Solid Earth
Discussions

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Interactive comment on "Can vesicle size distributions predict eruption intensity during volcanic activity?" *by* A. LaRue et al.

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Received and published: 28 July 2013

Comment 1: The authors have synthesized in platinum capsules the hydrous glass with powdered scoria and water (simulating the Eyjafjallajökull melt during Phase II of 2010 Eyjafjallajökull volcanic eruption), such that water concentrations dissolved in the melt would be between 1.7 and 4.1 wt.%. However, for the magmatic plumbing system of Eyjafjallajökull volcano, Keiding and Sigmarsson (2012; J. Geophys. Res., 117) estimated with the plagioclase-melt hygrometry (Putirka, 2008) a maximum average H_2O content of 1.8 wt.% (H_2O content range for the summit eruption: 1.2 -2.6 wt.%) in the benmoritic tephra, in agreement with melt inclusion observations. Why did the authors use higher water contents in their starting materials? Correct water content should be used for the experiments and this has consequences on the VSDs. Apparently, as

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observed by Baker et al. (2012), water content influences the resulting VSDs and the related power law exponents (i.e., glasses bearing 3 wt.% H2O are characterized by VSDs with power exponents of 0.6 to 1.0, glasses bearing 7 wt.% H2O show VSDs power exponents of 1.1 to 1.5; Baker et al., 2012). How much the variability of water content affects the VSDs? Is it possible to distinguish the water content effect on the experimental VSDs?

Reply: First, the paper by Keiding and Sigmarsson (2012) was not available when this research was performed in the Autumn of 2010 and Winter of 2011 so we used a range of water contents to bracket the magmatic water concentrations of the Eyjafjallajökull 2010 eruption. Second, Keiding and Sigmarsson (2012) measured the water content in three samples of sample EJ-2 which displayed a large variability (for example 0.6-2.6 wt% H2O). The broad range of water concentrations used in our experiments (1.7 to 4.1 wt% H2O) includes most of this range and allowed us to cover a representative water content range for the benmoreitic samples of Phase II. Third, as shown in our previous studies, the original water concentration of the sample has little effect on the final bubble (or vesicle) size distribution at large porosities (Bai et al., 2008, 2010, 2011). The variations of the power law exponent seen in the results of Baker et al. (2012) are not attributed to water concentration differences, but merely to stochastic fluctuations in the formation and growth of the bubbles. The lack of influence of water concentration on the power-law exponent of the VSD can be seen in the results of this contribution (see Table 1).

Comment 2: The authors synthesized the hydrous glasses at 1 GPa (corresponding to a depth of 40 km, assuming that 1 kbar is about 4 km depth) and simulated decompression tests at high temperature and 1 bar, with the approach of Bai et al. (2008; 2010). However, according to Keiding and Sigmarsson (2012; J. Geophys. Res., 117) pressure estimates yield an average value of 5.6-6.4 kbar ($\pm$1.5 kbar) for the basaltic tephra, and variable but lower pressures for the benmoritic samples ranging down to 0.6 kbar. The mafic magma mainly crystallized in the deeper crust (16-18 km), whereas

mingled magma from the summit eruption crystallized at more shallow crustal levels (2-5 km) suggesting multistage magma ascent. Why did the authors simulate magma decompression from 40 km depth? The authors must consider that decompression rate (here considered as the "jump" from the pressure of synthesis to 1 atm) controls the volatile supersaturation, which, in turn, influences the kinetics of bubble nucleation, and thus VSDs and VNDs (e.g., Toramaru, 2006; J. Volcanol. Geotherm. Res., 154; Rust and Cashman, 2011; J. Geophys. Res., 116). Cannot the piston cylinder be used at 0.5 GPa (usual minimum value of pressure) for the synthesis of the hydrous glasses? In this way the authors can simulate the decompression rate of the basaltic magma from about 20 km depth, even though the starting materials should display a different chemical composition (basaltic). If the authors want to use the composition of the natural scoria (benmorite to trachyandesite), they should decompress the hydrous glass from an initial pressure lower than 0.5 kbar, because more evolved magma of Eyjafjallajökull comes from shallow reservoirs (2-5 km $\approx$ 0.5-1.0 kbar; Keiding and Sigmarsson, 2012).

