

Here we reply to the comments of **Angiboust-Referee #1**. Below, we show these *in italic*, our responses to them in straight text.

Note: at the end of this document are available the modified figures and the Concordia diagrams, according to our replies to the two referees.

Summary statement

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.*

We certainly agree with Angiboust that linking the allanite age to the PT data of crystal growth is essential. For this purpose, we documented the microstructural context of allanite and all diagnostic mineral inclusions (section **4.4 Allanite textures and their microstructural relations** pg. 7, Table 5 and **7.1 Linking equilibrium conditions with time constraints** pg. 16). We note that, in all the dated samples, phengite inclusions in allanite cores invariably show the same composition as the phengite marking the eclogite facies foliation (Pg.7 lines 20-30). Both show the highest Si-contents (hence pressure) recorded by the sample. Phengite inside allanite also marks the same high-pressure foliation as in the matrix, and allanite grains are aligned with this same fabric. Allanite in samples FG1324 and FG1315 also contains inclusions of garnet the composition of which corresponds to the eclogite facies garnet (Pg.7 lines 20-30). For all these reasons, we conclude that allanite grew at eclogite facies conditions.

We also agree with Angiboust that “*Retrogression and fluid-rock interaction can easily reset these geochronometers*”. Therefore, the allanite grains chosen for age dating were first BSE-imaged (Pg. 4 lines 29-30; Fig. 4). BSE pictures are well suited to reveal allanite growth zones and replacement textures, where present, such as epidote overgrowths. We reported “These rims may reflect minor retrograde stages that weakly altered the eclogite facies parageneses as well. Again, a peripheral epidote rim is present.” (Fig. 4; lines 12-16 Pg. 7). Except for these local rims, our allanite crystal cores display no trace or evidence of replacement or re-equilibration. This is in line with the age data for allanite cores (Fig. 9) being very consistent and showing no trend that would indicate re-equilibration. Moreover, we rarely found evidence of a localised retrograde blueschist imprint – a clear difference to some previously reported samples in the literature – such as in sample FG12157 (pg.5 line 29). In that sample, allanite grains do have a rim (visible in Fig. 4c. noted on pg. 7 line 14), but no ages were obtained from such growth zones (as explained on pg. 15 lines 2-4).

Regarding zircon, we have no constraints to link the age data to the eclogite facies stages, but we note the agreement with the allanite age data, as pointed out by Angiboust. Regarding the particular question “*I see four Zrn overgrowths on figure 5d: which one corresponds to the peak and which one you date?*” we will update figure 5 and indicate the laser spots in the revised version. Our intent behind this figure is to show texturally most representative grains for each sample – that was the main criterion used to select each image. However, our descriptions of the rim types in the text were based on observations made in all of the grains of a particular sample.

Concerning the interpretation of the young ages obtained by FG1347, we consider the possibility illustrated on Pg 17 lines 16-18, on Pg 18 lines 27-33, and on Pg 19 lines 1-4, but we do not suggested “*complex and chaotic thrusting inside the Sesia zone to explain this*”.

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.*

Angiboust correctly remarks on the substantially higher temperatures proposed by our study, even though his assertion is at odds with well established estimates of the thermal effect of (de)hydration. That effect was documented as early as 1982 (e.g. Walther & Orville, *Contrib Mineral Petrol*, v.79, p.253). However, the reviewer's comment made us realize that this is not common knowledge. Hence we propose (a) to refer to the Walther & Orville (1982) results, and (b) to quantify the estimated thermal effect for our specific case, roughly as follows:

Walther & Orville (1982) analysed the thermal effect of (de-)hydration reactions during regional metamorphism and found it to be substantial. When applied to the present case of (re-)hydration, heat capacity data indicate that heating the Permian protolith requires ~1.0 kJ/K per kg of leucogneiss. The enthalpy released upon partial hydration of this protolith (producing the water content typical of these micaschists, 1.5 wt-% H₂O) adds ~77 kJ/kg in enthalpy. Such hydration should thus result in a temperature increase of some 80 °. This estimate lends credibility to the P-T estimates obtained here, which indeed are 60-90 °C higher than some maximum temperatures recently reported from other parts of the Sesia Zone, e.g. 575°C (Konrad-Schmolke & Halama 2014), 570-630°C for the Druer Slice (Regis et al. 2014), or >600°C for the Ivosio Complex (Zucali and Spalla 2011). In addition, Zr-in-rutile data reported by Kunz et al. (2017, their Table 3) gave 640 °C for one of the present samples (FG1249), confirming our results by an entirely independent method.

