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Interactive comment on "A review of analogue and numerical modelling in volcanology" *by* Janine L. Kavanagh et al.

O. Galland (Referee)

olivier.galland@fys.uio.no

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I read with great interest this manuscript, which is a review of the laboratory and numerical modelling methods applied to volcanic processes. Before reading it, I was somehow sceptical that such a review could be achieved, because it looked to me an amazingly ambitious task! However, after reading, I am amazed by the synthetic review work the authors have achieved, and this review is an excellent starting point for additional, more specific reviews on sub-fields listed in this manuscript. The authors start with historical perspective of modelling volcanic processes. This section is followed by a more technical, but very important, section on parameterization of modelling, which is the foundation for establishing and using models. Subsequently, the authors summarize both numerical and laboratory modelling used to study diverse lev-

els of the volcanic systems: (1) magma chambers, (2) magma intrusions, (3) lava lakes and domes, (4) volcanic flows, and (5) volcanic plumes. The authors conclude the review with very interesting and constructive prospectives for the future of laboratory and numerical modelling of volcanic processes. Undoubtfully, this review should be read by the next generation of scientists, and it is naturally worth being published.

Given the big challenge this review represents, there are some points that need improvement, with some major revisions of some sections. The following points list and describe the points that need to be improved. However, given that my expertise is focused on magma intrusions and laboratory techniques, I will provide more thoughtful comments on these sections.

Major comments

1. Analogue modelling. I am not a big fan of this term. The nomenclature of the other modelling techniques (numerical and theoretical) already highlights what they implement: numerical calculations and mathematical theory. The term analogue does not achieve this. In addition, the word analogue implies in geoscientists minds that the models are just analogues of the geological systems, and that their aim is to only reproduce the geological systems with very little insights in the underlying physical processes. This is a reason, among others, why "analogue" models have been disregarded, and sometimes for relevant reasons. I rather propose the term "laboratory modelling". I therefore suggest the authors to replace systematically "analogue" by "laboratory" in their manuscript, explaining at the beginning of the review that the term analogue has been used extensively but does not reflect (1) the way models are implemented and (2) the wish for the physical understanding. I hope this point is not too picky, but I think that the laboratory modelling community can benefit a lot of respect with respect to the numerical and theoretical modelling communities by giving the message that we are not only producing analogues of the Earth.

2. Section 3.2.1 on scaling. This section requires significant reworking as, I think,

it does not give a relevant picture of how scaling should be used, for the following reasons: - In this manuscript, scaling is restricted to laboratory models. However, scaling is extremely important, if not fundamental, in numerical and theoretical models. Indeed, scaling produced by dimensional analysis is essential for unravelling the physical behaviour of numerical and theoretical models through the definition of the key, fundamental dimensionless ratios that govern the physical behaviour of the modelled systems. The grouping of (dimensional) model parameters, such as lengths, viscosity, rates, etc, in dimensionless ratios reduces the number of parameters to test, and allows defining dimensionless scaling laws, which are the essence of our physical understanding of the modelled processes (Barenblatt, 2003). But overall, it is these dimensionless ratios that are the relevant parameters that govern the physics of the modelled systems, not the individual dimensional model parameters. This has been described in great details by Barenblatt (2003), and the procedure has been nicely explained by Gibbings (2011). Good examples of how scaling provides the physically relevant parameters while reducing the number of parameters are given by Bunger and Cruden (2011), Michaut (2011) and Galland and Scheibert (2013), as examples of models applied to the emplacement of sills and laccoliths. This definition of scaling is the result of dimensional analysis, and is an essential part of the parameterisation of both laboratory and numerical models. It is very important to express here that scaling is also an essential component of numerical models, because many numerical modellers are even not aware of this and the parameterisation of numerous numerical models is frequently irrelevant because of this.

