

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

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The manuscript “Lava flow hazard at Fogo Volcano, Cape Verde, before and after the 2014–2015 eruption,” by Richter and others, makes use of detailed topographic and surface area data to map lava flow inundation and assess hazards using the stochastic DOWNFLOW model. An exceptional amount of data are used in this analysis—the post-eruptive DEM required intensive field work with a TLS instrument in addition to the collection of a photogrammetric dataset. Despite the complicated nature of the topographic data, which span several orders of magnitude in terms of resolution, the analyses are well executed and demonstrate the synergy between rapid collection of topography and currently available lava flow pathway models. The DOWNFLOW simulations for before and after the 2014–2015 eruption yield important insights into hazards within the Cha das Caldeiras and have great relevance today, with the potential

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for application to land-use planning to minimize the risk from future eruptions. There are general lessons that can be applied to other basaltic volcanoes as well.

The writing is excellent, the figures are of high quality, and the paper is well referenced. The details and conclusions not only offer a great description of the 2014-2015 Fogo eruption, but also the complexities of lava flow hazards and human responses. I have only a few thoughts of consequence that I'd like to share with the authors. I also have a number of minor comments that I list at the end of this review and that mostly point out places where the text could be reworded for clarity.

I hope that the authors find this review helpful.

Best wishes, Mike Poland

Specific comments:

- I would have liked to see a little more background about Fogo's eruptive history, or perhaps a reorganization of the material that is presented. For example, in the introduction, the 1951, 1995, and 2014-2015 eruptions are mentioned first, and then the "major 1680 eruption" is brought in somewhat casually. This sounds like a key event in the volcano's history, yet its importance is hard to appreciate in the current context. It's not even mentioned in the "Geologic setting and eruptive history" section. I recommend that the entire second paragraph of the introduction (which starts with "Fogo Island features a prominent giant landslide") be integrated with the "Geologic setting and eruptive history" section. That way, the introduction covers only that material which is sufficient to understand the purpose and importance of the paper, and all of the information on past eruptions and geologic setting are contained within a specific section.

- In the introduction, I found it a little odd to see that "DOWNFLOW is known to work well at steep terrain" and then in the subsequent sentence "Here we apply the model to a rather flat area." This seems contradictory. Is this one of the first studies to apply DOWNFLOW to a low-relief area? If so, is that an outcome of the paper that the

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model is also appropriately used where topography is subdued (assuming the model parameters can be adequately explored and constrained)?

- There are a number of very short sections throughout the manuscript, and it might be better to eliminate or merge some of these just to keep from breaking the flow of the paper. For example, it seems to me that section 3.3.1 could be merged with section 3.3.2. It keeps the description of the DOWNFLOW simulation with no length constraint contained within a single section, thereby lessening the chance for later confusion (in my opinion). Also, section 4.4 (the single sentence before section 4.4.1) is not really necessary and could be deleted.

- I enjoyed the discussion about the challenges of placing length constraints on the lava flow simulation. That said, I was a little surprised there was no mention of the fairly well-known work of Walker (1973) and subsequent authors, who noted the correlation between effusion rate and flow length. This could be especially important given the restricted vertical spread of historic eruptions at Fogo (making it impossible to infer a correlation between flow length and vent elevation). Given that one of the really noteworthy results of this work is a volume for the 2014-2015 lava flow, the authors could calculate discharge rates. Overall, the eruption rate (43.7 MCM over 85 days) was 6 m³/s, but it was certainly higher during the first few days/weeks of the eruption, when the flow traveled farthest. Can the authors place some constraint on the maximum effusion/discharge rate for the early part of the eruption? This might allow them to use Walker's (1973) relation between effusion rate and flow length, and could also facilitate a more thorough comparison with the 1995 eruption.

- What is the impact of varying delta-h on the simulations? That parameter seems relatively well constrained, but a brief mention of how important that value is to the results (what happens if the value is changed to 3.5 m? Or 2 m?) would help reinforce the simulation results.

