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Interactive comment on “Polyphase evolution of a crustal-scale shear zone during progressive exhumation from ductile to brittle behaviour: a case study from Calabria, Italy” by E. Fazio et al.

Anonymous Referee #2

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The manuscript is very hard to read, and in my opinion, barely in a reviewable state. At one hand, this is clearly related to the authors difficulties with the English language, which is something that can be resolved. Terminology is used in a very confusing, wrong and very inconsistent way. This results in many sentences appearing out-of-context, free from any meaning or leave the actual meaning to the interpretation of the reader. Various repetitions in many parts do not improve the readability. At the other hand, I have difficulties to see what is the purpose of this study, which is due to the lack of a well defined aim followed throughout the manuscript. The manuscript combines a mixture of descriptions of regional aspects, structures, very interesting microstructures, petrography and adds orientation and LPO data and the application of

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laboratory derived relations. With those, the authors want to target the goal of this manuscript(911:25ff): to report the evolution of structural features, a comparison of meso- and microstructures, a kinematic analysis and stress field orientation. This is a lot of different things, however, I do not see how this is accomplished. It also remains unclear to me what are the novel findings in the working area or regarding the presented observations. It is not clear to me, what is new about the observed overprinting of ductile structures by brittle structures in an area where metamorphic rocks underwent exhumation up to the surface. If there are any novelties, it should have been presented much clearer. On top of that, I have serious doubts on the validity and correctness of some results presented in this manuscript, as will be outlined below (regarding section 4.2, 4.3, 4.4). In some occasions, it is also unclear what a certain method should contribute to the authors statements. Overall and unfortunately, the manuscript gives the impression of a wild collection of various field and microscopic observation without getting them wrapped together to a consistent and understandable story.

I recommend that this manuscript should not be accepted to the special issue of Solid Earth. The rocks presented seem to me like highly interesting study objects, if the purpose of the study is well defined. The observations have the potential to target many different aspects, however this needs to be done in an ordered, consistent and carefully documented way. I would encourage the authors to focus on just one clearly specified topic and re-write the manuscript, adding and clarifying data, omitting data unrelated to the actual topic and being very careful about speculations which are not based on solid observations and data. It should be clearly stated what is already known from the previous studies and what is new, separated into different sections. The descriptive part should be as free as possible from interpretations which belong into a discussion or interpretations section.

I will add some comments I made while working through the manuscript. Those will be not exhaustive but give an overview where major and minor problems from my point of

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view are situated. I do not think it is the job of a reviewer to give a full "correction" of a manuscript in that particular condition.

Abstract 910:L12 "...switching from compressive to extensional tectonics." vs L21 "...support a continuous compressional exhumation..." What is it?

Introduction 911:6/29 "...stress-field orientation.." I could not find any paleostress analysis in this manuscript, unless plotting 6 datasets (Fig. 4-6) of steep and flat faults (or fractures, this is unclear) is thought to accomplish that task. Or is it simply the interpretation that sigma 1 changes from a sub-horizontal to a sub-vertical orientation?

After reading the introduction, the reader knows that previous research has been done on the transition from ductile to brittle deformation in this area and will be curious what is going to be new about the findings to be presented.

913:22/29 "..Alpine-type metamorphism..." What should that be?

913:6-9 "The evolution of the overlapping tectonics... ." That is one of the sentences, I do not know what the authors are trying to say.

913:12 "NE-SW brittle extensional faults.." 915:25 "ENE-WSW axial plane..." There are many occasions in the text where the usage of technical terms is necessary, e.g. why don't the authors use strike or striking, plunge or plunging, dip or dipping? Would make it much easier. 918:6 there's suddenly orientation data in dip direction/dip notation (e.g. 260/38), however the authors add that it's right hand rule notation. So is this now an incomplete strike/dip notation? 917:15 "...SSE-NNW verging asymmetric folds.." I do not know how I have to imagine this? 918:9 "...mylonitic foliation oriented ENE-WSW...L having an average plunge of 210/25 (azimuth/dip direction).." Plunge is the vertical angle of a line with the horizontal, not an entire direction.

The entire manuscript lacks a methods section. However, the reader is informed 915:10 that "... contouring was used to plot the main foliation, fold axis... ." What method was used? Is it useful to contour data populations of eg. 6, 8, 11, 29 measurements (Fig.

