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Solid Earth
Discussions

Interactive comment on "Dynamic magma mixing revealed by the 2010 Eyjafjallajökull eruption" *by* O. Sigmarsson et al.

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This manuscript reports about the recent 2010 eruption of Eyjafjallajökull volcano (Iceland). Authors demonstrate that magma mixing events played a key role in both modulating the compositional variability of erupted products and in triggering the eruption. It is hypothesised that mixing occurred between a resident felsic magma and an incoming mafic magma (magmas) that became more primitive with time.

I read this work with great interest and I agree with the general picture that magma mixing is likely the most important process governing the compositional variability of studied rocks. Nevertheless, I have some suggestions that authors may want to take into account when preparing their revised version.

1) I suggest changing the title of the paper. In particular, I find the expression "dynamic magma mixing" a bit misleading. In my view any mixing process is "dynamic" by definition and I am not aware of any "static magma mixing" process. The concept of "dynamics mixing" is explained in the paper as the injection of progressively more mafic magma in the resident felsic magma. So why not using something like "Felsic magma chamber recharge by mafic magmas and magma mixing revealed by . . ."?. Of course this is just a possible solution and Authors can surely propose a sexier title, which reflects their idea and that does not resemble a tautology.

2) I suggest being careful in the use of the words "mingling" and "mixing". Although a certain degree of confusion exists in the literature, there is a general agreement to use the term "mingling" to indicate mechanical interaction (i.e. no chemical exchanges are involved), whereas "mixing" is commonly used to indicate both mechanical mingling and chemical exchanges. Given that from presented results it is clear that chemical exchanges between magmas occurred, I suggest using only the term "mixing" throughout the paper.

3) In the interpretation of data displayed in Fig. 3 it is assumed that samples erupted at different times were also produced at different times in the igneous system. However, this is only a possible solution of the problem. Several recent works, supported by detailed analysis of rocks samples, have shown that magma mixing processes are able to produce a strong modulation of compositional fields both in time AND space (e.g. Flinders and Clemens, 1996, Perugini et al., 2002; 2003). In particular, different dynamic regions, tightly connected to the fluid dynamics of the mixing process, can occur at the same time in the same system. In these regions the mixing efficiency can be highly variable. As an example, consider the injection of a mafic magma in a felsic magma chamber. The mafic magma can undergo extremely different degrees of interaction with the felsic magma. In one region of the system the mixing efficiency can be high and the mafic magma signature disappears quickly in favour of an intermediate composition. In another region, instead, the initial mafic magma composition can be

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preserved (see for instance Perugini et al., 2002; 2003). This being so, the plot of Fig. 3 does not necessary shown a temporal evolution of the mixing process. The compositional variability triggered by mixing could have been generated by the injection of a single batch of mafic magma that underwent different degrees of interaction in different dynamical regions. During the progressive evacuation of the magma chamber magma batches with variable proportions of the different dynamical regions where the mixing process occurred with different intensities may have been erupted in time leading to an "apparent" temporal evolution of the mixing process. I suggest the Authors to consider this alternative interpretation when discussing their data. 4) Data interpretation and discussion is entirely based on the classic concept that magma mixing processes produce linear variations between any pair of chemical elements. I would like to bring to the attention of Authors that this is not necessarily true. Recent works based on the study of natural samples, numerical simulation, and laboratory experiments (e.g. Perugini et al., 2006; 2008; De Campos et al., 2011) clearly indicate that linear trends can be expected only between elements having the same mobility (i.e. diffusion coefficients, D). If two elements have different mobility, curved or scattered trends can be observed in binary inter-elemental plots. This diffusive fractionation process is true for both major and trace elements (e.g. Perugini et al., 2006, De Campos et al., 2008). In the light of these considerations the data displayed in the CaO-MgO plot of Fig. 3, hence, does not necessarily need to be fitted by several mixing lines. A single mixing episode could produce the same scattering. I suggest inserting in the revised version of the paper additional binary inter-elemental plots for different major and trace elements. Given the obvious difficulty of displaying all possible binary plots, the Authors may want the evaluate the opportunity to include a colour-coded version of the correlation matrix for all analysed elements in which all correlation coefficients (r) of linear fitting between any pair of elements are displayed. This means to include one figure, with a lot of useful information on it, which, in my view, will increase the readability of the paper.

I Hope the Authors will find these comments useful in preparing a revised version of

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