For these reasons, we see no compelling reason to doubt the P-T results presented or the technique used, which – as explained in Lanari et al. (2017) – is based on the intersection of garnet isopleth, a well-established method.

- In the companion paper submitted to Solid earth, the authors report glaucophane inclusions in Grt 2 from sample FG12157 and propose PT conditions of 650°C and 1.4 GPa for this event. Glaucophane is not stable at these conditions. I would rather expect a barrosite. 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).*

This is a good comment but to model such polyorogenic garnets with drastic compositional variations between the pre-Alpine core and the Alpine rims, it was not possible to use a conventional approach, as extensively discussed in Lanari et al., (2017). For this reason we developed a new approach and software (GRTMOD) that accounted for fractionation of the previous growth zones and dissolution and precipitation. We also applied the method (GrtMod) used in this study with the same thermodynamic database to samples from the Mont Emilius (Burn, 2016) and we found conditions of 500-550°C in samples with similar bulk rock compositions of Sesia Internal Complex. The same temperature is also found with the same method in samples from the Zermatt-Saas unit (work in progress). This argument thus cannot be used to explain the temperature difference we obtained for the samples of this study. Any T estimate not taking into account garnet fractionation and resorption would produce shifted P-T conditions (e.g. Lanari & Engi 2017).

We discussed these higher T predicted by our thermodynamic modelling: at lines 12-15 pg. 19 we wrote “Furthermore, our samples in the IC indicate ~50° C higher temperatures than those reported so far from parts of the Sesia Zone (Fig. 11). This observation indicates a temperature increase in at least some of the external (i.e. north-westerly) units of the IC, which may be linked to effects such as shear-heating and (re)hydration in the pre-Alpine HT rocks (see also reply to the previous comment). All our T estimates are around 600°C, considering the uncertainties, as shown in Fig. 11b by the lines departing from the ellipses, except sample FG12157. Such variation of T probably reflects different tectonic sheets that experienced somewhat different P-T-t paths, as discussed in chapter **7.3 Assembly and exhumation of the Sesia Zone** (see further details in our reply to comment 3).

Glaucofane may not be stable at these temperatures in mafic rocks, but how much of a calcic component appears in amphibole at higher temperatures strongly depends on bulk composition, and barroisite may not develop in low-Ca rocks, such as our sample.

Regarding the absence of staurolite remarked by Angiboust, we note that (a) its stability field is not extensive at high pressures, and (b) in equilibrium phase diagrams calculated for the compositions of our samples no staurolite is predicted to occur between 1.3-2 GPa and 400-700°C, except for FG1347, where staurolite is predicted to be stable below 1.6 GPa above 600°C (Fig. 6d), but our PT estimates are outside that stability field of staurolite – and the prediction is in agreement with the assemblage we observed.

For the above arguments, we disagree that such higher T are caused by *disequilibrium problems in the thermobarometric approach*. To extract compositions for modelling, garnet growth zones were carefully selected after detailed microstructural analysis of the compositional maps; such areas where chemically homogeneous and uniform amongst different garnet crystals, they displayed no evidence of enrichment or depletion in the major elements. We should probably stress this point, adding few lines in the section **5.1.2 Garnet thermobarometry using GrtMod**.

We are aware of the recent paper by Angiboust et al. (2016) on Mt Emilius, but here we consider just the Sesia Zone *sensu stricto* (see pg. 3, lines 5-8). These two units occupy different structural levels in the Alpine nappe stack (more detail in Giuntoli and Engi, 2016 page 3): Emilius is located below the Combin Unit, the Sesia Zone *sensu stricto* is located above it. We agree with previous studies and consider both of them as fragments of the Adriatic passive margin (e.g. Dal Piaz 1999; Beltrando et al. 2014), but a direct correlation of their Alpine tectonometamorphic history is hazardous.

