Dear editors, dear reviewers,

I would first like to apologize for the chaos concerning the figure numbers and figure references in the text of the first submission, and the understandable confusion and frustration this caused during review of the manuscript. The problem was not due to a last minute rearrangement of the figures and I would like to give a short explanation how it happened:
The figures 3 and 4 were supposed to be continuing over two and three pages, respectively. Unfortunately, I did not notice during my final review before submitting the pdf, that the LaTex command I normally use for this did not work. This resulted in figures 3 and 4 ending up being figures 3 to 7, and pushing back any subsequent figures by three numbers. I noticed the strange figure numbers referenced in the text and eventually ended changing them by hand, but unfortunately did not realize that the problem were not the references to the figures, but the figure numbers themselves. The problem could be solved and the numbers and references are now in order.

All reviewer comments were taken into careful consideration and implemented to the best of our ability. This resulted in a near complete reorganization of the manuscript, with the greater part of the text rewritten or newly added, including changed and new figures. There are now several chapters that discuss the basis of the study approach as well as the way of interpreting the data. One of the new figures combines backscatter images with age data, adds chemical plots, and should thereby make the reasoning behind the age grouping better visible to the reader.
The discussion is now more focussed. The study results are highlighted and the discussion split into two chapters for better readability. One chapter compares hydrothermal monazite dating to thermochronometers, while the other discusses the results in a regional geological context.

We hope that the improved and restructured version of the submitted manuscript meets your aproval and look forward to any further suggestions you may have.

Sincerely yours,
Christian Bergemann

Comments reviewer 1:

Review of
Constraining metamorphic dome exhumation and fault activity through hydrothermal monazite-(Ce)
Christian A. Bergemann, Edwin Gnos, Alfons Berger, Emilie Janots, Martin J. Whitehouse
by Meinert Rahn

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 adjacent to it). These ages are used to decipher the exhumation and tectonic history of the Lepontine dome, as the ages are compared with other geochronological data supposed to represent the Neogene cooling history.
To me, there is no doubt that the provided data are interesting for publication in Solid Earth. However, for the moment the manuscript and submitted material has for the moment several critical shortcomings that I would recommend to fix prior to becoming acceptable, as I consider them critical, if the paper wants to have the impact the presented topic deserves and the general title promises. My major concerns are the following:

1. The title of the manuscript suggests that the monazite data provide new constraints on the tectonic and exhumation history of the Lepontine dome, while the discussion of the data mostly refer your data to already existing constraints of the dome exhumation and Tectonics. As such the focus of the paper is more on methodical aspects of monazite dating (e.g. monazite formation temperatures, relationship to other dating techniques and their closure temperatures).

2. There is throughout the paper a mess with the figure numbers. My assumption is that the authors may have changed these numbers shortly before submission of the manuscript. I invite the authors to check carefully all figure numbers when revising their submitted material. I also note that at several occasions the authors refer to figure 1 in the appendix, which I was unable to locate.

3. For the moment, the chapter ñResultsò is ultra-short and lacks important information. In your discussion chapter, you tend to describe your results at several places, which should be done in the ñResultsò chapter. The ñResultsò chapter should also be used to clarify, what data you will discuss in the ñDiscussionò chapter and which data will not further be discussed.

4.
a.My major concern is that the authors are rather vague with their methodical descriptions. Some of these details should be part of the ñIntroductionò chapter, of a new methodical chapter or part of the ñResultsò chapter. Let me summarize this in five points that I would expect the authors to provide more information about: a. The authors talk about the ñmonazite stability fieldò (e.g. p. 13, line 3; p. 15, line 6; p. 17, line 20), however, they never discuss, what they mean with ñstability fieldò. Note that the authors on p. 12, line 5, talk about ñdisequilibriumò, without clarifying what kind of ñdisequilibriumò they refer to. I would assume that this is not a ñthermodynamic stabilityò, but they rather consider a kind of temperature window, in which the cleft monazites were formed. If correct, it might more correctly speak about the ñmonazite

formation temperature windowò. This aspect is important, because in the ñDiscussionò chapter you compare the formation of monazite with the closure temperatures of lowtemperature thermochronology methods (which seems to suggest some kind of closure-T for cleft monazites).

b. The authors present BSE images for each on the investigated monazite crystals (their figures 3 and 4). However, it remains unclear what the visible colour changes mean within each individual crystals (no chemical data are given except for a few selected elements in the supplementary data file) and how the authors have chosen their analytical spots on these crystals. The only information is that the authors state that they have placed the SIMS spots were placed ñaccording to compositional domainsò (p. 3, line 29). Accordingly, we would expect that spots of same colour rings in figures 3 and 4 would always represent areas of same gray colour in the BSE image. This clearly is not the case for e.g.in the DURO1 crystal the yellow spots seem to only roughly follow a lighter lamella, but overlap with darker areas around, in the DUTH2 crystal the orange spots lie within a lighter rim, but spread into the darker centre next to it. The authors have to state clearly their criteria in how to assure that spots are not mixtures between to different generations of monazite formation.

c. The authors state that they have avoided measurements next to cracks and holes (p. 15, line 30). This statement is in contradiction to e.g. the red spots in BETT11, the blue spots in VANI6, the red spots in VANI5 etc. I assume that the criteria is more likely defined by the analysis itself showing a deficit in elements rather than the geometric vicinity. The authors have to clarify this issue.

d. The authors have to clarify on the basis of which criteria they have chosen the weighted mean ages out of the spot analyses. In Figure 5a (VANI6), it seems obvious that the orange group weighted mean age is formed out of all orange spots. Agewise, however, these spots seem to overlap with the gray spots. So, how have the authors separated between orange and gray? In figure 5b (BETT11), the four red spots show age overlap, but they are not combined to one weighted mean age. Why not? In figure 5c (DURO1), the four blue spots form a weighted mean age, but the gray spot next to it (same age) is not part of it. Why not? I could continue the same way for most of the diagrams in the figures 5 to 7. I am sure that there are good reasons for the authorsô choice of the weighted mean ages, but for the moment, this choice cannot at all be assessed by the reader and looks very arbitrary, not scientifically founded. The authors have to explain to the readership their selection criteria, and for such purpose, it may be needed to better illustrate the different compositional variations among the individual monazite analyses.

e. According to figure 1, there are three age groups (with some samples showing more than one). In figures 5 to 7, however, the authors have several samples with more than two weighted mean ages, in figure 8, the three age groups are no longer visible, and in your discussion chapter, you discuss a much finer distinction among the age groups (see also figure 9). We would recommend to the authors to clarify this issue of age groups in an early stage (e.g. in the results chapter and then stick to it throughout the entire discussion chapter. For the moment, the reader gets lost due to the many age groups and the inconsistency between the figures.

f. Figure 8 shows the ages again, but in probability density plots. Up to here (in particular in the figures 2 to 7, the reader has gained the impression that single spot

data are clustered to weighted mean ages. Here, however, the authors seem to have split the ages again in single spot ages to form new curves and density plots. The same is true in the ñDiscussionò chapter on pages 15 and 17: Sometimes, the authors refer to single spot ages and sometimes they refer to weighted mean ages. I do not understand why the authors refer to weighted mean ages at all, if they afterwards selectively use the information that fits best their arguments. The authors have to clarify their strategy in interpreting their results. They have to clarify the meaning of their ñweighted mean agesò in that sense. They also have to explain how uncertainties were calculated for the different types of ages.

