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

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Responses to comments by Reviewer Shea's comments

Shea's comments are in italics, our responses are not.

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Title: Can vesicle size distributions predict eruption intensity during volcanic activity?

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Reviewed: 07/31/2013 by Thomas Shea

Disclaimer on the review procedure: I found this system of open discussion and review a bit confusing in the context of classical review procedures. Before I completed my review, comments from other (more efficient!) referees were already posted, and I was informed that they were by email. Not too convinced this was customary, I inquired with the editorial staff about whether reviewers should be really reading other reviewer comments (which is not very fair to the authors in my opinion). They rightfully advised me not to look at those, which is the guideline I adopted for my review. Recommendation

This article contains an interesting textural comparison between natural scoria clasts produced by the recent Eyjafjallajökull eruption and degassing experiments performed at 1 atm. It is my assessment that there are several major issues that need to be addressed before this work is publishable. Summary of review General background:

Vesicles in volcanic pyroclasts can be sensitive indicators of the style(s) and rates of ascent and degassing of magmas. By comparing key textural features measured within natural pyroclasts to those obtained during controlled laboratory experiments, it is possible to characterize the conditions in which degassing occurs. Article: The authors

used tomographic imaging to characterize the vesicle sizes and numbers in 7 scoria samples from one phase of the 2010 Eyjafjallajökull eruption involving some water-magma interaction. They performed two types of 1-atm degassing experiments, (1) a set of runs involving no incorporation of external water, and (b) a set of runs aimed at understanding the effect of external water on bubble textures. They also characterized the experiments texturally and compared them to natural scoria. They conclude that their experiments reproduce the size distributions well irrespective of whether external water is involved, and that textural parameters such as power-law exponents

can be used to fingerprint eruption intensity. Concerns: Comparisons between experimental and natural pyroclast textures are difficult to achieve and the authors deserve credit for their work. Below, I express concern over aspects of this work that require a more robust analysis, including (1) the problems associated with their experimental degassing strategy, (2) the notion that the water/melt ratio they use would replicate typical phreatomagmatic interactions , (3) the inadequate degassing picture an isolated single parameter such as the power-law exponent provides, (4) whether these exponents can be used as proxies for eruption instensity, (5) the problem of fitting their data to such exponents and (6) the idea that this type of data can be used to predict eruptive behavior, and (7) the lack of visual aids to compare experiments and natural samples. If these issues can be addressed, I believe this could be a valuable contribution to advancing our understanding of degassing in mafic magmas. For most comments, I have tried to make short suggestions on how the problems can be handled whenever possible.

Main comments: 1. Experimental degassing strategy: 1-atm heating-induced degassing experiments are still in their early phase in terms of applications to bubble formation in magmas (e.g.; Bai et al. 2008, 2010; Brown et al. 2012 conference abstracts, although Bagdassarov et al. 1995 also had a similar conceptual setup). A key strategy when performing laboratory experiments is to isolate the important variables acting on the system, and the strategy employed by the authors is problematic in this regard. There are several assumptions that must be made for their degassing experiments to have some legitimacy IF the objective is to make comparisons with natural magmas. I noted the following problems:

a. Starting material: the authors chose to use hydrated melts fused at superliquidus temperatures to homogenize their starting charges. While this may be a valid approach to study crystal-free magmas such as rhyolites or, in less frequent cases, crystal-free/poor basaltic melts (e.g. the Stromboli golden pumice), the natural counterparts they use for comparisons are scoria, which not only contain microlites in abundance, but also plenty of oxides. The latter are known to strongly facilitate bubble nucleation

(e.g. Hurwitz and Navon 1994; Cluzel et al. 2008), which affects later bubble growth (e.g. Gonnermann and Manga 2007, 2012) and ultimately size distributions. Do their experiments contain oxides or are they oxide-free?