4. *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).*

As we wrote on pg 17 lines 16-20 “It thus appears that the **samples from the IC reflect several stages of allanite growth, probably because rocks of slightly different bulk composition produced allanite by different metamorphic reactions** (Engi, 2017). The three growth stages captured by our samples are at ~73 Ma, ~65 Ma, and ~56 Ma. The different P-T-t paths of Group1, 2 and 3 are interpreted to represent different continental sheets (Giuntoli and Engi, 2016) that experienced similar PT conditions but at different times (further discussed in section 7.3 Assembly and exhumation of the Sesia Zone).” and in section **7.3 Assembly and exhumation of the Sesia Zone** “The IC shows several tectonic sheets, from several hundred meters to a few kilometres in thickness (Giuntoli and Engi, 2016), some of which may have moved independently (Rubatto et al., 2011; Regis et al., 2014) at some stages of the evolution. Some of the samples studied, while taken at most a few kilometres apart in the field, recorded similar P-T paths but at different times, as reflected by the three age groups identified. **This age difference may reflect relative mobility between such sheets, which are notoriously difficult to delimit in this terrain** (Giuntoli and Engi, 2016).” We propose that these differences in P-T-t trajectories experienced by these samples may reflect different tectonic sheets and/or an interplay between tectonic mobility and several stages of hydration at eclogite facies triggering crystallization of the main parageneses (including allanite and zircon) to explain such different P-Tt paths. As visible from Fig. 1b, FG12157, FG1249 and FG1347 are located in the Croix Courma Sheet, as defined by Giuntoli and Engi (2016), because in the field no marker highlighting further tectonic boundaries were found inside that sheet (see section **3.3 Criteria used to subdivide units** pg. 5 of Giuntoli and Engi 2016 for the criteria used). However, fieldwork alone is insufficient to identify all the tectonic boundaries, as distinctive field markers are scarce in gneiss-terrains. Giuntoli and Engi (2016, Pg. 25) point out: “The present study confirms the presence of several such units and documents their spatial relation, though more such sheets probably remain to be discovered. ... We recognize how difficult it is to trace tectonic contacts, even major ones such as between the Internal and External Complexes, in areas where markers (e.g. distinctive marble trails) are missing and similar lithotypes are juxtaposed...”. Fieldwork used in conjunction with petrochronology enhances our ability to disentangle the evolution of complex tectonometamorphic complexes.

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. Agreed. We will thus refer to *Angiboust et al. (2014)* and add a few sentences to section 7.3 comparing our results with this model, as requested. We also agree to acknowledge *Beltrando et al., 2010 (Gond. Res.)* in the revised version, as it is a useful reference.

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

Zircon in the External Complex displays pre-Alpine magmatic oscillatory zoning, no Alpine overgrowth zones have been found. We will add this sentence in the revised version.

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

The initial starting guess is a technicality used for reasons discussed in Lanari et al. (2017, sections 5.2.3. **Stage 2 – go fast mode** and 6.3. **Automated strategy [1]: limitation of multiple minima and solution finding**). The specific purpose is not to miss a minimum in this part of the PT space. This function searches a solution around the starting guess and follows the gradient in the objective function; there cannot be two local minima at high pressure (see Fig. 8 in Lanari et al. 2017). We will add a clarification in the revised paper, but more importantly we reject Angiboust's conjecture that our P-T estimates "went astray".

P.11, L.7: isochemical twice

Deleted

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 microchemical 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.

This objection is justified, at least in part, but we attribute the predicted biotite saturation to inadequacies in the available solution models, especially for alkali-amphibole. In the present case, the discrepancy has no drastic implications, since all of the phases observed in the (high-variance) assemblage are in fact part of the model assemblage that buffers the phengite composition. In this sample, garnet and phengite are the most abundant mineralogical phases, and their composition is matched perfectly by the models used. The intersect at 550°C is no valid alternative, because it is completely out of the range of the isopleth intersections for garnet and phengite, while these match perfectly at the accepted intersect at 1.65-1.75 GPa and 600-650 °C. We stress that the high temperatures are specific for the present samples; the exact same method (and with similar bulk rock compositions) yields 500-550°C for eclogitic micaschists of Mont-Emilius, in agreement with Angiboust's results there. So the modelling results appear to be robust even though they predict biotite as an additionally stable phase for one of the present samples.