g. Figure 8 shows a kind of clustering of the single spot ages. In this plot the authors also show previous literature data (in gray), but these are not included in their clustering pattern (we do not know, whether this is the case for the curves in the inset below). In Figure 9, however, their interpretation includes all the literature data (e.g. for the Gotthard nappe and the Aar Massif). This is inconsistent. Either you use all data or you do not. The authors have to lay out their strategy on what data are to be interpreted and then stick to it.

h. Figure 2 shows nicely how the authors divise their samples into regional groups. However, in the ñDiscussionò chapter, their division seem to not make sense in many respects as they tend to again subdivide their division. I make two examples: (1) On p. 15, line 19, the authors refer to ñthe entire (north)eastern region that seem to act differently than the rest of the region. This ñsub-regionò is not well defined. (2) Figure 2 places sample DUTH6 to the edge of the ñCenterò region, but in figure 9, this sample rather behaves like the samples in region ñWestò, so why DUTH6 is part of the ñCenterò area? i. In chapter 5.3, the authors compare their data with data from other thermochronometers. However, this comparison is incomplete in that sense that sometimes ages are quoted, sometimes not, sometimes the authors only refer to the interpretation of the previous workers without referring to the geochronological evidence. This should be done in a more careful, systematic and transparent way. I recommend e.g. that they authors clearly state what time and methodical information they use for their discussion (e.g. they refer to K/Ar ages, ZFT and ZHe ages, but they do not use AFT or AHe ages.

j. The ñDiscussionò chapter starts with an interesting subchapter on hydrothermal monazite crystallisation. This is exactly the information needed to understand methodically the authorsô strategies. However, as far as I understand, this chapter is not a ñresultò but a initially chosen ñstrategyò on how the monazite ages are to be interpreted (it looks therefore misplaced in the ñDiscussionò chapter). The authors should somewhere clarify their strategy of the understanding on how monazite is formed.

5. From the title of the paper, the reader expects some new information about exhumation and tectonics within the Lepontine Dome. However, in such respect, the ñDiscussionò chapter has been disappointing for me. The authors support existing cooling/exhumation paths and tectonic events, but they have no courage to suggest any new ñeventsò. I agree that the paper title could be understood as ñConfirming metamorphic dome exhumationò, and I also agree that the problem with monazite dating is the fact that the ages cannot be related to a temperature value (closure temperature) in contrast to other methods. Nevertheless, I also see potential about

the information of the monazite ages that the authors seems to keep untouched. What e.g. is the function of the Rhone-Rhine line (e.g. in figure 9e, f)? Where do the new results show an extension of previous time windows or a focussing on smaller windows for existing phases of tectonic activity? In the end, the ñDiscussionò chapter does not seem to provide any new information. Looking through these comments (and the detailed comments below) I would recommend to the authors to thoroughly revise their manuscript (major revisions). For me, there is no doubt that this study would be an excellent contribution to Solid Earth. However, for the moment, publication of the extensive data set would fail to gain credibility among the readers, because so many methodical details are only vaguely described and therefore lack credibility.

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 grammar or English style corrections, this reviewer confesses to not be a native English speaking person; accordingly, suggestions concerning grammar and phrasing are to be seen as suggestions. I also highlight, if a comment is thought to be a suggestion (for e.g. clarifying a statement or shortening the text).

Page 1

Title The authors always refer to ñmonazite-(Ce)ò. This is ok as the IMA points out that monazites could be dominated by different REE in their formula. However, the authors never confirm that Ce is the dominating RE element in the formula of their 19 samples. The paper has no compositional data except for those in the supplementary data set. I assume that much of the division of the analysis spots (figures 3 and 4) is due to compositional arguments. However, the reader has no chance to assess this division. The reader has not even a chance to find out, whether the ñmonazite-(Ce)ò in the title is correct or not.

Title Looking back to the ñDiscussionò chapter, this reviewer has rather the impression as if the authors do not ñconstrainò but only confirm previous information on the exhumation and tectonic history of the Lepontine Dome. I suggest to thoroughly revise the manuscript and then decide on whether to change the title of the paper. The same might apply for the ñAbstractò.

Title The term ñhydrothermal monazite-(Ce)ò is varied in the manuscript. Sometimes, the authors refer to ñcleft monazite-(Ce)ò, sometimes to ñhydrothermal cleft monazite-(Ce)ò, and the reader gets puzzled about the different expressions. It would help to clearly state somewhere in the manuscript that these monazites all come from clefts. I am not so happy about the term hydrothermal as it suggests that these monazites have formed in a flush of ñhotò fluid, the temperature of which might not have been in equilibrium with the surrounding rock. If so, any later comparison with closure temperatures of thermochronologic systems may not be useful, as the temperature of the monazite formation may have been completely different from the temperature controlling closure of other geochronologic methods.

Line 2 The authors refer to the ñCentral Alpine Lepontine metamorphic domeò, which is a rather unusual term. First, figures 1, 2 and 9 show that the study area does not cover the south of the dome (along the Insubric Line), but its entire E-W extension

(so, ñCentralò may not be needed). Second, the Lepontine Dome is an Alpine structure, so, I would suggest that there is no need to use the term ñAlpineòhere.

Changed

Line 4 Suggestion: delete ñTheòat beginning of sentence

Deleted

Line 5 There seem to be a double space between ñbetween and ñ19ò

This was due to an error in the command for ~, that was now replaced in the entire document

Line 6 The authors use the abbreviation ñMaò (= mega annum) for both, specific moments in time and timer periods. Please check the instructions for authors that suggest to use ñMaò for time spotsò, however, ñMyrò for time periods. I fully support such instruction. Accordingly, the authors should write here ñlifetime of 2 to 7.5 Myrò

This was changed

Line 6 Suggestion to put ñcombined with age distributionòinto brackets.

Changed

Line 9 Suggestion: ñIn the east and south of the northern Lepontine domeò

Changed to "In the north-east and south-west of the Lepontine dome"

Line 9 Suggestion: delete ñtheòbefore ñunitsò

Deleted

Line 9 Suggestion: add ñ, respectively,òafter ñMaò

Added

Line 10 Suggestion: ñat 15-10 Ma. Cleft monazitesé ò

Changed

Lines 10/11 There seem to be a mismatch with the statement ñyoungestòin line 10 and ñA last phaseò in line 11. ñYoungestò means ñlast phaseò, thus these phase should be the sameé Furthermore, I would suggest using the term ñage signalò

instead of ñphaseò as the latter already suggests that this has been a distinct tectonic or thermal ñeventò

"Youngest" was changed to "younger". "Phase" was changed to "age group"

Line 11 Suggestion: ñalong the Simplon Faultò One further note: If you quote names of domes and faults, make sure that they are constantly with upper case or lower case. So, either Lepontine Dome or Lepontine dome, Simplon Fault or Simplon fault.

Lines 11/12 Add hyphen: ñstrike-slipò

Added

Line 12 Suggestion: ñfaults along the Rhine and Rhine-Rhone faultsò

Line 13 Do you mean ñstabilityò or ñformationò?

