nature portfolio

Peer Review File

Trigger mechanism and propagation dynamics of pyroclastic density currents in basaltic volcanoes

Corresponding Author: Dr Francesco Zuccarello

This manuscript has been previously reviewed at another Nature Portfolio journal. This document only contains reviewer comments and rebuttal letters for versions considered at Communications Earth & Environment.

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Zuccarello,

We are delighted to say that we are happy, in principle, to publish a suitable version of your revised manuscript titled "Unravelling the trigger mechanism and propagation dynamics of pyroclastic density currents in basaltic volcanoes" in Communications Earth & Environment.

We therefore ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor,
Communications Earth & Environment
Consulting Editor, Communications Sustainability

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Reviewers' comments:
Reviewer #1 (Remarks to the Author):

Overall comments:
We thank the authors for carefully considering all the comments and we have enjoyed reading the revised manuscript. A small number of minor points are listed below.
Authors: We also sincerely thank you for your constructive comments, which have been invaluable in improving the quality of the manuscript.

In line text comments:

Line 98, 100: add UTC
Authors: Done.

Line 104: lava, not the lava
Authors: We eliminated "the".

Line 111: during the later stages of the paroxysm
Authors: Done.

Line 140: for this thinnest deposit.
Authors: Done.

Line 190: missing space after Md
Authors: We added the space.

Line 203 (and throughout): from a communication perspective, lava fountain is advantageous over fire fountain.
Authors: The reviewer is right. We changed fire fountain in lava fountain.

Line 231 & Figure 7: could you please provide the values of Jaccard Index for the three simulations that you present in Figure 7 (also add them to Fig 7)? Since you're using that index to define the best fit, I think it would be beneficial for the readers to see what is the difference between the J values for the three simulations.
Authors: We added the J index for the three simulations showed in the figure. We chose to change the visualization of the same simulations from a 3D perspective to better show the topography influence on the propagation of the granular flow considering the variations of the input parameters.

Line 270: of the rock mechanical properties
Authors: Done.

Line 398: m/s^1?
Authors: We corrected in m/s.

Figures/Tables:

Figure 1: you have missed (d) in the panel labelling. Also please add the detail you provide about the colour scales in the pt by pt to the caption.
Authors: We added the color bar for each thermal image indicating the minimum and the maximum temperature processed by the camera.

Figure 4: it's hard to see the a-d letters, consider adding white background boxes.
Authors: We modified the figure, hope that now it is clearer.

Figure 5: If possible, suggest adding boxes around each panel.
Authors: Done.

Figure 6: Suggest adding a scale bar for the upper 2 panels. The arrows are also hard to see, can you make them thicker/bolder.
Authors: We added the scale bar and made the arrows thicker.

Figure 8: in all other figures you have units in brackets.
Authors: We added the units in the brackets.

Natalia Lipiejko & Thomas Jones.

Reviewer #1 (Remarks on code availability):
No issues with code availability

Reviewer #2 (Remarks to the Author):

Dear colleagues,

After reading the revised version of this paper, I appreciate the efforts made by the authors to address most of the reviewers' concerns. However, I found some of the answers about the issues found in the numerical modeling section a bit vague and superficial, some containing false comments (the Pouliquen-Forterre rheology is not restricted to 'slow currents') or not even addressing the concern raised (e.g. effect of topography not taken into account by the model). I was also surprised to see that another paper entitled 'Dynamics and hazards of pyroclastic avalanches at Etna volcano (Italy)' was published by the same authors a few weeks ago in Annals of Geophysics (doi:10.4401/ag-9158). This already published paper uses the same data as this one (February 10, 2022 PDC deposit at Etna) but provides a much more robust modeling approach involving the use of Monte Carlo simulations with an ensemble of model input parameters, with varying (i) avalanche volumes, (ii) initial positions, (iii) velocity, and (iv) rheological coefficients. This work is basically what should have been included in the second part of this paper to make it innovative and robust enough to be published in Nature Communications. I am now wondering of the usefulness of publishing another paper about the same events but with poorer and weaker modeling results?

Authors: The points raised by Reviewer #2 have been addressed in the manuscript and the cover letter that goes with this submission. In the submitted paper, in fact, we have emphasised the importance of a multidisciplinary approach in analysing the mechanisms that led to the formation and propagation of the PDC, and have made it clear how this work differs from the one that was recently published in Annals of Geophysics. Since the study mainly focused on a process of defining a set of input parameters while explaining the numerical model, the latter actually complements it.

"However, I found some of the answers about the issues found in the numerical modeling section a bit vague and superficial, some containing false comments (the Pouliquen-Forterre rheology is not restricted to 'slow currents') or not even addressing the concern raised (e.g. effect of topography not taken into account by the model)."

Authors: Concerning the limitation to slow currents, we remind that it was first O. Pouliquen in his pioneering work to limit the validity of the proposed local rheology to regimes of intermediate flow velocity (or thickness) and slopes (see Fig. 2 in: O. Pouliquen, "Scaling laws in granular flows down rough inclined planes", Phys. Fluids 11, 542 (1999); doi: 10.1063/1.869928). Indeed, in rapid granular flow regimes the rheology would be dominated by interparticle collisions and saltation. This is the so-called liquid-gas transition in granular flows, in which the mu(I) rheology ceases to be valid (Andreotti, B., Forterre, Y., & Pouliquen, O. (2013). Granular media: between fluid and solid. Cambridge University Press, pag. 246).
We reiterate our opinion that this paper was not aimed at discussing such complex aspects, and that our comment was aimed at strengthening our position that the problem of the rheology of granular avalanches is far from being solved. Calibration of an empirical rheological law is still a valid approach for specific use cases.

Concerning the topography, we have clearly specified in our response (and adding the citation to our previous benchmarks) that the topographic model is considered in our simulations. We do not comment about the possible limitations of assuming the hydrostatic shallow-water approximation (not accounting for vertical components of acceleration), which are addressed in our published works in the references.

Reviewer #3 (Remarks to the Author):

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer #3 (Remarks on code availability): No issues with the code availability.
Authors: We greatly appreciate the time and effort that Reviewer #3 dedicated to reviewing our manuscript.