TEKTONIKA

Review Report

Pistone et al., Can Pseudotachylytes Form via Fracture-Induced Decompression Melting Under Hydrous Conditions?, TEKTONIKA, 2025.

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1st Round of Revisions

Decision Letter (30 November 2023)

Dear Dr. Pistone and co-authors,

Thank you for submitting your manuscript " Can pseudotachylytes form via fracture-induced decompression melting under hydrous conditions?" to Tektonika. We have now received 2 reviews of your manuscript. Based on these reviews, along with our own assessment, we believe this paper has the potential to make a significant contribution to the understanding of the origin of pseudotachylytes and will make an interesting discussion paper for the community. However, the reviewers have identified some aspects of your work which we believe need to be addressed in order for us to consider your paper for publication. Our decision is therefore: Resubmit for review.

We ask that you now prepare a revised manuscript where you carefully address each reviewer's concerns and editorial comments. In particular, Reviewer #1 questions the assumption of equilibrium required for the thermodynamic modelling, so we ask that you explain further the evidence to validate your approach and any limitations or uncertainties in this. Reviewer #2 asks for an isochron diagrams to be included, and both reviewers suggest some substantial editing to some sections (particularly section 2) to remove text that is not relevant and to enhance the focus of the writing.

The reviewers have provided annotated manuscripts and a review report, and a list of comments from the editor are attached to this email. All these comments should be responded to in a traditional response letter, with a detailed explanation given for how changes have been made to the manuscript. We ask you to submit both a copy of your revised manuscript, with changes clearly marked, and a clean version, during your resubmission.

Tektonika values your contribution and hope that you will be able to address the reviewers' concerns and submit a revised manuscript within 2 months (please note that Tektonika does not impose a time limit on submitting revised manuscripts, this suggested timeline is provided as a guide only). If you require additional time for your resubmission, please don't hesitate to get in touch with the editorial team to discuss a revised timeline. Feel free to reach out at any stage if anything is unclear.

Thank you for giving us the opportunity to consider your work.

Yours sincerely,

Dripta Dutta (Associate Editor)

Janine Kavanagh (Executive Editor

Comments by Reviewer 1 (Elisabetta Mariani)

Introduction

This manuscript reports and discusses results from thermodynamic calculations (assuming equilibrium) determining the P-T path of a pseudotachylyte from the Balmuccia peridotite. Based on observations of the sub-micron scale chemical signature of the melt (using STEM EDS) the Authors interpret fracture-induced decompression melting in hydrous conditions as an important process facilitating the formation of pseudotachylyte. To my knowledge this process has not been previously reported in this context.

The manuscript highlights are: the identification of spinel and opx chemistries as separate “droplets” in the melt at the sub-micron scale. The evidence that only under hydrous conditions a melt of composition similar to that observed in the field can be produced in the author’s thermodynamic model.

I report my main comments below, and I also annotated and commented on the manuscript document directly. This work is certainly providing a valuable opportunity for new discussions on the formation of pseudotachylytes and it is therefore an important contribution to the structural and metamorphic geology (and wider) communities. Therefore, I recommend it for publication in Tektonika after revisions have been made.

I should also say that the manuscript assumes a pseudotachylyte system in equilibrium and that this assumption is necessary to build the thermodynamic models shown in Fig. 3 and 4. However this assumption of equilibrium is not currently documented in previous literature and there might be valid objections to it. Thermodynamics is not my direct field of expertise, therefore I hope that there is another reviewer with this knowledge able to comment to some depth on the validity of such assumption and its limitations in the context of this manuscript.

Main changes

1) Geological Settings and history: I found this section a longer read than it is perhaps necessary. I suggest that more focus is given to this section by emphasising those tectonic events that are more directly relevant to fracture-induced decompression melting in peridotite and the environmental conditions that may facilitate it, with less emphasis on the broader history of the peridotite, which is also comprehensively reported elsewhere in the literature.

2) Methods: If fieldwork was carried out, are there any particular approaches to field work that should be described here? This would provide context for the hypothesis for decompression melting. What and how many samples did you study and where did they come from (give grid coordinates).

3) Crystallisation and recrystallisation seem to be used interchangeably. It would be helpful to provide clear definitions of what process is described by each of these terms.

4) Similarly, the terms flash melting and decompression melting are not clearly

defined and in this particular study the boundaries between the two become blurred. This might be because the decompression melting described here is rapid (a handful of seconds?), giving rise to flash melting. I suggest both terms are defined at the onset and their use in this study explained.

5) In fig. 2, how did you confirm that spinel and opx are in a glassy state and they are not for example cryptocrystalline? This seems important to demonstrate in support of your interpretations of thermodynamic calculations.

6) The assumption of equilibrium in a pseudotachylyte is necessarily a point for further discussion as these systems are not currently understood as equilibrium systems. Yet this assumption underpins the thermodynamic models in Fig. 3 and 4 and therefore the results, discussion and conclusions of this study. Given the truly rapid changes of environmental conditions during Pst formation I do find it difficult to think of this system as a system in equilibrium. Even rocks in their solid state are often not in equilibrium.

I encourage the authors to define what they mean by near-equilibrium in the pseudotachylyte melt, provide further supporting evidence for their assumption of equilibrium and expand on both their reasoning and on the limitations of their assumption.

As thermodynamics is not in my direct field of expertise, I hope there will be another reviewer with this expertise who can comment further here.

Other changes

See the document: “Pistone_Tektonika_2023_EM” that I have provided and make the changes suggested (main changes in short text version are included here too).

