Review of **Lava flow hazard at Fogo Volcano, Cape Verde, before and after the 2014-15 eruption** by Richter et al.

The study of Richter et al. presents an interesting analysis of the Fogo 2014-15 lava flow eruption. The two main point of the papers are 1. The presentation of the methodology to update the topography of the DEM using ground-based laser and photogrammetry, leading to the derivation of the lava flow volume, and 2. The impact of the new topography on the spatial distribution of lava flow modeled using a probabilistic approach.

The manuscript is of overall very good quality. It is well written and supported by informative figures and tables. The text is however quite long and could be shortened by 15-20% without loss of content, avoiding unnecessary broad information in the introduction, moving some technical details in appendix and avoiding the many repetitions between the observations and the discussion.

I recommend that this manuscript be accepted for publication in NHESS after minor revision of the text in order to address the comments listed hereafter.

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. 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 later 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.

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 overestimation 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 ~0.5 even without considering the issue of length.

Minor comments:

- Page 3 , line 7: spell out TLS when first used in main text
- 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.
- 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 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?
- 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).
- 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.
- Section 3.2.4: mention already here the spatial resolution of the post-eruption DEM produced
- '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.

- 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?
- 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.
- Page 8, line 2: justify the bandwidth of the Gaussian kernel. This bandwidth can have a major impact on the resulting PDF map.
- Equation 2: explain why the resolution of the DEM Δx and Δy need to be taken into account in this equation.
- 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).
- 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?
- 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).
- 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).
- 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.
- 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.