- In this manuscript, the scaling is presented only as the discussion of the across-scale relevance of laboratory-scale models to the geological-scale natural systems. This definition of scaling is indeed the pillar of Hubbert (1937) and Ranberg (1967). However, this definition of scaling is too restrictive, as described in the former section. In addition, this definition implies that the physical relevance of the models are only based on how they reproduce geological systems, without focusing on the physical understanding behind the models through the identification of scaling laws, which is often

an argument used to disregard laboratory models. This definition of scaling is also “attached” to the word “analogue” (see point 1), and this is why I intend to modify this nomenclature towards a more process-oriented approach instead of a “reproduction-based” approach. The definition of scaling used in this manuscript is called “similarity” by Barenblatt (2003), which means it discusses how the laboratory models are physically similar to their geological prototypes. The discussion on the similarity between the models and the geological systems is actually based on the equality of the dimensionless ratios defined in the dimensional analysis (see former paragraph) oth in the models and the geological systems, as explained by Barenblatt (2003) and summarized by Galland et al. (2017). The advantage of this approach of “scaling” (i.e. dimensional analysis+similarity) is that it leads to display the model results in dimensionless forms, which is scale-independent and therefore directly comparable with geological-scale natural data. An example is given by Galland et al. (2014a).

I therefore suggest the authors to restructure the section 3 according to the following structure: - Keep somehow the same introductory paragraphs below the heading 3.0 Parameterisation . . .; - A section 3.1 on scaling, as an essential part of both laboratory and numerical model parameterization; - A section on numerical (and theoretical) models, by explaining how scaling derived from dimensional analysis is essential to extract the physical behaviour of the models; - A section on laboratory models, by explaining how scaling derived from dimensional analysis is essential to establish the experimental strategy. Subsequently the similarity principle can be explained in detail to discuss how the lab-scale models are physically representative of their geological systems. To help the authors, I recommend using the detailed description of this “workflow” in our review (Galland et al., 2017). With my co-authors, we spent very long discussions to produce a consistent and didactic description of scaling procedure, so we would be happy that this work could be used.

3. Section 6.1 Analogue models of sheet intrusion. This section seems greatly influenced by the research methods of the authors. This is fully understandable. However

this might not be properly balanced and unfair with respect to the literature. For example, the section 6.1.1 “Gelatine models of hydraulic fractures” is much longer and much more detailed than the section 6.1.2 “Compacted granular materials and viscous indenters”. This difference does not reflect the relative relevance of the literature. For example, in the sub-section “Impact of mechanical layering of the crust on magma intrusion”, the authors discuss the study of Le Corvec et al. (2013) as reference for magma-fault interactions using gelatine models. However, gelatine models are not designed to study these complex interactions, as it is impossible to simulate faulting in gelatine; as a result, the “fault” in gelatine models is made by a pre-cut with a knife, the geological relevance of which is discussable. In contrast, granular materials spontaneously simulate faults and tensile fractures, and they have been implemented to study the mechanical interactions between magma intrusions and active faulting (Ferré et al., 2012; Galland et al., 2007; Galland et al., 2006; Galland et al., 2003; Montanari et al., 2010; Musumeci et al., 2005). The authors should definitely add a sub-section in section 6.1.2 regarding magma-fault interactions. In addition, the heading of the 6.1.2 section is misleading, as it mentions “viscous indenters”. However, the authors focus more on the laboratory methods associated with models made of compacted granular materials, but do not provide the references that specifically describes models of viscous indenter (Abdelmalak et al., 2012; Mathieu et al., 2008). In addition, Galland et al. (2014a) show that models of granular materials are necessary to address the emplacement of dykes versus cone sheets, whereas cone sheets are very rarely modelled in elastic models. Given that the authors discuss in details the processes unravelled by gelatine models of elastic tensile fractures, the authors should also discuss the processes related to (1) magma emplacement as viscous indenters and (2) magma-fault interactions.

The discrepancy between the gelatine and granular models highlight the importance of the rheology of the host rock on magma emplacement. Gelatine is purely elastic, whereas granular models are mostly plastic. However, the Earth crust is visco-elasto-plastic. I think it is important here that the introductory paragraphs of section 6.1 dis-

cusses that so far laboratory models have addressed end-member rheologies for the Earth’s crust, addressed by different types of models, and the conclusion of section 6.1 should be that we need to move towards model materials of more complex rheology to fully address the dynamics of emplacement of sheet intrusions.

