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Solid Earth Discussions

Interactive comment on "First experimental evidence for the CO_2-driven origin of Stromboli's major explosions" *by* A. Aiuppa et al.

Anonymous Referee #2

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General comments The discussion paper "First experimental evidence for the CO2-driven origin of Stromboli's major explosions" by Aiuppa et al. presents an extraordinary and detailed dataset of CO2 emissions from Stromboli, the like of which has never been produced for a volcano. The authors must be congratulated for this, which represents a huge investment of time in instrument development and field site maintenance. It is through making such detailed observations that great strides are made in understanding eruptive behaviour. The central idea put forward in the current paper, i.e. that a) large paroxysmal explosions are driven by CO2 rich gas and b) enhanced CO2 fluxes are a precursor to these explosive events, has been published previously by the authors and by Allard and colleagues. This current paper discusses the idea in detail and presents a larger dataset and the paper is certainly scientifically valuable for this reason. I would have expected, however, given the number of papers that the lead

author in particular has published on this topic previously, the modelling to be a more developed and to bring us further towards constraining the deep degassing processes. The mass balance approach is a valuable constraint on what might be causing the observed patterns in gas release, but does it have to arise from foam accumulation? Why not changes in magma flow that might arise from fundamental changes in magma supply and volatile content at depth? The paper is well written and the figures all clearly presented and necessary. Some more specific comments follow.

Specific comments Title: I am not sure that “experimental” is the correct word for the title; would “observational” be better?

Figures 2, 3, 4: the authors state the accuracy of the CO2 flux measurements as $\sim$40%. What is the precision? I would imagine considerably better, but a detailed treatment of it is absolutely necessary for this paper given the emphasis on interpreting changes and variability in the CO2 flux.

Page 418, top: the prediction that the magnitude of the explosion should be proportional to the enhanced degassing rate prior to the explosion (Allard, 2010) could be tested using this dataset. Is there a relationship between the mass of CO2 accumulated/leaked and the seismic energy released by the explosion perhaps, or some measure of erupted volume? Likewise is there a correlation between the time frame for accumulation and leakage, and the magnitude of the explosion? These kinds of analyses would lend support to a gas accumulation model.

Page 419: the observation that mass balance is satisfied by the CO2 degassing patterns strikes me as absolutely crucial but at the same time, I do not see why foam accumulation is the only way that this criterion can be satisfied. Surely magma accumulation and ensuing pressure changes interacting with a storage system could reproduce such changes? Has this been explored in detail? Such behaviour has been observed in silicic systems and is consistent with a model of pressurisation and non-linear magma flow, proposed by Slezin, Melnik and co-workers.

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Page 421: it is indeed interesting that much of the CO2 release for the paroxysmal events could occur prior to the main explosive event. This would be consistent with explosive events at Kilauea and elsewhere however, where the shallow magma acceleration is caused by H2O degassing, in the manner proposed by Wilson, Head, Parfitt and co-workers in several publications. Perhaps CO2 provides an initial impetus and H2O takes over at low pressures?

Technical corrections Page 412, line 24: “open-vent” delete last “s” Page 414, line 11: delete “top of” Page 418, line 21: change to “for most of the volcanic gas discharge” Line 23: change to “whose fast ascent, followed by bursting, drive the”

Interactive comment on Solid Earth Discuss., 3, 411, 2011.