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

M. J. Heap (Editor)

heap@unistra.fr

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Dear Dr. Kendrick,

I have now carefully read the reviews, your responses, and the new manuscript. Overall, I think you make a compelling argument for the presence of pseudotachylytes (frictional melting) within the magmatic column. This is a novel (and perhaps controversial at this early stage in its gestation) idea and is exactly the type of stimulating science we would like to publish in Solid Earth. My decision is accept, after the following (minor) comments have been considered.

1. Reviewer #1 has concerns regarding the temperature monitored by the infrared camera. While I do not contend your reasoning, I think it would be appropriate to

mention (perhaps in the figure caption) that no cooling correction was applied to these data.

2. I have some comments regarding the CT data. There is, for me, some discrepancy between Figure 3A and 3B (or at least some missing explanation). How was the porosity selected exactly? Are they the voxels that have a grey level of 0? Judging by the two reconstructions it looks like a range of grey level was judged to be void space. What was this range? This is an important point because the porosity highlighted in Figure 3B could include low density minerals (if such minerals are present). Further, if the size of a crystal/grain/mineral is below the resolution of the scan (the resolution should also be indicated in the figure caption) then a number of voxels could represent an average of solid and void space. Such voxels could, in this case, have been interpreted as porosity leading to an overestimation. Can you provide more details please, perhaps in the figure caption? Interpreting porosity from a scan of a multimineralic material that potentially contains microporosity is an extremely difficult, borderline impossible, task. Is this how you estimated the porosity of the pseudotachylyte to be 1 vol.% (line 115)? The manuscript is littered with statements which emphasise that the pseudotachylyte has a "negligible porosity". Is this just inferred from the CT, or measured?

3. Line 93. Remove the asterisk. This is a programming notation.

4. Line 93. "...incrementally higher pressures from 5-50 MPa..." should be reworded. "...at every 5 MPa increment from 5 MPa to 50 MPa..."?

5. Line 94. Heap et al. 2014 has now been published in Solid Earth. This citation should be updated. Heap, M. J., Baud, P., Meredith, P. G., Vinciguerra, S., and Reuschlé, T., 2014. The permeability and elastic moduli of tuff from Campi Flegrei, Italy: implications for ground deformation modelling. Solid Earth, 5, 1-20.

6. Line 95. "...3 times less permeable..." Do you mean "...three orders of magnitude less permeable..."?

7. I think, for those who may still argue that the feature could be an injection vein, that the paper would benefit from some of your discussion in response to the comments of reviewer #2. For example, "…injection veins tend to be on the order of cm's length, and tend to have the morphology of a half-bell, tapering rapidly from the vein base and less rapidly toward the tip (see Griffiths et al., 2012, EPSL)…"

8. Line 181. I had to read this sentence a fair few times. I think "…for example, the 235 m of displacement preceding…" would help the flow of the sentence.

9. Line 217. I would change "low" to "lower".

10. Line 529-530. "…permeable porous network…" The link between porosity and permeability is certainly not straightforward. For example, microcracked granites with a low porosity can have high permeabilities. Just because a material has low porosity does not mean that it automatically has a low permeability. I would remove "…and its influence on the permeable porous network."

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