Line 13 ñdirectlyòinstead of ñdirectò

Changed

Line 15 ñexperienceòinstead of ñexperiencedò

Changed

Line 16 Suggestion: Start line with ñFor the Lepontine Dome, this evolution is an interplay é ò

Changed

Line 16 Suggestion: ñmotion alongòinstead of ñactivity ofò

Changed

Line 16 Suggestion: ñdefineòinstead of ñdominateò

Changed

Line 17 Suggestion: ñthe western edge of the domeòinstead of ñthe western parts of the areaò

Changed

Note that throughout the text, you frequently use ñin this areaò, but you are not very specific, what area exactly you are referring to. I would expect to get some references here.

Line 17 The statement ñAlthough much of the (thermo)chronological history of the area is well knownòis rather cryptic. Does this refer to the fact that there are a lot of data concerning the post-peak metamorphic history? Or does this mean that the tectonic history is known in detail? Since you do not provide any references and the statement is rather broad, the reader cannot assess the meaning of this statement.

Line 18/19 Interestingly, here you refer to AFT and AHe ages, while in the Discussion chapter, you do not refer to any of the existing studies including such data and you do nowhere cite any of them. This is ok with me (as e.g. AFT and AHe ages may already be influenced by topographic evolution. Nevertheless, there is a mismatch between this statement and the discussion of your data afterwards. Finally one suggestion for a rewording: ñexisting cooling ages of the Rb-Sr, fission track (FT) and (U-Th)/He systemsò

Good point, this was left over from an earlier version. The references were taken out for exactly that reason, the sentence has been changed now to avoid confusion.

Line 20 Suggestion: ñ(e.g. Parrish, 1990). It is highly resistantéò

Changed

Line 21 ñby diffusionòinstead of ñthrough diffusionò

Changed

Line 22 not clear, what ñgeologically reactiveòmeans

Changed to "Nonetheless, monazite remains reactive after crystallization, as it can experience dissolution-recrystallization facilitated through hydrous fluids..."

Page 2

Line 1 ñbyòinstead of ñthroughò

Changed

Line 1 What do you mean by ñmediationò?

Changed to "facilitated through", meaning that the monazite remains susceptible to these changes as long as the cleft remains fluid filled and within a certain temperature range.

Line 3 Suggestion: ñoccasionally contain monazite-(Ce). They represent voidsé ò

Changed

Line 7 Suggestion: ñwereòinstead of ñcould beò

Changed

Line 11 Suggestion: ñé tectonic activity. Accordingly, fissures and clefts are é ò

Changed

Line 13 ñinteractedòinstead of ñinteractsò

Changed

Line 13 Suggestion: Start new sentence with ñDissolution and precipitationé ò

Line 14 Suggestion: ñled toòinstead of ñcausingò

Changed

Line 18 Suggestion: ñby using secondary ion mass spectrometry (SIMS)ò

Changed

Line 19 At the end of this introduction, I miss a paragraph that presents the aims of this study. I addition, I miss an introduction to the methodology of monazite dating, not the technical issues, but the prerequisites that define the strategy of the study.

Such a paragraph was added and further explanations added in a later chapter.

Line 22 Add ñEuropeanòto ñAlps

Added

Line 23 This bracket lists examples of metamorphic domes, but their allocation is once to a country (ñAustriaò) and once to the part of an orogeny (ñwestern Alpsò). I would suggest referring either to Switzerland/Austria or to eastern and western Alps.

Changed to "Eastern Alps"

Line 25 The references listed here have a strange ordering, either alphabetic or with decreasing age. Please check the authorôs instructions.

This was adjusted

Line 26 The statement that the Western and Central Alps had a ñcomplex tectonic and metamorphic historyò is not very convincing. ñComplexò with respect to what? Orogens tend to be complex anyway (fortunately, this keeps us going to find out more details of this history!), but the statement is too vague, too general. You may start the chapter with such a statement.

Furthermore, I refer to the detail that Schmid et al. (2004) did not use the term ñCentral Alpsò, but divided the Alps into western and eastern Alps.

True, there is no consistent terminology in this case. If consulting for example A. O. Pfiffner's "Geology of the Alps" (2014) there is the division into Western, Central and Eastern Alps.

Line 31 You may put ñin excess of 650°C in some regionsòinto brackets.

Done

Page 3

Line 1 not clear, what you mean by ñstaggered exhumationñ, I am doing research in the field of exhumation for 20 years, but have never heard of this expression.

Staggered was used here in its meaning of one process starting somewhat later than the other. To avoid confusion, it was replaced by "exhumation starting first in the Ticino and then the...".

Line 1 I would recommend to add references to this statement

Line 4 Suggestion: ñlater in time at 18-15 Ma

Changed

Line 5 I would recommend to use the term ñnormal faulting, rather than ñdetachmentò, as the term ñdetachmentòhas no direction.

Changed

Line 9 Here, the term ñSimplon Fault zoneò is used, but in line 11 ñSimplon shear zoneò. If these terms describe different zones, then the authors should explain the difference.

Line 10 ñAlpineòinstead of ñalpineò

Changed

Line 14 From figures 1 and 2 it is evident that the study area is the northern Lepontine dome. Nevertheless, the exhumation of the Lepontine Dome cannot be described without the Insubric Line. (15 km of differential vertical exhumation!). The authors should consider including the Insubric Line in their ñGeological settingò

A sentence was added here

Line 15 ñareaòinstead of ñAreaò

Changed

Line 16 Suggestion: ñThe study area comprises the northern half of the Lepontine é ò

This only true for the eastern part of the Lepontine, while the west is completely covered, leaving out the south and south-west. If it is preferred, the statement could be changed to "... the western and northern parts of..."

Line 17 ñForcola Faultò

Changed

Line 17 The ñVal dôOssolaòis not visible in any of the figures.

The place name was removed from the text to avoid cluttering on the map.

Line 18 Suggestion: ñAar massif to the north (see Fig. 1 for the tectonic position of the samples)é ò

Changed

Line 19 Should not Janots et al. 2009 be added to this list, as it also contains monazite ages?

Janots et al. 2009 was omitted, since it contains ages of metamorphic monazite instead of hydrothermal. However, due to changes in the text later on to avoid confusion, the references to Janots et al 2012 and Bergemann et al 2017 were removed here, since the samples are discussed, but sampling sites lie outside the Lepontine dome.

Line 20 The statement that the four groups correlate ñto tectonic subdivisionsò cannot fully be assessed by the reader: First, there is no tectonic subdivision between central and western part (the Ticino and Toce domes are separated by what tectonic feature?). Second, In figures 1, 2 and 9, the Forcola Fault ends south of the study area, and if the authors refer to the extension of the Forcola Fault to the north, the sample VALS would be located in the central part. How about separating along the western rim of the Adula nappe?

Changed

Line 22 ñboundò instead of ñboundedò, as it is related to ñto bindò

Changed

Lines 23-25 I would shift the last sentence to the beginning of the next chapter.

The sentence was moved

Line 28 Note that there are different spellings of ñbackscatterò. In figures 3 and 4, you write ñback-scatterò.

Changed in all cases to "backscatter"

Line 28 Suggestion: ñBackscatter electron (BSE) images were used to define spots suitable for analytical investigations.ò It is, however, unclear to me, how you defined the different groups (of different colours) and how you have chosen the analytical spots. This needs more information.

Explanations were added

Line 29 The statement ñaccording to compositional domainsò cannot be understood without further information: How do you define a compositional domain? What compositional data were available? I refer to the fact that a change in RE element (but otherwise constant composition of the monazite) may not really show a difference in colour in the BSE imageé It would be helpful to know what analytical data were available, and which of these data were used to ñdefine the compositional domainsò.