With best wishes,

Betty Mariani

Other comments

Line 216 - What and how many samples did you study and where did they come from (give grid coordinates)

Line 407 - I understand what you are trying to demonstrate: that the use of STEM EDS is important in this study. Why not say it explicitly with reference to the technique rather than using long sentences such as in this section? Referring to nanoscale seems excessive as Fig.2 is in the order of ~1 um scale. But citing the technique directly gives a better idea of the resolution power and activation volume.

Line 416 - Again, in Fig. 2 you are dealing with micron to sub-micron scale analyses, so referring to nanoscale is pushing it. But mentioning STEM EDS will help the reader see the value in the data.

Line 502 - How did you confirm that spinel and opx are in a glassy state and they are

not for example cryptocrystalline? This seems important to demonstrate in support of your interpretations below.

Line 619-620 - Presumably the amount will also depend on the amount of fluids available? I would like to see some discussion on what considerations drove the decision on the wt% fluids to use in Fig [5].

Minor Text Edits

Line 125: The Balmuccia Massif is one of the largest peridotite slivers

Line 132: along with the Baldissero

Line 456 - Incomplete or confused sentence – I think I understand what you want to say but really a schematic of this process to refer to would help. Include such schematic in Supp Material and reference it here.

Comments by Reviewer 2

Line 96 - Where are these glassy pseudotachylytes from and where is the evidence of their glassy nature? XRD?

Line 341 - Would it be possible to present more evidence supporting the glass interpretation?

Line 1100 - "I visited this outcrop and the geometry of these A-type veins is quite unusual. The margins are not straight … Would it be possible to see an XRD diagram of the vein material?"

Figure 3 - Decompression melting during seismic rupture should be adiabatic due to the low thermal diffusivity of rocks in general. Therefore the PT path of the lherzolite should go down along the vertical axis …

Comments by Associate Editor

1) Apart from the first paragraph, the 'Introduction' sounds incoherent. It is particularly unclear how the discussion in the second paragraph is related to the objectives outlined at the end.

2) I recommend adding sub-sections in Sec. 3 corresponding to the different methods and then grouping the results in Sec. 4 accordingly.

3) Figure 1: What do the regions bounded by yellow dashed curves represent in A and B? Please explain in the caption.

4) Figure 2

a) Since there are several sub-figures, please label them (a), (b), (c), etc.

b) What does the inset represent? And, from which location has it been captured?

c) Please label the x-axis of the plot and the two ends of the yellow horizontal line on the x-axis.

d) The stippled vertical line perhaps marks the point where the yellow horizontal line intersects the dashed curve in the above image. Please write it down in the caption.

5) Other minor comments

a) Lines 48-50 sound repetitive

b) Line 52: What type of alteration and what do they alter to? Please list a few examples.

c) Line 70: Please define flash melting before this.

d) Line 152: Perhaps it should be 'composed of' instead of 'composed by'

e) Line 177: 'the presence of' could be deleted

f) Line 193: Please add a ',' before 'respectively'.

g) Line 203: Maybe 'first-order' instead of 'first order'

h) Line 229: 'are estimated'

i) Line 253: 'temperatures'

j) Line 296: ',respectively,'

k) Line 339: technical details such as the beam diameter can be avoided from the 'Discussions'

l) Line 346: Sounds more like an observation than interpretation and therefore should be moved to 'Results'.

6) Please provide caption for each supplementary figure.

Authors' Reply to Reviewer 1

We thank the reviewer for appreciating our research contribution and for sharing their critical analysis along with inputs applied to the annotated version of the manuscript. We have attentively considered all the reviewer's suggestions and integrated the edits, corrections, and requested additional information throughout our revised version of the manuscript. Below we provide our responses to the suggested main changes, including detailed clarification about why we describe the pseudotachylyte formation as a near-equilibrium process.

Main changes

1) Geological Settings and history: I found this section a longer read than it is perhaps necessary. I suggest that more focus is given to this section by emphasising those tectonic events that are more directly relevant to fracture-induced decompression melting in peridotite and the environmental conditions that may facilitate it, with less emphasis on the broader history of the peridotite, which is also comprehensively reported elsewhere in the literature.

We agree with the reviewer's point. We have removed any information that constituted a sort of "dead weight" during reading the section dedicated to the geological setting and tectonic history of the Ivrea-Verbano Zone (section 2 reported in lines 120-188). We present the main aspects concerning the petrology and mechanics of formation of the pseudotachylytes and their host rocks of the Balmuccia Peridotite Massif.

2) Methods: If fieldwork was carried out, are there any particular approaches to field work that should be described here? This would provide context for the hypothesis for decompression melting. What and how many samples did you study and where did they come from (give grid coordinates).

We have added in-depth information about our strategies for outcrop characterization and sample selection at the beginning of the Methods section (lines 191-209).

3) Crystallisation and recrystallisation seem to be used interchangeably. It would be helpful to provide clear definitions of what process is described by each of these terms.

We have removed this confusion by removing "pseudotachylyte recrystallisation" from the text. We now only apply "crystallisation" intended as formation of new mineral phases upon cooling.

4) Similarly, the terms flash melting and decompression melting are not clearly defined and in this particular study the boundaries between the two become blurred. This might be because the decompression melting described here is rapid (a handful of seconds?), giving rise to flash melting. I suggest both terms are defined at the onset and their use in this study explained.