Response The natural samples contain very few crystals, as documented in Sigmundsson et al. (2010) who stated that crystals were rare in the summit samples, less than 2 % and as mentioned in manuscript. Thus, our experiments are appropriate to the problem studied. Note that Gonnermann and Manga (2007) argue that crystals should slow coalescence of bubbles which might affect bubble size distributions, but that coalescence is only one of the mechanisms that create power-law, bubble-size distributions. The other accepted mechanism for the power-law distribution is multiple nucleation events, which might be enhanced by crystals. Additionally, Bai et al. (2011) experimentally demonstrated that samples containing up to 38.5 % crystals still displayed power-law distributions, as did their experiments in crystal-free systems. (Note: We could not find the Gonnermann and Manga, 2012, paper cited in the comment, on the Web of Knowledge nor on Gonnermann's web page: http://earthscience.rice.edu/department/faculty/gonnermann/Site/publications/.)

b. Syn-degassing temperature variations: Degassing in the runs presented herein occurs by heating a hydrous glass to a high final temperature. It is mentioned that the strategy for 1-atm experiments is the same as Bai et al. (2008), wherein charges were heated at rates of 42 C/min (not to confuse with the 100C/min mentioned in the present ms which applies only to the 1-atm furnace, not the in-situ observation runs I presume?), which resulted in run durations 8-25 min until quenching. If they applied the same procedure, this implies that the temperature varies (increases) during melt vesiculation, and cannot be considered isolated from other variables, chiefly the decrease in solubility from the pressure at which initial materials were synthesized to 1-atm conditions. Temperature will affect vesiculation since it modulates whether growth is viscosity or diffusion controlled. I also note that even the final temperature appears to

not be fixed but variable (page 794 "samples were heated until they produced vesicles, generally between 925 C and 1042 C, and the melt was then quenched.").

Response The reviewer is correct that temperature varies during bubble growth in the type of experiments reported in this study. There is currently no way to avoid this problem in our experimental apparatus. However, the impact of this variable temperature is minor for two reasons. The first is that the effect of temperature on the water solubility is minor (see summary in Baker and Alletti, 2012). The second is that the viscosities of the melts are low enough to demonstrate bubble expansion and coalescence even during the short experimental durations, indicating that the growth of the bubbles is not limited by diffusion.

c. How does ΔT translate to ΔP? In classical degassing experiments, samples are heated and decompressed to a final pressure before quenching. This allows some tracking of the observed vs. predicted vesicularity, H2O content, bubble size, and permits the experimentalist to determine the relationship between decompression rates and bubble number densities (Hurwitz and Navon 1994; Mourtada-bonnefoi and Laporte 2004; Toramaru 2006; Cluzel et a. 2008; Hamada et al. 2010). The problem with these heating-induced degassing experiments is that, while one may estimate an effective integrated ΔP (pressure at which the starting melts were prepared minus 1-atm), it is not easy to translate heating rates into devolatilization rates (or decompression rates if one wishes to apply classical nucleation equations). Before these types of experiments are used for comparative purposes, it would be great to try and quantify what the rate of reaction of the system is (rates of degassing with time). Suggestion: I realize the authors cannot go back and completely change their experimental strategy or run many more sets of experiments for this particular contribution. However, it would be useful to discuss some of these points both in the presentation of the methodology as well as in the discussion sections. If the strengths and weaknesses of this experimental approach are identified, the reader gains a better understanding of why there is

so much variability in the experimental results (e.g. vesicularities from 6-89 %).

Response

The effect of ΔT on ΔP is minor for these experiments due to the small effect of temperature on water solubility in magmatic melts (see Baker and Alletti 2012 for a summary). The largest effect on the supersaturation of water is the difference between the dissolved water concentration and the equilibrium value at 1 atm (0.1 wt %). The absolute pressure difference between synthesis conditions and 1 atm has very little effect (Mysen and Richet, 2005, Silicate Glasses and Melts: Properties and Structure, Elsevier, 560 p.). Thus we calculate supersaturation pressures by the difference between pressure at which the synthesized melts would have been in equilibrium with a fluid of pure H_2O and 1 atm. These supersaturation pressures have been stated in the revised manuscript. The reported experiments were specifically designed to be complementary to "classical degassing experiments" in order to study the formation of bubbles and their size distributions over short time scales as the vesicularity increases (hence the variations in vesicularity seen in the experiments), something difficult to do at high pressures without multitudes of experiments. Determining the rates of reactions ("rates of degassing with time") is a very interesting idea, but such rates cannot be well-characterized using this experimental procedure because of the varying temperature during the experiments as well as the loss of H_2O from the system. The best that can be done is to measure bubble-growth rates in 4D tomographic experiments (see Baker et al., 2012), but the present set of experiments were performed before such techniques were available. However, most importantly, we are not interested in the rates at which degassing occurred, but only on the resulting bubble-size distributions. As the reviewer states, there is a "relationship between decompression rates and bubble number densities" but because we cannot control the decompression rate at this time we can only use the bubble/vesicle number densities in a semi-quantitative manner, and that is why we instead concentrate on the vesicle size distribution (as explicitly stated in the title and in the abstract) and its value in understanding volcanic processes.

