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Discussions

Interactive comment on "Dome instability at Merapi volcano identified by drone photogrammetry and numerical modeling" *by* Herlan Darmawan et al.

Anonymous Referee #1

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Review of the manuscript: Dome instability at Merapi volcano identified by drone photogrammetry and numerical modeling by Herlan Darmawan, Thomas R. Walter, Valentin R. Troll, Agus Budi-Santoso

Dear Editor,

The paper of Darmawan et al. focuses on the potential destabilization of the frozen dome of Merapi volcano by rain. It first calculates the morphology of the summit dome of Merapi volcano, then the dome stability and, finally, the pyroclastic flows that can result from a collapse. My review follows the structure of the manuscript: title, morphology calculation, stability and pyroclastic flow modelling.

Title:

the title must be changed to indicate that the studies focuses on the effect of rain and is not a comprehensive study of all the mechanisms than can lead to the collapse of active domes, as suggested by the current title.

Morphology:

The data obtained from Terrestrial Laser Scanning (TLS) and from drone are impressive and shows the power of such methods for calculating the morphology of dangerous areas. For the topography, most of the data were already published and the novelty here, is to extract the topography profile as well as the fumarole locations from visible images. Another novelty is to couple thermal images to confirm the fumaroles locations. My only doubt on this part is the temperatures accuracy given by the authors. According to variation of the atmosphere humidity, the composition of magmatic gas and the variable distance from the camera (it seems that a mean distance of 300 m is taken into account for the correction instead of the real distance, calculated with the DEM) and the pixel size, an error of the temperature of only 3°C seems very accurate. Could the authors give more details on how they have obtained this accuracy estimation? If not, it can be stated that the temperature is approximate, which is enough for the needs of the manuscript. Line 29 of section 3.1 must also be modified: as fumaroles activity is also related to rain, a punctual observation of more visible fumaroles can be related to seasonal changes and not necessarily to an increase of the activity.

Factor of safety:

This section is essentially based on the work and the model of Simmons et al. (2004). The novelty is the application to Merapi. My main criticism is that it is not easy to understand the calculations that have been done, and that some formula are perhaps wrong.

First, it must be explained why the authors focus on a small portion of a frozen dome.

All the summit, including the frozen dome, is cut by fractures. The crater flanks are very steep and can also collapse. The whole summit must be studied for a complete study of destabilizations.

Even if the safety model has been developed by others, the reader needs some information to understand what has been done (even briefly). Among the questions: what are the basis of the model? Why is there a link between the depth of water percolation and the distance between fractures to the square? (this is probably related to the surface that supplies the fracture, but why to the square?). Why the fracture widths are not taken into account in the percolation depth calculation? The "forces" must be more clearly explained and I recommend to expand and to detail the scheme of Fig. 6. The formulation of Fw and Fv are correct but it needs explanations: why a coefficient 0.5? Explain why, to calculate the force of the volcanic gas Fv, the density of the liquid and not of the gas is used (I have understood only by reading related papers). The "forces" W and F are not real forces (in N) but forces per meter in (N/m). It might be called a force but after being defined correctly.

In Equ. 2, C must be a force per meter. Is it the same as Cs, in tab. 1, both called "cohesive strength" but with a unit of stress (Pa)? The authors used the formulation of Simmons et al. and reproduce a probable typo in the formula (Eq. 1 of Simmons et al., 2004): Cs was probably C*s (in N/m in this case). Are the results obtained with a correct formula or with C instead of C*s? Because s = 100 m, using C instead C*s will significantly change the results. I cannot understand what is Fu and how it is calculated. If it is the water pressure at the base of the dome, the "force" must equal the pressure at the base of the fracture multiply by the dome surface, and it must be: Fu = d*cos(a)*rhow*g*s (neglecting the gas density). Where does the coefficient 0.5 come from? A progressive pressure decrease to the front? Why is it called the "uplift force from the volcanic gas", if it is related to the pressure of the liquid water only?

Once all these points will be fixed / clarified, the other point is the sensitivity of the model. The authors say that the "calculation requires careful parameter justification"

(section 5). As several parameters seem estimated roughly, other graphs like that of Fig. 6 are needed to explore the model sensitivity to the cohesive strength, the temperature, the volume rate of the rain, the fracture spacing, etc on the stability. I think that friction angles of 60° are not realistic and it can be replaced by a friction angle of 20° and 40°.

Modelling of pyroclastic flows:

The volume that can collapse in this manuscript is small and the lava dome is cold for several years (except because it transmits the temperature of the gases). In this case, why do the authors expect the genesis of pyroclastic flows? Even pyroclastic surges are evoked (4.1, p. 11, line 1-2). This assumption seems surprising and needs to be explained clearly. Works cited (e.g. Elsworth et al.) focus on an active and hot lava domes. The limitation of the numerical model used must also be presented. For example, the deposits of figure 7 are not compatible with pyroclastic flow deposits. They accumulate at the foot of the volcano forming piles more compatible with small rock collapse. Because the volume is small and the dome is cold, it is probable that a collapse will form a rock avalanche and not a pyroclastic flow but this must be explained and parametrized clearly.

If the authors want to simulate pyroclastic flows, a long debate exists about the models and the approaches used for pyroclastic flows and, today, models of pyroclastic flows are not reliable enough to be presented without discussions and caution. In this context, two points seem very worrying to me: 1) the work recently published by Kelfoun et al (2017) on the same theme (numerical simulation of pyroclastic flows) and on the same volcano (Merapi, 2010) is neither cited nor discussed. It cannot be ignored even if the model seems to reproduce correctly a pyroclastic flow emplacement with a physics that differs from the physics of the present manuscript. 2) the references to Charbonnier et al (2013) are partial. Their work is cited to justify that Titan2D is a tool that makes good simulations of pyroclastic flows, avoiding discussions on the model limitations. However, even if they have shown positive features, Charbonnier et al. have also shown the limitations of the models. For example, they wrote: "Titan2D is not capable of reproducing the runout distances and areas covered by the actual events over the highly complex topography" (discussion, 5.3.2). Is it compatible with its use in the present manuscript? A model is never perfect and it is why the limitations of the approach and of the results must be clearly and honestly discussed. The remarks of SC1 on the interactive discussion are also significant: for example, is the simulation able to stop? If not, what criterion has been chosen?

The quality of the DEM used, which seems to be very noisy, and the consequences on the results needs to be discussed too. Finally, given all the limitations of the approach and because the shape of the volcano has not changed from the last eruption, I wondered something similar to SC1: does the numerical model presented give results more confident than a rough estimation based on the past experience of Merapi's eruptions?

My conclusion is that, even if the data are interesting, they have been already partially published. The calculation of the stability is not new (except that it is applied to Merapi), not detailed enough and, maybe, partially wrong (C/Cs and Fu). The study is focussed on a very local problem: the collapse of a small part of a frozen lava dome by rain. The simulation of pyroclastic flows is based on a questionable assumption (a cold lava dome can create pyroclastic flow) and, the limitations of the model used and the results are not detailed enough. In the current state, I think that the paper cannot be published and it must be deeply reworked before publication.

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