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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.

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We very much appreciate the detailed and constructive comments provided. We incorporated all suggestions into the manuscript; however, some remain to be fully included in the final version of the manuscript:

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

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that the entire second paragraph if 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.

Author reply: We have integrated the second paragraph of the introduction with the second section on the “Geologic setting and eruptive history” as recommended (page 3, line 11-17; page 3, line 38 – page 4, line 6). We have also restructured the second paragraph of section 2 and added a little more information on the 1680 eruption for clarity (page 3, line 11-13). At the same time we made the attempt to not add much additional text, as suggested by reviewer 3.

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

Author reply: Indeed, DOWNFLOW has been applied mostly in steeper terrain in the past (e.g. at Mt. Etna, Mt. Cameroon, and Nyiragongo Volcano). Our study shows that the code also works well in rather flat areas (the Chã). We agree that this is an outcome of our work and not the motivation to use DOWNFLOW for the Fogo case. We have deleted the sentence from the introduction and moved it to the very beginning of our discussion about the DOWNFLOW performance (Sect. 5.3).

- 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

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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.

Author reply: We have merged sections 3.3.1 with 3.3.2 and have deleted the single sentence of section 4.4.

- 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.

Author reply: Cappello et al. (2016, JGR) calculated a maximum discharge rate for the 2014-2015 eruption of up to 20 m^3s-1. We can also constrain the lava flow length using the maximum and average effusion rates for the 2014-2015 eruption and compare this to the 1995 eruption. However, when creating hazard maps we need a lava flow length constraint that is valid for a future eruption. In this case we don't have any information on effusion rates. This is the reason why we chose an empirical approach for the length constraint. That said, we will incorporate this comment and refer to the works of Walker (1973) and Cappello et al. (2016) in the final version of the manuscript.

- 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.

Author reply: We have added the information that for $\Delta h < 2.5$ m and > 4 m, the fit significantly decreases to Sect. 3.3.1. We will also change the discussion accordingly in the final version of the manuscript.

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

Author reply: A valuable comment. We will discuss the challenge of knowing future vent locations and emphasize the importance of such knowledge for hazard assessments in the discussion of the final version of the manuscript.

- 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.

Author reply: We have replaced the separate panels of Figure 7 with a single map that shows the flow outline at different times. We have also adjusted the text accordingly (Sect. 4.1). This way we lost the actual coherence information. For the reader to be able to refer to our coherence interpretation, we will move the coherence images to Appendix D in the final version of the manuscript. We will also add coherence information for the very end of the eruption (19 January – 10 February 2015) there.

- 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 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.

Author reply: We will emphasize this point more in our conclusion in the final version of the manuscript.

- 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

Author reply: In the discussion (Sect. 5.2) we have added the information that the DEM difference method gives a bulk volume while the Ferrucci et al. (2015) and Cappello et al. (2016) both calculate DRE lava flow volumes.

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).

Author reply: We have deleted the sentence "Based on this, we discuss how our study can help improving the general understanding of basaltic lava flow behavior." from the abstract.

- 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.

Author reply: We have changed the reference to the updated version of the FLOWGO code (page 2, line 13).

- Toward the end if 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.

Author reply: The 5 m spatial resolution is referring to both DEMs. We have made this clear by adding "both" to this sentence (page 2, line 35).

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

Author reply: We have deleted the last paragraph of the introduction.

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

Author reply: We have corrected this.

- 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.”

Author reply: We have reworded the paragraph for clarity.

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

Author reply: We have corrected this.

- 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.

Author reply: We have deleted the sentence about the time delay between the orbital paths of the satellite to avoid confusion and we have added a sentence on the main decorrelation factors in the study area at hand.

- 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.

Author reply: Indeed, there are three major scanner locations from which scans were collected (Beco, Saia, and Amarelo). At Beco and Portela, multiple positions were occupied with different fields of view. We are referring to Table 1 when stating that we collected GPS data at five of the scanner positions. In Table 1 we show for which scans we collected GPS data. We have now added the information that GPS data was collected for one position on Monte Saia and for four positions on Monte Beco to the text (Sect. 3.2.2, page 5, line 24-25). However, we will change the text in the final version of the manuscript as suggested to make the whole 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).

Author reply: We have corrected this.

- 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.

Author reply: We have removed the mention of the 82,000 vents in Sect. 3.4.

- In section 3.4.2, the phrase "which integrates up to be 1" should probably be reworded to something like "which sums to 1."

Author reply: The integral of the PDF over the entire domain is 1. The pdf does not sum to 1. We decided to delete the second part of the sentence as it does not add anything but confusion.

- In section 3.4.3, right after equation 2, “extents” should be changed to “extends.”

Author reply: We have corrected this.

- 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.

Author reply: We now mention in Sect. 4.1 that we assume the flow thickened after it stopped advancing, as this is what we observe from the active lava flow in January (Appendix C).

- 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”.

Author reply: We firstly compare the point cloud to the pre-existing DEM. Then we compute a 5 m grid from the point cloud and compare this to the pre-existing DEM, which gives a slightly smaller RMSE. We have reworded the sentence to make this clear.

- 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?

Author reply: The area is indeed 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. We have also restructured the paragraph to first talk about the thickness (including the maximum thickness) and then about the lava flow volume.

- 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.

Author reply: We have deleted the sentence "We find intriguing similarities between the simulation and the lava flow thickness in several areas, including the initial flow (through point #1 in Fig. 8a), as well as the NW (#2), W (#3), and S (#4) lava lobes" from Sect. 4.3. We now only mention this correlation once in the discussion (Sect. 5.3).

- 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.

Author reply: We have added the sentence "Our hazard maps show the probability of lava flow invasion, i.e. the likelihood that a future lava flow will inundate a specific pixel." to the beginning of Sect. 4.4.1.

- 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.

Author reply: We have reworded this. We consider a lava flow hazard of 10 % rather high (referring to the "Fogo scenario").

- 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.

Author reply: We will add the references mentioned here.

- 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.

Author reply: Ferrucci et al., (2015) and Cappello et al., (2016) estimate vesicle-free volumes, while we refer to the bulk volume (estimated using the DEM difference method). We have added this information to the discussion and also refer to the often assumed vesicularity of 25 % for aa flows. However, this doesn't fully explain the different values. Future studies are needed to address this issue.

- 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.

Author reply: We are here talking about "the magma", referring to the (subsurface) dike propagation, rather than the lava flow paths on the surface. We have done some rewording to avoid misunderstandings.

- 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.

Author reply: We have deleted the sentence "We conclude that the next lava flow will very likely change the lava flow hazard within the Cha again."

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2016-81/nhess-2016-81-AC1-supplement.pdf

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