We have better differentiated flash or frictional melting from fracture-induced decompression melting upfront in text (in the introduction section). Frictional or flash melting (we then apply "frictional melting" throughout the manuscript text) is first defined in lines 74-77; decompression melting is detailed in lines 96-114. These lines

were originally placed in the discussion section. Thanks to the reviewer's suggestions in the annotated manuscript version, we moved these lines to the introduction.

5) In fig. 2, how did you confirm that spinel and opx are in a glassy state and they are not for example cryptocrystalline? This seems important to demonstrate in support of your interpretations of thermodynamic calculations.

Both glassy components were identified geochemically; chemical profiles were measured by STEM-EDS. The number of oxygens calculated from these indicates a glassy arrangement of such oxygens. Moreover, from a phase habitus standpoint, these glassy droplets display curvilinear boundaries that great differ from the suspended mineral phases such as clinopyroxene as reported in Figure 2. Following reviewer #2's comment, we considered XRD analysis to identify the potential cryptocrystalline phases reviewer #1 warns about. The major drawbacks when analysing nanominerals or looking for cryptocrystalline phases using XRD is that when the crystallite size decreases to nanoscale dimensions and is present in very low volume fractions, the XRD peaks broaden or are not revealed. This makes the method unsuitable for quantitative analysis in our study based on previous research attempts (Di Genova et al., 2018; 2020a; 2020b; Holder and Schaak, 2019; Schuller et al., 2023).

6) The assumption of equilibrium in a pseudotachylyte is necessarily a point for further discussion as these systems are not currently understood as equilibrium systems. Yet this assumption underpins the thermodynamic models in Fig. 3 and 4 and therefore the results, discussion and conclusions of this study. Given the truly rapid changes of environmental conditions during Pst formation I do find it difficult to think of this system as a system in equilibrium. Even rocks in their solid state are often not in equilibrium.

I encourage the authors to define what they mean by near-equilibrium in the pseudotachylyte melt, provide further supporting evidence for their assumption of equilibrium and expand on both their reasoning and on the limitations of their assumption.

As thermodynamics is not in my direct field of expertise, I hope there will be another reviewer with this expertise who can comment further here.

We thank the reviewer for giving us the opportunity to better detail the petrological, thermal, and physical conditions that justify the treatment of pseudotachylyte formation as a near-equilibrium process. Specifically, the near-equilibrium conditions for forming glassy pseudotachylytes is based on the quantitative comparison between the timescales of heat diffusion, chemical diffusion, and solidification and the timescales of rapid melting and cooling required for pseudotachylyte formation. We provide a detailed quantitative description in section 5.2 (see lines 408-480). We also anticipate in the first paragraph of the Methods section that thin natural pseudotachylytes (i.e. with thicknesses on the millimetre scale) represent an ideal case where near-equilibrium conditions can be achieved during frictional sliding, comminution, melting, and pseudotachylyte formation, with efficient heat transfer in a limited mass of material, mass conservation (without major apophyses), and identifiable unmelted and melted

components in the pseudotachylyte and the surrounding fractured rock. Hence, near-equilibrium conditions are petrologically quantifiable using thermodynamic constraints – i.e., Perple_X modelling in our study (see lines 203-209).

Other comments

Line 216 - What and how many samples did you study and where did they come from (give grid coordinates)

We collected five pseudotachylytes chips (with the host rock around) (lines 264-265). Only three samples were selected for thin section preparation and petrographic inspection and microstructural analysis (line 280). We then focused on analyzing geochemically one pseudotachylyte at both microscale to nanoscale (lines 285-286). In the previous revision round, we specified the GPS coordinates (lines 262-263). We have added the grid coordinates on the large geological map of the Ivrea-Verbano Zone in Figure 1. In Figure 1 we have separated the foliation data from the lineation one for improved readability of the figure. We have also added a new figure (Figure 2) showing a digital illustration of a field tracing of the outcrop of the Balmuccia Peridotite Massif located in the north bank of the Sesia river. This illustration shows the orientations of the A-type pseudotachylytes, which also help the reader understand the orientation of the pseudotachylyte reported in Figure 3A.

Line 407 - I understand what you are trying to demonstrate: that the use of STEM EDS is important in this study. Why not say it explicitly with reference to the technique rather than using long sentences such as in this section? Referring to nanoscale seems excessive as Fig.2 is in the order of ~1 um scale. But citing the technique directly gives a better idea of the resolution power and activation volume.

STEM instrument works at atomic to nanoscale by definition. Capturing phases and structures at microscale is possible but it is not the main analytical goal of this instrument. By analogy, electron probe microanalyzer (EPMA) captures structures and geochemical information at microscale, but it also offers sample overview at millimeter scale. Also here, the EPMA's primary goal is to undertake microscale analysis. We agree with the reviewer that citing a key reference can help comprehend the analytical capabilities of the STEM instrument. Please, see line 287, in which we refer to the work by San Gabriel et al. (2024) who use the same exact Hitachi STEM model adopted in our study.

Line 416 - Again, in Fig. 2 you are dealing with micron to sub-micron scale analyses, so referring to nanoscale is pushing it. But mentioning STEM EDS will help the reader see the value in the data.

After the latest revision round, Figure 3 shows field scale and microscale view of the pseudotachylytes. If the reviewer refers to Figure 4, we remind the point made above

for the reviewer's comment related to line 407 from the previous revised manuscript version. In the caption of Figure 4, we mention the STEM-based SE imaging of pseudotachylyte glass and accompanying EDS analyses.

Line 502 - How did you confirm that spinel and opx are in a glassy state and they are not for example cryptocrystalline? This seems important to demonstrate in support of your interpretations below.

