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Solid Earth
Discussions

Interactive comment on "Constraining metamorphic dome exhumation and fault activity through hydrothermal monazite-(Ce)" *by* Christian A. Bergemann et al.

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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 becom-

ing 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 "Results" 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 "Results" chapter. The "Results" chapter should also be used to clarify, what data you will discuss in the "Discussion" chapter and which data will not further be discussed.

4. My major concern is that the authors are rather vague with their methodical descriptions. Some of these details should be part of the "Introduction" chapter, of a new methodical chapter or part of the "Results" chapter. Let me summarize this in five points that I would expect the authors to provide more information about:

4a. The authors talk about the "monazite stability field" (e.g. p. 13, line 3; p. 15, line 6; p. 17, line 20), however, they never discuss, what they mean with "stability field". Note that the authors on p. 12, line 5, talk about "disequilibrium", without clarifying what kind of "disequilibrium" they refer to. I would assume that this is not a "thermodynamic stability", 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 "monazite formation temperature window". This aspect is important, because in the "Discussion" chapter you compare the formation of monazite with the closure temperatures of low-temperature thermochronology methods (which seems to suggest some kind of closure-T for cleft monazites).

4b. 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 "according to compositional domains" (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.

4c. 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.

4d. 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 authors' 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.

4e. 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.

4f. 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 "Discussion" 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 "weighted mean ages" in that sense. They also have to explain how uncertainties were calculated for the different types of ages.

4g. 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.

4h. Figure 2 shows nicely how the authors divise their samples into regional groups. However, in the “Discussion” 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 “the entire (north)eastern region that seem to act differently than the rest of the region. This “sub-region” is not well defined. (2) Figure 2 places sample DUTH6 to the edge of the “Center” region, but in figure 9, this sample rather behaves like the samples in region “West”, so why DUTH6 is part of the “Center” area?

4i. 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.

4j. The “Discussion” chapter starts with an interesting subchapter on hydrothermal monazite crystallisation. This is exactly the information needed to understand methodically the authors’ strategies. However, as far as I understand, this chapter is not a

“result” but a initially chosen “strategy” on how the monazite ages are to be interpreted (it looks therefore misplaced in the “Discussion” 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 “Discussion” 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 “events”. I agree that the paper title could be understood as “Confirming metamorphic dome exhumation”, 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 “Discussion” 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.

For detailed comments to the manuscript, see attached pdf file.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-10/se-2019-10-RC1-supplement.pdf

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-10, 2019.