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Solid Earth Discussions

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

Anonymous Referee #1

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General Comments.

The article deals with the compositional changes displayed by the products of the 2010 Eyjafjallajökull eruption, and their interpretation in terms of interaction processes between mafic and felsic magmatic end-members. The manuscript represents a good contribution to the scientific progress, although reference to previous work is limited, especially in the Introduction section. The analytical data are of good quality, and are discussed and used, although partially, to make quantitative modelling of mingling/mixing processes, hypothesized to have occurred during the eruption. As a matter of fact, even though the interpretation of geochemical data is sound, and the proposed scenario of magma reservoir dynamics occurred just before and during the eruption is reasonable, the manuscript suffers from a lack of clarity in the use of analytical data.

An effort is required to the Authors to improve the strength of their article in order to make it a useful contribution to the understanding of triggering processes of explosive volcanic eruptions.

Specific Comments.

1 - One main criticism to the article is the confused use of the terms mixing and mingling. It seems that the two words are used with the same meaning, starting from the abstract and throughout the text. I am sure that the Authors are aware that: mixing implies a complete chemical homogenisation of two or more magmas with different composition, resulting in a new, hybrid magma in which complex zoning patterns in phenocrysts and/or mineralogical disequilibrium is the only evidence of the process; mingling is just a mechanical mixing among different magmas, each of them possibly carrying its crystal load, giving rise to magmas in which the composition of end-members is still recognizable (see for instance Clynne, 1999, J. Petrol. 40-1, 105-132). The Authors are encouraged to follow this distinction in their article in order to avoid confusion.

2 - The second problem is the complete lack, in the Introduction (section 1), of reference to the literature concerning precursors of volcanic eruptions and, more in particular, mingling/mixing as a trigger mechanism for explosive volcanic eruptions, i.e. the main topic of the article. Personally, I have never read the introduction of a scientific article with no cited references at all! Even though any selection of articles to cite can result to be difficult just in light of the vastness of the specific literature concerning the main topic of the manuscript, at least some of the most recent examples of volcanological and petrological work testifying for mingling/mixing as a trigger process of past eruptions should be cited. The papers by Sparks et al. (1977) and Eichelberger (1980), present in the Reference List, but not cited in the text, would likely fill this gap at least partially. I would suggest citing here, for instance, the papers by Pallister et al. (2008) and Nakamura (1995) that the Authors cite later in the Results and Discussion section, as examples of recent explosive eruptions likely triggered by min-

gling/mixing processes. I would also suggest the Authors to cite papers like Suzuki and Nakada, 2007 (J. Petrol. 48-8, 1543-1567) for Haruna volcano in Japan, and Tonarini et al., 2009 (Lithos 107, 135-151), for Campi Flegrei in South Italy. This because the comparison between the eruption occurred in 2010 at Eyjafjallajökull volcano, well documented by the Authors through the analytical data presented and discussed in this manuscript, and well documented eruptions occurred in historical times in other active and dangerous volcanoes worldwide could be very useful in accurately defining the best petrological and geochemical tools to monitor mingling and/or mixing as fundamental magmatic processes occurring during explosive volcanic eruptions. All of that can be much useful to the scientific community, as well as the civil protection agencies, in assessment of volcanic hazards and mitigation of volcanic risks.

3 - The description of the eruption phases and samples needs some clarification. It is not very clear which eruption phases were explosive and which ones were effusive in the description given in section 2 “The Eyjafjallajökull 2010 eruption”. The reader realizes that the eruption extruded both lavas and tephra only when it reads section 3 “Samples”. In the latter, the sample/s representative of a historical eruption collected “from a soil section” (page 594, row 15) must be better described, as their analytical record could have been compromised by pedogenetic processes. The Authors should provide the reader with the label of these latter sample/s in order to easily individuate them in Tables and Figures.

4 - Analytical data need some careful control and evaluation, as well as a better use. Oxygen isotope data. The Authors say they analyzed “individual grains, bulk monomineralic fractions, and glasses” (page 596, row 19), but only five δ18O data are reported in Table 1, with no indication about the type of analyzed material (i.e., mineral or glass). The Authors should provide more detailed information. Furthermore, the Authors report δ18O data with a 1 ‰ error in the Results and Discussion (section 5, page 599, rows 25-27), much larger than the sd’s reported in Table 1. With such a large error it seems impossible to discuss any variation of oxygen isotope data that may have occurred during magma fractionation from mafic to silicic composition, as the Author do. The Authors should clarify this point. Also, the δ18O value of 5.8 reported in the text (row 25) for sample EJ-1 does not correspond to the number given in Table 1.

