Reply to: Anonymous Referee #3
On the manuscript: Seismogenic frictional melting in the magmatic column
by J. E. Kendrick, Y. Lavallée, K.-U. Hess, S. De Angelis, A. Ferk, H. E. Gaunt, D. B. Dingwell, and R. Leonhardt

Original reviewer comments are in dark green and replies to comments are in black.

We would like to thank reviewer #3 for the helpful comments that led to the improvement of the manuscript's legibility and message.

General Comments

In this manuscript the authors describes a layered structure developed in a andesitic lava block sampled in a block and ash flow deposit from the 2010 eruption of the Soufriere Hills volcano (SHV), Montserrat island. Petrography and petrology of this layered microstructure is analyzed while magnetic and calorimetric measurements are performed to characterize the pseudotachylyte nature of the main band. Although the presence of pseudotachylytes in the volcanic products issued from extrusion and/or explosion of andesitic events is recognized from a long time - as illustrated by the number of references cited in this manuscript – this paper is novel as it includes an experimental approach in deformation to link the frictional melting produced in the discrete structure to the periodic seismogenic rupture in the ascending magma. I really appreciate this approach. From the general point of view, the spelling is correct and the figures are well designed.

The main results of this considerable work in terms of volume of datas and analytical techniques involved are: the pseudachylytic nature of a part of the layered structure is evidenced; this structure is a shear band along which crystal cataclase (cataclasite s.s) taken place; the pseudotachylyte is linked to the seismogenic rupture in the ascending magma and finally the pseudotachylyte is an impermeable barriers as demonstrated by the simulation realized on medium resolution tomographic 3D images. From this last conclusion, the authors discuss the potential influence of the pseudotachylyte on degassing capabilities of the magmatic column during ascent of the magma followed by an eventual explosive activity and/or dome formation. Finally, I particularly appreciate the experimental approach to link the repetitive seismicity frequently recorded during magma ascent and the development of pseudotachylyte along conduit walls.
The authors conclude that the presence of pseudotachylyte documents the evidence of frictional melting along conduit walls of an ascending viscous magma.
The reviewer has highlighted the key findings of the study and notes the novel aspect of the experimental element to the study, which the authors find to support the analytical observations. Although it is of no consequence to the paper, we would like to note that what is said above, the formation of pseudotachylytes in volcanic extrusion/ explosion has only very recently been identified (Kendrick et al., 2012), and that prior to that it was only mentioned in 4 publications in any volcanic scenario- landslide/ caldera collapse/ block and ash flow (Grunewald et al., 2000, Kokelaar, 2007, Legros et al., 2000 and Schwarzkopf, 2001). We would also like to make clear that permeability measurements were made physically on the sample in the permeameter from the Rock & Ice Physics Laboratory at UCL.

1- The main major comment comes from the general organization in the manuscript between the different investigations (petrography/petrologic descriptions, geophysical characterization, magnetism, Permeability determination from tomographic imaging,

experimental test to characterize the mechanism of slip. . .), the description of the used methodologies and the interpretations. In that sense I follow the suggestions proposed by the first referee: I would prefer to see a rigorous separation between the analytical techniques, the results and the interpretations, and finally the conclusion. On the whole this manuscript need a moderate revision.
This comment agrees with the reorganisation suggested by reviewer #1, and hence I hope the reviewer will be satisfied with the changes made and detailed further in the earlier responses.

2- From this point a view an example is given by the term of "pseudotachylyte" that is used as early as in the petrographic investigation. I am not convinces that the authors can characterize strictly the pseudotachylitic nature of a part of the structure only from the observations made in the field or even in thin section. At this scale of observation this part of the shear band may be confused with an ultracataclastic vein . . .?
There are many differences between a pseudotachylyte and an (ultra-)cataclasite, the authors hope that it is clear from the manuscript in its modified form explicitly what methods can be used to identify these contrasts. As for terminology, we must refer to the pseudotachylyte early in the text to introduce the make-up of the shear band, and as it was a suspected pseudotachylyte, proven to be so during the study, the authors feel that referring to it as such is justified (just like referring to a silicate magma shown to have 58% silica as an andesite).

