Reply to: Anonymous Referee #1
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.

Firstly, the authors would like to thank reviewer #1 for their constructive comments. The reviewer has highlighted a number of key aspects which together with the other reviews has improved the clarity and quality of the manuscript.

This manuscript describes microstructures developed in the magmatic column of the Soufriere Hills volcano during the 2010 eruption by integrating data from calorimetry and magnetic measurements, high velocity friction experiments, and structural and petrological data from an erupted rock sample.
The main results of the manuscript are:
1) Evidence of frictional melting (i.e., formation of pseudotachylyte) within the magmatic column. 2) Pseudotachylyte presents an isothermal remanent magnetization interpreted as due to local electric currents in the magmatic conduit. 3) Pseudotachylyte is an impermeable barrier that may influence the degassing at the Soufriere Hills volcano. 4) Intermittent frictional melting and formation of pseudotachylyte may be linked to the long period drumbeat seismicity at the Soufriere Hills volcano.
The reviewer has identified the key aspects of the paper accurately, but in doing so highlighted the fact that there was previously too much emphasis on the permeability and degassing in the manuscript. This, in light of comments from the editor as well, has been reworked in the text, with the implications toned-down.

The number of thermal and magnetic measurements reported in this manuscript and the microstructural and petrological data represent an important volume of work. These new data pose a number of intriguing questions regarding the dynamics of magma facture and ascent during dome building eruptions and may be of interest to a broad range of geoscientists. Nevertheless, I think the data should be presented in a more organized way and in more detail regarding especially the methods and result sections. In particular, I suggest being more cautious in distinguishing between results and interpretation (discussion). I suggest for major revisions before final acceptance of the manuscript for publication.
With the substantial reworking of the text that the authors have undertaken following the comments from the reviewers and editor, we are confident that the data is now presented in a more logical and organised manner. In particular the distinction between result and interpretation is more clearly made. The results are more thoroughly discussed, and observations that may have been glanced over in figure captions are now stated in the text.

Major comments:

I) Sample description
The description is too scarce and does not systematically lead to the authors' conclusions and mainly to the deformation mechanisms associated with the structures. This lack of information affects the clear understanding of the manuscript and precludes the comparison between the results of this study and previously published ones. More description of the observed structure is required.

The authors are unclear as to which "previously published" studies the reviewer refers to, however, the comment has been adhered to, and a more thorough sample description is now given.

- Section 2.1 is dedicated to the petrographic description of the host rock and the shear band and provides a qualitative mineralogical description of the host rock and the pseudotachylyte. However, to be consistent, the authors should also provide a specific description for the cataclasite: at the moment, the description is limited to the microstructural assemblage of the cataclasite (i.e., "a combination of the pseudotachylyte and host rock").
A more detailed description of the cataclasite has been added, along with more thorough characterisation.

The manuscript should also include a quantitative description of the mineralogy of the three main structural units (host rock, pseudotachylyte, and cataclasite), e.g, Vol% or Wt% of Feldspar, amphibole, etc.. The microstructure of each unit (host rock, pseudotachylyte, and cataclasite) should be also described. What is the average shape, distribution and orientation of the grains with respect to the wall rocks? What is the grain size range? If the shear band consists of interlayered of pseudotachylyte and cataclasite, what is the layer thickness? How are the contacts/boundaries? Are injection veins present? ... In the same way, there is no legend on fig.1. Could the authors add any information (pseudotachylyte, host rock, lenses, ect. . .) on the photo that may help the reader?
The description of microstructures has been broadened, especially, shape/ distribution/ orientation etc. is noted. The grain size range was provided before, but was listed sporadically for the units, this is more clearly defined and compared now, as is the layer thickness and description of boundaries. We have quantative data for wt % crystallinity and grain size, but only for the sample as a whole (host, cataclasite and pseudotachylyte together), if the editor would like us to add this, it's possible. We have not annotated Figure 1 as the reviewer suggests as the scale is not appropriate for the labels suggested. Instead the figure caption has been expanded, and the text explains in detail the features "pseudotachylyte, host rock, lenses etc." as requested.

