Review of "Can vesicle size distributions predict eruption intensity during volcanic activity?" by LaRue et al., submitted to Solid Earth

General Comments:

The manuscript is well written, linear and fluent to read. I like the idea to compare similar natural samples to extract information on eruption intensity of a certain volcano whose activity is less known and studied. I also like the multidisciplinary approach to combine laboratory work with field observations. However, matching experimental results with natural microstructures is not always straightforward; particularly when the experimental conditions do not match with the general conditions of a natural scenario (water contents, decompression rate, temperature variation, …). For instance, I do not agree with the setup of the magma/water interaction (MWI) tests, since the authors have proposed a water/magma mass ratio that is too high given the scenario of Phase II of 2010 Eyjafjallajökull volcanic eruption (see more in Comment 3). Moreover, there are other points concerning the experimental work that request more clarification (see the other comments). I find the discussion of the manuscript a bit "misleading" because the comparison between natural scoria from Stromboli and those from Eyjafjallajökull induces the authors to propose an identical volcanic process occurring at Stromboli for Eyjafjallajökull volcano, without considering the effect of magma mingling/mixing, which is the trigger and the dominant process of 2010 Eyjafjallajökull volcanic eruption (see details in Comment 5). I find the title quite provocative and it would be great to use the portable X-ray micro-CT of Tuniz et al. (2013) to analyze natural samples in situ. But, such an advanced technology was not used in this work; on the contrary, the authors conducted sample tomographic analysis, image segmentation, data processing and quantification with a classical long work timescale and they did all this amount of work after the explosive eruption of Eyjafjallajökull volcano. Therefore, the authors did not *predict* with the VSDs the intensity of Eyjafjallajökull volcanic eruption during its activity, but they "*quantified*" it based on the VSDs results in the "post mortem phase" of the eruption. The authors also claim this in their abstract ("Such behavior implies that continued activity during Phase II of the Eyjafjallajökull eruption could be expected and would have been predicted, if our VSDs had been measured in real time during the eruption"). Volcanic forecast during eruptive activity based on VSDs can be attempted by the authors with more persistent and "regular" volcanic activity, like that one at Stromboli volcano. Eyjafjallajökull volcano is one of the many volcanoes characterized by eruptions of limited duration. To date, volcanic prediction during long duration eruption is quite difficult; for short duration eruptions the volcanic forecast is terribly difficult! I would suggest to change the title in "*Are vesicle size distributions indicators of volcanic eruption intensity?*". The manuscript can be proposed for publication IF the following points are revised.

Specific Comments:

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 observed by Baker et al. (2012), water content influences the resulting VSDs and the related power law exponents (i.e., glasses bearing 3 wt.% H_2O are characterized by VSDs with power exponents of 0.6 to 1.0, glasses bearing 7 wt.% H_2O 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?

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 (±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 ≈ 0.5-1.0 kbar; Keiding and Sigmarsson, 2012).

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 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).

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.

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?

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)?

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?

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?

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.

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".

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.

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

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.

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.

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.