This was an error, the sentence should have stated "according to visible domains" and has been corrected. These should represent differences in concentration of the heavier elements.

Line 30 Suggestion: ñAs far as possible, spot measurements next to cracks or holes were avoidedò.

Changed

Line 30 Looking at e.g. figures 5a, 5j, or 5l, this statement cannot be understood. In the corresponding crystals, there are measurement spots directly next to cracksé

That is unfortunately, despite best efforts, not always avoidable. The grains are gold covered for the SIMS measurements, due to the large grain size not visible in their entirety when placing the measurement spots, and viewed at an angle due to the machine construction. Smaller cracks and inclusions may therefore be covered up and orientation points lie outside the visible area during measurement spot placement. If the data appears undisturbed, the measurement is still used.

Line 31 Suggestion: ñin such areasò

Changed

Page 4

Figure 1 This figure is not yet finished. First and foremost, there are no coordinates, there is no scale and there is no north direction. I would recommend to enlarge the map on top (a), and enlarge the labels (b) to (e). The legend contains the abbreviation ñmztò that is not explained in the caption. According to the thick black line in map (a), the profile (c) should end at the border to the Aar Massif, but in (c), it extends into it. Profile (d) has gray units that do not show up in the map (a). Each of the profiles has its own scale, thus, scales should be added to figure parts (b) to (e). Faults are not labelled. One methodical problem is that the authors in this figureôs legend refer to three age groups, but in the discussion chapter (and in figure 9), there are more age groups. This is inconsistent. Do you need to mark the different groups in this figure already?

The requested changes were made and the reference to age groups taken out.

Figure caption
Line 1: switch the two references, as they are not in correct order, line 4: There is no Wiederkehr et al. 2008 reference in your reference list. Do you mean the 2009 reference? Line 4: Why does the caption uses a different text for profile (e)?

References were switched, the caption text for (e) was made identical to the others.

Page 5

Table 1 In this table, all samples are given names with upper case spelling. However, in figures 1 to 7, the authors sometimes use lower case or upper case. In addition, sometimes, there is a gap between name and number, sometimes not. I recommend adapting the names throughout the paper and figures and tables, and I would use a space between name and number, as several names end with ñò, which then might be interpreted as a ñ1ò For some of the samples, you note coordinates with ñ-ò What

is the difference? If the location of the sample is not sure; I would rather recommend to reduce the digits after the comma of the angle minutes (e.g. sample SALZ2) rather than adding a ñ-ò

The sample names were standardized to upper case with a space between name and number.

Page 6

Figure 2 The frame in the inset does not fit to the area of the large map. Similar to figure 1, coordinates and north direction are missing, the abbreviation ñmztò should be explained, The label of the ñCentovalli Faultòis partly covered with blue colour.

Line 4 Suggestion: *byòinstead of ñwith

Changed

Line 7 delete ñpresentedò

Changed

Line 8 Suggestion: ñreportedòinstead of ñgivenò

Changed

Lines 10/11 Suggestion: ñé pending). Table 2 provides an overview and Figures 3 and 4 show measurement positions and the division of the analysis spots into different age groups, represented by different colours.

Changed

Lines 11/12 The statement ñAs there are difficulties with the U-Pb system for hydrothermal monazite-(Ce)òis very cryptic. What difficulties do you mean? And how have the authors dealt with these difficulties?

Explanations were added to the text.

Line 13 ñsee discussion in chapter 5.2ò

Line 13 I note that the ñResultsò chapter only contains these four lines. I strongly recommenddescribing the results. What should the reader see? The next pages are figures 3 to 7and table 2, and if the reader goes through these many data, there is no guarantee that the reader will come to the same conclusions as the authors about what is important and what is not. As noted in the general comments above, I have rather collected doubts about the usefulness of the chosen strategy of the authors and the credibility in their chosen analytical spots, derived weighted mean ages etc. I would argue that a careful description of the results would help much not losing the reader half way. You should add a description on how the spots have been selected,

grouped in the diagrams of figures 5 to 7, how the weighted mean ages were afterwards grouped. It may help starting with figure 8 instead that shows an overview of all ages.

The results chapter was expanded and the figures combined into a larger figure which now also includes a chemical plot for each sample.

Page 7

Figure 3 Many of the colours of the spots are hard to be distinguished (e.g. figure e). All sample names should be similar to tables 1 or 2. For several sub-figures, the numbers are too small to read (perhaps enlarge figure to full page). You should clarify that this BSE images show all spots made for this study.

A remark that the images show all spots made for the study was added.

Figure caption ñBack-scatterò or ñBackscatterò? (see comment to page 3, line 28). You have to add the information that the grains shown are from the south and west areas only, e.g. ñBackscatter electron images of all studied cleft monazite-(Ce) grains from the South and West areas.ò Line 1: ñcontentò instead of ñcontentsò, ñColoured ovalsòinstead of ñSpotsò

Changed

Line 2: ñdifferent colours indicate different chemical é ò Lines 2/3: ñindicates those data, for which a weighted mean 208Pb/232Th age could be calculated.

Changed

Page 8

Figure 4 For this figure, see comments to figure 3 and figure 3 caption. Again, it should be clarified in the caption that these samples are from the Center and East areas.

The same changes as former figure 3 wered made.

Page 9

Table 2 The caption of this table does not fit. The information given here should be placed elsewhere. Most of the numbers in the ñFigureò column are wrong. Obviously, the authors have changed the figure numbers in a late step and not adapted text and tables.
The last column to the right lists the range of single grain ages. I do not understand why there are empty lines in this column as the range can also be given to those age groups that are combined to a weighted mean age.

The age range given does not correspond to the age groups, but is of the entire sample. To avoid confusion this column was moved.

For sample
ñGRAESER1ò, the number of points for the older weighted mean age should be 6 instead of 5 (see figure 3e), and the second weighted mean age is missing in figure 5e.

Changed

I would recommend to draw lines between the four different areas (South, West, Center, East).

Page 10-12

Figures 5-7 Looking at these diagrams, I have many questions about how you define a weighted mean age. In some cases, all spots of the same colour form such a weighted mean age (e.g. 5a, 5b, 5g). More frequently, the weighted mean ages are calculated for a selection of ages of the same colour only (see e.g. 5b, 5e). In some cases, it is obvious that only one colour bar iw far off (e.g. in 5e, red), but in other cases (e.g. 5b, red), I do not know, why one age has not been included, even though the vertical bars overlap. Finally, there are examples, where age bars overlap, but were not used to calculate a weighted mean age (e.g. in 5e, young yellow ages, in figure 7s, gray bars in the figure centre). The authors have to explain their selection procedure in detail, otherwise there is no credibility to their ages at all.

Explanations were added to the text and the figures combined into a larger figure which now also includes a chemical plot for each sample.

In figure 6p, one vertical bar has a lighter blue colour; why?, In figure 7s to the left, there is a short bar without colour.

Changed

Page 12

Line 2 To me the content of chapter 5.1 is misplaced. The reasoning given here is not the result of the study, but a strategy that the authors have chosen beforehand, as far as I understand. If there are observations, features, analyses that support your reasoning, then they should be presented as results and used to interpret your ages properly. For the moment, the content of this chapter should be transferred elsewhere, e.g. in a new chapter on methodology.