We show the glassy nature of the pseudotachylyte (with suspended clinopyroxene minerals) in the Results section using mineralogical and microstructural observations in Fig. 3C-D and geochemical and nanostructural observations in Fig. 4. In short, the glassy nature is given by the transparent to weakly absorbent aspect of the central portion of the pseudotachylyte (Fig. 3C) (see the revised lines 403-405), the glassy phase revealed by BSE imaging via SEM and EPMA (Fig. 3D) and WDS analysis via EPMA including the low totals owing to the presence of water (which is absent in olivine, clinopyroxene, orthopyroxene, and spinel) (lines 437-442 and EPMA data table in supplementary materials), the curvilinear contacts of the glassy phases analysed via EDS with the STEM instrument (Fig. 4) along with the lack of stochiometric oxygen coordination (lines 437-441).

Line 619-620 - Presumably the amount will also depend on the amount of fluids available? I would like to see some discussion on what considerations drove the decision on the wt% fluids to use in Fig [5].

The reviewer's point is not clear here: what "amount" does depend on the amount of fluids? To the best of our understanding here, we emphasize that the concentration of water used in the Perple_X calculations whose results are displayed in Fig. 6 comes from previous assessments led by other authors (Obata and Karato, 1995; O'Hara and Sharp, 2001; Ferrand et al., 2018) reporting that relatively unaltered pseudotachylytes are hydrous glasses (up to 1.5 wt.% H2O) containing vesicles, suggestive of fluid exsolution during pseudotachylyte melting at temperatures and pressures at which excess water is not soluble in the melt (lines 670-674). Moreover, with respect to anhydrous conditions (section 5.3), the mineral+melt assemblage and compositions are consistent with the presence of water (section 5.4). We also highlight that the concentration of water here applied is important to match the structural and chemical evidences in lines 656-663.

Minor Text Edits

Line 125: The Balmuccia Massif is one of the largest peridotite slivers

Done.

Line 132: along with the Baldissero

Done.

Line 456 - Incomplete or confused sentence – I think I understand what you want to say but really a schematic of this process to refer to would help. Include such schematic in Supp Material and reference it here.

We do not understand the schematic the reviewer requests here. We add further emphasis on large undercooling (see lines 538-544, which are lines already reported in the previous revised version of our manuscript), which helps suppress the formation of new minerals during frictional melting process along with efficient thermal diffusion (lines 528-531 and 553-556).

Authors' Reply to Reviewer 2

Line 96 - Where are these glassy pseudotachylytes from and where is the evidence of their glassy nature? XRD?

We share the same response to one of the comments presented by Reviewer 1, who presents a similar question in their comment related to line 504 (see above). Regarding the reviewer's proposal of XRD analysis to reveal the glassy nature of the pseudotachylyte, we share our response we already gave to Reviewer 2 in our previous rebuttal letter (reported above). Here is the specific response:

Following reviewer #2's comment, we considered XRD analysis to identify the potential cryptocrystalline phases reviewer #1 warns about. The major drawbacks when analysing nanominerals or looking for cryptocrystalline phases using XRD is that when the crystallite size decreases to nanoscale dimensions and is present in very low volume fractions, the XRD peaks broaden or are not revealed. This makes the method unsuitable for quantitative analysis in our study based on previous research attempts (Di Genova et al., 2018; 2020a; 2020b; Holder and Schaak, 2019; Schuller et al., 2023).

Now, by flipping the attention from crystallites (highlighted by the reviewer in the previous review round) to glass, XRD technique is also unsuitable to offer the bullet proof that the pseudotachylyte is glassy because, if the clinopyroxene grains suspended in the pseudotachylyte are large enough to be detected via XRD, the resulting spectrum (in the 2theta-intensity plot) may show clinopyroxene peaks but they do not demonstrate that the psedotachylyte is glassy vs. crystalline. As already shown by the other data other than XRD, the pseudotachylyte detailed in our study is a glass with suspended clinopyroxene minerals.

Line 341 - Would it be possible to present more evidence supporting the glass interpretation?

We have offered all the possible evidences in the light of the means available to us. If the reviewer wants to see XRD data, we emphasize the unsuitable application of this technique in demonstrating the glassy nature of pseudotachylytes.

Line 1100 - "I visited this outcrop and the geometry of these A-type veins is quite unusual. The margins are not straight … Would it be possible to see an XRD diagram of the vein material?"

We believe that we have replied to the reviewer's point in our previous responses.

Figure 3 - Decompression melting during seismic rupture should be adiabatic due to the low thermal diffusivity of rocks in general. Therefore the PT path of the lherzolite should go down along the vertical axis …

If the reviewer refers to Figure 5 (renumbered after adding the new Figures 1 and 2 in the present revision) which shows the P-T plot under anhydrous conditions, we include decompression as a potential mechanism that contributes to generate pseudotachylyte (section 5.3) and agree that the pressure should decrease by going down in a diagonal way during heating (temperature increase) produced by frictional heating and melting.

Authors' Reply to Associate Editor

- Apart from the first paragraph, the 'Introduction' sounds incoherent. It is particularly unclear how the discussion in the second paragraph is related to the objectives outlined at the end.

We thank the reviewer for their point. We have improved the Introduction section by better differentiating flash or frictional melting from fracture-induced decompression melting. Frictional or flash melting (we subsequently employ "frictional melting" throughout the manuscript text) is first defined in lines 74-77; decompression melting is detailed in lines 96-114. The latter lines were originally placed in the discussion section.