- I was struck by the fact that Pico Pequeno, where the last two eruptions have taken

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place, is not the densest concentration of vents – not by a long shot! This is reminiscent to me of the situation at Campi Flegrei, where Bevilacqua et al. (2015) found that the most likely location of a new vent in that caldera was not where the most recent eruption occurred. This has little direct relevance, except with underscoring the importance and challenge of knowing something about vent location when estimating hazard.

- I really like the coherence maps showing lava flow area, but wonder if those might be summarized in a single figure? The maps dominate the figures (in terms of their size and the space they take up), even though they are not necessarily the most critical figures in the paper. One option might be to move the coherence images to the appendix, and replace the separate panels of Figure 7 with a single map that shows the flow outline at different times (because there is no look angle artifact with coherence, different dates could be combined on a single map). This would make it easier to compare what are now separate panels and show the spatio-temporal growth of the flow at a glance. A disadvantage is that one would not necessarily “see” the small flow that was active in mid-January, since it is mostly contained within the existing flow. That’s why the coherence images could remain in the appendix, but the areal growth of the flow could be represented in a single manuscript figure that would communicate that information more succinctly. Also, why do the coherence images not go through the end of the eruption, in early February? If they show no change in the flow (aside from incoherence due to cooling and rapid subsidence), that would be helpful in constraining the effusion rate (i.e., that there was little volume erupted during late January – early February). This is already hinted at in section 4.2, but there is little evidence given to back up claim of a post-January 16 erupted volume of 0.05 MCM.

- The manuscript does a good job of pointing out that topography must be updated in order for subsequent DOWNFLOW runs to be relevant, and indeed, this is borne out by the results – some areas had a 0% chance of inundation based on the pre-eruption DEM, but as the 2014-2015 eruption progressed, the modified topography

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caused these areas to become inundated. I think this could be stated more explicitly, however. Lava flows make their own topography, so the failure of the simulation to exactly predict the flow coverage (the “0%” probability areas) is really more a reflection of the changing topographic conditions than of the capability of the simulation, right? This is a major conclusion of the paper, but it is somewhat spread out between sections 4 and 5. The authors might want to make it more explicit. Perhaps even address the question about iteratively updating topography (with TLS?) during a crisis so that successive DOWNFLOW runs will be more representative of changing conditions. That seems like an unstated conclusion, given the datasets the authors used and the simulations they ran.

- Was any adjustment applied to the volume calculation to account for vesicularity? For aa flows, ~25% is often assumed. If no adjustment is made, the authors should be explicit that the volume is a bulk volume, and that vesicularity is not accounted for. This might explain a small percentage of the difference with the Ferrucci volume. . .

Technical recommendations:

- I suggest deleting from the abstract the sentence “Based on this, we discuss how our study can help improving the general understanding of basaltic lava flow behavior.” Such a discussion really doesn’t exist in the manuscript, and I don’t think anything is lost from the abstract if this sentence is removed (in fact, the abstract becomes tighter).

- The authors reference “Harris and Rowland, 2001” in the introduction section to highlight their FLOWGO code, but maybe referencing their 2015 update to that code would be better? That paper is included in the AGU monograph on Hawaiian volcanism.

- Toward the end of the introduction, it is unclear whether “featuring a 5 m spatial resolution” is referring to the pre-eruption DEM, the post-eruption DEM, or both.

- Is the last paragraph of the introduction (“In the first section of this paper. . .”) necessary? I think the introduction might be more powerful if it were to end with mention that

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this work is the first to use TLS data as a base to develop probabilistic hazard maps.

- In the first paragraph of section 2, note that Bordiera reaches a height (not an elevation) of 1,000 m above the Cha.

- The first sentence of the second paragraph of section 2 is awkward, and should be rephrased. It implies that reports of eruptive activity exist for the period ~1500-1660, but afterwards there was less information? This lesser-known period would include the time period of the “major 1680 eruption.”

