

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

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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

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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. Further, I'm not sure whether I would consider 10-16 m² 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.

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.

Page 1661: I find it odd that Laumonier et al. (2011) is not cited. Page 1664: “close proximity” is a tautology. Page 1666 and 1667: “MPa” is a stress, not a load! Page 1667: “Multi-parametric”?