Reply: Once again we point out that the paper of Keiding and Sigmarsson was not published when we performed the experiments. As we discuss in the original manuscript (page 793, lines 16-21) the purpose of our degassing experiments was to simulate instantaneous decompression at 1 bar in order to investigate vesicle growth and the evolution of vesicle size distributions under sub-surface-like conditions during a volcanic eruption. The most important control on the decrompression is the difference between the concentration of water dissolved into the starting glasses at 1.0 GPa and the solubility at 1 atmosphere because the structure of the glass is virtually insensitive to changes in pressure over this range (Mysen and Richet, 2005, Silicate Glasses and Melts: Properties and Structure, Elsevier, 560 p.). These water concentrations correspond to supersaturation pressures between 30 and 110 Pa (Baker, D.R. and Alletti M. (2012) Fluid saturation and volatile partitioning between melts and hydrous fluids in crustal magmatic systems: The contribution of experimental measurements and solubility models. Earth-Science Reviews, v. 114, pp. 298-324.) and thus are similar to the

pressure of the shallow magma chamber. This information is included in the revised manuscript.

Comment 3: The authors conducted magma/water interaction (MWI) experiments to test whether the phreatomagmatic process could affect the VSDs or not. These are very interesting experiments, but, in my opinion, they were set up in the wrong way if the authors planned to simulate the Phase II of 2010 Eyjafjallajökull volcanic eruption. Since the early experimental work of Sheridan and Wohletz (1983; J. Volcanol. Geotherm. Res., 17) and Wohletz (1983; J. Volcanol. Geotherm. Res., 17), the maximum efficiency of magma fragmentation because of MWI was found at 0.3 in water/magma (WM) mass ratio. In the diagram reported in Figure 1 in the work of Wohletz (1983; J. Volcanol. Geotherm. Res., 17), depending on the water/magma mass ratio, there are three fields of volcanic activity: Strombolian (WM mass ratio < 0.1); Sutseyan/Vulcanian (0.1 < WM mass ratio < 3.0 or extended to 50 by Sheridan and Wohletz, 1983); Submarine (WM mass ratio > 3.0 or 50). In the MWI tests of this manuscript the authors used a WM mass ratio of about 1500, which is clearly simulating a submarine volcanic process. As the authors claim, the water inundation provides a rapid quench of the heated (and already vesiculated!) samples without observing any change in the final VSDs (no surprise with so much water). Why did the authors use so much water to inundate the samples? The high amount of external water used in the experiments is not consistent with the natural scenario of Phase II of 2010 Eyjafjallajökull eruption. As reported by Gudmundsson et al. (2012) and also described by the authors in the introductory paragraph, Phase II of 2010 Eyjafjallajökull volcanic eruption was mainly magmatic (Strombolian with scoria and lava production), with some episodes of wet explosions due to residual melting of Gígjökull glacier. The passage from Strombolian to phreatomagmatic activity and viceversa is worldwide known and very common in volcanic area close to the sea (e.g., see the cases of Monte Nuovo, Italy; Di Vito et al., 1987; Bull. Volcanol. 49) or covered by an ice cap (Ruapehu, New Zealand; Houghton and Hacket, 1984; J. Volcanol. Geotherm. Res., 21), like in the case of Eyjafjallajökull volcano. This change from Strombolian to phreatomagmatic