- I recommend adding sub-sections in Sec. 3 corresponding to the different methods and then grouping the results in Sec. 4 accordingly.

In the Methods section, each paragraph clearly separates one method from another. The Results section narrative is constructed to present the results in a cohesive manner by avoiding redundant description when applying one method or another to address the same data or result.

- Figure 1: What do the regions bounded by yellow dashed curves represent in A and B? Please explain in the caption.

Those regions are populated by olivine minerals that were displaced by an offset of 1 cm. We have added such information in the figure caption.

- Figure 2

1. Since there are several sub-figures, please label them (a), (b), (c), etc.

As we tend to mention all the results displayed in Figure 2 altogether, we have renounced to specify each panel with a letter. Moreover, the reviewer's suggestion of labeling panels with letters works great when each panel shows the same item (e.g., SE image, chemical plot, or chemical map). In our case, we have an SE image (plus SE image inset of cpx), a chemical plot, and X-ray distribution maps of the three most representative major elements, which are easily discriminated without additional labeling.

2. What does the inset represent? And, from which location has it been captured?

That SE image inset was captured in another portion of the glassy pseudotachylyte made of Al-Cr-spinel as also specified in the supplementary materials. Unfortunately, we have not managed to find the same sample portion capturing the copresence of clinopyroxene minerals and glassy droplets of orthopyroxene and Al-Cr-spinel at nanoscale.

3. Please label the x-axis of the plot and the two ends of the yellow horizontal line on the x-axis.

It was already labeled with "micron". The yellow line indicates the chemical profile in this figure. We now specify this piece of information in the figure caption.

4. The stippled vertical line perhaps marks the point where the yellow horizontal line intersects the dashed curve in the above image. Please write it down in the caption.

Done.

- Other minor comments

1. Lines 48-50 sound repetitive

We have removed any redundant information in the first paragraph of the introduction (see lines 42-59).

2. Line 52: What type of alteration and what do they alter to? Please list a few examples.

We removed this sentence from the manuscript as irrelevant in the context of this work.

3. Line 70: Please define flash melting before this.

We define “flash” as synonym of “rapid” in lines 68-72. We then define frictional melting in lines 72-74. Finally, we clarify the use of frictional or flash melting in lines 74-77. We then apply “frictional melting” throughout the manuscript text.

4. Line 152: Perhaps it should be ‘composed of’ instead of ‘composed by’

Corrected.

5. Line 177: ‘the presence of’ could be deleted

Corrected. See line 326.

6. Line 193: Please add a ‘,’ before ‘respectively’.

Done.

7. Line 203: Maybe ‘first-order’ instead of ‘first order’

Done as suggested.

8. Line 229: ‘are estimated’

Done.

9. Line 253: ‘temperatures’

Done.

10. Line 296: ‘, respectively,’

Done.

11. Line 339: technical details such as the beam diameter can be avoided from the ‘Discussions’

Removed the detail referred to the beam diameter used during the EPMA analysis of the glassy pseudotachylyte.

12. Line 346: Sounds more like an observation than interpretation and therefore should be moved to ‘Results’.

We have rectified this point throughout the discussion by mentioning, in places, observations to highlight the corresponding interpretations.

- Please provide caption for each supplementary figure.

We have provided captions for the table and each series of images included in the supplementary materials.

2nd Round of Revisions

Decision Letter (17 June 2024)

Dear Dr. Pistone and co-authors,

Thank you for resubmitting your manuscript "Can pseudotachylytes form via fracture-induced decompression melting under hydrous conditions?" to Tektonika. We have now reviewed this new version of the paper, your response to the concerns points raised and have obtained two new reviews of this revised version of the paper. Based on the reviews, we conclude that the manuscript requires Moderate Revision.

Reviewer 1 is happy with most of the changes you have made though is not satisfied that all the comments raised have been addressed and states that your response letter does not respond to all the points previously raised. We also agree with this assessment. We have also consulted a new reviewer and they have raised several critical points that we suggest must be addressed in the manuscript. Additional emphasis/information on the field/outcrop observations and structural settings of the studied pseudotachylyte are needed, and your assumption that flash melting of pseudotachylyte is an equilibrium or near-equilibrium process requires additional justification. Some observations / analytical results are currently missing from the 'Results' section and have rather been explained under 'Discussions', so some text rearrangement is required. The comments by both reviewers are appended below this letter.

We request you prepare a traditional rebuttal containing point-by-point responses to the new comments raised below by Reviewer 3. In addition, as there are comments from the previous round of reviews that were not previously addressed point by point, we ask that you now also provide these for Reviewer 1 and 2. Please also provide justification(s) for disagreeing with and thus not incorporating any of the suggestions. Please submit a revised manuscript incorporating the recommendations, and a copy with all the changes marked (e.g. using tracked changes) is also required. Tektonika values your contribution, and we look forward to receiving your revised manuscript by 17 August 2024.

Please get in touch with us if you have any questions.

Best regards,

Dripta Dutta (Associate Editor)

Janine Kavanagh (Executive Editor)

Comments by Reviewer 1 (Elisabetta Mariani)

I am happy with most changes implemented, although I should say that not all of my text box comments/suggestions in the manuscript document have been implemented. This is fine, but the reasons why have not been explained in the rebuttal document.

I encourage the authors to go through this process and also look out for typos at the same time as, re-reading the manuscript I have spotted a few.

Comments by Reviewer 3

Dear Editors, dear Authors,

I have carefully scrutinized the paper "Can pseudotachylytes form via fracture-induced decompression melting under hydrous conditions?" submitted (for a second revision) by Pistone et al. to the international journal Tektonika.