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6)? Why are rose diagrams for fractures used, which are supposedly "steep"? What is steep and what is flat?

There is an inconsistency about the subscripts in the authors deformation phases. E.g. D1E in the text, D1H in the figures and tables. That is not necessary.

915:21 "Structural investigations ...suggest syn metamorphic deformation... ." Citation?

LAF/HAF: in places in the text and figure captions this abbreviation is used for low/high angle joints (916:8), low angle fractures (Fig 3), thrusting (918:16), low angle faults (929:23). These are all different things and should not be mixed.

916:22 ".. to ultra-mylonite (Fig 3b)..." No, I cannot see this in the photograph.

919 Petrography of the APU and PT estimates: As these results come from the literature, these should not be mixed with your own results. Is this entire section (919:7-22) just a citation?

919:23-25 "At the sub-centimetric... ...depicting at places a cataclastic flow." I do not know how I can understand this sentence.

919:29-920:4 So basically, the authors find in their rocks all dynamic recrystallization mechanisms. Isn't that something to elaborate on? Where? What is the overprinting relation? Most likely one can tell from these much more than just reporting their presence.

920:5 Why do veins need to suggest hydraulic fracturing?

920:7 Chessboard pattern in quartz grains together with deformation lamellae? This is usually not encountered together. Do all grains with very HT microstructures also show these LT microstructures? Is there a (regional) pattern?

920:21 And suddenly a pseudotachylite appears. How is the nature of these PST confirmed? Where do those occur?

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920:25 "...micron size dip-slip at thin section scale" What dip?

921:2 "White mica fish are diffuse." What does this mean?

921:2-5 The whole sentence seems like a collection of words.

Section 4.2 Quartz LPO: Unfortunately I have to say, that most of the statements within 4.2 (921:14-19) do not make any sense. A polefigure presents directions, in the current case the quartz c-axis. It has at first hand, nothing to do with the operational slip system. A coincidence with a peripheral maximum roughly perpendicular to the foliation with the activity of basal-a slip can be shown in many cases. Equally a coincidence of a central y- maximum with the activity of prism-a slip has been observed. However, these are interpretations and simply a c-axis polefigure alone is not enough to make claims about slip systems. The interpretation to use polefigures as a temperature indicator is equally difficult. A simple relation of recrystallization mechanism, LPO-geometry - temperature, like the one presented by e.g. Stipp et al., 2002 has been shown not to be universally applicable (e.g. Heilbronner & Tullis, 2006; Pennacchioni et al., 2010; Menegon et al., 2011). LPO might also reflect strain, orientation of parent grains and recrystallization mechanisms as well. Quartz LPO are not a thermometer. There is a correlation of the opening angle of c-axis cross girdles with temperature, however the underlying mechanism and the universal applicability is still unclear (see Law 2014 for an overview).

What does the quartz vein contribute? Simply because this vein is also in one of the CIP analysis does not mean it has to necessarily make it into the manuscript.

Section 4.3 Quartz deformation mechanisms To me, the first sentence (922:1) does not make any sense. The grain size obtained from the grain boundary map shown in Fig. 11a has obviously nothing to do with the underlying c-axis orientation image. Accordingly, the obtained grain size is unrelated to the presented samples. Why is there a histogram presenting the axial ratios of grains obtained from the map in Fig. 11. It is not mentioned nor is there any reference on that. What does it tell the reader? What

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urther information is obtained from that? The authors observe different recrystallization mechanism in different microstructural positions. However, no further information or interpretation is drawn from that. The transition from bulging to subgrain rotation recrystallization has been observed by Stipp et al, 2002 at a certain temperature in a specific rock deforming at a specific rate under specific conditions. This was never intended to act as a universal thermometer.

