (and in particular precipitation of monazite) occurs. Thus I would make a more cautious statement. Note the implication of your statements here. Monazite within the fissures MAY record tectonic events that can be dated. But monazite formation is not guaranteed at all, and important tectonic events MAY be missed, and also other fluid forming events MAY also cause monazite formation. The reduced formula “monazite formation = tectonic event” is simply wrong.

Page 7

Figure 3 I find this figure difficult. If forms the basis for the interpretation of all the other BSE images inn figure 5. For such purpose, the three samples do a rather moderate job. Image B is difficult to interpret, as the colour differences are weak and disturbed by the combination of the different photos that have different colours and colour contrasts. I cannot follow the description of this image. In addition, here starts the mess with the different terms such as zonation, sector zonation sector-like zonation etc. If these descriptions and classifications are important, you have to more carefully describe what you mean by these terms.

Figure caption Line 1: “BSE images …”. Line 3: I would not use the word “pristine”, as it implicates that you know that this is pristine. How do you know? Lines 3/4: Suggestion: “(B) Partially.preserved sector-like zonation…”. Lines 6/7: Suggestion: “(C)” Multiple rims combined with …”. Line 8. Suggestion: “core” instead of “central part”. Line 9: Is “inner rim” equal to “central part”/core?

Lines 5/6 Here, you mention a temperature window and stability field for fissure monazite. My question is: Does it matter, if such a temperature window (similar to a closure temperature) is defined. Would it not be more favourable to the method to leave it open, in particular, if you assume that there might be a temperature disequilibrium between fissure fluid and surrounding rock wall? I find it rather contradictory, if you speak about a "temperature window and stability field" first and then about the monazite formation to be largely "temperature independent".

Line 8 Suggestion: "catalysed" or "triggered" instead of "aided"

Line 9 Whatever means "primary or secondary" is not defined in your discussion about the BSE images or data. You only talk about formation and alteration, which may not be the same.

Page 8

Line 3 You refer to figure 3c, but strictly speaking you should state "as assumed for the crystal in figure 3c".

Line 6 "possess"

Lines 9/10 Suggestion: Most of the samples were provided by mineral collectors (see Table 1 for location details", as monazite-(Ce) is uncommon in clefts and often …" I would avoid the term "hydrothermal cleft", these clefts have formed due to tectonic movements in a brittle rock regime, but not originated from hydrothermal activity. Furthermore: I note that there seems to be no information about the orientation of the fissures that these samples have been taken from. Thus, any arguments from such fissure orientation has to remain speculative, as this observation has not been collected (or at least not systematically).

Line 11 Suggestion: "level of a central section across the grain"

Line 12 Suggestion: "were obtained, using …"

Line 15 Suggestion: Start sentence with "Spot measurements next to cracks …". If your spot analysis includes a crack or inclusion, how do you know? What would be the methodical procedure to sort out analyses with cross contamination with a volume that is not a monazite? I just want to make it clear. If there are spot analyses that contain cracks or inclusions they should be sorted out before calculating ages, as these ages may have no geologic meaning.

Lines 22-29 I am not a U/Th/Pb specialist, and I do not understand your careful explanation on how common Pb was corrected. Similar to me, however, I expect all non Pb-specialists to get to know what the issue about the common Pb is. Later on (p. 13) you mention that some grain analyses have high to very high common Pb values, and the reader wants to know whether this means anything with respect to precision or reliability of the ages. Thus your detailed explanation here is good, but what is the consequence with respect of the ages afterwards?

Line 31 "possess"

Page 9

Table 2 no comments

Page 10

Line 4 Suggestion: "from comparison"

Line 6 add space before "to capture"

Lines 7/8 Suggestion: Delete sentence “The number of domains …”. This is not a scientific issue, but an organisational, and I am sure that Martin Whitehouse is not amused to be critized…

Line 9 If the high Th/U ratios are the argument, then it does not matter whether this high ratio results from low U contents. I would therefore delete “at low U content”

Line 10 “of” instead of “oft”

Line 15 Suggestion: “showing the compositional …”

Line 19 “as these could be shown to date tectonic activity”: I have not gone through the references papers, but I would simply be more cautious about the relationship between tectonic activity and monazite formation. How about: “as it has been shown that monazite formation is closely related to tectonic activity”?

Line 29 I would not speak about “a problem” but address the issue directly: “Large parts of the Lepontine Dome …”

Line 30 Why “more than two”? Suggestion: “several”, this leaves the number more open.