This paper suggests a new possible mechanism for the formation of pseudotachylytes in the deep crust/upper mantle conditions, studying the fist (pre-Alpine) generation of pseudotachylytes (so called A-type pseudotachylyte) in the peridotite of Balmuccia (Italy) located in the Ivrea-Verbano zone, in the Southern Alps domain of the European Alps. The Authors proposes how the the formation of A-type pseudotachylyte is linked to fracture-induced decompression melting under hydrous conditions using mostly petrological (and equilibrium thermodynamics) and geochemical constraints. This proposed aspect, neglecting the previously suggestion of T increase related to shearing, sounds new, interesting and quite provocative. So, concerning these points, the manuscript of Pistone and co-authors is more than welcome on an international journal. Anyway, I see some several key-points not yet clearly addressed in the current version of the manuscript, as detailed explained below. In summary, I suggest major revision for this paper, before it could be accepted for be published on an international journal.

Main comments

1) General and local geology. The description of general geology of the Alps, and particularly of the local structural settings around Balmuccia is poorly addressed in the current version of the paper. I am not asking for a revision of the Alps (and of the Ivrea zone) but a better general description, accompanied with geological figure of the W Alps and a zoom around the area of Balmuccia peridotite body, is really needed. take present how not all the readers may be familiar with Alps and Balmuccia.

2) Structural settings and structural analysis of the Balmuccia peridotite. I can understand how this paper rely on previously collected data, however more care should be addressed to the structural settings of the studied pseudotachylytes.

A more in-depth description of the field/outcrop occurrence of studied fault rocks, associated to their microstructures should be given in the paper, accompanied with several meso-and micro photos, and possibly geological outcrop sketch showing the fault rocks occurrence and geometry. Indeed, looking to the available images in the literature a genesis for pure Mode I fracture (as stated in Line 100-105) seems unlikely. My suggestion is to test your theoretical models with constrains from the field, starting from the latter.

3) I suggest including in the paper a short revision of models proposed for the genesis of deep-seated/formed pseudotachylytes, including key papers such as Menegon et al., 2021 (https://doi.org/10.1098/rsta.2019.0416) or Menegon et al., 2017 (https://doi.org/10.1002/2017GC007189). Such effort would justify your

statement/proposed model

4) Despite I read the manuscript several times (see e.g., L203-209), it is still not clear to me how (flash) melting of pseudotachylyte could be considered as an equilibrium, or near to equilibrium, process. I think this should be netter justified, also including some few references. Indeed, despite I am aware of thermodynamic modelling to quantify cooling of the melt or the conditions in the rocks before the pseudotachylyte formation, I guess many Authors consider this such as a non-equilibrium process (see Spray 2010). I final suggestion concern the Perplex output. I suggest to re-draw manually the curve of the pseudosections, those small steps in the field boundaries are not nice to see.

6) Mixing of data/results with the discussion. This represents a main problem in the paper. Actually, the thermodynamic modelling for peridotite melting as well as the timescale for melting solidification of the veins, is not reported in the "results" but given directly in the "discussion" part of the paper. The reason why it is not clear to me. I guess first you present the data and the models and their results, later on you discuss their implications.

Minor comments

In this part I am going to list my minor comments, according to the Line in the paper.

L62 I think here more references are needed, including Mengon et al., 2021; Scambelluri et al., 2017; Pennacchion et al., 2020; Toffol et al., 2024

L87-92 Here you should spend more words explaining why this is inconsistent.

L165 “Liguro-Tethyan” is not a correct form, please replace with Alpine Tethys or Liguria-Piemonte ocean;

Authors' Reply to Reviewer 1

I am happy with most changes implemented, although I should say that not all of my text box comments/suggestions in the manuscript document have been implemented. This is fine, but the reasons why have not been explained in the rebuttal document.
The comments and suggestions reported directly in the first submitted version of the manuscript were interpreted as minor suggestions to take or reject without needing a specific reply or comment from our side. In the light of the recent revision, we have applied substantial changes that the original reviewer's comments and suggestions are no longer applicable. If Reviewer #1 still thinks that we must owe a reply, we are happy to receive her guidance on the specific comments they want that we reply to. To this goal, we kindly ask for an independent document in which Reviewer #1 lists that points that we are supposed to address.

I encourage the authors to go through this process and also look out for typos at the same time as, re-reading the manuscript I have spotted a few.
Remaining typos were removed from text during the last round of revision.

Authors' Reply to Reviewer 3

Dear Editors, dear Authors,

I have carefully scrutinized the paper "Can pseudotachylytes form via fracture-induced decompression melting under hydrous conditions?" submitted (for a second revision) by Pistone et al. to the international journal Tektonika.

This paper suggests a new possible mechanism for the formation of pseudotachylytes in the deep crust/upper mantle conditions, studying the fist (pre-Alpine) generation of pseudotachylytes (so called A-type pseudotachylyte) in the peridotite of Balmuccia (Italy) located in the Ivrea-Verbano zone, in the Southern Alps domain of the European Alps. The Authors proposes how the the formation of A-type pseudotachylyte is linked to fracture-induced decompression melting under hydrous conditions using mostly petrological (and equilibrium thermodynamics) and geochemical constraints. This proposed aspect, neglecting the previously suggestion of T increase related to shearing, sounds new, interesting and quite provocative. So, concerning these points, the manuscript of Pistone and co-authors is more than welcome on an international journal. Anyway, I see some several key-points not yet clearly addressed in the current version of the manuscript, as detailed explained below. In summary, I suggest major revision for this paper, before it could be accepted for be published on an international journal.