Strontium and Neodymium isotope data. The description of analytical techniques is a bit uneven: for instance, the nature of the filament is given for Sr and not for Nd; the procedural blank is given for Nd and not for Sr, which is more critical than Nd. The Authors should homogenize the information.

Trace element data. All the analytical effort to acquire trace element contents of melt inclusions deserves some further use of the data (Table 3). For Instance, I would suggest to prepare a Primitive Mantle-normalized spiderdiagram of the most primitive MI’s, and compare them to equivalent Icelandic whole-rocks.

Two whole-rock samples of evolved composition are missing in the plots of Figure 4, when compared with data in Table 1. The Authors should include them in the plots or explain why they are not used.

To show the time progression of the mingling process during the course of the eruption, a chemostratigraphy with some petrological indicators (see Tonarini et al., 2009 for an example) would be much more useful than the simple double-Y plot of Figure 5.

Minor Comments.

Page 598, rows 10-11: A largely crystallized groundmass is not always a consequence of a high gas content of the magma; it may depend upon the cooling rate of the magma during the latest stages of extrusion.

Page 599, rows 20-21: The statement “Both the plagioclases and the clinopyroxenes display an inverse chemical zonation (e.g. Fig. 2e-f) ” is contradicted by the examples of Figure 2, panels a and c. The Authors should revise that statement, explaining better what they are referring to.

Page 599, rows 5-6: The compositional variability of the composite sample EJ-2 should

be described also through its MgO and CaO contents, and not only through its SiO2 content, not shown in Figure 3.

Page 599, rows 7-8: “mechanical mixing or mingling”: mingling IS a mechanical mixing (see Specific Comment N° 1); “older silicic melt”: how can the Authors state that this silicic melt was older than the evolved basaltic one?

Technical Corrections.

The term “benmoreite” for an alkaline rock compositionally intermediate between alkali basalt and trachyte should be used throughout the text instead of the much less used word “benmorite”. The same should be for “benmoreitic”, instead of “benmoritic”.

Reference list: the Authors use abbreviations (e.g., Earth Planet. Sci. Lett.) in a few cases, and full name of journals in several cases. These should be homogenized as required by Solid Earth.

Page 594, row 3: change “The magma produced is of a benmoritic. . .” with “The extruded magma was of benmoreitic. . .”

Page 594, row 23: put “a” before “Cameca SX100”

Page 596, row 4: put “an” before “induction furnace”

Page 597, row 11: change “residue” with “residues”

Page 597, row 13: change “chemistry,” with “chemistry;”

Page 597, row 14: change “was” with “were”

Page 597, row 19: change “where” with “were”

Page 597, row 20: change “8.735209” with “8.375209”

Page 597, row 25: change “session” with “sessions:”

Page 598, row 1: change “in run and lower” with “in run, and were lower”

Page 597, row 11: put “so” before “no blank”

Page 598, row 16: change “of a benmoritic” with “of benmoreitic”

Page 598, row 26: change “analysis” with “samples”

Page 598, row 27: change “ratios” with “ratio”

Page 600, row 17: put “an” before “older”

Page 601, row 27: change “Monserat” with “Monserrat”

Page 603, row 28: change “B. Volcanol.” with “Bull. Volcanol.”

Page 604, row 3: change “Ac.” with “Acta”

Page 604, row 6: change “Ac.” with “Acta”

Page 604, row 8: change “Ac.” with “Acta”

Page 605, Table 1: Are Sr and Nd data of sample EJ-5 actually referring to sample EJ-6 instead? That would make a much better sense than measuring δ18O on the most evolved sample, and Sr- and Nd- on another, less evolved sample. I suggest the Authors to carefully check the numbers in all Tables.

Page 606, footnote to Table 1: change “Apastrophes” with “Apostrophes”

Page 610, caption to Fig. 2: put a semicolon before “b)” the scale bar of panel f cannot be the same as that of panel e

Page 611, caption to Fig. 3: The Authors should explain the meaning of “oligoclase melt”, described in a different way in the text.

Interactive comment on Solid Earth Discuss., 3, 591, 2011.