Line 31 Suggestion: “a reason for large age scatter”

Lines 31/32 Further above you state correctly that the monazite ages are formation ages. Here you talk about “age resetting”, which is a term coming from the closure temperature concept. If alteration changes the age (and we do not know how), the age is geologically meaningless.

Line 35 The statement here differs from the statement on p. 6, lines 23/24. I consider this statement much better. This is what actually happens: precipitation of monazite due to disequilibrium and the system trying to re-gain equilibrium.

Page 11

Figure 4 I still do not understand the arguments on the basis of which the different colour bars are chosen on the basis of the probability density plots. I understand that you apply a threshold value at 1 age (which cuts out the youngest bumps for the red, green and blue curves). But then: For me, the blue curve has a “plateau” at 7 to 10 Ma in my eyes, but obviously not in your terminology. This needs more clarity in your figure caption (lines 3 and 4).

Page 12

Line 4 the statement “especially in larger grains” cannot be assessed. How/where do I see this and why is this the case.

Line 5 If you state that “the entire dataset of each region was plotted according to …”, this might also be interpreted in such a way that you simply smear out geologically meaningless ages. The choice what is a good analysis (providing a good age) and what is not a good analysis (providing a useless or doubtful age) has to be made before aggregating the ages.

Line 6 add comma, after “Fig. 4”

Line 7 What means “a significant number of ages”?

Lines 10-12 I like this statement, but of course such a statement only works, if only reliable analyses were used. If everything fails and all weighted mean ages are artificial clusters, then this is a statement that would still be true.

Lines 12/13 The distinction is only proposed here. But the proposal has not been put into action, if one e.g. compares with p. 20, lines 22-24. Thus you do not distinguish between “precise” and “approximate ages”.

Line 17 Suggestion: delete “covering the time”

Lines 19/20 Something seems to be missing here. I do not understand this sentence.

Line 22 Is a low Th content an argument to discard an analysis? This is one of the critical points: How do you discard an analysis (see e.g. figures 5l, 5t). You have to specify this somewhere. Otherwise, the reader thinks that the quoted ages are the only "reasonable ages" (leaving it to the authors to decide what is "reasonable"…)

Lines 25-27 Suggestion: Cut long sentence in two: Line 25: Start with "Th contents are generally…" and cut at line 27 after "ppm)", then restart "This results …"

Line 27 Can you be more specific: "up to several hundred, e.g. by quoting the highest value?

Lines 29/30 Suggestion: Delete "above 70%" and shorten to "contents up to a maximum …"

Line 30 "With the exception of sample BLAS 1": P. 13, line 1 states that GRAESER 3 also shows "no clear sign of alteration"…

Line 32 Start new line with "(1) Sector like zonation". For the now coming list of all samples, divided into 5 categories, I have four general comments: (1) I wonder, whether this list would better to be placed in a table. At the moment this is a long list that the reader cannot really assess. (2) I would disagree with many of your descriptions or simply cannot see what you mean. And this has got to do with (3): Many if the terms you use, such as zonation, sector zonation, sector like zonation, oscillatory zonation, oscillatory-complex zonation, ring zonation, complex zonation, alteration, alteration features, diffuse pattern are not clear to me. Figure 3 does only partly help the reader to understand. Finally (4): Does it all matter? Does this grouping into 5 categories help anything? The reader simply wants to know: Do I understand how the authors have grouped their single spot analyses and derived weighted mean ages from. The five categories do not help in this respect

Line 33 Here it starts: What are "some signs of alteration"? Where? What am I supposed to see? What do you mean by "complex zonation"? Which is the "inner part of the grain"? What is the difference between "primary" and "secondary"?

Page 13

Line 1 What would be "clear signs of alteration"?

Line 2 What is the consequence of the high common Pb contents to the age?

Line 5 What are these "strong signs of alteration"? Where is "in places"?

Line 7 Suggestion: "… are relatively low (1600-10800 ppm)." Comma after "elevated".

Line 8 delete "that"? What do you mean by "strong alteration features"?

Lines 9/10 What is the effect of this "common Pb" to the ages and its credibility?

Line 10 comma after "elevated"

Line 11 Text should be shifted to the left.

Line 12 BLAS 1 is not the only grain that has the feature of young ages in the core. This is an issue for several grains. However, it is not discussed anywhere. It must be discussed, otherwise the reader will think that your method does not work.