We thank the reviewer for appreciating out work and for sharing their constructive comments.

Main comments

6) General and local geology. The description of general geology of the Alps, and particularly of the local structural settings around Balmuccia is poorly addressed in the current version of the paper. I am not asking for a revision of the Alps (and of the Ivrea zone) but a better general description, accompanied with geological figure of the W Alps and a zoom around the area of Balmuccia peridotite body, is really needed. Take present how not all the readers may be familiar with Alps and Balmuccia.

We agree with the reviewer's point. We have added a new figure (Figure 1) that shows the geographical location of the Ivrea-Verbano Zone in Italy, the geological map of this crustal section, and the structural map of the Balmuccia Peridotite Massif with the specific location of the pseudotachylytes considered in our study.

2) Structural settings and structural analysis of the Balmuccia peridotite. I can understand how this paper rely on previously collected data, however more care should be addressed to the structural settings of the studied pseudotachylytes.

A more in-depth description of the field/outcrop occurrence of studied fault rocks, associated to their microstructures should be given in the paper, accompanied with several meso-and micro photos, and possibly geological outcrop sketch showing the

fault rocks occurrence and geometry. Indeed, looking to the available images in the literature a genesis for pure Mode I fracture (as stated in Line 100-105) seems unlikely. My suggestion is to test your theoretical models with constrains from the field, starting from the latter.

Following the previous Reviewer #1's recommendations, we offer a succinct overview of the geological setting without iterating information contained in previous works that we cite in text. The new lines 142-157 offer a more in-depth contextual information on the structural character of the Balmuccia Massif and how the pseudotachylytes are arranged based on the previous work by Ferrand et al. (2018). We removed the Mode I fracture from text. We also remind an important point here: in this contribution we do not reconstruct the specific mechanics of stress configuration that should lead to the formation of the pseudotachylyte, but we suggest how decompression should accompany friction melting using petrological and geochemical analyses of pseudotachylytes.

3) I suggest including in the paper a short revision of models proposed for the genesis of deep-seated/formed pseudotachylytes, including key papers such as Menegon et al., 2021 (https://doi.org/10.1098/rsta.2019.0416) or Menegon et al., 2017 (https://doi.org/10.1002/2017GC007189). Such effort would justify your statement/proposed model

We thank the reviewers for suggesting these works. Both of them focus on mylonitized pseudotachylytes formed under anhydrous conditions, which appear to be very different from the glassy pseudotachylytes formed under hydrous conditions here investigated. Indeed, these papers propose a rheological model based on "grain-size reduction due to the formation and subsequent recrystallization of pseudotachylytes to a fine-grained (< 30 μm) polyphase aggregate" (Menegon et al., 2017) and ultramylonites (Menegon et al., 2021). We do cite the papers in the revised version of our manuscript (see lines 51 and 577-580), to highlight the likely formation of fine-grained pseudotachylytes and ultramylonites under anhydrous conditions.

4) Despite I read the manuscript several times (see e.g., L203-209), it is still not clear to me how (flash) melting of pseudotachylyte could be considered as an equilibrium, or near to equilibrium, process. I think this should be netter justified, also including some few references. Indeed, despite I am aware of thermodynamic modelling to quantify cooling of the melt or the conditions in the rocks before the pseudotachylyte formation, I guess many Authors consider this such as a non-equilibrium process (see Spray 2010). I final suggestion concern the Perplex output. I suggest to re-draw manually the curve of the pseudosections, those small steps in the field boundaries are not nice to see.

To improve clarity on the (near-)equilibrium nature of pseudotachylyte formation, we have strongly revised section 5.2 by detailing the processes (listed in bullet points a-e) needed to satisfy the equilibrium conditions, followed by the calculations of timescale of heat and elemental diffusion and cooling (using equations from published papers) based on the maximum thickness (5 mm) of pseudotachylyte investigated in our work.

To the best of our knowledge, there are no available references that propose a similar approach for quantifying the formation of pseudotachylyte under equilibrium conditions. Regarding the pseudosections generated via Perple_X, we have improved the display of the phase boundaries in both plots by manually interpolating the "step edges" from the original lines generated by the Perple_X model.

6) Mixing of data/results with the discussion. This represents a main problem in the paper. Actually, the thermodynamic modelling for peridotite melting as well as the timescale for melting solidification of the veins, is not reported in the "results" but given directly in the "discussion" part of the paper. The reason why it is not clear to me. I guess first you present the data and the models and their results, later on you discuss their implications.

We thank the reviewer for this critical point. We now clarify in the Methods section that the calculations of timescales of thermal + chemical diffusion and cooling, and the use of the thermodynamic modelling are meant to be part of the discussion because both sets of calculations are based on the input data in terms of field and analytical data presented in the Results section. These sets of calculations represent the implications following the collected observations and analyses presented in this study.

Minor comments

In this part I am going to list my minor comments, according to the Line in the paper.

L62 I think here more references are needed, including Mengon et al., 2021; Scambelluri et al., 2017; Pennacchion et al., 2020; Toffol et al., 2024

We confirm that we added the suggested references in lines 63-65.

L87-92 Here you should spend more words explaining why this is inconsistent.

We have added a clearer statement in lines 93-95.

L165 "Liguro-Tethyan" is not a correct form, please replace with Alpine Tethys or Liguria-Piemonte ocean.

Corrected accordingly.

