Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-120-AC3, 2018

Natural Hazards and Earth System Sciences Discussions
Open Access

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

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Received and published: 9 August 2018

Reviewer: 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.

Printer-friendly version

Discussion paper

Response: We appreciate this comment and made appropriate changes. We have changed the title to: “Structural instability of the dome at Mt. Merapi volcano identified by drone photogrammetry and modeling.”

Reviewer: 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.

Response: Yes, part of the data is published earlier. In this new study we added new close up views of a photomosaic generated from new drone overflights. This new data adds further information about the location of fumaroles. Another very important new dataset is the use of high resolution thermal infrared maps. These were generated by a superzoom lens and image mosaicking. The results allow is identifying precise positions of gas escape. This gas escape follows a structural pattern already inferred from optical data, and weakly expressed in the Terrestrial Laser Scanning results. Therefore this paper presents a number of novel and innovative methods. In the revised version we improved this description and made the novelties further clear.

Reviewer: 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.

Response: Accepted comment and changes made. We follow the reviewers suggestion and describe the temperature as approximate, which is indeed enough for the needs of the manuscript. Nevertheless we assess the temperature uncertainty following (Spampinato et al., 2011). By varying parameters of emissivity, distance, reflection temperature (Trefl), atmospheric temperature (Tatm), relative humidity (RH), computed transmission, external optics temperature, and external optics transmission, we could assess the uncertainty. The uncertainty was obtained by choosing one pixel in the same area, varying one parameter, and then calculating the RMSE (see table 1 below). Based on the calculation, we found that increasing emissivity by 0.01 may influence the apparent temperature of 1.04°C. Other studies of the dome rock emissivity at volcanoes (Merapi, Carr et al., 2016) and Colima (Walter et al., 2013) suggested that the emissivity may be in the range of 0.95 and 0.98, therefore, we estimate that the uncertainty of the thermal pixel value is $\sim$3°C. However, in order to improve the manuscript, we accepted the suggestion from the reviewer by clarifying that the temperature is approximate in the revised version.

Reviewer: 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.

Response: Accepted comment and changes made. Following language proofreading, some of the unclear phrasing might already improve the clarity of the text. Furthermore, we improve the description of the safety equation (FS). Factor of safety is widely used to calculate slope instability and it is calculated by dividing resisting forces to driving forces that acting on a failure plane (α). The conventional model such as Slice, Swedish, Bishop's methods are commonly used to calculate slope instability. However, in an active lava dome, some additional forces may influence the resisting and driving forces. The FS equation used by us is based on Simmons's work, which aims to calculate dome instability during intense rainfall. Rain water may build up gas (Fu), vaporize the water (Fv), and add water forces (Fw). The FS from Simmon et al. (2004) considers the uplift force that may reduce the resistance force (W.cos(α)) and the water and vaporized water forces that may add the driving force (W.sin(α)). Therefore, we do not think that the equation is wrong, but we improved the text flow. As the current Merapi lava dome is influenced by degassing and rainfall activities, we used the FS equation from Simmons et al. (2004) to estimate the failure plane inclination (α), therefore, we were able to quantify the volume of source collapse. In order to make the equation more understandable, we clarified each parameter in the method (section 2.2) in the revised manuscript. We also re-calculate the factor of safety by using Swedish and Bishop's method to compare our FS results in the revised version.

Reviewer: 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.

Response: very good comments. Obviously our description of the horseshoe shaped fracture and the instability tests applied for a particular dome sector were not clear. We focus on a small portion of the Merapi dome because we find a structural weakening due to hydrothermal alteration at the southern part of the lava dome. This structural weakening is evidenced based on digital elevation models showing a horseshoe shaped crater, a fumarole expression following this horseshoe shaped pattern, and degassing of hot fluids along such a horseshoe shaped fluid pathway. It has been explained in the introduction that hydrothermal alteration may weaken dome rocks and promote dome collapse (L7-13, page 2). We improved the description of the horseshoe shaped fracture in the revised version. However, the idea to assess dome instability of the whole summit is good and accepted. We added instability analysis at the western flank in the revised manuscript as this area is also subjected by progressive hydrothermal alteration. Previous studies also suggest that the dome collapses were dominantly to the west-south west direction in 1900's (Voight et al., 2000).

Reviewer: 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?

Response: We introduced the factor of safety model in the revised version to describe the application of this model to the readers. As mentioned before that the factor of safety is generally used to assess slope stability. The model is calculated by comparing resisting force to driving force that acts on a failure plane. We added the basic concept of factor of safety in the revised version, compare our FS results from Simmons et al. (2004) to the FS results from Swedish and Bishop's model.

Reviewer: 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?).

Response: The instability of the square is influenced by water percolation (d) and the fracture spacing (s). Sensitivity tests of these two parameters suggested that increasing fracture spacing slightly decreases the factor of safet, while increasing water percolation (d) three times may reduce the stability 0.16 to 0.27 (Simmons et al., 2005).

Reviewer: Why the fracture widths are not taken into account in the percolation depth calculation?

Response : a critical question, which also leads us to further improve the discussion section of the paper. The water percolation is calculated based on equation 1 that consider a fracture spacing (s) on the block and dome properties and neglect the fracture width parameter. The equation considers that the dome properties (temperature, heat capacity, and thermal diffusivity) have stronger control toward water infiltration than the fracture width (Fig. 6). Therefore fracture width is not necessarily to be taken into account in the percolation depth calculation.