5. Section 4.0 Magma and Lava rheology. The authors list the main rheologies used for magma. For this, the authors provide equations. I would also recommend the authors to compile a rheological plot figure that illustrate the stress/strain rate relations for the rheologies listed here. In addition, both in numerical and laboratory models, the rheology of the host rock when considering magma intrusion also plays a major role. Given that the Earth’s crust behaves visco-elasto-plastic, it is a challenge to encompass such complexity in models, however it is crucial for addressing the complex physics of magma emplacement. So far, mostly end-member rheologies (elastic, plastic, viscous) have been implemented, and it is time to combine them. The authors can refer to the review by Galland et al. (2017).

6. Before section 8.0 Volcanic lava flows. The authors could include a section dealing with modelling processes within, and controlling the formation of, explosive vents (maar-diatremes). There is a significant laboratory and numerical literature on the topic, which is highly relevant in this review (Galland et al., 2014b; Gernon et al., 2009; Haug et al., 2013; Nermoen et al., 2010; Ross et al., 2008).

Minor comments

- Lines 206-207: the authors mention that the propagation behaviours of dykes and sills are controlled by the stiffness of the host rock, but the viscosity of the magma and the strength of the host rock play an equivalent role. Please add these points.

- Lines 343-344: the authors can also add the important effects of bubble fluid pressure.

- Section “Magma chamber failure”. Add and describe study by Cañón-Tapia and Merle (2006).

- Line 519: correct “Vegeteline” to “Vegetaline”.

- Section “Interaction of magma-filled fractures with a stress field”: add and describe references to Hyndman and Alt (1987). In the same section

- Section “Sills and Laccoliths” (lines 664-682): add references to numerical models of Malthe-Sørenssen et al. (2004) and Zhao et al. (2008). These need to be listed and discussed here, because they implement Discrete Element Models (DEM), with are fundamentally different than all the other mentioned already, which are based on thin plate approximation theoretical models. DEM models allow more realistic fracturing of the host rock, in contrast to thin plate models. In addition, DEM models address sills and laccoliths of any size, whereas thin plate models are only valid for sills of radius 5 times larger than their depth. Add also reference to Thorey and Michaut (2016), which implement an original thermo-mechanical model for sill and laccolith emplacement.

- Section 6.3 “Testing magma intrusion models”: add reference to Spacapan et al. (2017), which provides detailed, very high-quality structural observations that document the relevance of the “viscous indenter” model. Also refer to Spacapan et al. (2016), who provide detailed field observations of dykes emplaced within pre-existing faults, and who also show that the emplacement of dyke swarms can be controlled pre-existing fault arrays oblique to the principal tectonic stresses.

- Lines 705-713: the authors discuss the relevance of laboratory models to testing geodetic models. The authors should include references to Kavanagh et al. (2015), Galland (2012) and Galland et al. (2016).

- In section 8.1.1 Analogue models of lava flow dynamics. The authors should also refer to (Garel et al., 2012, 2014), which describe well-controlled and quantitative laboratory experiments of cooling lava flows.

- Line 870-879: the authors list experiments of columnar jointing, whereas the heading of the section is “Flow indicators”. The authors should add a new heading “columnar

jointing”.

- Line 902: correct “reflect the whether” to “reflect whether”.

- Line 908: correct “for example using Monte Carlo” to “for example Monte Carlo”.

- Line 1207: correct “depending the application” to “depending on the application”.

- Lines 1235-1245, section “Utilise a multi-disciplinary approach”, please refer to Burchardt and Galland (2016), which is a review that discusses the limitations of the commonly separated disciplines of Earth sciences, and concludes that these limitations can be overcome by other methods when combines, i.e. an explanation of the added value of multidisciplinary research applied to volcanic systems.

In sincerely hope these comments and suggestions will help the authors to improve their review. I am looking forward to reading the revised version of this contribution, and will be happy to give it to read to my students.

Best regards,

Olivier Galland

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