In light of the comments of both reviewers, this seems like a good idea. Accordingly, chapters 5.1 and 5.2 were moved to the methodology section and combined to give a better introduction into the approach used.

If you argue that ñlater reactions may be aided by secondary porosity and fracturing induced by the previous dissolution-reprecipitation/recrystallization eventsò, then show evidence for it. Show what you mean, add another figure like figs 3 or 4 to show corresponding features. For the moment, the entire chapter 5.1 is a black box to the reader, as the authors do not present any evidence for dissolution, reprecipitation, recrystallization. The authors should better define, what they mean with these processes and present examples for it.

The chapters were moved, reorganized and expanded

Line 5 Suggestion: ñlater reactions may occurò

Changed

Line 5 The term ñdisequilibriumò is hard to understand. ñDisequilibriumò with respect to what?

The sentence was changed to state ñ.. brought into chemical disequilibrium with the hydrothermal mineral phases and the surrounding wall rock.ò

Line 5 Suggestion: ñThe may be caused by a tectonic event for a é ò

Changed to "These may be..."

Line 10 Suggestion: ñphase may precipitateò

Changed

Line 12 Suggestion: ñfluid remains connected to a é ò

Changed

Line 12 Suggestion: Start sentence with ñA dissolution é ò as this process has not been defined.

Changed, and the previous sentence was moved to follow this one.

Page 13

Line 2 There is no 2018 reference for Grandô Homme et al. in the reference list. Do you mean the 2016 reference?

The reference was adde to the bibliography

Line 8 The statement ñplaced é according to growth domainsò cannot be assessed by thereader. The authors should show their arguments, e.g. with one example. How

do you define a growth domain? How do you define what is old, what is younger? Do ages from different growth domains fit to the measured ages (same age order)? Have you tested the order?
Line 9 The statement ñon the basis of chemical compositionò cannot be assessed by the reader. What chemical information did the authors gather and what chemical arguments did they use? This information is of major importance if forthcoming studies should apply the same methodology.
Line 11 The reference to ñFig. 7ò should probably be ñFig. 5-7ò

Explanations were added to the text and the figures combined into a larger figure which now also includes a chemical plot for each sample.

Lines 11/12 The statement ñIt appears that dissolution-precipitation may largely preserve the chemical composition of an affected crystal partò cannot be assessed by the reader. Is this an assumption? If not, what is the evidence you found? What are the conclusions drawn from it?

References were added, An explanation on the implications of this was added.

Line 12 ñareas ofò instead of ñareas withò

Changed

Line 13 Not clear, what you mean by ñthisò (ñDespite this,é .ò)

Changed

Lines 13/14 The statement ñonly in a few, clear casesò cannot be assessed by the reader. Which cases? And what means ñclearò?

The text was adjusted

Line 14 Suggestion: ñage calculation, to avoid é ò

Changed

Line 15 Suggestion: ñsingleò instead of ñdistinctò

Changed

Line 18 Suggestion: ñNew growth on an existing crystal results in sharp chemical (or colour) boundaries between growth zones.ò Note however, that this is only the case, if the growth zones differ in composition. Thus, it is an argument, but it does not need to beé

This is indeed only the case if the growth zones differ in composition. It is, however, safe to assume that the fluid composition will change at least slightly in almost every case.

Lines 18/19 I wonder whether this discussion on the dissolution-reprecipitation should be shifted to the chapter 5.1, where these issues are raised already.

Moved

Lines 20/21 Suggestion: Start sentence with ñAccordingly, events of monazite growth may noté ò

Changed

Line 21 Suggestion: ñIf looking at the weighted mean ages only.ò

Changed

Line 21 Suggestion: ñTo avoid age mixtureò

Changed

Line 22 What is the reason for a 0.5 Ma time interval? And use 0.5 ñMyrò (instead of ñMaò),

Changed to Myr

Line 22 Does the content in the bracket mean: ñsee at figure 1 and the appendixò?. Or is there a figure in the appendix (which I have no access to)?

Lines 22/23 Suggestion: ñIn a subsequent stepé ò

Changed

Line 23 What do you mean by ñplateauò? Could the plateau be simply made of several distinct intervals that a time step of 0.5 myr is not sufficient to resolve individual age peaks? Is there geological evidence that would support a plateau due to continuous monazite formation? Can we see this in the BSE images (e.g. continuous colour gradients)?

This argument can be made for all dating techniques applied to any type of mineral grain that underwent stepwise crystallization. With respect to the data presented here, when considering the precision of the individual measured ages an interval size of 0.5 Myr is reasonable. A smaller interval size would indeed likely result in the identification of more age peaks, which would, however, suggest a higher accuracy

than supported by the precision of the data. While it is likely that such smaller events exist, as also argued in the text, they cannot be resolved in time with the currently availabe techniques. Currently, the only way to identify phases that closely followed one another, is through changes in the crystal chemistry.

Line 24 The authors should clarify, whether their interpretation is based on their ages only, on their ages and the already published ages together, or on the weighted mean ages only. From here on, there is mix between all types, which does not enhance the credibility of your interpretation.

This was adjusted

Lines 24/25 The statement ñto visualize distinct events or phases of tectonic activity (Fig. 8) is difficult to understand. First, the visualization is dependent on your chosen time interval (see comment above). Second, you have to clarify for figure 8, what you do with the gray ages (literature data): Are they included or not? If not, why not? Third, you have to specify the different colours used in figure 8, there are colours of different intensity (in the West area three shades, but no explanation).

This was adjusted

Line 25 Suggestion: ñare possible to be obtained for each grainò

Changed

Line 29 ñlistedòinstead of ñgivenò

Changed

Line 30 Suggestion: Start sentence with ñAn alternative reason é ò

Changed

Line 30 I suggest that the authors speak of a ñplateauòif they claim a ñspread out age patternò. The nomenclature should be clarified.
Lines 30/31 Suggestion: ñtectonic activity by multiple smallé ò

Changed

Line 31 ñIn such a case é ò

Changed

Line 31 Do you really mean ñreprecipitateòor only ñprecipitateò?

Both, changed to "(re-)precipitate".

Line 32 Suggestion: ñThis may lead to é

Changed

Line 32 Can you specify more precisely, what you mean by ñunclear crystal zonationsò?

This was adjusted

Page 14

Figure 8 In this figure, there are black and gray data. It is obvious that the gray data were not used for the colour coding of the denser and less dense age areas. The authors should clarify on what data set they base their interpretation. Does the inset (with the curves) show the ages of this study only or all data?

Both, figure and inset, show only the data of this study, as the literature data is from the Aar Massif and the interior Gotthard Nappe and was only included for additional information. After careful consideration, the references and data points were removed from this figure.

The authors talk about the ñcompete data setò. What is the complete data set? What are we expected to see?

"Complete" was deleted to avoid confusion

What is the meaning of the different colour shades? Howe were they defined (what are the boundaries between the shades)?

An explanation has been adde to the figure caption: "Darker colors represent peaks or plateaus, that indicate individual events or phases of monazite formation/alteration due to tectonic activity. Lighter shades indicate fewer ages recorded, due to either only reduced tectonic activity or mixing ages."

(only the West area has three different shades (why?).

This was a mistake and has been corrected to two shades

Figure caption
Line 4: Suggestion: ñfrequencyòinstead of ñumberò. The inset shows a probability density plot, not a histogram.