2. Simulations of phreatomagmatic interactions: While I appreciate the idea behind their water-melt interaction experiments, the information resulting from this portion of the ms is not very useful. It is widely known that the type of magma-water interaction and the efficiency of any fragmentation resulting from this interaction are controlled to a first order by the ratio of water/magma (Wohletz and McQueen 1984; Wohletz 1986, 2003). Below a water/magma ratio of 1, the efficiency of energy conversion from vaporization is high (max around 0.3-0.4), and at higher ratios, it decreases drastically. The water/melt ratios utilized in the present contribution is >1500, effectively meaning that you are basically quenching a melt to a glass almost instantaneously, without significant transfer of energy to any fracturing/fragmentation event. I would not call this a simulation of phreatomagmatism since you are comparing your 'dry' control experiments to a complete extreme of the water-magma interaction spectrum, both end-members representing scenarios in which basically we would never expect anything to happen (aside from fast quenching). But these experiments have likely very little in common with the type of phreatomagmatic interaction that occurred during the 2010 Eyjafjallajökull eruption. Suggestion(s): I think these experiments can be used to examine somehow the effects of early quenching vs. longer dwell times on the vesicle textures. The idea that these runs correspond to simulations of real life phreatomagmatic interactions is deceptive.

Response The melt-water-interaction experiments were designed only to investigate the effect of added water on the bubble-size distributions, not to investigate fracturing and fragmentation. They were not meant to fully simulate all processes occurring in phreatomagamtic eruptions that have been well-studied in the past. The use of the high water/melt ratio was due to our concern that if we used low ratios, the water would evaporate and leave the furnace before any interaction with the melt. This point is now included in the revised manuscript. 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 guiding idea was that the high water/melt ratio 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 only change vesicle number densities (producing a likely increase) without having an important effect on vesicle size distributions.

3. The interpretations rely too highly on power-law exponents: The 3D bubble volumes recovered from the microCT scans are shown in a cumulative vesicle size distribution (CVSD) format, which is now widely used to identify certain processes controlling bubble nucleation and maturation (Blower et al. 2002; Klug and Cashman 2002; Adams et al. 2006; Bai et al. 2008; Polacci et al. 2009; Carey et al. 2009; Constantini et al. 2010; Shea et al. 2010, 2012). In most cases, it was found that natural samples yield power-law relationships in the main portion of the CVSD curve. This is the textural parameter that the authors chose to focus on here, but there are several processes unifying to generate and modify CVSD curves (see comment 4). It is easy to see how utilizing only one parameter to compare natural and experimental samples can be problematic: the range in values for their power-law exponents 'd' can show some overlap but because there are more than one way to modify these exponents, we are left with little confidence on the textural comparison. To illustrate this, below I plot the range in exponent values found in a few studies involving the products of explosive mafic magma eruptions as well as the experiments from Bai et al. 2008 and from the present paper: From this plot is can be seen that, although not perfect, there is a good overlap between the experiments from both this paper and those of Bai et al. (2008) and the Eyjafjallajökull or Stromboli scoria. This is what the authors concluded. Now if the other two textural parameters available from those studies, vesicularity and bubble

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number density, are plotted against the exponent or each other, the overlap between the experiments and Eyjafjallajökull scoria is not maintained (see page below).