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eruption (i.e., from purely magmatic to phreatomagmatic eruption style) is strongly related to the change of water/magma volume ratio < 0.3. Why do not the authors play with WM mass ratio < 0.3? The authors collected natural scoria samples on 8 May 2010, did not they? This would suggest that the interaction between magma and water was minimal or nil. I strongly recommend to the authors to consider a new set of experiments where the WM mass ratio is less than 0.3. Given the low water mass interacting with the heated sample, the authors might observe some changes in VSDs due to cracks generated by MWI, increasing gas permeability. There is an interesting paper by Trigila et al. (2007; Bull. Volcanol., 69) showing MWI at high temperature and high pressure and how water inhibits bubble vesiculation, but produces cracks instead. This magma cracking induced by the contact with external water might affect the VSDs, also when bubbles are already vesiculated (like in the authors' experiments).

Reply: We appreciate the reviewer's concern about the amount of water we used in the magma-water interaction experiments. We first wish to stress that we were not studying the effects of water on the fragmentation of the samples, but only on the bubble size distributions. Experimental limitations did not allow us to use low water/melt ratios because we had to inject the water into the furnace and were concerned that at low water/melt ratios all of the water would evaporate during passage through the furnace to the sample and leave the furnace before having an opportunity to interact with the melt. This point is now included in the revised manuscript. Instead, we took what might be considered a "traditional experimental approach" (similar to classic experiments on the effect of water on the melting relations of rocks, e.g. Yoder and Tilley, 1962, Journal of Petrology) by using a very high water/melt ratio to ensure that we had water reaching the sample and interacting with it. Our hope was that this high water/melt ration would have the greatest effect possible on the bubble size distribution. We found that a melt/ratio of 1500 did not significantly affect vesicle size distributions, the only change being related to a decrease in vesicle nucleation reflected in the lower vesicle number density values in comparison to those of the control experiments (without any interaction with water). Our results indicate that reducing the melt-water ratio would

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only change vesicle number densities (producing a likely increase) without having an important effect on vesicle size distributions. Furthermore, by performing the control experiments we have done what the reviewer asked us to do in the comment: "new set of experiments where the WM mass ratio is less than 0.3." In cases where the "the interaction between magma and water was minimal or nil." the results of our control experiments can be applied, but the differences in the VSD's between the control and MWI experiments is minimal. In addition, it is important to note that we did not find any of the crack features mentioned by the reviewer in our natural or experimental samples.

Comment 4: The authors performed two sets of experiments: magma/water interaction (with water inundation) between 800 and 1000 °C; and control experiments (no water inundation) between 925 and 1042 °C. Why did the authors choose these temperatures for the experiments? Are these simulated eruptive temperatures for the Phase II of 2010 Eyjafjallajökull eruption? If not, were the temperatures depending on the chosen experimental technique? Please, I invite the authors to clarify this point.

Reply: The temperatures were chosen to based upon our previous experience (Bai et al, 2008, 2010, 2011, Baker et al. 2012) that the temperature of the experiments has no discernible effect on the VSDs when bubbles are grown about the glass transition (see papers of Bai et al., 2008, 2010). The MWI experiments were performed at lower temperatures in the hope of prolonging the life of the furnace whose interior was innundated with water multiple times during the study.

Comment 5: The authors are very familiar with the scoria samples of Stromboli volcano, given the extensive 3D textural research of such scoria specimens(Bai et al., 2008; 2010; Polacci et al., 2008; 2009; 2010). Naturally, the comparison between the Icelandic scoria with the Italian ones represents a good strategy to constrain the results and part of the interpretations. However, I find the discussion in the section 4.3 a sort of "Strombolian vision" of Eyjafjallajökull volcanic system. The authors claim that "the explosions were triggered by a continuous inflow of magma and gas from depth into a shallow magma reservoir, similar to results based upon trace element and isotopic