- At the end of section 2, I suggest replacing the word “reenacted” with “renewed.”

- In section 3.1, the phrase “ranges between ~12 h (for the ascending and descending pairs 57/64 and 148/155) to ~6 days” is awkward and should be reworded. I think the authors mean that consecutive ascending and descending data are offset by 12 hours, but the two sets of A/D pairs are offset by about 6 days. In any case, this might be confusing to readers who don’t regularly work with TSX data. Also in this section, it might be useful to mention that vegetation is not an issue in the Cha, so coherence really just reflects steep slopes and surface change. This is stated much later in the manuscript, but should probably also be explained here.

- In section 3.2.2, I was a little confused by the scanner locations and positions. It appears that there were three major locations from which scans were collected—Beco, Saia, and Amarelo—and at two of these locations, multiple positions were occupied (presumably with different fields of view). Is that right? After the three locations are mentioned, it is stated that “At five of the scanner positions” GPS data were acquired. It is not clear if these 5 positions are distributed between the Beco and Amarelo sites, or represent all of the positions at these sites, etc. I recommend that a little rewording be done here to make the procedure easier to understand.

- Toward the end of section 3.2.2, the phrase “here used methodology” is awkward and should be reworded (“methodology used here” would be fine).

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- In section 3.4, I would delete mention of the “~82,000 vents,” since it only raises the question of how that number was determined. Since this is explained in section 3.4.3 in greater detail, the earlier mention can be removed.
- In section 3.4.2, the phrase “which integrates up to be 1” should probably be reworded to something like “which sums to 1.”
- In section 3.4.3, right after equation 2, “extents” should be changed to “extends.”
- In section 4.1, did the flow thicken after it stopped advancing (after December 23, 2014)? From figure C, it looks like it did (at least, the active lobe in January did), and that would probably be worth stating directly.
- In the first paragraph of section 4.2, I didn’t understand what was meant by the error being smaller “when comparing post-eruptive and pre-eruptive grids”.
- In the second paragraph of section 4.2, isn’t the area calculated from the coherence maps, and not the topographic difference? Also, note that the area given here (4.84 km²) differs from the area given at the end of section 4.1 (4.85 km²). Finally, perhaps the maximum thickness of the 2014-2015 flow could be given along with the average thickness, instead of at the end of the section?
- In section 4.3, I thought it was a little awkward to bring up the apparent correlation between the simulation and the thickness, since it is not raised again until well into the discussion. Maybe wait until the discussion to note this similarity? That way, it doesn’t get in the way of the description of the simulation results.
- The first time a percentage is given in terms of a DOWNFLOW result (in section 4.4.1), it might be useful for the authors to offer a brief explanation of what that percentage is referring to—for example, is it the likelihood that a future eruption will inundate a specific pixel? Just so that the reader is clear on the meaning.
- Toward the end of section 4.4.2, “remarkable hazard” is an awkward phrasing that should be reworded. It’s unclear if 10% is remarkable because it is so low or so high.

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- The idea of “catchment” maps is a good one. The authors may wish to reference some work along the same lines at HVO by Frank Trusdell and Jim Kauahikaua, who use that technique for hazard assessment on the Island of Hawaii.
- At the end of section 5.2, is there any indication why the volume derived from the topographic difference is so much greater than that of Ferrucci et al., 2015? The idea that volumes inferred from thermal data might be so much different from those determined by topographic differences is a little unnerving.
- In the third paragraph of section 5.4, the authors raise the question of why the 1995 and 2014-2015 flows followed such similar paths. But isn't the answer “topography”? Can it be anything else? It's unclear to me what type of “future studies” might actually address this question.
- In the conclusions section, I would recommend deleting the second-to-last sentence. “We conclude that the next lava flow will very likely change the lava flow hazard within the Cha again.” This sounds rather grand, but is also pretty plain for all to see, and the point is made more effectively earlier.

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