Reviewer: 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).

Response : We added detail explanation of forces that influence the FS calculations in the revised version. Fw and Fv are water and vaporized water forces, respectively. The coefficient of 0.5 is to calculate the geometry volume of Fw that influence the block (see Fig. 6). In general, force can be expressed by multiplying mass and acceleration/gravity acceleration (F = m. g, where m = v×, so F = v××g). In the equation, it is mentioned that:

Fw=(0.5×d2×cos(α))×w×g.

We inferred that the 0.5 is a constant that used to calculate the geometry volume of Fw that influence the block stability (see Fig. 6 where Fw are represented as triangle prism object). We used the density of water to calculate Fv as Fv represents the force of vaporized water from rainfall that interacts with hot dome interior.

Reviewer: 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.

Response : accepted comments. We corrected, converted all parameters in SI units and re-calculated the Factor of Safety in the revised version.

Reviewer: 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.

Response : Accepted comments. We corrected Cs to C*s, converted all parameters in

SI units, re-calculated the FS from Simmons et al.(2004) and compared the FS results from Simmons et al to the FS calculation based on Swedish and Bishop's methods.

Reviewer: 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?

Response : The uplift force (Fu) is produced by water vapor and volcanic gas that released upward through the fracture to atmosphere. As mentioned before that the coefficient 0.5 is a constant that used to calculate the volume of Fu that influence the block stability (see Fig. 6 where Fu are illustrated by triangle prism object). Fu=(0.5$\times$d2$\times$cos(α)$\times$s)$\times$w$\times$g according to Simmons et al (2005)

Reviewer: 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°.

Response : accepted comments and suggestions. In the revised version, we clearly described the limitation of the FS method and the sensitivity of the parameters. We recalculated the factor of safety by using friction angle of 25° and 45° according studies from Simmons et al (2005) and Husein et al (2014).

Modelling of pyroclastic flows:

Reviewer: 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.

Response : Accepted comments. We changed the terminology from pyroclastic flows to rock avalanche as the collapse of southern frozen dome is likely to produce rock avalanche than pyroclastic flow. However, we think that it is vital to assess the potential hazard zone in a case of the southern dome collapse as many cases suggested that hydrothermal alteration may weaken the rock and trigger a collapse. In addition, sand mining intensively occurs at the southern flank with radius of 5 km from the summit of Merapi. This is the reason why we assess the potential hazard of the southern Merapi dome sector. The idea to assess the potential hazard in this manuscript is vital for hazard assessment in the future.

Reviewer: 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?

Response : Accepted comments. We changed the terminology to rock avalanche as suggested in the previous point. We still used Titan2D to model the rock avalanche as titan2D is well-validated to model granular avalanches over natural terrain (Patra et al. (2005); Pitman et al. (2003). The suggestion to add the work from Kelfoun et al (2017) is accepted and added in the discussion. We agree that model is never perfect, therefore, we added more detail on limitation of the Titan2D to simulate debris avalanche in complex topography as also suggested by Charbonnier (second reviewer). The remark of SC1 is also correct that Titan2D has limitation to stop the simulation. The simulation cannot perfectly stop, even it is reached the maximum time simulation. In order to fit realistic model, we extended the maximum time simulation up to 1 hour which is long enough for rock avalanche duration and set validated/corrected friction angle parameter as this parameter control the run out and distribution of the rock avalanche.

Reviewer: 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?

Response : We realized that the DEM used for Titan2D model is noisy as it was merged with TanDEM-X from Kubanek et al., (2013). During DEM reconstruction, TanDEM-X may produce random noise and grazing signal in complex topography area. In order to reduce the noise, we filtered and re-interpolated the DEM and re-run the model by using filtered DEM. Historically, volume with VEI 1, may produce debris avalanche/pyroclastic flows less than 5 km from the summit of Merapi (Voight et al., 2000). In our results, the maximum run out distance is about 4 km from the summit. We think that our results represents typical geophysical mass flow that occurs in Merapi.

Reviewer: 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 focused 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.

Response : We appreciated the comments and suggestions from the reviewer and thank you very much for the work to improve the manuscript. We have deeply reworked and re-analyzed the results and revised the manuscript based on the suggestions and comments from the reviewers.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2018-120, 2018.

emissivity	distance	T_{refl}	T_{atm}	RH	Comp trans	Ext. trans	Ext. temp	Refer temp	Min (°C)	Max (°C)	Avg (°C)	RMSE (°C)
0.95	300	0	20	45	0.91	1	0	20	7.3	137.7	34.7	0
0.96	300	0	20	45	0.91	1	0	20	7.3	136.7	34.4	**1.04**
0.95	1200	0	20	45	0.91	1	0	20	7.3	137.7	34.7	0
0.95	300	10	20	45	0.91	1	0	20	6.8	137.5	34.3	0.67
0.95	300	0	19	45	0.91	1	0	20	7.5	137.7	34.8	0.22
0.95	300	0	20	55	0.91	1	0	20	7.3	137.7	34.7	0
0.95	300	0	20	45	0.9	1	0	20	7.2	138.6	34.8	0.91
0.95	300	0	20	45	0.91	0.99	0	20	7.4	138.6	35	0.95
0.95	300	0	20	45	0.91	1	10	20	7.3	137.7	34.7	0
0.95	300	0	20	45	0.91	1	0	10	7.3	137.7	34.7	0

Fig. 1.