

**Reply to: M. J. Heap (Editor)**

**On the manuscript: Seismogenic frictional melting in the magmatic column  
by J. E. Kendrick, Y. Lavallée, K.-U. Hess, S. De Angelis, A. Ferik, H. E. Gaunt,  
D. B. Dingwell, and R. Leonhardt**

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

Dear Dr. Heap,

The authors would like to start by thanking the 3 reviewers who have helped to improve the clarity and structure of the manuscript. We have taken all due care and attention to ensure that all comments and queries have been attended to, and feel that significant improvements have been made to the flow of the text and organisation of its content. We have restructured the method, results, discussions to more clearly distinguish between one another, as requested by reviewer's #1 and #3; we also addressed reviewer #2's concerns that the shear band may represent an injection vein; removed figure 7, which was deemed unnecessary bearing in mind the ratio of figures to text; and finally removed the emphasis on the permeability linked to degassing, as requested by the reviewers and editor, as this was not the primary goal of the study. In addition to these major comments we have also made a number of minor changes to satisfy the other requests of the reviewers and yourself, and have responded individually to each comment below and in the 3 other replies. We hope that you will consider the changes made to the manuscript to be sufficient for publication in *Solid Earth*, and look forward to hearing from you shortly.

Kind Regards,  
Jackie Kendrick and co-authors.

After careful consideration of your manuscript "Seismogenic frictional melting in the magmatic column", based on the three reviews, my decision is that the contribution requires major revision before it can be accepted in *Solid Earth*. Please now address the comments of the reviewers (and my comments below). In particular, please pay careful attention to the comments of reviewer #2 regarding whether the feature presented in this study is a pseudotachylyte or an injection vein. Stylistically, I feel that the paper suffers from the short format (see also the comments of reviewer #1). I would also remove the references from the abstract.

My comments relate primarily to the permeability measurements. Firstly, there is no indication from the authors as to the orientation of these features. I suspect, if the authors' interpretation is correct, that they will be subparallel to the conduit wall. They therefore may influence degassing into the host rock, but will have very little influence on vertical degassing. In fact, if the 2 m is a representative length for this type of feature, they will also impart little influence on horizontal degassing. The authors mention that they will "significantly influence the efficiency of the degassing network", without offering any further information. How will they influence degassing exactly? Regardless of their orientation, they will not impact degassing if they can't develop significant lengths. The authors do not mention the expected lengths of these features, or how many of these features they would expect in the conduit.

It is true that no lengths at formation are inferred, and neither are numbers, but if the shear bands are linked to the seismicity then there must be many. We direct the editor to the

answers to reviewer #3 and to the newly drafted manuscript for a more appropriate discussion of the capabilities of this permeability study.

Further, I'm not sure whether I would consider 10-16 m<sup>2</sup> as a "impermeable barrier". The "three orders of magnitude" difference quoted by the authors is for an effective pressure of 5 MPa. At more relevant pressures the difference is much less. This is because the permeability of the host rock is controlled by microcracks (which close at higher pressure). Such microcracks probably formed as the material cooled. Therefore, the microcrack facilitated permeability at lower pressures for the host rock is probably not representative of the material in the conduit. I would argue that, looking at these data, the feature has little influence on the permeability. I also find it suspicious that the authors do not mention the length of each of the samples. I suggest that they (1) give the lengths and, (2) show photographs of the core samples. I would further suggest that they reconsider the use of these data.

As mentioned previously the application of the permeability data to the volcanic scenario has been altered, and the authors find it now presents a more balanced view of the shear band's involvement with degassing. We have added the core lengths and a schematic of each core as a panel in figure 4, but unfortunately we do not have photographs as in 2 cases the sample ruptured at high pressure.

There are quite a few sentences that state these features will have "an important influence on magma ascent dynamics" and "may have important implications for eruption dynamics". But, what are the implications? I'm left unconvinced by their influence on permeability in the conduit.

As mentioned previously the text has been changed substantially, and you will find that it now represents a more balanced view of the shear bands involvement in outgassing.

Page 1661: I find it odd that Laumonier et al. (2011) is not cited.

This was an oversight, and the reference has been added, along with a number of other relevant references.

Page 1664: "close proximity" is a tautology.

Changed

Page 1666 and 1667: "MPa" is a stress, not a load!

Changed

Page 1667: "Multi-parametric"?

Spelling changed