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

Corrected

P.13, L.16: chloritoid

Corrected

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

Our mistake, the correct (asymmetric) uncertainty is 0.6 +/- 0.2 GPa. Corrected

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).

This is correct, but not at all in disagreement with what we state (pg. 17 lines 31-32): "Comparing P-T-t data for the IC and EC, we note that Group 2 (in the IC) and Group 4 (in the EC) recorded the same age data of ca. 65 Ma, but very different metamorphic conditions". We could modify the sentence at *P.19, L.28*, stating "at ca. 65 Ma" (instead of 75-60 Ma). We do not claim to know which of the two units entered the subduction zone earlier – we do not have data to constrain this. But we discuss that in the same time range

these two complexes were experiencing very different PT conditions (~1 GPa and 100-180 °C less in the EC, as stated on line 29 pg. 19).

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 ingress (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.

We disagree with this comment: As we stressed in our reply to comment #1, all the evidence we have from the dated samples indicates eclogite facies, none of it blueschist facies. Even though we have no reasons to doubt the results reported by Konrad-Schmolke from the Tallorno shear zone, it is an excellent idea to include samples from there in a petrochronological study using allanite and zircon. However, we have not extended our study to samples from there.

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.

Indeed, these two sample localities are not far apart, and we found no markers highlighting a tectonic boundary between them in the field. However, absence of evidence is not evidence of absence – especially in such a high-strain gneiss terrain – so we really cannot exclude the possibility of a tectonic boundary. Please, note our reply to comment #3 as well.

Fig.2: External complex (not ENTERNAL)
Mistake, corrected

Fig.4: which ones are EC / IC ?
Specified in the revised version to make the figure clearer

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.
Yes, the sample is FG1347, the error was in table 5 and we corrected it, in the revised version. We will update our figure to show the spot locations, as requested.

Fig.6: EC/IC should be given (in the title, close to the sample number)
Good advice, we will update the figure in the revised version

Fig.9: Th-isochron diagram for FG1420: why so much uncertainty? Lead loss or fluid-rock interaction?
Neither one of these reasons, the large uncertainty is due to the high common lead content of the sample.

Table 1: please provide EC garnet composition as well
OK, modified in the revised version

Table 2: why you give the average composition? If so, you should mention the associated standard deviation
These values are based on the area selected for each growth zone (using X-ray compositional maps in XMapTools). We can add the associated standard deviation in the revised version.

Table 3: please give totals for chlorite composition
Ok, added in the revised version

Table 4: please give totals for phengite composition
Ok, added in the revised version

I hope these comments helped. There is 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.

We thank Samuel **Angiboust** for his many constructive comments.

Below are available the modified figures and the Concordia diagrams that will be added in the revised manuscript, according to our replies to the two referees.