II) Magnetic measurements
I am not entirely convinced by the logic behind the discussion in section 2.2 concerning the possible magnetization mechanism (high local electric currents in fault) for the SHV pseudotachylyte. Indeed, previous studies on fault-related pseudotachylytes showed that they have an anomalously high initial magnetic susceptibility (MS) and natural remanent magnetization (NRM) suggesting large electric pulses involved in the magnetization.
The MS and NRM are respectively _ 10:1 and _ 300:1 higher in the pseudotachylyte than in the host rock (Ferre et al., Tectonophysics, 2005*). According to the NRM measurements shown in Fig. 6, it seems to me that the NRM reached similar values for both the pseudotachylyte and the host rock. Can the authors flesh that out more in the main text?
Moreover, the authors should assure the reader on the repeatability of their measurements. In fact, there are no error bars reported in the plots of magnetic measurements.
Could the authors provide a more sound statically basis that rigorously quantifies the differences between NRM, ARM and IRM measurements?
Regarding first the Ms (saturation magnetisation) and NRM (natural remanent magnetisation) values: The reviewer is right that Ferre et al. found much higher values (e.g. 300:1 for NRM) in the pseudotachylte. This could be because the host rock in the Ferre paper was a granite while in our cases we are dealing with volcanic rock, in which the host rock is relatively strongly magnetised and already has a high number of magnetic particles

(which in the Ferre case build only in the pseudotachylyte). We also found lower values in our earlier paper (Kendrick et al., 2012, JSG).
As to the discussion of the results, this has been modified and now offers a better explanation: Note the remanence of the pseudotachylyte is comparable to the IRM, but the demagnetisation pattern of the NRM (remanence) and the IRM are similar for the pseudotachylyte or of NRM and ARM for the host rock. I.e. for smaller alternating fields NRM of pseudotachylyte drops suddenly just as IRM does. For higher fields it decreases more gently just like IRM does. For the NRM of the host rock: it decreases more continuously over the whole spectrum of alternating field values – just like ARM does.
The authors can provide a supplementary table if the editor sees fit, but the authors do not feel it is necessary for the manuscript, and can assure the editor that standard experimental procedures were followed.

The authors should also be quantitative in the appendix A2. They said "There are more FeTi oxides (which are the magnetic carriers) in the pseudotachylyte than in the host rock, but they are smaller, and there seems to be a tendency for higher Ti content in the magnetic carriers in the vein (5 : 1 rather than 10 : 1 Fe : Ti)". What "more FeTi oxides" means (x 2, x 10, x 100)? What "smaller" means?
This was not quite accurately explained previously and suggested importance where there was none. In fact, in the host rock there are a large and small population of FeTi oxides – at an approx. 10μm and 100μm scale, the larger may be either ilmenite or FeTi oxide (see QEMSCAN image in figure 6) with higher iron content (5-10:1 Fe:Ti) while the smaller are all in the range 5-10:1 Fe:Ti. In the pseudotachylyte the large (100μm) fraction are not present, but there are a larger number of magnetic minerals, these range from 1-10:1 Fe:Ti (covering the whole range of composition of those in the host rock) but are all <10μm as a result of grain size reduction. This is consistent with the evidence in figure 5c (the cataclasite) which shows an FeTi oxide locked in breakdown, showing the transitional state between larger magnetic carriers in the host, and more frequent smaller carriers in the pseudotachylyte (the cataclasite has intermediate-sized carriers). However, this information has no implications and as such the authors choose to omit this.

III) Permeability measurements Measurements were made on an erupted block (where was this block in the volcanic edifice?), i.e. on non-oriented samples. It is consequently difficult to conclude about the pseudotachylyte effect on degasing. I strongly suggest rephrasing the paragraph 3 and the abstract in a more careful way.
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. 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). The relevant statements have been rephrased to reflect this information.

IV) High velocity friction experiments
1) The infrared camera used to estimate the temperature during the high speed friction experiments measured the temperature (1) from outside the slipping zone (so a cooling melt) and (2) with a resolution probably lower than the thickness of the slipping zone (so an average temperature between the cooling melt and the nearby solid wall rocks): as a consequence these two limitations resulted in an underestimate of the temperature in the slipping zone. I recommend to the authors to precise the corrections they probably made for the temperature estimation. It would be also very useful to present the mechanical data

associated with this temperature measurement. Indeed, the temperature increase in a slipping zone is mainly governed by the mechanical power (shear stress*slip rate). I suggest adding a new figure: Shear stress versus time or slip.
The thermal camera does represent a minimum temperature, as is mentioned in the manuscript, but this is a result only of the melt cooling on the surface. The relatively high axial stress of 10MPa results in a continued and steady production of melt, which is ejected from the slip zone (see supplementary video) and so is always relatively fresh, no correction was made for the cooling effect, and the monitored temperature was used for the model. Regarding the resolution, it is clear from the right hand panel of the supplementary video, which is the thermal video, that the resolution is high enough to accurately monitor the temperature- the maximum observed temperature along the slip zone was used in the model and we do not believe this resulted in any additional underestimation of the temperature. A plot of shear stress, and additionally shortening, over slip has been added as panel b in Figure 10.