In both plots, I illustrated some possible paths during various bubble-related phenomena. Note that in the top plot bubble growth is not shown because it is not inferred to modify either the power-law exponent or the bubble number density. This figure shows that the experiments from this paper do not really overlap with Eyjafjallajökull scoria when the other textural characteristics are examined. I believe this illustrates the notion that one textural parameter alone is not useful to decipher bubble vesiculation processes, and that the authors need to make a more thorough analysis of their data and what they mean. Suggestion(s): In my opinion it is essential to really dig deep into what makes the textural parameters they measured (BNDs, power law exponents, potentially bubble sizes) vary, and how they can be reconciled with both the processes acting in their degassing experiments as well as during the 2010 Eyjafjallajökull eruption. This analysis is currently insufficient.

Response This contribution is a test of the hypothesis that vesicle-size distributions can be used to understand and interpret volcanic phenomena; we have stated this explicitly in the title of the contribution as well as in the abstract. We fully understand that there any many mechanisms that affect the power-law exponent and have reported upon them in some of our past papers. As well, we recognize the importance of measuring other parameters such as vesicle number density. However, as discussed in the manuscript (and in this review), the vesicle-size distribution of the Eyjafjallajökull samples and the experiments are similar and allow us to compare the two to learn more about the eruption process. Our results demonstrate that the answer to the hypothesis this study set out to investigate is "yes". We acknowledge that the power-law distributions and the experiments do not completely simulate the natural situation and that they have limitations, as discussed in the manuscript. The data in the paper and in this review demonstrate a correlation between eruption intensity and the power-law expo-

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C421

nent. The only exceptions appear to be 4 data points from Ambryn volcano reported in Polacci et al. (2012); however, these samples are believed to be associated with high-intensity, fire-fountain events and are thus from more-intense-than-normal activity and therefore consistent with the correlation between power-law exponent and level of intensity.

Additionally, the figures in the review demonstrate that the Eyjafjallajökull samples are remarkable for their low vesicle-number densities (VNDs). As we indicated in the manuscript, the experimental VND's are much higher and we attribute the low VNDs to long (minutes to hours) maturation times of the bubbles in the magma chamber before eruption. These VNDs are very interesting and no doubt providing us with additional information about volcanic processes, but they are not the subject of this contribution and the current experiments were not designed to investigate VNDs and their evolution with time.

4. Can power-law exponents be used as proxies for eruption intensity? Another problem (directly related to comment 3) is the lack of explanation for what the power-law exponents derived from their samples really mean. In paragraph 4.3, it is argued that since power-law exponents in low intensity explosive activity are lower than those measured in the products of higher intensity eruptions, they can be used as a proxy for eruption intensity. The first issue with this is that as mentioned above there are several processes acting in concert to generate the curves from which the power-law exponents are derived (e.g. Gaonac'h et al. 1996; Blower et al. 2002; Shea et al. 2010).

Prolonged nucleation or high nucleation rates tend to increase the negative slope of the power-law portion of the curve and thus increase d.

Response Yes, this has been shown theoretically by Blower et al. (2001, GRL) and experimentally in analogue systems by the same authors (2002 JVGR), but only up to some apparently limiting value.

C422

Bubble coalescence reduces the number of small and medium-sized vesicles in favor of larger ones and thus decreases the negative slope.

Response In many experiments we have observed bubble coalescence and do not see it resulting in a lower power-law exponent (i.e., we have not detected that bubble coalescence "decreases the negative slope"). The best example of this behavior can be seen in Baker et al. (2012) in which we see the bubble-number density decreasing as coalescence occurs; in these samples the power-law exponent remains approximately fixed at a value of 1.

In turn bubble collapse has an opposite effect, sacrificing large bubbles in favor of smaller ones and thus increasing the slope.

Response The effect of bubble collapse on the distribution is a very interesting one because it depends upon the susceptibility of different sized bubbles to collapse. If large bubbles are more susceptible and they form part of the distribution then there will be a tendency to increase the slope as the reviewer suggests. However, the same experiments cited immediately above (Baker et al. 2012) demonstrate that even with some collapse of bubbles, the size distribution remains a power-law one.