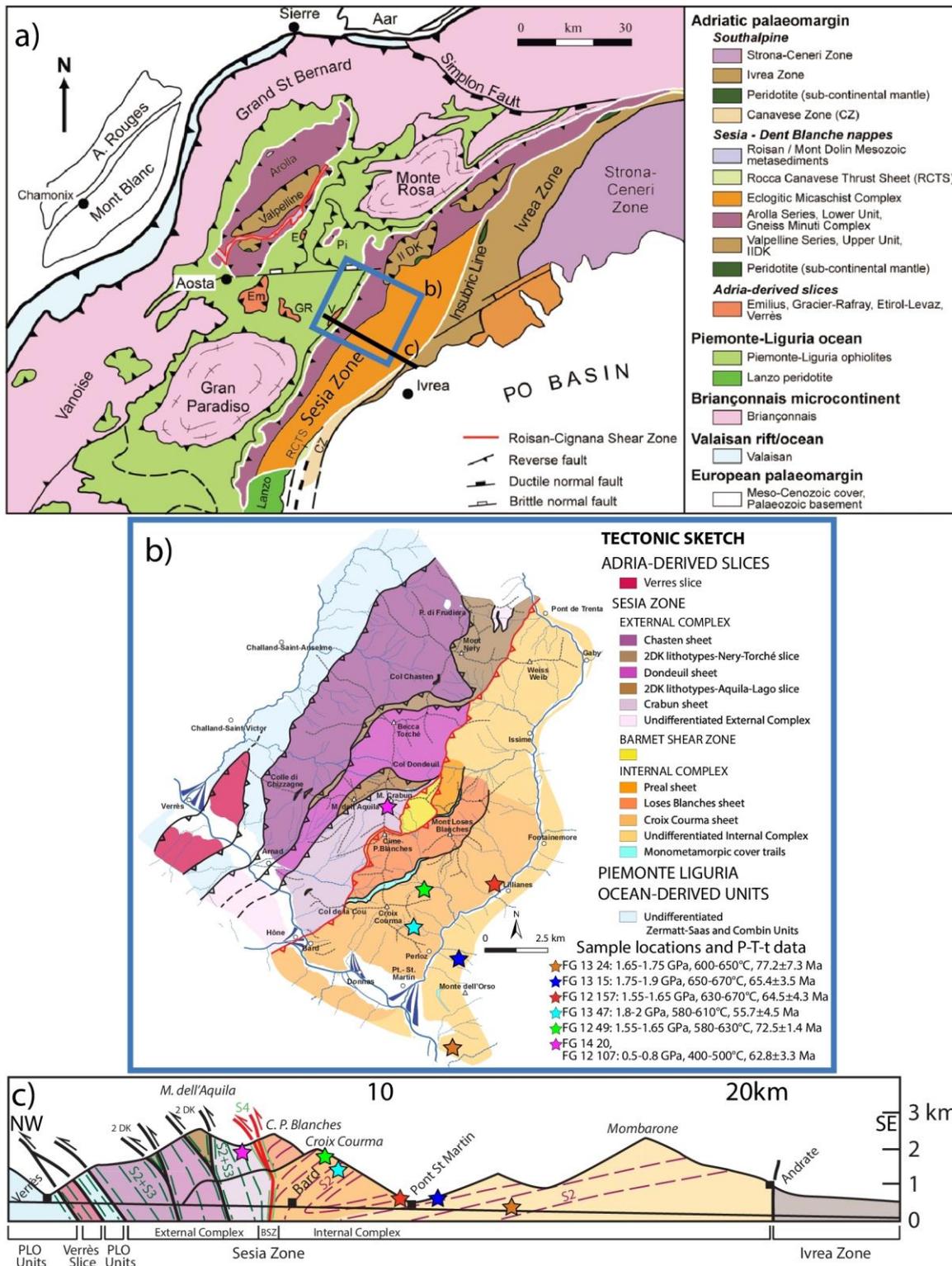


Figure 1: (a) Simplified tectonic map of the Western Alps (modified from Manzotti et al., 2014). (b) Tectonic sketch of the study area (modified from Giuntoli and Engi, 2016) with sample locations and P-T-t data (this study). (c) Cross section

through the study area (location show in a) with **projection of the studied samples**. Foliation traces: violet indicates the eclogite facies foliation (S2) of the IC, dark green indicates the composite epidote blueschist-greenschist facies foliation (S2+S3) of the EC, dark green indicates the greenschist facies mylonitic foliation (S4) at the contact IC-EC; **BSZ Barmet Shear Zone, PLO Piemonte-Liguria Oceanic unit** (modified from Giuntoli and Engi, 2016).

