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Discussions

Interactive comment on "Lava flow hazard at Fogo Volcano, Cape Verde, before and after the 2014–2015 eruption" *by* N. Richter et al.

N. Richter et al.

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Author reply: We thank the reviewer for very detailed and constructive comments and we will revise the manuscript accordingly.

Major comments:

1. My main concern is that the authors stress throughout the manuscript the relevance of their study for risk management, the rapid acquisition strategy of their method and the role of the HART of GFZ in assisting local decision makers during the crisis management and in training local residents. Although I do not doubt that the high quality products presented might be relevant for crisis and long term risk management, it is unclear in the manuscript to which extent the local actors were indeed informed about these results and how much these maps were used to inform the local population.

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Either the authors have taken actions to ensure that the scientific results have a direct impact on risk management and information to local population, and they should describe it, or they have not (yet) done so and assume that a scientific publication is sufficient to have an impact. In the latter case, the argument of the impact of the research for risk management should be downscaled in the paper (eg. page 2, lines 1-6, page 12 lines 5-10, page 15 lines 37-38). As pointed out in the introduction, residents tend to re-occupy zones invaded by lava flows but from experience I don't believe accurate lava flow hazard maps can make a difference without major investment in education and communication actions. This is mentioned by authors (page 2, line 6) but this should be clear in the discussion and conclusion.

Author reply: This is a valid point. We do agree with this comment and we will downscale the relevance of this study for risk management in the revised manuscript. In the revised version, we focus on hazard assessment and provide comprehensive hazard maps. Neither vulnerability nor preparedness have been investigated by us. However, our hazard maps might be valuable for risk management, provided that they are scientifically peer reviewed and subsequently communicated to the local decision makers. We took two important steps to assure that our results are accessible. Firstly, we chose to publish in an open access journal. Secondly, we collaborated with Sonia Silva (who is an author on the manuscript), the leading local volcano scientist. She interacts with the local decision makers and will be able to use our results and communicate them further.

2. DOWNFLOW: I am quite familiar with the approach and capabilities of the DOWNFLOW code. In some places the authors should be more careful in the description of the DOWNFLOW results and be critical. On p 10 (lines 4-10), authors highlight the good match between the DOWNFLOW simulation and the outline of the actual flow. Where this is true for some areas (points 1, 2, 3, 4 on Fig. 8), this is not so true for zone 5 where the probabilities of DOWNFLOW are much lower than other zones located at shorter distance (Western and Southern border of the calder). Zone of over-

estimation of the DOWNFLOW simulations should also be described in this part of the results. Also in the discussion (section 5.3), authors should not only highlight the capabilities but also the limitations: between the Northern and NW branches, a lot of pixels have a low probability of lava flow invasion but were not invaded, whereas the opposite is true for Zone 5. These uncertainities, and their cause, should be highlighted. When discussing accuracy (page 13, line 31), quantitative values should be provided: the reader should be informed that 'very good' simulation have accuracy parameter of $\sim$0.5 even without considering the issue of length.

Author reply: We agree this is a valid comment and we will include changes following from this remark in the final version of the manuscript. We will highlight uncertainties and give quantitative values for accuracies.

Minor comments:

- Page 3, line 7: spell out TLS when first used in main text

Author reply: We accept this comment and changed the text accordingly (we now spell out TLS on page 3, line 7).

- A recently published paper by Cappello et al. (2016, JGR, DOI: 10.1002/2015JB012666) also discuss lava flow modelling for the Fogo 2014-15 eruption. As that publication use a physically-based model, a comparison of the advantage and limitation of the two approaches in the discussion would be useful.

Author reply: We accept this comment and incorporated changes accordingly. While the purpose of Cappello et al. (2016) is near real time lava flow hazard estimation during the 2014-2015 eruption, our ultimate purpose is to create hazard maps that are valid for the next eruption of Fogo Volcano. We include this very recent study in the revised manuscript and compare the different approaches in our discussion.

- Paper by Albino et al. (JGR, 2015) presents a volume estimate for the Nyamulagira 2011-12 lava flow eruption using a TanDEM-X DEM. The author could compare the

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accuracy of their DEM comparison and volume estimate with the one presented by these other authors. Why was the TanDEM-X technology not applied in the case of the Fogo eruption to derive the post-eruption DEM?

Author reply: We accept this comment and incorporated changes accordingly. When using the DEM difference method for lava flow volume calculations, errors in volume only occur due to the quality and the resolution of the DEMs. Albino et al. (2015) used TanDEM-X DEMs to estimate the volume of the lava flow of the 2011-2012 Nyamulagira eruption to be 305.2 +/- 36.0 x 106 m3. Their error therefore corresponds to 11.8 % of the total lava flow volume. Our error corresponds to 11.9 % of the total lava flow volume. The achieved DEM qualities are therefore comparable, which we discuss in the revised version of the manuscript. We still have to note that our pre-eruptive DEM is of lower resolution and quality as compared to our post-eruptive DEM. We used ground-based techniques, because TanDEM-X bistatic data is not available for Fogo after the 2014-2015 eruption.