studies (Sigmarsson et al., 2011)". I would recommend the authors to read the paper of Sigmarsson et al. (2011) a bit better, because Eyjafjallajökull volcanic eruption has been triggered and driven by magma mingling/mixing between basaltic (the major input) and felsic magma (the remobilized batch). The trace element and isotopic studies of Sigmarsson et al. (2011) confirm the mingling/mixing process occurring during 2010 Eyjafjallajökull eruption. Magma mingling/mixing has been proved by several workers (e.g., Sigmundsson et al., 2010; Keiding and Sigmarsson, 2012). Keiding and Sigmarsson (2012; J. Geophys. Res., 117) collected freshly fallen tephra from 17-20 April 2010 activity (sample EJ-3; at the transition between the end of Phase I and beginning of Phase II) and 5 May 2010 activity (sample EJ-5; at the transition between the end of Phase II and beginning of Phase III). They observed a change in composition from benmorite to trachyte due to rapid magma mingling (without an effective homogeneization; Sigmarsson et al., 2011). I think the authors should mention this magmatic process in the discussion. To better compare with the activity at Stromboli volcano, the authors could also consider the intermingling between the magma producing the golden pumice and the member giving the black scoria. Do the authors think that magma mingling/mixing can affect the VSDs?

Reply: The scoria samples we investigated come from the second phase of the eruption. In the original manuscript we didn't mean to discuss the eruption trigger by mixing/mingling of basalt with the residing rhyolitic magma body; instead we meant to mention conduit processes that might have happened when the eruption was well established. In the revised manuscript we have rephrased the sentence reported by the reviewer (page 798, lines 19-22 of original manuscript) in order to better reflect what we intended to say: 'Phase II of the Eyjafjallajökull eruption thus appears similar to Stromboli's normal activity, because the explosions were supplied by a continuous inïňĊow of mingled/mixed magma and gas from depth in the shallower conduit (Sigmarsson et al., 2011). '

We also changed a similar sentence in both the revised abstract and revised summary,

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respectively:

'The comparable VSDs and behavior of Phase II of the Eyjafjallajökull 2010 eruption to Stromboli are interpreted to be a reflection of similar conduit systems in both volcanoes that are being constantly fed by the ascent of mingled/mixed magma from depth.'

'We interpret the VSDs in the scoriae from the 2010 Eyjafjallajökull Phase II eruption to reflect gas-melt withdrawal from an open-conduit volcanic system supplied with deeper mingled/mixed magma.'

Comment 6 (to link to Comment 2): The authors simulated magma decompression by using high pressure designed samples (i.e., synthesized at high pressure in piston cylinder) with high heating rate (about 100 °C/min). However, magma decompression can occur near isothermal conditions in the timescale of months (e.g., Blundy and Cashman, 2005; Geology, 33) or, when it is very rapid, is accompanied by (adiabatic) cooling due to rapid gas expansion (e.g., Mastin and Ghiorso, 2001; Contrib. Mineral. Petrol., 141). Martel and Bureau (2001; EPSL, 191) performed in situ high pressure and high temperature bubble growth experiments in silicic melts in a hydrothermal diamond-anvil cell. They performed cooling rate experiments and, from the cooling rate they estimated the corresponding decompression rate by using the equation of state of water. Given the opposite approach of the authors (heating rate experiments), do they think that there is a difference between the experimental VSDs generated by cooling (Martel and Bureau, 2001) and the VSDs generated by heating? If so, how much sure the authors would be to correlate their experimental VSDs generated by heating and sudden decompression (from the initial correct pressure; see Comment 2) with the natural VSDs produced by volcanic activity (adiabatic cooling due to gas expansion)?

Reply: At the present time all available data (both experimental and natural) indicates that our experimental techniques produce vesicle size distributions similar to those found in nature created by decompression at constant temperature. We are planning to expand our research to build the equipment necessary for constant temperature,

decompression experiments, but have not yet acquired the funds to build the necessary equipment. Therefore, at this time our experiments are the state of the art for the study of VSD's formed during degassing, and with water-interaction, at shallow, crustal pressures.