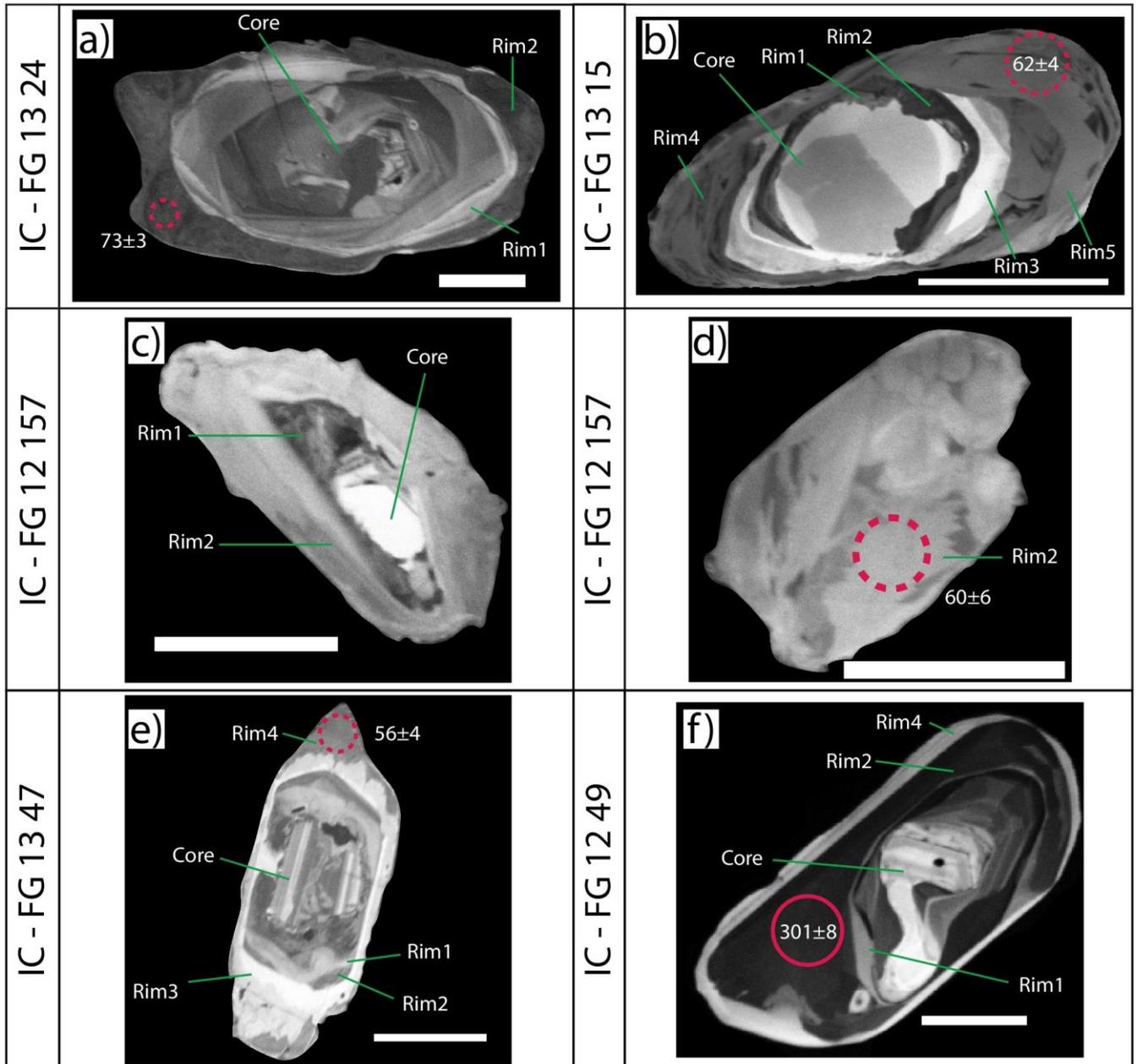


Figure 5: CL-images of zircon textures in the samples from the Internal Complex. Typically detrital cores show more or less resorption and are followed up by one to five metamorphic rim generations of different CL responses. The scale bar in all images is 50 µm. **Solid circles correspond to 32µm LA-ICP-MS spots, while dashed circles are 16µm spot sizes. The dates are individual $^{206}\text{Pb}/^{238}\text{U}$ spots analysis given in Ma.**

S10 Concordia diagrams of the zircon Alpine age data

Concordia plots of individual $^{206}\text{Pb}/^{238}\text{U}$ spots analysis of Alpine dates given in Ma (using the software Isoplot- Ludwig, K.R., 2003. Isoplot/Ex version 3.0. A Geochronological Toolkit for Microsoft Excel. Berkeley Geochronological Centre Spec. Pub., Berkeley 70.).

