Review of Rheological control on the dynamics of explosive activity in the 2000 summit eruption of Mt. Etna by Giordano et al

The authors present an integrative textural and rheological study of erupted products from the 2000 eruption at Etna volcano, and use numerical modelling to interpret the conditions that lead to the extraordinary eruptive dynamics. The approach is very interesting and innovative, and provides an integrated view of eruptive dynamics. Indeed in the future, it will become useful to use such a multifaceted approach at other volcanoes. Here, however, interpretation and discussion come of a little short owing to the lack of a few important parameters.

The main deficiencies of the model is that fragmentation is solely related to bulk viscosity and strain rate, and does not consider 1) the presence of dissolved volatiles, 2) a fragmentation criterion from bubble overpressure, and 3) a failure criterion for the presence of brittle crystals in magma. These three factors are pivotal to transition in eruptive styles.

1) Explosive eruptions are notoriously driven by volatiles (whether in the magma or exsolved). Likewise, the Giordano et al (08) model is very strongly dependent on H_2O and F content, but these are not accounted for. Arguably, a review of volatile in the glass and in melt inclusion would help quantify exsolution (at least in 1-D) along the conduit.

2) Once volatiles are exsolved, the bubbles evolve significantly in basaltic systems - especially with such intermediate crystallinities. Bubble coalescence occurs at variable depths in the conduit and has important effects on the eruptive dynamics (see Szramec et al. JVGR 2006; Castro and Gardner, Geology 2008).
Interestingly, the density of bubbles in each eruptive product appears to be the same, if calculated against the bulk magma+crystal volume (which is the way this should be done). This similarity, if true, may be used to justify the use of a uniform fragmentation criterion. Please discuss Spieler et al 2004.

3) None of the magma fragmented in the simulation; this is because basaltic melts require very fast strain rates to fragment. The addition of crystals increases the brittle character of magma (Cordonnier et al EPSL 2009; Lavallee et al 2007, 2008). Viscosity is therefore not the best way to gauge how brittle magma becomes with increase crystals.

Lasty, I have some reservation as to whether the codes for the simulation are too ‘uni-dimensional’ to explain the highly non-steady dynamics of the observed eruptive style transition.

Other comments

Overall:
The text needs to be tightened significantly and is redundant. The structure of many sentences is heavy and reading is tortuous.

Abstract.
It is not obvious from the abstract alone how the petrographical and rheological model confirms the eruptive dynamics. The intro of the abstract should describe the sequence of events and the evolution of the characteristics of erupted magma; eg, crystallinity (phenocrysts vs micorlites) SiO2 content. On Line 17: if you want to draw this conclusions, the intro of the abstract should most likely mention this flow or else

Introduction.
SEC: seldom used. No need to introduce this abbreviation. It confuses the reader
A conclusion does not belong in the intro. Moreover, this conclusion fully neglects the importance of volatile, which had just been argued as an important input parameter of the model.

Section 2.
This section needs rephrasing.

Section 2.1.
You say that paroxysms initiate with a Strombolian phase, then 8 lines later, each paroxysm is preceded by lava flow. Which is it then?
On page 23: one sentence never ends.

Section 2.2.
The description of bubble size is rather vague. Do you mean that bubble size distribution is bimodal or that the general sizes of bubbles is contrasting for each eruptive styles? It is not clear from Fig. 2 either. A bubble size distribution analysis would be necessary to support these important statements.

If (as mentioned) the vesicularity analysis has been performed on a crystal-free assemblage, then it means that the vesicularity is actually the same, which is worth noting. In general, the crystallinity is accounted for on a vesicle-free assemblage, however the vesicularity is calculated with respect to the bulk so that is should be equivalent to the porosity, which can be precisely measured with a pycnometer. Please do so.

How homogeneous are the crystals? Are there signs of resorption in the crystals (perhaps from fluxing in magma recharge or viscous heating

Interstitial glass in fire fountains products are less evolve than Strombolian scoria: Figure? Table? Is that the reason for the CaO/MgO ration in Table 1?

Section 3.
Very tortuous.

Section 3.1
The intro is repetitive.
Prediction in Fig. 3: The Giordano et al (08) model is very strongly dependent on H_2O and F content, which are not accounted for in this study (at least not in Table 3). Initial and end content of volatiles in the melt inclusions and glass, respectively, are necessary to validate the hypothesis that the crystallinity is truly responsible for the change in rheology.
Also, how can the model use only one averaged chemical composition? Each eruptive products were said to be petrographically different and as such are very unlikely to be chemically similar.

When discussing Einstein-Roscoe: vanishing is clearly the wrong term. They can extrapolate to intermediate crystallinities.

Inferred temperature. If you have glass, what is Tg?

Section 3.2

Why using spheres when Fig 2c and 2d show an absence of equant crystals?

In Fig 4. What is the effect of crystallisation on the SiO_2 content in the interstitial melt and therefore on the viscosity changes? This needs to be shown in such an integrative model.

In Fig 5. water contents: how were these measured? Glass? Inclusions?

Section 3.3.

This section is once more repetitive.

The short discussion of crystal aspect ratio (2^{nd} par.) does not belong in this section. Plus, how do you come up with this mere 5% deviation if different crystal shapes are used. I can assure you that this is not the case especially within the liquid-solid transition at intermediate crystallinities relevant for the eruption you describe.

Conduit diameter: Can’t direction observation or geophysics help constrain this better? Also, the diameter may vary significantly between each eruptive phase; especially in a cinder cone.

When discussing Fig 6. How can fragmentation never be met? An increase crystallinity does increase the brittle character and decrease the stress needed to fragment. This observation sounds erroneous or shows the drawbacks of the Conduit4 code; please explain.

When discussing Fig. 6 & 7. I agree that Fig 7 shows a different dynamics, but this is not obvious from fig 6. Moreover, why not comparing with natural discharge rate?

Fig. 7. what is the point of the 2 wt.% water line with 3 points only?

Can the model account for viscous heating?

Section 4.

There is no such things as multiphase viscosity? Unless you consider the viscosity of crystals, which you don’t.

Your discussion neglect SiO2 and volatiles. Please comment.

The first sentence in the application of your model to the eruptive events (2^{nd} par) need to be rephrased. The end of this paragraph is interesting and nice to read.

Tephra cone “characterised by a minimum in mechanical resistance” . do you mean low cohesion? Low strength? How would that affect the conduit diameter used in the simulation? This is not a stratovolcano; the diameter would likely be strongly affected by pressure variation from exsolved volatiles and variations in discharge rates.

What is up with those dykes and lava flows? They have not been described anywhere in the paper. Please remove, this is distracting.

I certainly hope that the comments were constructive and helpful to this very interesting manuscript. I look forward to see a later edited version.

Best regards,

Yan Lavallée