3rd Round of Revisions

Decision Letter (18 June 2025)

Dear Dr. Pistone and co-authors,

Thank you for resubmitting your manuscript "Can pseudotachylytes form via fracture-induced decompression melting under hydrous conditions?" to Tektonika. We have now reviewed this new version of the paper along with your response to the comments raised. We still are of the opinion that your manuscript has the potential to make a significant contribution to the understanding of the origin of pseudotachylytes and is of interest to the readers of Tektonika. However, whilst several of the concerns raised by the reviewers and editors have now been addressed, we have identified some points which are yet to be responded to or that we would like further clarification on. Additionally, as the manuscript was first submitted in 2023, the literature cited needs some updating to the state of knowledge in 2025. Currently only two papers from 2024 are cited and none from 2025.

We would like to offer you the opportunity to address the remaining concerns listed below and ask that you submit a traditional rebuttal letter containing point-by-point responses. Please also provide justification(s) for disagreeing with and thus not incorporating any of the suggestions. A revised manuscript incorporating the recommendations and a copy with all new changes marked (e.g. using tracked changes) is also requested.

Given the remaining concerns listed below are moderate, we ask that you resubmit your manuscript within 1 month.

Thank you for giving us the opportunity to consider your work and please get in touch with us if you have any questions.

Yours sincerely,

Dripta Dutta (Associate Editor)

Janine Kavanagh (Executive Editor)

Comments by Reviewer 1

Because these were originally annotated in the main manuscript file during the 1st round of revisions, they are appended below the Comments by Reviewer 1.

Comments by Reviewer 2

Because these were originally annotated in the main manuscript file during the 1st round of revisions, they are added under Comments by Reviewer 2.

Additional editorial team comments

The bullet point list presented in lines 450-475 requires a sentence to introduce it or the use of sub-headings for each point described in the preceding list.

The literature cited needs some updating with more recent papers added up to the state of knowledge in 2025. Currently only two papers from 2024 are cited and none from 2025.

Lines 80-81: "Recent experiments using cubic-anvil apparatus have simulated ... "

Line 82: "a pressure of 1.1 GPa and have shown..."

Line 108: "surpass their solidus" - I think the use of 'own' sounds redundant.

Line 146: northward

Line 176: I think 'serve' sounds as a better alternative to 'act' in this context.

Line 217: micro-

Line 218: "and ensured..."

Line 238: I think it should be 'times'

Line 318: "wide range of pressure..."

Figure 1: Please put a coordinate grid on the map with the 10 km scale.

Authors' Reply to Reviewer 1

Because these were originally annotated in the main manuscript file during the 1st round of revisions, they are appended below the Authors' Reply to Reviewer 1.

Authors' Reply to Reviewer 2

Because these were originally annotated in the main manuscript file during the 1st round of revisions, they are appended below the Authors' Reply to Reviewer 2.

Authors' Reply to Additional editorial team comments

The bullet point list presented in lines 450-475 requires a sentence to introduce it or the use of sub-headings for each point described in the preceding list.

Added in lines 524-526.

The literature cited needs some updating with more recent papers added up to the state of knowledge in 2025. Currently only two papers from 2024 are cited and none from 2025.

It is unclear which specific papers the Editor/reviewers refer to. In line with the content of our manuscript, we have included 4 works on pseudotachylytes published in 2023 (1), 2024 (2), and 2025 (1) along with other 19 works published prior to 2023.

Lines 80-81: "Recent experiments using cubic-anvil apparatus have simulated ... "

Done.

Line 82: "a pressure of 1.1 GPa and have shown..."

Done.

Line 108: "surpass their solidus" - I think the use of 'own' sounds redundant.

Done.

Line 146: northward

Done.

Line 176: I think 'serve' sounds as a better alternative to 'act' in this context.

Done.

Line 217: micro-

Done.

Line 218: "and ensured..."

Done.

Line 238: I think it should be 'times'

Done.

Line 318: "wide range of pressure..."

Done.

Figure 1: Please put a coordinate grid on the map with the 10 km scale.

Added.

Acceptance Letter (23 July 2025)

Dear Dr Pistone and co-authors,

We are pleased to inform you that both the Associate and Executive editors have reviewed the resubmitted revision of your manuscript, " Can pseudotachylytes form via fracture-induced decompression melting under hydrous conditions?" and are satisfied with the changes you have made in response to the reviewers' comments.

We are happy to confirm that your manuscript is now accepted for publication in ТЕКТОНІКА. The next steps will involve production processing, during which you will receive proofs for final review before publication. Our production team will be in touch to oversee this process.

We have one request that during the copyediting stage you add one sentence on the criteria used to select the three samples for petrographic studies and the one for STEM analysis. A statement explaining your selections would be appreciated by the readers.

You also raise an important question regarding co-authorship and recognition of the work completed by a student who has not provided consent to be co-author. Thank you for raising this to our attention. We have discussed the issue with the Tektonika editorial board and suggest that because the student has not given consent to be co-author that consent cannot be assumed, and instead they should be thanked for their contribution in the Acknowledgements section. If during the copyediting stage the author team are able to make contact with the individual and obtain their consent to be co-author then of course we can add them to the author list at your request.

Thank you for your contribution to ТЕКТОНІКА and we hope you will consider publishing with us again. We have greatly appreciated your cooperation throughout the review process and look forward to sharing your work with our readers.

Congratulations on this impressive piece of work, and thank you again for submitting your work to ТЕКТОНІКА!

Yours sincerely,

Dripta Dutta, PhD, Associate Editor

Janine Kavanagh, PhD, Executive Editor