Bubble growth is inferred not to affect the slope of these distributions. While it is ordinary that bubble number densities would correlate somewhat with eruption intensity (e.g. BNDs can be indirectly linked with magma decompression rates dP/dt), it is unclear why coalescence or collapse would be any hallmark of intensity.

I would thus be much more careful about trying to make 'd' values proxies for eruption intensity. For illustration purposes, the figure on page 3 of this review shows that values of d between Ambrym 2008, Fontana, Stromboli 2007 (paroxysm) and Etna122BC pyroclasts almost overlap around d=1.4-1.5. Clearly the Fontana and Etna122BC Plinian

eruptions cannot have had the same intensity as those of Ambrym 2008 and Stromboli 2007. In their study of Ambrym scoria, Polacci et al. (2012) also recognize that other parameters such as crystal content and processes like permeability development modify cumulative size distributions. Suggestion(s): It might be better to trade off power-law exponents being proxies for intensity for a more detailed data analysis of bubble number densities, vesicularities and sizes, as also suggested above.

Response We agree with the reviewer that vesicle-size distributions do not tell the whole story, but we demonstrate that they are important evidence that aid in our understanding of volcanic processes and that their exponents, in general, correlate with relative eruption intensity.

We have substantiated the importance of bubble size distributions in many previous papers (e.g., by Bai et al. and Polacci et al.). Furthermore, other authors (e.g., Gaonac'h et al., 1996, who first revealed the presence of power-law distributions) have also shown their utility. As discussed in the manuscript, and mentioned in the reviewer's comment, it is the relatively more-energetic eruptions at each volcano that demonstrate the increase in the power-law exponent. The figures supplied by the reviewer demonstrate that the only volcano that appears to have an anomalously high power-law exponents is Ambryn (see Polacci et al. 2012). There are only 4 Ambryn samples that display high power-law exponents and their high values are attributed to their association with high-intensity, fire-fountain events and are thus from more-intense-than-normal activity and therefore consistent with the correlation between power-law exponent and level of intensity.

As we stated previously, our experiments simulate near-instantaneous depressurization and their vesicle/bubble number densities cannot be directly applied to the natural ones (as we discuss in the manuscript). However, as we have demonstrated multiple times in previous papers (cited in the current manuscript) the vesicle size distribution exponents created by these types of experiments accurately reproduce natural ones

and provide insight into the eruption processes. Furthermore, our previous papers demonstrated the importance of both multiple nucleation events and bubble coalescence in the formation of power-law bubbles size distributions (e.g., Bai et al., 2008).

We found corroborating evidence for the increase in the power-law exponent with increasing explosivity in analogue systems (Blower et al., 2001, GRL). We have now included that reference and the following sentence into the revised section 4.2. "VSDs and volcanic eruption intensity" of the manuscript: Furthermore, Blower et al.'s (2001) laboratory experiments of bubble growth and simulated volcanic eruptions using analogue materials demonstrated that the power-law exponents describing the VSD's of their samples increased with increasing explosivity; they attributed the increase in the power-law exponent to multiple nucleation events.

5. Problems with fitting power-law exponents: While two out of three curves shown for natural Eyjafjallajökull scoria convincingly display straight portions within the log-log CSVD plot (Fig. 2a and 2b), I would be more skeptical about fitting such curves to CVSDs in Fig. 2c, and even more so in the experimental curve shown in 3a. This makes the reader wonder what the other fitted curves look like. I imagine that the uncertainty in fitting those curves is one of the reasons for the wide range in d exponents presented. Correlation coefficients shown in the figures are not very meaningful since they are user defined (by selecting only a small portion of the curves). I would remove those. Suggestion(s): I believe the authors should show more, if not all CVSDs (their plots can be decreased in size and combined into a matrix of CVSDs), which would make the comparison between nature and experiments more convincing.