2) I am not entirely convinced that the authors can use the following reasoning: the bulk temperature increase in a slipping zone with an initial temperature condition of 25 C is similar to the bulk temperature increase in a slipping zone with an initial temperature condition of 800 C. Indeed, the (static and dynamic) shear stress may be temperature dependent (see, i.e. Chester, JGR, 1994*) as well as the deformation localization. The authors should comment about this in the main text.
*Chester, F.M., Effects of temperature on friction: Constitutive equations and experiments with quartz gouge ,J Geophys Res., 99, 7247-7262, 1994
While the reviewer is correct in that we do make the assumption that the magma will behave the same at 25 and 800 degrees, it is not without basis. It has been shown that magmas will behave as brittle solids if the deformation rate exceeds the timescale of relaxation i.e. the glass transition. At the strain rates investigated here, magma at 800 degrees is brittle, and we extend this similarity to infer that its frictional behaviour may be equally similar to that of its rock equivalent. We intend to experimentally test such friction conditions as soon as complete a system capable to achieve a controlled high-temperature atmosphere.

Minor comments:

P1662: I see only a petrographical description paragraph 2.1. Please remove petrological.
This has been removed.

P 1663 line 5-14: It would be good to precise, why do the authors use the HS-DSC technique? Is this paragraph useful for the manuscript?
The HS-DSC technique is more sensitive, and hence more likely to reveal a phase at low abundance levels. The authors do not feel it is necessary to justify the use of this method, which is an accurate way to make the measurements in question.

P1664-1665. It would be good to make explicit here that the magnetic and permeability measurement were made on non-oriented samples.
The authors feel that it is now clear in the text that the samples are not orientated with respect to initial position in the conduit. They are, however orientated with respect to the vein.

P 1663 line 14: It would be very useful to present the LS-DSC measurements in a figure in sup. mat for example.

The authors feel that the LS-DSC maps add little to the manuscript, and that the more interesting data is presented in the HS-DSC plot.

P1664 line 9: Please, precise the low coercive material present in the host rock and the pseudotachylyte.
The carriers are FeTi oxides. [see above]

P 1664, 1665 line 19: Please, precise the origin of the electric currents in the fault.
Static stress builds up in the fault to create the high electric current. Indeed, rare earthquake lights are thought to be a result of stress-induced electrical currents that flow rapidly to the surface, according to a new study (Robert Thériault et al. Prevalence of Earthquake Lights Associated with Rift Environments Seismological Research Letters, January/February 2014, v. 85, p. 159-178, doi:10.1785/0220130059).

P1665 line 2-3: Please, move these lines to the section 2.1.
Much of the text has been re-written and relocated as a result of other comments, this text is no longer in the same place.

P1165 line 7: Please, give permeability values.
This section has been substantially modified. Additionally, the values are given in the permeability figure 4 and discussed further in the text.

P1665 line 8: "the presence of chlorite. . .." Please, move the shear band description to the section 2.1
The section has been altered, although this statement was just re-iterating information that was given earlier, and verifying the fact that we see evidence for degassing in the mineralogy.

P1666 line 9: Please, give the acceleration and deceleration of the HV friction experiment.
The HVR reached the target velocity in acceleration and deceleration in less than 0.1 seconds.

P1666 line 22 to 25: Please, explain how "the frictional properties of melt differ greatly from gouge"? HV friction exp. performed with gouge and cohesive rock showed +/- the same peak friction and steady state friction values.
In fact melt does not have a frictional behaviour, instead the shear stress (or resistance) is controlled by the viscosity of the melt. The viscosity-controlled sliding has been shown to greatly differ to sliding along a non-molten surface, be that gouge or rock-rock, which as the reviewer notices, have been shown to have similar peak and steady state friction values to one another (e.g. Dietrich and Kilgore, 1994, Del Gaudio et al., 2009).

P1667 line 1: please, remove "unequivoqual".
Removed during reworking of the text.

P1667 line 23: please, remove "inextricably".
Removed during reworking of the text.

Fig2.B. C: unnecessary figures. Again, samples are not oriented.
The samples are from an unorientated block, as is stated regularly. The figure still shows a sense of shear and so not unnecessary, however mention in the text was lacking, but this is now added.

Fig3: If possible, show figures with a higher magnification.
The authors feel that the scale presented is the most informative for showing comparisons between the samples.

Fig9: Please, put some error bars.
A thorough description of the errors involved in the permeability measurements including machine errors and its significance compared to natural variability (very little) is made in the recent Solid Earth Discussions article Heap et al. (2013) for the same permeameter at UCL.