- The authors argue that ground-based technology are ‘more flexible’ (page 3, line 5): I find this argument a bit weak, as ground-based technology require to access inhospitable volcanic area during or directly after an eruption. This is also contradicted by the discussion where the TLS approach is described as ‘time consuming and challenging’ (page 13, line 10).

Author reply: We refer to the sentence before, satellite data need to be tasked and ground-based methods are more flexible with respect to their acquisition time and date. For instance, we produced the first post-eruptive DEM from ground-based data in January 2015, while the next (and only other) available post-eruptive DEM data was acquired on 20 June 2015 by the Pléiades satellite, more than 5 months after the end of the 2014-2015 eruption. In section 5.2 we compare ground-based TLS to ground-based SfM techniques and conclude that TLS in this comparison is more time consuming and logistically challenging. We have done some rewording to avoid misunderstandings.

- HART: authors repeatedly highlight the action of GFZ-funded HART initiative (page 4, line 15-18; page 12 line 3-10) and the rapid-response character of the action (page 4, line 16). Although HART is for sure a nice initiative I don't think this paper should aim at giving so much publicity for it. The author should also justify why they consider that the topographic survey is part of a 'rapid response': as eruptions are often separate by several years, this survey could be done once it is clear that the eruption is finished and access to the site is secured.

Author reply: We accept this comment and changed the text accordingly (i.e. we reduced the publicity for the HART initiative).

- Section 3.2.4: mention already here the spatial resolution of the post-eruption DEM produced

Author reply: We included the spatial resolution of the post-eruptive DEM in this section (in the sentence: "We generate a DEM featuring a 5 m spatial resolution from more than 164 million TLS data points and mosaic it with the pre-eruptive DEM").

- 'filling algorithm': in the methodology (page 6, line 33; page 10, line 10), as well as the discussion section (page 13, line 33), the authors refer to a 'filling algorithm' integrated in DOWNFLOW. More information should be given about this aspect. DOWNFLOW being a probabilistic model with no explicit lava flow thickness and topography adaptation, I don't really understand what is meant by 'filling algorithm' except for the possibility of the simulation to continue beyond actual pits in the DEM. How this is implemented in the algorithm should be explained in details. The observation that higher probabilities are found in pits, corresponding to thicker flow accumulation, although interesting, is not a surprise as it is a simple results of the topography-control on the lava flow paths. I would not say that these similarities are 'intriguing' (page 10, line 5), they are rather expected and logical based on the modelling approach.

Author reply: We added more detail on the filling algorithm to the text. We also added relevant references. The "filling algorithm" simply adds (deposits) a certain thickness when there is a local minimum. A local minimum is defined as follows: During the simulation of one steepest decent path, the topography is randomly perturbed (within the interval Δh). And if the simulation is in a minimum, the topography is perturbed again. If for 10 times the simulation is still stuck in a minimum (i.e. the simulation doesn't find a steepest decent path to follow downslope) a certain thickness is added. (Of course the number 10 can be changed, but we think 10 is not too little (not to introduce a local minimum just due to the perturbation) and not too big (because it will use CPU in these evaluations). Typically a 'very' small thickness is added (in our case 0.01 m) during iterations. This value can be adjusted in the DOWNFLOW input file. The smaller the value the longer it will take for the simulation to run. But the smaller the value, the better is the filling. According to a suggestion from reviewer 1, we do not longer mention the comparison of the thickness and the simulation in section 4.3. We only mention this in the discussion now.

- Section 3.3.2: the optimal Δh value is defined based on the maximization of the best fit parameters, using the actual lava flow as reference point. In the discussion, authors argue that this parameter has a wide range of fit (page 14, line 19), although figure 4 actually suggests that for Δh <2.5 m and >4 m, the fit significantly reduces. How confident are the authors that this Δh value will also be optimal for the future eruption?

Author reply: We agree and we have added the information that for Δh < 2.5 m and > 4 m, the fit significantly decreases to Sect. 3.3.1 (Sect. 3.3.1 and Sect. 3.3.2 are now merged). We will also change the discussion according to this comment in the final version of the manuscript. The Δh value represents the characteristics of the lava. According to previous applications, this parameter differs for different volcanoes, but not so much between different eruptions of one volcano. We will mention this and add references to the discussion of the final version of the manuscript.

- Section 3.4.1.: Authors should clarify that they assume a linear decline in probability from the minimum to the maximum length, similarly to previous application of DOWNFLOW. Bonne et al. (Int. J. Remote Sensing) demonstrated that for Mt Cameroon, a

Gaussian probability decrease better fitted observed lava flows' lengths.

Author reply: We accept this comment and incorporated this into the manuscript.

- Page 8, line 2: justify the bandwidth of the Gaussian kernel. This bandwidth can have a major impact on the resulting PDF map.