Line 13 What means "minor signs of alteration"?

Line 14 What means "shows signs of alteration"? Which ones? I do not understand or see what you mean by "the zonation is diffuse in places like the centre and part of the rim".

Line 15 I do not understand what you mean by "primary zonation is cut in places". What means primary zonation? Are there examples of "secondary zonation"?

Line 16 Text should be shifted to the left. What are "altered zones" and how do I detect them?

Line 17 What means “featureless”? I see many features… and you state that they are altered areas. How can you see alteration, if there are no features?

Line 18 What means “oscillatory-complex zonation”? To me, I would expect that zoning is either oscillatory or complex. In addition I see no oscillation in figure 5a.

Line 19 What “feature” are we talking about? I do not understand what you mean by “secondary zonation”.

Line 20 Text should be shifted to the left. What “alteration features” do you mean? If you talk about “features”, does this always refer to alteration?

Line 22 What do you mean by “diffuse pattern”? I see only areas of homogeneous colour.

Line 23 Not clear what you mean by “sector-like” and by “complex zonation”. These terms have not been defined elsewhere.

Line 24 Text should be shifted to the left.

Line 25 The description says nothing about the alteration. Where do I see alteration and is this alteration interpreted to fall into this category?

Line 26 What do you mean by “diffuse pattern”? Does this concern zonation or alteration? I do not understand, why DUTH 3 is in category 4.

Line 27 For this sample you state “strong zonation in the altered parts”. What kind of alteration do you mean? Up to here, the reader has the idea that zonation is something that has got to do with monazite growth (i.e. it should be a primary feature), but here zonation seems to form out of alteration…

Line 29 It should be noted that this sample is not found in figure 5, but in the appendix (see table 2). What do you mean by “remnants of sector zonation”?

Line 31 What do you mean by “diffuse internal structure”? If diffuse, how do you know that this sample has to be put into the group of “weak zonation with strong alteration features”?

Line 32 Suggestion: “This may lead to …

Line 32 Shouldn’t it be “<3300 ppm”? For this grain, the authors do not describe the internal features, but only discuss the

Pages 14

Figure 5 In the following, I want to illustrate why I have so strong difficulties in understanding the authors’ choice of colour grouping among the spot analyses and derivation of weighted mean ages. We need clear rules on how the distinctions were made and how then the spots for the weighted mean ages were chosen. Since the authors do not (as required in the first review) provide any clue, how the grouping and weighted mean age calculation was done methodically, I tried to find any hints to understand. In the following I formulate many questions to illustrate why I do not understand your method. Graphically: The mean age boxes are too small to read.

Figure 5a Why are the 7 orange spots not grouped to another weighted mean age?

Figure 5b Why are the yellow spot analyses not in an ascending order (right diagram)?

Figure 5c How do you separate chemically between yellow, green and blue sports (could e.g. spot 12 be blue or 13 be yellow)?

Figure 5d This sample and your choice of grouping are transparent!

Page 15

Figure 5e Why are the red spot analyses not listed in ascending order (right diagram)?

Figure 5f Why is the spot with the highest U content green? Why has spot 29 not a blue colour?

Figure 5g This sample and your choice of grouping are absolutely clear!

Figure 5h Why has spot 20 been discarded? Why did you not calculate a weighted mean age for the 6 yellow spots? Why no mean age for the four red spots?

Page 16

Figure 5i Why are spots 3 to 5 not blue (in particular 5)? Why are these three excluded from the weighted mean age for the yellow samples? If they are excluded, shouldn't they have a different colour?

Figure 5j Why is spot 15 not blue?

Figure 5k Why is spot 3 not blue? Why is spot 14 not included in the weighted mean age calculation of the yellow spots?

Figure 5l Why are the spot analyses 3, 14 and 16 discarded from further age calculation? Why is there no weighted mean age for the orange spots?

Page 17

Figure 5m This figure has a mess between the BSE image and the diagram to the right. Spot 6 is red in the image, but yellow in the diagram. Spot 7 is blue in the image, but red in the diagram. Spot 10 is yellow in the image, but blue in the diagram. Spots 19 and 20 are blue in the image, but red in the diagram…

Figure 5n The label for spot 2 is shifted to the left in the diagram to the right. The here quoted weighted mean age was not found in figure 7

Figure 5o Why is spot 5 red and not blue? Why do you not calculate a weighted mean age for the blue spots?

