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Discussions

Interactive comment on "Sensibility analysis of VORIS lava-flow simulations: application to Nyamulagira volcano, Democratic Republic of Congo" *by* A. M. Syavulisembo et al.

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The paper illustrates the calibration of the VORIS code (for the simulation of lava flows) to the 2010 eruption of Nyamulagira volcano. The work presented is an interesting premise of what could be done with the software and the data available to the authors. But beyond what is presented a significant amount of work is missing. The current discussion paper is too concise in places, and it is necessary to refer to other works for a proper assessment of what is presented here. Several concerns are listed below, starting from the title onwards.

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1) Title.

The title reads "sensibility analysis", a wording which is of rather limited use in numerical modeling. Probably the authors meant what is usually called "sensitivity analysis" (e.g. Bilotta et al. 2012). But what is presented is not a sensitivity analysis, but a calibration of the code to a single case. A sensitivity analysis is usually performed after that a model has been already validated to a given test case (e.g. Bilotta et al. 2012, Tarquini and Favalli 2013), and the authors are willing to explore how the result changes upon moderate variations of input parameters (which are to be considered because of the intrinsic uncertainty in the system). But this is not the case.

2) Extent of the application.

In the work presented the authors take as reference topography the SRTM1 DEM. This DEM was acquired during a survey carried out on February 2000 (Rabus et al. 2003). According to the very good map published by co-authors of the present work (Smets et al. 2010), there are additional 5-6 lava flows (beyond the one considered) that could be simulated on the SRTM topography, because they formed after the survey. These are the lava flows formed from eruptions in 2000, 2001 (probably two separate flow fields), 2002, 2004 and 2006. In addition, if older flows are actually so thin (consistently 3 m on average according to Smets et al. 2010), it is also possible that some lava flows older than the SRTM survey could be simulated in spite of the presence of the lava deposit (as it has been the case at Mt Cameroon, according to Favalli et al. 2012).

It is also worthy to mention that, according to the cited map of the Nyamulagira volcano, it appears that the 2006 lava flow originated mainly from a vent very close (if not coincident) to the vent of the lava flow formed in 2010. The fact that the 2010 flow field coverage coincides to a large extent to the 2006 one (the cited map of Nyamulagira) appears to confirm that the 2006 lava deposit is thin, and hence it has only a minor effect on the paths of subsequent flows.

Provided the above, the presented preliminary calibration should be followed by the

simulation of other flows, to validate the preliminary calibration or to refine it. I have to say that, without this missing work, what already presented results to be a very light and incomplete work.

3) Geological setting.

In the description of the geology of the volcano (current section 2) a wide spectrum of the past activity is mentioned, with SiO2 percent ranging from 43 to 56. But what is the (possibly more narrow) spectrum of the more recent activity? It is not even mentioned what kind of lava constitutes the 2010 lava flow which is being considered. Without a careful analysis of the spectrum of possible future scenarios any result is of limited significance, because lava flows between 43 and 56 percent emplace according to a very different dynamic, resulting (presumably) in a very different calibration of the VORIS parameters.

4) Code/methodology.

4.1) the authors highlight that lava flows are typically fed from eruptive fissures. There-fore, they should describe how the VORIS code copes with a similar feature (since they use only a single-point vent), or explain that the emission from a fissure is adequately approximated by a single-point vent in the case(s) considered.

4.2) Tuning (or calibration) of the code and the relative presentation.

The existing literature provides useful caveats regarding how to perform a calibration and how to evaluate the result of simulations (e.g. see references listed at the end and references therein). A very general consensus exists on the use of a specific index "i" to evaluate simulation results (e.g. Bilotta et al. 2012):

i = (R intersection S) / (R union S)

where "R" is the coverage of the real flow and "S" is the coverage of the simulation (in case, with an additional square root applied). The index i effectively summarizes in a single value the accuracy of the simulation (increasing from 0 to 1), and is much

clearer than the multiple plots used by the authors (e.g. fig. 3). I encourage the authors in expressing their results by using the above index.

4.3) The authors should also discuss in more detail the point of the runout of lava flows. They show only a best-fit value in the calibration of a single case, but of course lava flow length can vary significantly, and this variation should be tackled as much as possible in the perspective of using the code in a volcano observatory (as specified in the conclusion). The existing map of recent lava flows provides a mean to explore flow length statistics and to check if some correlation holds with other parameters (e.g. vent elevation, see e.g. Favalli et al. 2009, 2012).

5) DEMs.

In the DEMs section the authors simply state that it is necessary to perform a separate calibration for each DEM considered (which is rather obvious). I suggest to strengthen substantially this section or otherwise to substitute it with the above statement, specifying that the work is carried out using the best available DEM.

6) Availability of the code and consequent application (this point is just a comment, not an issue).

The authors highlight the benefit of the free availability of the VORIS code as opposed to other codes which "only exist in the literature". The free release of the code is undoubtedly a commendable option. I appreciate it. Maybe the ideal solution could be the sharing of a code through the establishment of a cooperation with the developers (supported by some project, hopefully), an option which should minimize misuse guaranteeing the accuracy of the critical data produced. I am not sure if the worst scenario is not having a lava flow simulation or instead having a poorly assessed one.

7) Figures.

See previous comment 4.2 for figures 3, 5 and 7. The multiple frames in figures 2, 4 and 6 are a good option. If you label each frame with a given letter, it would be easier to

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refer to a specific frame. To improve the readability please try also to use consistently the same scale factor in the different frames (e.g. fig 6), and use a bigger scale bar (I am not able to read the existing one). I have a further concern with figure 6: could you justify why in the first frame the runout is much shorter than in the second one while in the subsequent frames the runout monotonically decreases?. Consider current figures 8 and 9 according to comment 5. In figure 8, third frame, the number of iterations appear to be 50 times smaller than in previous frames (according to the labels).

8) final remark.

The authors, in their conclusive section, state that the work presented illustrates that the VORIS code "is perfectly adapted for lava flow hazard assessment performed at Goma Volcano Observatory". The existing literature already showed that VORIS is a valid tool for hazard assessment purposes. But the novelty of the present work is only in the application presented (i.e. as a simple calibration to a single lava flow, so far), and this application is too light to demonstrate that the authors are using the code with the necessary care. I encourage the authors in undertaking a deeper and more comprehensive analysis to better exploit both the potential of the VORIS code and their substantial knowledge of the volcano.

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