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Interactive Comment

Interactive comment on "Seismogenic frictional melting in the magmatic column" *by* J. E. Kendrick et al.

Anonymous Referee #1

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Comments 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

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 in-

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terpreted 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 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.

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.

- 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"). 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?

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 $\sim$ 10:1 and $\sim$ 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?

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

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oxides" means (x 2, x 10, x 100)? What "smaller" means?

*Ferré, E. C., Zechmeister, M. S., Geissman, J. W., mathanasekaran, N., and Kocak, K.: The Origin of high magnetic remanence in fault pseudotachylites: theoretical considerations and Implication for coseismic electrical currents, Tectonophysics, 402, 125–139, 2005.

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.

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.

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

Minor comments:

P1662: I see only a petrographical description paragraph 2.1. Please remove petrological.

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?

P1664-1665. It would be good to make explicit here that the magnetic and permeability measurement were made on non-oriented samples.

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

P1664 line 9: Please, precise the low coercive material present in the host rock and the pseudotachylyte.

P 1664, 1665 line 19: Please, precise the origin of the electric currents in the fault P1665 line 2-3: Please, move these lines to the section 2.1.

P1165 line 7: Please, give permeability values.

P1665 line 8: "the presence of chlorite. . ..” Please, move the shear band description to the section 2.1

P1666 line 9: Please, give the acceleration and deceleration of the HV friction experiment.

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.

P1667 line 1: please, remove "unequivoqual".

P1667 line 23: please, remove “inextricably”.

Fig2.B. C: unnecessary figures. Again, samples are not oriented.

Fig3: If possible, show figures with a higher magnification.

Fig9: Please, put some error bars.

Interactive comment on Solid Earth Discuss., 5, 1659, 2013.

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