Section 4.4 Quartz paleopiezometry Despite it is unclear to me what the differential stress estimate obtained by recrystallized grain size paleopiezometry and the calculation of a strain rate adds to the overall interpretation, this section has several problems. First, as mentioned above, the grain size estimate is unrelated to the sample, rendering the results wrong. Second, the authors use apparently the same grain size and hence differential stress to infer a strain rate at two different temperatures (924:15ff). What should that mean? If they had used, eg. one grain size related to a confirmed higher temperature recrystallization and one to a confirmed lower temperature recrystallization, one could have made some point about the rheological development during exhumation. The entire reasoning that results are "realistic" is questionable. Of course, one gets always reasonable differential stresses out of a piezometer calibration that is actually fitted (for bulging and subgrain recrystallization) for grains sizes that are usually encountered on planet earth. And of course, one gets reasonable strain rates when these differential stresses are inserted into a flow law fitted for stress-strain rate conditions on planet earth. Hence the argumentation that these numbers are "valid" because shear zones on earth seem to deform somewhere at strain rates between 10-9s and 10-15s (924:12) is not very convincing. What does it contribute to the story?

Section 4.5 Brittle microstructures It is unclear to me how all these different phases are differentiated. The referenced Fig.11 seems not to be related. I fail to see a clear documentation of the overprinting relations mentioned. I also never heard of the "fragile" regime. How do breccias and pseudotachylites get together? Why are the latter not documented in any form. Why do some veins only have to form during an extensional

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tectonic event?

Section 5 Discussion Again, I fail to understand the first sentence. 926:3 "..a non-coaxial plastic-type deformation..." I'm not entirely sure where this should point to, but I have not seen any data on this in the manuscript. The discussion in large parts is composed of repetitions (926:5-9; 926:15-18), speculations (926:12 "..causes over-pressure."; 926:19 "..enrichment of fluid pressure" (?) and various claims without a reference (927:4 "...should be coincident with σ_1 ..."). There is a lot of interpretation about the veins, fractures, changes in physical conditions, which I find difficult to relate to the presented data. Given the amount of observations presented on the ductile evolution, there's very little interpretation on that.

Section 6, conclusions is in large parts repetitions without clarifying what the authors want to state. 929:9 "...opposite finite strain products..:" Despite I don't know what this should be, I did not find any strain analysis in the manuscript. 929:14ff "..peculiar orientation of such veins..." If this is so peculiar, it should have been worked out more clearly in the relevant section. Why do veins developing parallel to a preexisting anisotropy have to be related to a compressional stress field?

Some notes on the figures: Fig 3: The figure would benefit, if the drawings on the photographs would not cover too much the actual features. Fig 3b: If LAF is in this case "low angle fracture", ok, if it is "low angle fault", I do not see how this should work. Is the displacement coming out of the plane or why are the different layers not displaced?

Fig 4/5/6 What is the requirement to be a HAF or a LAF? I find this arrow bending around the large polefigure quite confusing. What should it tell me? Also, abbreviations should be consistent with the text. How are hierarchies in the HAF established? Is there any statistics behind this?

Fig 7/8/9 Beautiful structures: There could be much more taken out of those!

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Fig 10 The presented LPOs and COIs originate mostly from recrystallized layers. What is the reference frame for them? Foliation, shear zone boundary? This is important when using the external pole figure asymmetry to deduce shear sense.

Fig 10h” and 15: I don’t know what this should tell us. If there’s a preferred orientation of quartz growing in a vein, this sample is not large enough to be representative. Where does the sigma 1 inference come from? There is no Fig. 9h. What does this displacement arrow show? To me it look like some grains are continuous from the ribbon grains to the vein. What do the gray values indicate?

Fig 11. Simply by looking at the grain boundary map and the COI, it is obvious that the obtained grain size has not much in common with the analyzed sample. Why are the masked out parts also segmented? Some areas appear to be highly over-segmented while other lack segmentation. I suspect something went very wrong doing the grain boundary segmentation.

Fig. 13. How is the direction of the principal stress axes established? Are we looking orthogonal to the stylolite plane. If this is a re-opened stylolite, how does that fit into the interpretation?

Fig. 16. The mica stacks (also in Fig. 14) are very interesting. However, the conceptual drawing (inset Fig. 16e) makes them form at compressional sites. How is this supposed to work? What does the color-coded segmentation contribute? What does the presence of an - indeed - complex fold tell us? Nothing about all that in the text.

Fig 17. How are the stress axis established? It’s not clear to me from the text. Are those only 2D? What about local deviations, especially when looking into ductile flow in a heterogeneous rock? Again, the uppermost middle sketch shows the compressional sites where mineral growth is observed. Where do the pressure estimates come from?

Interactive comment on Solid Earth Discuss., 7, 909, 2015.

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