3 –There is also sometime a lake in the description of the different parts of the shear band. In particular, a quantitative analysis of the textural characteristics in the pseudotachylyte, the cataclase and the host rock is required.
We have significantly edited the bulk of the descriptions of the sample to provide more information as the reviewer requested. However, we find that a full quantitative analysis is beyond the scope of this paper, given the extent of other techniques used.

This work is based on one example found in the field and the authors conclude about the importance of this type of structure (i)- because first, is an impermeable barrier that possibly act on (limit?) the degassing process of the magmatic column and second (ii) express a melting linked to the repetitive seismicity that taken place in the magma along the conduit wall. I agree with these conclusions (already evoked and described in previous publications). Therefore such structures should be very common in the blocks issued from the magmatic column/dome and sampled in the block and ash flow. Is there any systematic field observations and calculations made by the authors about the volume of materials affected by theses shear bands ?. It is clear that these structures are discrete and initially located only along the conduits walls or along shear zones, if present, in the magmatic column. The volume of magma affected by the frictional melting is therefore not necessarily very large but the number of shear bands should be significant to express the large number of earthquakes recorded during the eruption.
This comment is very similar to the comment from reviewer #2 and as such the authors will repeat the relevant part of the response: *The reviewer makes an interesting point again, and the authors agree that with such a link between seismicity and fracturing then the process is relatively common. I think that a key point here is that these features undoubtedly are more common than we have observed, but their chance of survival and subsequent deposition on the surface of a block in an accessible part of the block and ash flow at the base of the volcano is slim (assuming that they may form within ~2m of the conduit margin in a ~30m conduit, and that the conduit margin is then overprinted by gouge formation at shallow depths and that, during dome collapse particles from μm's to 10's meter scale are formed, leaving only a small*

percentage accessible for study). With the ongoing eruption at Montserrat the study of the in-situ dome is not yet possible, although this would aid the investigation into shear bands significantly.

Others minors comments or questions

What was the influence of the porous fraction probably present during formation and development of this shear band along the conduit wall? this point remains strongly speculative but could be potentially evoked.

This is a really important point highlighted by the reviewer, and the answer is- we just don't yet know how to interpret the pre-eruptive porous structure from a description of the eruptive products. There is not yet a comprehensive model for the development of the permeable porous network during magma ascent, so with or without shear zones, we have little idea as to the porosity at the depths where seismicity occurs. As to the effect of porosity (once it's known) on the development of shear bands, this has been investigated (see e.g. Wright 2009, and Laumonier et al., 2011) and imperfections /pores can act as nucleation sites for strain localisation, but these processes are far from well understood at yet, especially considering the additional presence of crystals.

The pseudotachylytes are often characterized by injection veins along which secondary brittle injections often take place in the host rock or in the cataclase zone. Is these structures have been observed ?

This comment is also very similar to a previous question from reviewer #2, so we will copy the relevant portion of the text:

While the magma is able to behave as a brittle solid and fracture at strain rates exceeding the timescale of relaxation, as soon as the slip event is over the strain rate is such that the magma is able to flow. This material property, unique to glass, may also explain the lack of injection veins emanating from the shear band, as the immediate return of the andesite into its fluid state would not allow the brittle propagation of fractures for injection veins.

This sample was not in place. Is there any information from the relationships between the orientation of the shear band and the other structures such as the mineral fabric in the host rock that constrain the original orientation of this band with respect to the conduit wall (and therefore the flow) geometry ? this point is relatively important to support the control on the bulk permeability of the magma by the pseudotachylyte (see fig.9).

Again, this point was raised by reviewers #1and #2 and the relevant text is repeated here: *The reviewer is correct, measurements were made on an erupted block. The block was erupted during a block and ash flow from a partial dome collapse in 2010, so its original location cannot be traced. As to speculation of the orientation with respect to the conduit margin, we know from many previous studies that shear zones can form along conduit margins, especially during the extrusion of highly viscous magma during dome eruptions. But we also know that strain localisation in one area can result in stress build-up in the adjacent rocks and magma, and so related fractures forming simultaneous to the principal slip surface are real possibility, and to achieve a slip distance of just 15cm to form melt is also not unfeasible. While the orientation is important to establish the exact effect of the vein on degassing, it is clear that the sample records a relic degassing pathway, for example due to the presence of chlorite shown here and other evidence (this is discussed in more detail in Plail et al., 2014 EPSL).* Hence we can confirm its origin was in the conduit and its relation to degassing, but not further information.