Response

All additional natural measurements and experimental results are now included in the Supplementary Figures, which demonstrate that the figures originally chosen for the paper are good examples of the entire suite of measurements. As discussed in the

responses to the other reviewers, power-law distributions demonstrate cut-offs that restrict the region over which the power-law applies. The low cut-off is often due to imaging resolution and the high-cut off is due to sample size. Thus, we, as well as all others, can only fit the power-law over a limited range of vesicle sizes. Nevertheless, we observe similar-valued power-law exponents over the same size range in natural and experimentally produced samples. And, as discussed in this paper, there is a consistent correlation between the power-law exponent and the eruption intensity. The correlation coefficients shown are important because they demonstrate the goodness-of-fit of a power law to the data over the range selected.

6. Predicting eruption intensity: I do not grasp what is meant by "predicting". I doubt that anyone would expect bubble size distributions to predict anything in terms of future behavior; they are a reflection of degassing during magma ascent, which changes significantly during a single eruption and even during a single eruptive phase. A prediction requires at the very least some sort of time-series of measurements. I understand the idea that portable microCT scanners could be used in the future, and, if software advances allow rapid data treatment, potentially obtain VSD data of one or two pyroclasts fairly quickly. But even then, it is known that the products of a single eruptive phase can be highly variable in their textural attributes (e.g. Houghton and Wilson 1989; Klug and Cashman 1994; Polacci et al. 2003; Shea et al. 2010). This is why it is difficult to envision real-time textural investigations: they require thorough sampling (i.e. obtain a texturally representative collection of pyroclasts) and data analysis, making them unsuitable for rapid near real-time procedures. If the authors wish to preserve the 'prediction' notion in this paper they need to provide stronger arguments that there some practical validity to this.

Response

First, we have changed the wording of the title and in the text from "predict" to assess, which is a better description of the hypothesis investigated. Second, current microCT

scanners and software operated by expert users can obtain a quantitative VSD from a 2 x 2 x 1 cm pyroclast in less than 3 hours. Thus, 8 samples per day can be processed with a single scanner using current technology. The question of sample heterogenity is a problem for all textural studies of volcanic materials, but during an active volcanic crisis samples can be rapidly investigated under a binocular microscope in the field and representative samples chosen for X-ray microtomography. Thus, we maintain that real-time sampling and analysis is possible using X-ray microtomography.

7. Images of the experiments: This is perhaps a more minor comment but the comparison between natural and experimental samples would be much more convincing if the authors showed images (or perspective views like in Fig.1) from their heating experiments.

Response

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.

Minor comments and edits/typos.

Title: It should probably be changed and not include the word 'predict'.

Response The word "predict" has been changed to "assess"

Terminology: I would make it clear right from the beginning that you are dealing with cumulative size distributions CVSDs and not size distributions VSDs in the classical sense (which are plots of ln(n) vs. size).

Response We have now made it clear at multiple places in the manuscripts that we are working with cumulative distributions.

Section 2.1, Page 792 It is mentioned that 8 representative clasts were selected to undergo textural characterization. There are, however, only 7 listed in the Table.

Response

Apologies for our mistake. It has been fixed in the revised manuscccript. *Section 2.2,*

Page 793, line 17: The sentence is confusing because it appears as though 'inundating the sample with water' is linked to 'simulating approximately instantaneous decompression'. Needs rephrasing. . .

Response The sentence has been changed to make it more clear to the reader.

Section 3, Page 795, line onwards 8: Your vesicularity variations are so large in between different samples that it deserves an explanation either here or in the discussion. What process is responsible for producing a run with 6% vesicles vs. 89%? I understand why the WMI vesicularities would be lower than the control experiments because of the water quenching they experience. But what about intrinsic variation within each series?

Response

The variations are simply due to the growth durations of the experiments which were controlled by a combination of the stochastic nature of bubble nucleation as well as variations in heating and quenching when water was injected into the system. This has now been explained in the revised manuscript.

Section 4, Page 796, line 18: The word 'effect' is vague here, what kind of effects are you referring to?.

Response

We have changed the wording by replacing "effect" with "may be affected"

Section 4.3, Page 798, line 7-11: In the intro you mention that Phase II plumes reached 7 km…a bit confusing for the reader.

Thomas Shea

Response As mentioned in the introduction, the plume that reached 7 km during phase II was anomalous, most were much lower in altitude. We have changed the working in Section 4.3 to indicate that "almost all" ash clouds only reached 3-4 km in height.