Solid Earth Discuss.,
https://doi.org/10.5194/se-2017-88-RC1, 2017

Interactive comment on "Deeply subducted continental fragments: II. Insight from petrochronology in the central Sesia Zone (Western Italian Alps)" *by* Francesco Giuntoli et al.

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Received and published: 2 October 2017

General comments

The manuscript "deeply subducted continental fragments: II. Insight from petrochronology in the central Sesia zone (W. Italian Alps)" from Giuntoli et al. deals with reconstructing P-T-t paths of the individual tectonic elements building up the Sesia zone. 40 yrs of geological investigations in this region enabled the identification of three main units, which according to recent work (e.g. Rubatto et al., 2011), may have more complex individual P-T-t histories than earlier thought. The authors combine modern thermobarometric tools together with allanite in situ dating to yield new P-T-t paths for

various samples across the Sesia zone. The paper is well written, clear, in good English and concise. This contribution, which constitutes one step forward to improve our knowledge about this tectonic zone, presents an interesting dataset and should for this reason be published in Solid Earth after moderate revisions. Yet, I don't fully agree with the tectonic interpretation and I have some comments about the proposed P-T estimates which strongly differ from previous work. These points should be carefully addressed before re-submission.

1. It is a quite risky and challenging task to date allanite and conclude about the meaning of the obtained age since we understand so little about its P-T stability field as well as the effect of retrogressive fluids on its replacement and/or re-equilibration. One important finding of this paper comes from the nice agreement between Aln and Zrn ages as shown in Table 5. This match does not mean that both methods date the metamorphic peak (I see four Zrn overgrowths on figure 5d: which one corresponds to the peak and which one you dated?). Retrogression and fluid-rock interaction can easily reset these geochronometers and the agreement between these two methods would, in that case, just mean that both minerals have been affected in the same way by the same event. In other words, a blueschist-facies retrogression event accompanied by fluid influx (Konrad-Schmolke et al., 2011) would easily explain the 56 Ma age data obtained for sample FG1247. No need for complex and chaotic thrusting inside the Sesia zone to explain this. I thought this point of view was important to mention and I would like to see some words about that in the discussion to show the reader the existing debate on the meaning of such ages.

2. I have been very surprised by the very high temperatures proposed by the authors. These estimates strongly depart from the previous estimates (c.100°C warmer on average). The re-hydration heat production is not enough to explain such enormous amount of heat needed. In the companion paper submitted to Solid earth, the authors report glaucophane inclusions in Grt 2 from sample FG12157 and propose P-T conditions of 650°C and 1.4 GPa for this event. Glaucophane is not stable at these

conditions. I would rather expect a barroisite. And we would have staurolite everywhere in all metapelitic rocks which is not the case in Sesia zone rocks. Last, the Raman thermometer on organic matter estimates by Giuntoli & Engi (2016) yields 520-615°C which is clearly less than what is presented here and in better agreement with previous works (Regis et al., 2014). This suggests some disequilibrium problems in the thermobarometric approach and makes me doubt about the robustness of some of these estimates. Maybe pre-alpine garnet resorption yields an artificial Mg-enrichment in the vicinity of the dissolution front and artificially "boosts" the temperatures towards higher values? Further discussion is needed here. I also suggest that the authors take a look on the recent paper by Angiboust et al. (2016) about Mt Emilius eclogitized granulites. Similar X-ray maps on similar rocks have been already published and these results should be used for comparison and discussed in both manuscripts. Note that the P-T conditions (obtained by conventional THERMOCALC average P-T mode: 500-550°C, 2.1-2.4 GPa) are sensibly different. Peak ages for Emilius-like slivers above the Zermatt-Saas unit are in the range 50-60 Ma (Weber et al. 2015; Fassmer et al., 2016). 3. There is a lack of field constrains to support the alleged P-T-t differences reported between the different group of samples (1, 2 and 3) inside the Internal Complex. I recommend to make a better use of the extensive and high-quality field dataset from Giuntoli & Engi (2016) to better highlight the link between P-T-t gaps and individual structural sub-units (if any).

Specific comments

There are a number of important references relevant for your study area which should be cited. P.2,L.10 and P.19,L.22: Angiboust et al. (2014). This paper proposes a vision significantly different than yours on the geodynamics and emplacement of the internal nappes (in line with previous works from Pognante, 1987 and Polino et al., 1990). This model should not be neglected in your work and some words presenting these models and comparing them to your results are needed here (in particular in section 7.3). I also believe that the paper from Beltrando et al., 2010 (Gond. Res.) should be

acknowledged in the geological setting section.

P.8: no Zrn found in the External Complex? Please state this explicitly.

P.17: why initial starting PT guess of 650°C? maybe this is the reason why your P-T estimates went astray. . .

P.11, L.7: isochemical twice

P.11, L.20: 6 vol.% of biotite is a lot for a biotite-free sample. I would not use phengite silica isopleths to constrain the peak with so much biotite predicted. I would rather consider this attempt as a fail and try with another sample or with a different micro-chemical domain. If you take the other intersect at 550°C (Fig.6a), the biotite amount would surely be lower – and thus closer from actual petrological observations.

P.13, L.7: "chlorite IS retrograde and recordS"

P.13, L.16: chloritoid

P.13, L.25: I am very puzzled by the meaning of 0.6 +/- 2.0 GPa pressure estimates. . .

P.19, L.28: in the same time range (75-60 Ma)? But 15 Ma is a lot of time! these two units may easily have been subducted diachronously (with the EC entering the subduction zone much later than the IC; see my comment above and the attached references).

P.20, L.30: I see no evidence here for peak, eclogite-facies metamorphism at 55 Ma. This allanite/zrn ages could date the blueschist-facies overprint associated with exhumation and fluid ingression (Pognante 1987, Halama et al., 2014). I would advise to follow the same "petrochronological" strategy in the Tallorno shear zone and see what Aln and Zrn tell you.

Fig.1: FG1347 and FG1249 are very spatially close but have very different P-T estimates. Have you noticed any tectonic boundary between them? See my comment #3.

Fig.2: External complex (not ENTERNAL)

Fig.4: which ones are EC / IC ?

Fig.5: sure about the reference FG1347? (or rather FG1247?) Please provide spot location where the ablation holes have been made. This is very important to understand the meaning of your Zrn ages. Fig.6: EC/IC should be given (in the title, close to the sample number)

Fig.9: Th-isochron diagram for FG1420: why so much uncertainty? Lead loss or fluid-rock interaction?

Table 1: please provide EC garnet composition as well

Table 2: why you give the average composition? If so, you should mention the associated standard deviation

Table 3: please give totals for chlorite composition

Table 4: please give totals for phengite composition

I hope these comments helped. There is a still a long way until we really understand how the Sesia zone formed. This contribution does not solve all the problems but it provides some elements of the puzzle and raises important questions for future works.

Paris, 30/09/2017

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