Figure 5p Why is spot 8 not yellow? Why are the red spots not in ascending order in the diagram to the right? Why are the spots 9 and 10 not a separate group?

Page 18

Figure 5q How do you explain, why the yellow spots (distributed all over the crystal) form one group and e.g. spot 6 does not belong to this group? Why are the red spots not in ascending order in the diagram to the right? Why haven't you calculated a weighted mean age for the orange group?

Figure 5r Why is spot 13 not yellow? Why is spot 19 not yellow? Why is spot 20 not yellow? Based on what argument do your define the green group?

Figure 5s Based on what do you separate the blue spots? Why are the spots 20 and 24 not yellow?

Figure 5t Why don't you define a weighted mean age for the orange group? Why not for the yellow group as well? Why are the spots 29 and 32 not red?

Page 19

Figure 6 Nice figure!

Figure captions Lines 3/4: Suggestion: "Note that rock-forming monazites give considerably higher T-max than…"

Line 2 “occur typically in a temperature window of…”: My figure above shows that this is not true (and this is also visible in figure 6), if you really want to compare with thermochronology data. I see the fissure temperature relatively separate from the temperature in the surrounding rock. One part of the “disequilibrium” that occurs in fissures is the fact that the surrounding rock has a lower temperature and the fissure fluid will be cooled quickly and therefore precipitate minerals out of the fluid.

Line 3 “independent of the local cooling rate”: Interestingly, this statement comes after quoting references. So what is the reference to this statement?

Line 4 Suggestion: “are slightly older or equal to the ZFT ages”

Lines 6/7 Delete “based on Steck et al. (2013)”, as this reference is already quoted in the figure caption.

Line 7 No brackets needed for “Fig. 6”

Line 8 “starts to crystallize” is wrong for the rock-forming monazite, as the latter are crystallized much earlier, but their isotopic system closes earlier than the fissure monazite.

Line 11 I disagree. The authors assume that the fluid temperature in the fissure is always in equilibrium with the temperature of the surrounding host rock. This definitely is not the case.

Page 20

Lines 1/2 I disagree. If you look at figure 5, some fissure monazite ages are even in the range of AFT ages.

Line 2 The “(1)” is not necessary (also delete “(2)” in line 5 and “(3)” in line 10.

Line 3 space before “2011”

Lines 8/9 Perhaps one may here refer to figure 6, where the jump in cooling ages is visible. I agree with the authors that the jump get much smaller toward the south, but here data are two variable to definitely state that there is no jump.

Line 9 Suggestion: “across” instead of “along”

Line 14 “exhumation/cooling rates were low”: What means low? Can you quantify? The Alps exhume low with respect to Taiwan or Scandinavia, but there are still faster areas than others…

Line 15 I suggest to shift the references to the end of the sentence.

Lines 16-20 What I am missing is a counter example. Stating that this is true for areas of slow exhumation should be proven by a case, where it is different and exhumation is fast. The absence of evidence is not the evidence of absence…

I turn your argument around: If the fissure monazite ages are close to the K/Ar ages of white micas, and we assume that fissure formation only occurs in a brittle regime (i.e. below 300 °C, where quartz becomes brittle, then having similar age this would rather mean fast cooling from white mica closure (at 350°C) to fissure formation (not earlier than at 300°C).

Line 24 Do we talk about a spot age or a weighted mean age? From here on, this should carefully be distinguished (as stated on p. 12, lines 12/13).

Lines 27/28 Again: Do we talk about spot ages or weighted mean ages?

Line 31 Suggestion: “age record with the oldest ages being diachronous …”

Line 33 Delete “After this,” and start sentence with “The age record…

Page 21

Line 5 Be more precise: "… at the SW edge of the …"

Line 5 Suggestion: "separated from the Western region

Line 6 Suggestion: "shows an age range similar to the eastern region". Why is it here "eastern", but one line above "Western"? Upper case or lower case?

Lines 8-10 I am missing here the age jump across the RSF, furthermore the possibility of an age offset across the Forcola fault. Your figure 6 also suggests a "jump in cooling ages" across the Forcola Fault, not only the RSF.

Lines 11-33 From here on, all the regions are discussed again, and it is not obvious why. This part seems to be repetitive and I would suggest to combine it editorially with the former paragraphs. First discuss the single regions, then discuss the general trends.

Line 16 "Forcola" instead of "Forcoloa"

Line 17 Rather than only quoting the method "AFT", you should also quote the age.