Comment 7: In Figure 1 a plagioclase is displayed in the 3D rendering. Did the authors find a significant presence of phenocrysts? If not, what about the presence of microlites? If the authors have found significant crystal and/or microlite contents in the scoria samples, what is the influence of the crystals and/or microlites on the VSDs?

Reply: We did not observe significant concentrations of either phenocrysts or microlites in our samples. In the caption to Figure 1 we cite Sigmundsson et al. (2010) who stated that crystals were rare in the summit samples, less than 2 %. We have now included this information in the revised figure caption to Figure 1. Sigmarsson et al. (2011) provide a list and compositions of crystals found in samples erupted from the summit, but do not provide information on the quantity of these crystals.

Comment 8: As well explained by Bai et al. (2008) VSDs and power-law relationships are generally affected by vesicularity (i.e., increase of vesicularity generates an increase of the power-law exponents), coalescence and outgassing, and temperature increase (from power-law to exponential relationship between VND and vesicle volume). What about the effect of melt viscosity on the VSDs? Viscosity is a critical factor that is not mentioned by the authors. I would suggest describing the role played by viscosity on the VSDs in the discussion part. Also, it would be great to specify the melt viscosity range used in the experiments during bubble nucleation and growth. Would the authors be able to constrain the viscosity of the experimental charges?

Reply: The effect of viscosity on vesicle textures and distributions is a field mostly unexplored in petrology and volcanology. Our experiments in melts ranging from hydrous albite to High-K basalt (see discussion in Bai et al., 2008) demonstrate no effect of viscosity on the VSD's, however we think that additional experiments on this question

would be useful. Addressing this question thoroughly requires performing 3D X-ray microtomographic experiments at a synchrotron facility equipped with un ultrafast tomographic end station in order to be able to follow vesicle size, shape and distribution with time, and repeat the experiments for a range of magma composition. Notwithstanding the importance of the topic, this is beyond the scope of this paper, although we will investigate it in the very near future.

Comment 9: Can the authors add a summary figure where all the cumulative bubble size distributions are displayed in the same diagram? It would be easier for a reader to immediately observe the close match between natural VSDs and experimental ones. This will strengthen what the authors propose.

Reply:

Although in principle this may seem like a good idea we find that the superposition of all of the data creates a very confusing image for the reader and therefore prefer not to include a summary figure as suggested by the reviewer.

Technical Comments: Lines 25-26, page 790: "...predicted, had our VSDs been measured..." has to be corrected in this way: "...predicted, if our VSDs had been measured".

It has the same meaning, therefore no correction is needed.

Lines 10, page 792: The authors use "1 bar" to specify the room pressure conditions of the experiments. Later the authors use "1 atm". Both units are naturally correct; but, for a better congruence, I would suggest the same unit. In this case, I would suggest "bar" since the authors use "kbar" for high pressure values.

We changed to bars.

Lines 7, page 793: Substitute "1 atm" with "1 bar".

Change made.

Line 23, page 799: "(Tuniz et al., 2012)" must be changed in "(Tuniz et al., 2013)". I suggest to the authors to check again the references prior to proceeding for resubmission of the manuscript.

Oops – our mistake. Referenced were double-checked.

Fig. 1: Could you add a white arrow to highlight where the plagioclase is? This can help the reader to immediately find the plagioclase. I would also suggest using the plagioclase as a scale bar in your Figure.

An arrow has been added to in Figure 1 to indicate the plagioclase crystal. However, because this is a perspective view we cannot accurately add a scale bar because the scale changes from front-to-back and side to side, that is why we prefered to state the length of the sample in the figure caption.

Fig. 2a-2b-3a-3b: Since you are showing cumulative vesicle size distributions from natural and experimental samples, why do not you show the corresponding 3D samples you analyzed by ImageJ and Blob3D? This would be a clear correlation between analyzed microstructures and quantified VSDs results.

The quantitative comparisons provided by the VSD's are more powerful than any image that we can show of the experimental samples and based upon our experience they are the best method of showing our results.

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