Author reply: This is a valid point. We discuss this in the revised version of the manuscript and we included a study published by Bartolini et al. (2013) who suggest how to calculate an optimal bandwidth. According to an equation from that paper (Bartolini et al., 2013) the optimal bandwidth for our case study would be 3600 m, i.e. we would have only one maximum in our distribution, centered on the cone itself. However, we think that the cone is clearly not the maximum of probability of vent opening considering recent eruptions. Therefore, we chose the value by trials, not to have the distribution too undersmoothed or too oversmoothed.

- Equation 2: explain why the resolution of the DEM Δx and Δy need to be taken into account in this equation.

Author reply: In equation (2) we have a product of probabilities (the uppercase P are probabilities, which we state more clearly in the revised text). The probability of vent opening in a pixel is rhoVj Δx Δy. This is because rhoVj is not a probability, but is a PDF (probability density function), that is a probability divided by an area. So the probability of having a vent in a given pixel is rhoVj Δx Δy. We will include this in the final version of the manuscript.

- Fig. 8a: why is the color bar of this figure not presented in a quantitative way (with percentage) similarly to Fig. 9 and 10. This would be much more interesting. Actually the color scale of Fig. 10 is the most interesting one, as it also enable to know what is used as lower threshold (pixel with no color).

Author reply: Fig. 8a shows the DOWNFLOW simulation output, i.e. a grid with the same cell size as the input DEM, where each pixel value gives the number of n steepest decent paths overlaying the cell area. The highest n values reflect the most likely paths followed by the lava downhill. In such a single DOWNFLOW simulation (with N number of runs), the lava flow length is not constrained (all steepest decent paths run until they reach the end of the dataset) and therefore this map does not reflect a “lava flow hazard”. To avoid confusion with a hazard map, we chose to color scale the map according to a “high” or “low” number of times that a pixel is crossed over the total number of runs. In our opinion, a quantification of this number does not add relevant information. In the revised version of the manuscript, we have done some rewording to explain this better. We also added relevant literature for reference, e.g. Tarquini & Favalli (2015). In contrast to the “Fogo scenario” hazard maps (Fig. 10), the “Pico Pequeno scenario” hazard maps (Fig. 9) contain areas of both, 100 % and 0 % probability of invasion. This is why we only introduce a lower threshold of probability in Fig. 10 (0 % probability only occurs on top of cones or at some places along the Bordeira wall in the “Fogo scenario”). In Fig. 9, no color is 0 probability of invasion (because there is a restricted area of vent opening in Fig. 9).

- Fig.8b: this is a key results of the study which could be better valorized. An histogram of the thickness distribution should be provided. The color bars suggest values from -12.8 to +52.7 m: what proportion of the thickness are negative, how could this be explained and how does this impact the total volume? Author mention a maximum thickness value of 35m: why does the color bar goes to 52 m then?

Author reply: We will provide a histogram of the thickness distribution. Negative values occur at the edges of the flow, where ground subsidence is observed due to loading (also obvious from InSAR data). Also, negative values occur close to the vent due to vent opening. The lava flow thickness is indeed maximal ∼35 m, but the cone at the eruption site built up to be maximal 52.7 m. We will add this to the text and provide an explaining figure.

- Section 4.2: the aeral coverage of the lava flow on lines 18 and 28 (page 9) are not matching. Please provide an histogram of the thickness values (at the moment you give the mean and some max value, but you don’t provide the distribution nor the minimum values that are probably below zero: Fig. 8b).

Author reply: The area we use is calculated from the coherence maps. We actually also used the DEM difference map to check the area and the value was slightly smaller (as we are missing data at the westernmost tip of the northern lava lobe). We have therefore deleted the area from Sect. 4.2. As mentioned above, we will provide a histogram of the thickness values in the final version of the manuscript.

- Page 11: the pre-post hazard map comparison is interesting at the caldera scale for the ‘Pico Pequeno’ scenario. It is less relevant for the ‘Fogo scenario’ as the changes are minor and similar to the ones observed in previous scenario. I advise to shorten or cut lines 11-18 (page 11).

Author reply: We agree and we will shorten the paragraph.

- Page 12, section 5.1: this section could be largely reduced as it brings little new information. The technique of coherence loss to map new volcanic products is indeed quite standard and does not disserve a long discussion. I disagree with the sentence (line 26-): “as the resulting extent of the mapped and the simulated 2014-15 lava flows match almost perfectly”. Looking at fig. 8a it is obvious that this is correct only for specific zones andonly for the early emplacement stage of the lava flow.

Author reply: It is true that the good fit is only correct for specific zones and only for the early phase of the eruption. We will shorten and reword this paragraph in the final version of the manuscript accordingly. We will also mention here the comparison to Cappello et al. (2016), who achieved a better fit using a deterministic model.

- Page 15, lines 17-28: this paragraph does not relate at all to the presented results. Although I know that the modelling approach might enable to simulate the influence and optimal location of a barrier, this is not done in this case, and I doubt this would be practical solution for the Cha caldera, since the eruption probability and the settlements are dispersed. The example of Chirico et al. (2009) mentioned is a good example of a purely theoretical modelling exercise with no applicability on the ground. I would advice to cut this paragraph.

Author reply: We agree and we will shorten the paragraph (not mentioning the barriers anymore).

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