Line 18 "late circulation of hot fluids": Here the possibility of hot fluids circulating is mentioned, but this is all the time the case. Fissures fill because hot fluid enters the fissures and get cooled and this causes precipitation. Why is this only mentioned here? Simply because this far outside of the temperature range quoted e.g. on page 20, line 17 (and twice before)?

Line 20 Suggestion: "and then runs …"

Line 20 What does "runs parallel to that of …" mean? "Parallel" with respect to what? If you mean "cooling", then state that "it cools with the same rate than…"

Lines 22/23 Suggestion: "After this time, temperature conditions must have decreased due to exhumation …"

Line 23 "around 16-15 in the Gotthard nappe". Can you add a reference? What ages do you refer to?

Line 25 You are referring to ZHe ages of Price et al., but it is not clear what area you are referring to. Price et al. have not produced ZHe ages from the Lepontine Dome nor from the Gotthard massif (northern Adula nappe is not within the Lepontine Dome).

Line 26 I am not sure how the Price et al. data can date the "decoupling of the Gotthard nappe". Your "regions" are combining the northern Lepontine Dome with the southern Gotthard nappe (Figure 7, thus they are treated as being "coupled"). If in the end, your data sets show a difference between Lepontine Dome and the Gotthard nappe fissure ages, then you should show this in figure 7 and revise your data regions (which, as a result out of your dating, would be ok).

Line 28 "until" instead of "to"

Lines 28/29 "as the samples … 9 Ma (Fig. 7d)": I do not see the link. If it was fast, why should it show continued age spread? Needs clarification.

Lines 30/31 Suggestion: Start sentence with "In some areas it records a change from…"

Lines 33/34 In this statement you argue with the direction of the opened fissures/clefts. However, you do not provide any information about the fissure/cleft direction of your samples (see e.g. tables 1 and 2). Thus, this discussion looks odd to the reader: Why should we discuss something, we have no information on? In addition, I got lost in these two lines. Is there something missing? Perhaps you have to simply the long sentences.

Page 22

Figure 7 Figure has been much improved. I found two errors: Sample TAMB 1has two ages in figure b, but the younger age is erroneously again quoted in figure c. In addition, the mean age of BLAS 1 is missing in figure c.

Figure captions.. Line 1: What means “age”? Spot age or mean age? Line 4: Suggestion: “that could be identified within the corresponding grain. Ages are ordered into six time intervals.” Otherwise, one may misinterpret that you have ordered your spot ages according to age and then calculated weighted mean ages…

Page 23

Line 2 The reference should be „Figs. 7e-f”.

Lines3-5 I note that the different ages are quoted with different precision: 11 Ma (based on three spot ages of 12, 12 and 10.5 Ma, should you therefore rather quote an age range?), 16.8 Ma and 11-10 Ma. In addition, the 11-10 Ma is based on spot ages that range up to 11.5 Ma (Fig. 5d). Be more careful with the numbers you are quoting, and be evenly precise when you quote them.

Line 8 Can you be more precise with the reference? Which part of figure 5? Which sample?

Lines 10-24 Again, there is a repetitive paragraph that again runs through the same results. There is basically nothing new that comes in this paragraph.

Lines 10/11 I do not know what the statement “is related to the exhumed deeper part of the Simplon Fault” means. What is the “deeper part of the Simplon Fault”?

Line 11 The reference ”Hartel and Herwegh 2012” is missing in the reference list.

Line 12 If you talk about a “switch in deformation style”, then you should also state from which to which state the system switched.

Line 19 “where strong late-stage hydrothermal activity occurred”: What means “strong”? Can you add a reference to this statement?

Line 21 “indicate a resetting …”. Do you mean a local resetting? This would be in line with hydrothermal activity…

Lines 21/22 “These phases of strike-slip deformation”: This comes with surprise that you talk about a direction of fault movement. Where does this information come from? Is the direction of motion important to this study? Why is hydrothermal activity related to “strike-slip”?

Line 28 “regional” instead of “regionsal”

Line 31 “record” instead of “records”

Line 32 The here again quoted (and to my taste wrong) temperature range is not a result of this paper. You show yourself in figure 6 that the range is larger and discuss it on p. 21, lines 17-19.

Page 24

Lines 1-6 Here again, similar to the Discussion chapter: This part is repetitive to previous statements in the summary. This part could be shortened.

I have not checked the appendices and the references, as the major revision of the paper will also imply serious changes in these parts.

Meinert Rahn, August 15, 2019