Changed

Page 15

Line 2 Suggestion: Ătend to show sharper zonationñ

Changed

Line 6 The ñmonazite stability fieldò has not been defined. If the authors talk about a ñfieldò, there should be at least two parameters that define the stability ñfieldò. But I assume that they rather mean a temperature range in which monazite in clefts are formed. It has nothing to do with the thermodynamic stability of monazite.

Line 7 ñ2.5 to 7 Myrò

Changed

Line 7 The bracket should refer to Figs. 5-7 and table 2.

Changed

Line 9 In the bracket I would more specifically refer to Fig. 9a and b.

Changed

Line 10 If the authors start here to separate another area (the ñortheasternò area), the reader wonders why they do not specify such area in figure 2é

Change to "eastern and central"

Line 13 For the sake of completeness: Tony Hurford (in Hunziker et al. 1992) has produced a zircon FT age for the Splügenpass, not far from your sample locality, which is 20. 3 Ma, There are also AFT ages for this locality in Hunziker et al. (1992, 15.3 Ma) and Rahn (2005, 16.9 Ma).

Added to the text and in a new figure

Line 13 Start new paragraph after ñRubatto et al. 2009).ò Start new paragraph with ñAt the same timeé ò

Line 12-18 In this discussion you sometimes refer to geochronological data, sometimes to references that quote time intervals for a specific ñeventò. I would argue that such time intervals are also based on geochronologic data. Thus, the authors may as well quote those data or at least refer to the dating method used. The presentation so far is rather inconsistent.

The discussion was reorganized

Line 14 The sentence starts with ñAt that timeé ò(or my suggested change) but then a time interval of 19-18 Ma is quoted. Is this the same time interval? Is this the relict of a former statement?

Sentence was changed

Line 15 It is not clear, what the authors mean by ñAfter thisò What does ñthisò refer to?
Line 15 Suggestion: ñAfterward, temperature decrease due to exhumation, é ò

Changed to "After this time"

Line 16 The ñValsertalòis not visible in any of the figures. Thus, a reader not familiar with Swiss geography is lost. My suggestion: ñstarted ar 16 Ma near sample VALS (Figs. 2, 7t) é ò

Changed

Line 18 Suggestion: ñbiotite recrystallization ages (Wiederkehr et al., 2009).ò

Changed

Lines19/20 Suggestion: ñafter which the record ends around sample VALS where cooling belowé ò y the way, I refer to the apatite FT age by Rahn (2005), which for this locality is 8.6Ma.

Changed

Lines 20/21 Suggestion: ñThe sample VALS age range of 16-12 Maé ò

Changed

Line 21 I do not understand the ñperfectlyò, in particular if the time interval in line 22 is only ñca.ò

"perfectly" was taken out

Line 23 ñwithin the domeò

Changed

Line 25 ñFig. 6mòinstead of ñFig. 4mò

Line 27 The statement ñas fault gouge ages seem to typically coincide with the end of monazite-Ce) growthò is rather strange. This should be carefully illustrated. If you have a fault gouge, you are in a process of brittle deformation. But to form cleft monazites you have to be in a brittle deformation state anyway. Is this an important statement? If yes, the authors have to show more evidence for it or more specifically

refer to a study that has shown such a coincidence. Here I do not know where ñbelowò is.
Line 29 Start paragraph with ñ40Ar/39Ar cleft muscovite agesé ò

Changed

Line 30 Stop sentence after ócrystallizationòand restart with ñFurther westé ò

Changed

Line 31 reduce to ñ2017, 2019), however, ZFT ages predate é ò

Changed

Line 33 The statement ñsuggests slow cooling ratesò cannot be assessed by the reader. What is the arguments for slow cooling?

A chapter on this was added

Page 16

Figure 9 I like this figure very much. To me, it is the heart of this study. Note that similar to figure 2 (same inset), the frame in the inset does not fit to the chosen map outline.

The inset frame was adjusted

Line 2 of caption: ñquotedò instead of ñgivenò

Changed

Line 2: Since the study area mostly extends in a EW direction, the term ñouter regionò is difficult to understand. Metamorphically, the inner region would be in the South towards the Insubric Line, not towards the Simplon Line.
Changed to " Note the shift over time from the southern and eastern regions of the Lepontine dome to the central and western areas and finally to the areas close to the shear zones bounding its western limit."

Page 17

Line 1 Suggestion: Ăcontinued deformation and monazite formationñ Is this what you mean?
Line 1 The statement ñas the systems closedò is not clear to me. What ñsystemsò are you talking about?
Line 2 The term ñthe lower end of the closure temperature windowò is wrong. The closure temperature sensu Dodson (1973) is the moment when a geochronologic system closes, and for one cooling history, there is only one closure temperature, not a closure temperature window. You may argue that the closure temperature of a

geochronologic system may vary depending on the cooling rate (see e.g. Bernet 2009 for the ZFT system), but then you are talking about different cooling histories. Do not mix the term closure temperature with the terms partial retention (He methods) or partial annealing zone (FT methods).

This part of the discussion was rewritten

Line 4 double space before ñ17ò

Adjusted

Line 4 Suggestion for bracket: ñ(VANI 6, Fig. 5a)ò

Line 5 The authors make a jump in their discussion from S of the RSF to N of the RSF, which is hard to understand and follow.

The discussion was changed

Line 9 ñFig. 9c, dò

Lines 14/15 I do not understand this statement. Next to the RSF, there are weighted mean ages as low as 7.2 Ma (sample VANI 5).

VANI 5 is located in the foot wall and not in the hanging wall

Line 15 Suggestion: ñCorrespondingly, the 12-10 Ma phase also marksé ò

Changed

Line 16 The first bracket should be ñ(Figs. 5c, d and 6j)ò, the second ñ(Fig. 6k)ò

Line 17 Suggestion: along the eastern side of the RSF tend to predate, but still are in agreement withé ò

Changed

Line 18 Suggestion: ñIn the vicinity of sample VANI 6 south of the RSF (Figs, 2, 5a), ZFT ages show a scatter é ò

Changed

Line 20 The statement ñleave the hydrothermal stability field é ò should be avoided. Fact is that you have no younger monazite. This should not be mixed up with monazite no longer being stable.

Changed

Line 20/21 Here you should refer to ñFig. 9e and fò

Line 21 Suggestion to start a new paragraph before ñThe numberò

Changed

Line 21 The ñclear age patterns within the crystalsò remain completely cryptic to me (see comments further above). I do not know, how the authors have sorted out what is a cluster to be combined to a weighted mean age. They have to provide this information in the ñResultsò chapter to gain credibility for their study.

More detailed explanations concerning the dating of hydrothermal monazite were added at an earlier stage of the manuscript.

Line 21 Rather than ñstaggeredò, I recommend using the word ñstackedò or ñclusteredò

Changed

Lines 24/25 The content in the bracket should be ñ(Figs 5b, d and 6j-l)ò

Lines 27/28 I do not know why the authors here refer to results from the Mont Blanc Massif. The only reason might be that they want to point out that there is a tectonic link by the Rhone Line, which starts in their study area and reached west to the Mont Blanc Massif. If yes, they should make this link. Otherwise, I would suggest deleting the entire sentence. Alternative suggestion: ñOverall, the 10-7 Ma time interval é along the extended RSF system, as far as to the Mont Blanc Massif (é ò

Changed

Line 29 Suggestion to add ñFig. 6lò to the bracket.

Line 31 Add reference after ñoccurredò

Line 32 The ages are ñ6.4-5-4 Maò

Changed

The part of the manuscript referred to by the following comments was rewritten to be more focused and thus better understandable.

Line 32 The reference to a ñmuscovite ageò is cryptic. What muscovite has been dated? From the host rock or from clefts or fault gouges?

Line 33 The statement ñof the areaò is cryptic as well. What area?

Line 34 The statement in this line (ñsimilaré ò) is not clear. What does this statement tell us? I do not understand your reasoning. Note, this is your last sentence of the discussion. Do not stop with a loose endé

Page 18

Lines 2/3 Is this first sentence a conclusion out of this study? My suggestions would be to start with: ñHydrothermal cleft monazite-(Ce) provides an important recordé ò You should not use the word ñfissureò here, after having used the word ñcleftò only so faré I would have preferred ñfissureò

Lines 3/4 Suggestion: ñprovides a important record of shifting tectonic activity associated with the regional exhumation history.ò Delete ñwithin the monazite stability fieldòas this is a term that has not been defined.

Lines 5/6 The statement ñthat age clusters within individual crystals from a simple exhuming area have a less clear age distribution than samples from fault zone areasò is not clear to me. First of all, you state that the Lepontine Dome has a ñcomplex metamorphic and tectonic historyò(see above), thus you should not refer to ña simple exhuming areaò Second, this statement, if quoted in the ñConclusionsò chapter, should have been prepared in the ñDiscussionò chapter, but I cannot see such a discussion.

Line 6 Here, the authors talk about ñfast exhuming areaò What is considered to be ñfastò? The authors did not distinguish between slow and fast exhumation before. Why do they do this in the ñConclusionsòchapter?

Line 6 Do you really mean ñrecrystallizationò? I realize that the authors have not explained where the monazites are crystallized and where they underwent recrystallization. Therefore, the statement remains cryptic to me. The authors should carefully define the different processes and explain, what pattern they generate and what this means for the age interpretation.

Line 7 If you state ñin these areasò, the reader does not know where. Try to be more specific.

Line 7-9 These statements have not been discussed in the previous chapter.

Line 10 This is the only place where the authors use the term ñ232Th-208Pb monazite-(Ce)ò, Why only here?

Line 10 double space before ñ19ò

Line 11 The temperature range given here is not a result of this study. If yes, this should be presented with more clarity in your ñDiscussionòchapter.

Line 12 double space before ñ19ò

Line 12 The bracket content should be ñ(Fig. 9)ò

Line 13 Suggestion: ñWithin the Lepontine Domeé ò

Line 14 Here you refer to the ñeastern Gotthard nappeò, but on p. 15, line 17, you refer to the ñsouthern edgeòof ité

Line 14 What do you consider to be ñslow exhumationò (see also comment further above concerning ñfast exhumationò)?

Line 15 The sentence here seems to be incomplete, something does not fit, at least I do not understand.

Line 19 ñSwiss National Science Foundationò

Changed

Line 21 Suggestion: ñin providing monazite-(Ce) material for this study.ò

Changed

References There are several references listed that are not in the text: Frisch 1979, Frisch et al. 2000, Glotzbach et al. 2010, Keller et al. 2006, Kralik et al. 1992, Putnis 2002, Schmid et al. 1996.
References There are some type errors, e.g. at Keller et al. 2005 and 2006, check spaces between first names, e.g. with Steiger and Jäger 1977 (What is ñS.o.ò?), with Townsend et al. 2000 (is the name ñDAndreaò correct?). Meinert Rahn, March 10, 2019

Changed

Comments reviewer 2

Review of Bergemann, Gnos, Berger, Janots and Whitehouse, Solid Earth Ms. Feb 2019:
Constraining metamorphic dome exhumation and fault activity through hydrothermal monazite-(Ce).
Reviewer: Fraukje Brouwer, VU Amsterdam, Netherlands, 11 March 2019

General assessment

The manuscript presents and extensive new dataset of cleft monazite ages that are an important addition to exiting geochronological work in the Alps. In addition, the study presents an interesting analysis of the relationship between the duration of tectonic events and the spread in ages recorded in individual monazite crystals. The paper certainly falls within the scope of Solid Earth, but has significant shortcomings in its presentation and therefore **I recommend that it undergoes major revision before being accepted for publication in Solid Earth**.

The manuscript in its present state has three major shortcomings:

1) The data are presented and grouped in multiple ways that are not always clarified to the reader, which makes it impossible for the reader to judge whether the interpretations are sound.

2) The figure numbers appear to have been switched around several times during the preparation of the manuscript leaving many incorrect references, including a on-existing figure in the electronic supplement, making it nearly impossible to find the correct data.

3) Section 5.3 is not clearly argued and organised and needs to be revised to clarify the reasoning of the authors. The abstract suggests the results of the study include major new findings, but if fact the results mostly confirm existing age information. To me, the value of the paper is more in the applicability of cleft monazite ages and the different expression of faster and slower tectonic processes in this dataset.

Note: This review was performed after the review of Dr. M. Rahn became available. I have tried to avoid duplication. I agree with most of his comments and suggestions. Numbers between brackets below (1) are marked in the appended annotated manuscript.

Specific comments

Throughout: figure numbers and references to them are a mess throughout the manuscript. This needs thorough checking.

 The title is too general and not entirely on-topic. Metamorphic dome is rather unspecific. Please add an indication of location and perhaps time (Alpine). Given that the applicability of the method is not restricted to metamorphic domes, it may be better to rephrase the title altogether.

The title was changed to " Dating exhumation and fault activity of the Lepontine Dome and Rhone-Simplon Fault regions through hydrothermal monazite-(Ce)"

Page 2

(4) It would be good to add a sentence or two at the end of the introduction that elaborates on the aims of the study.

This was added

(6) The more generally interested reader may have no idea where we are. I suggest to move Fig. 2 here and to add a reference to this figure to section 2.1. As indicated by Dr. Rahn, Figs. 1 and 2 need to be completed with coordinates, an indication of North, etc.

Changed

Page 3

(10) The samples were grouped ñroughly correlating to tectonic subdivisionsò is a vague statement and leaves the reader unable to judge the criteria that were applied. This might give an unfortunate impression of arbitrary grouping, which renders the paper less persuasive.

This was adjusted

(12) Regarding Figs 3 and 4, it would be good to mention briefly in the text, and not only in the figure caption what characteristic causes the zoning and how that is thought to be related to age information.

More detailed explanations were added to the text

Page 4

(13) See annotated manuscript for necessary edits to figure 1. The term ñGeological-geometric in the caption is unclear. Perhaps best replaced by ñGeometryò

Pages 7 and 8

(15) The images and all lettering in figures 3 and 4 should be enlarged so the reader is better able to assess the placement of the spots. Dr. Rahn mentions justified concerns regarding the placement of spots across boundaries between compositional domains. Some of the spots within one apparent compositional zone have different colours and it is not clear why that is the case (e.g., grains Duro2 and Klem1). The

caption mentions ñthe color of the frameò but it is not entirely clear what that refers to. Is it the box around each weighted mean age result? Please clarify.

Changed

Pages 10-12

(18) In addition to Dr Rahnôs comments. Please add spot numbers so the ages can be matched to the spots in figures 3 and 4. Enlarge lettering for readability; 6 pts at full size printing is usually considered minimal. I printed the pdf to A4 and most figures are too small in one way or another. The meaning of grey bands in these figures is not clear to me. Are the colours matched with those in figs 3-4?

Spot age numbers were added and colors unified

Page 12
(19) The content of section 5.1 is more fitting for the introduction than for the discussion.
Page 13
(22) The decisions behind the groupings are not really explained and therefor the reason has no way to judge whether these decisions are sound, or not. In addition, as indicated in figure 3 some spots within the same apparent chemical domain (based on BSE, other compositional data that may have been used is not available to the reader) are marked with different colours and therefore apparently assigned to different groups for reasons not indicated. The groupings need to be argued more clearly to convince the reader.
(23) ñto calculate, whenever possible, weighted mean domain ages (Fig. 7).ò Should this be figure 8? It is unclear to me what determines whether a weighted domain age can be calculate or, in fact, how this is done. This needs more explanation. It seems that some of this explanation is actually in the paragraph following this reference. It would be better to first explain the procedure and then present the calculated ages.
(24) ñIt appears that if dissolution-precipitation may largely preserve the chemical composition of an affected crystal part, this would mean that areas with different chemical compositions may have reprecipitated simultaneously.ò What is the basis for the assumption of preservation? Has this been shown in the literature? Or do the data somehow suggest this? This needs to be explained better. For the second part of the sentence, I do not understand the reasoning either. I am not an expert on monazite dating, but if the authors want the reader to trust the validity of their interpretations, they need to argue their assumptions and decisions more clearly.
(26) There is no figure in the appendix. Has this figure been moved to the inset of Figure 8? Please correct accordingly.

The chapter was moved, reorganized and expanded to provide a better explanation tot the reader.

Page 15

(29) This is certainly not clear from Fig 2 or 7, and perhaps refers to Fig 8. If so, the statement that the age ranges within grains are generally longer in the Eastern and Southern domain does not appear to be supported. This could also refer to figs 5-7 (I now note that the panels are numbered continuously through figures 5-7, which is rather confusing), but there I do not see a consistency in the graphs to support this statement either. This leaves me at a loss as to the basis of this this statement. This needs to be clarified.

Changed

Page 16

(31) The shadings in Figure 9 render the ages illegible and this figure needs editing for clarity. It also seems that the age ranges are idealised to an extent: in 9b a 13.6 +/- 0.4 age is included in the 15-14 Ma range and in 9c a 13.4 +/- 0.3 age further West is included in the 13-11 Ma range. The 13-11 Ma area in 9c includes the area coloured in 9b, which contains almost exclusively ages >13 Ma.
The colouring is persuasive but the averaged ages do not appear to match the areas all that closely. From the caption it seems that the shaded areas are based on all ages from each sample, but the weighted mean average ages are based on a selection of those. Such, presumably unintentional juggling with the data makes it almost impossible for the reader to judge the value of the results and interpretations, which is very unfortunate. The authors need to do a better job in presenting their results to convince me that their interpretations are valid and can be used to underpin a tectonic scenario.

The figure was changed and explanations added in caption and text.

Page 18

(36) The first sentence of the conclusions is a bit awkward. Please rephrase.

Changed

(37) ñage clusters within individual crystals from a simply exhuming area have a less clear age distribution than samples from fault zone areas, or fast exhuming areas.ò This apparently main conclusion is new here and was not that clearly presented in the discussion. It would be good to add a couple of sentences specifying the argument and its conclusions. The same goes for the next sentence.

Taken out, this was an unfortunate phrasing.

(38) The conclusions presented here paint a much clearer picture than section 5.3. The regional references (to the various faults and domes) are less clear in 5.3. Section 5.3 needs a thorough rewrite, and perhaps splitting in two sections to present the arguments more clearly. The first part could argue the conclusions about slow vs. punctuated events leading to broader and narrower age ranges, respectively, whilst the second part would present the tectonometamorphic development of the study area (leading to the conclusions in the second paragraph of section 6).

The paper was reorganized and should hopefully be more focused now.

Technical corrections

Many suggested corrections for spelling and grammar and indicated in the annotated manuscript. In addition, please consider the following numbered comments.

The following techical suggestions were implemented in the manuscript.

Page 1
(1) Earlier in the Abstract the authors argue that using cleft Mz is superior to other dating techniques because it is not cooling based. It then seems somewhat inconsistent to highlight cooling in line 8. Better to say exhumation only.
(2) The final sentence is very general and it would be better to be more specific as to what kind of information can be derived by dating of cleft-Mz.
Page 2
(3) Alpine is used a lot here (once also without capital), but the processes considered in the study are not likely to be restricted to Alpine orogenesis or the Alps orogeny. It would be better to phrase this a bit more generally.
(5) This sentence is very vague. Either be more specific, or leave out.
Page 3
(7) I am not sure what is meant by óstaggeredô exhumation.
(8) This reference to Central Lepontine is unclear, because the next sentences refer to more specific areas that are indicated in Fig. 2.
(9) Number figures in order of mention in the text. The current figures 1 and 2 should be swapped. This is consistent with comment (6) above.
Page 5
(11) Please add the groupings to Table 1.
Page 6
(14) Please check figure references. This should probably Figs 5 through 7.
Pages 7 and 8
(16) On both pages colour and color is used in the same sentence. Please use either British or American English spelling consistently.
Page 9
(17) The caption of Table 2 does not describe its content. Please correct. The text that is now in the caption is in fact a note.
Page 12
(20) Suggestion to rephrase: ñé existing grain. Alternatively, dissolution-reprecipitation may cause precipitation of a secondary monazite-(Ce) phase from the fluid film at the surface of the primary phase.ò
Page 13
(21) The reference to Grandô Homme et al., 2018 is not in the bibliography.
(25) It would be good to add a reference supporting these statements.
(27) ñanother reasonò is confusing here, because in lines 17-18 prolonged tectonic activity is already mentioned as a possible reason for age spread. In addition, the description ñé prolonged phases of low-intensity tectonic activity of multiple small deformation eventsé òis very vague. Please revise to address both these issues.
Page 15
(28) Correct references. Presumably Fig. 8 and Table 2.

(30) Clarify the location of the Rhine-Rhone line in the text and give it the same font size in figure 9 as all other faults
Page 17
(32) panels a and b are in figure 5, j and k are in 6. All figure references need to be thoroughly checked and corrected.
(33) ñclear age patterns within the crystalsòis a very vague criterion, which can not be judged by the reader. Please be as specific as possible.
(34) Again, ñstaggeredòis used in a sense that is not entirely clear to me. It would help if the authors clarify to which part of Fig 8 this refers.
(35) The mention of hydrothermal gold mineralisation is very random and appears to have little relationship with the rest of the study. Consider leaving this out.
Pages 19 and further - Bibliography
Missing from the reference list:
Milnes (1974)
Not referenced in the paper:
Frisch (1979)
Frisch et al (2000)
Glotzbach
Keller et al (2006)
Kralik et al
Putnis 2002 and 2009
Schmid et al (1996)
Possible mistakes:
Grandô Homme et al. 2016 or 2018?
Steiger and Jaeger: Title is ñSubcommittee on Geochronology: Convention é .ò
Wiederkehr et al. 2008 or 2009?