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Geoscientific Model Development Discussions

Interactive comment on "IMEX_SfloW2D 1.0. A depth-averaged numerical flow model for pyroclastic avalanches" *by* Mattia de' Michieli Vitturi et al.

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This manuscript presents the mathematical basis of a new depth-averaged flow model for geophysical flows as well as some simulations that show the capabilities and the accuracy of the code.

The two main strengths of the new model are: 1) that the equations are written in global Cartesian coordinates, with the z-axis parallel to the gravity. 2) that the source code is available for the community.

The manuscript is clearly written and well organized. To me, from both a modelling and

a volcanological points of view, this is a good article that merits to be published after minor revisions.

I have read the review of RC1 and I agree with nearly all his comments. I won't repeat all the points he has raised and I just focus on the most significant points and additional comments.

1) Avalanches, PDC and the field case used :

I recognize that there is no real consensus in the volcanological community to name the different "currents" that are observed: pyroclastic flows, avalanches, surges, etc. However, I think that definitions of the paper will add to the confusion. PDCs is a general term that was introduced to include all the "currents". It only means that the "currents" move because their densities are higher than their environment, whatever their physics. In this definition, an avalanche is a density current. The authors can use the term "avalanche", but they do not need to redefine (nor to use) the term PDCs.

I also want to point out a related source of potential confusion in the manuscript. During my first reading, even if the authors use the term "avalanche", I thought that the subject was the simulation of "pyroclastic flows". I was thus surprised that the dome collapses were forgotten as a source of the "avalanches" in the abstract. The difference between "pyroclastic flows" and "pyroclastic avalanches" is in the runout: the runout of a "pyroclastic avalanche" can be simulated as a dry granular material. Additional phenomena occur in the "pyroclastic flows" that gives to them the very high fluidity observed in the field. I agree with the authors: the field case used (section 5) is a pyroclastic avalanche.

If the authors want to focus on pyroclastic avalanche only, they need to explain more clearly the difference with the "pyroclastic flows" in the introduction. However, I think that the authors can extend the application domain of their model to pyroclastic flows (in this case, they must include dome collapses and vulcanian explosions in the description of their origins). In both cases, to avoid future misinterpretations of the best values they have obtained at Etna (mu = 0.3-0.4) in the debate of the fluidity of pyroclastic flows, it is important that they recall the “avalanche” nature of the natural phenomenon simulated in section 5 and that they describe at least briefly the topography characteristics: runout (2300 m) but also elevation differences (~1200 m) and mean slope (~25°). Page 16, line 8, for example.

2) Equations:

To me, the equations are clearly presented and seem to be correct. The tests are relevant and convincing. However, the manuscript is more a mathematical paper with strong implications in volcanology rather than a volcanological paper. Because RC1 and I are not mathematicians, the opinion of a specialist of these systems of equations and of their numerical resolution is required.

Three points are not clear for me and must be explicitly explained in the text: - Where the valleys turn rapidly (in an almost horizontal plan), do the system of equations presented take into account the centrifugal force that will increase the apparent gravity and then the retarding stress? - Is the modular structure of the code (Page 1, line 16) compatible with a rheology that does not include a term related to the flow velocity if we want to model a purely frictional or plastic flow, for example? Or do the user need to add an artificial viscosity (or turbulence) and, in this case, how can be determined the lowest viscosity required? - It is possible to use the full resolution of a topography or does the initial interpolation smooth it (lines 9-10, page 6)? The resolution has a strong influence for flow simulations in narrow valleys.

Other minor comments:

- Page 1, line 25: better to use the past: ‘the French avaler, which meant “move down the valley” ‘. Today it has been replaced by “dévaler” and the meaning of “avaler” has evolved and it is used for “swallow” (move down but to the stomach).

- Page 2, line 17: category

- Page 3, line 7: conservative

- Page 3, line 14: perhaps you could explain the origin of the name of your code.

- Page 6, line 12-14: I do not understand this sentence. Could you explain it a little further?

- Page 13, line 16: the friction coefficient is not related to a Voellmy-Salm rheology. For clarity, please indicate that you are using another rheology, that of Kurganov and Petrova (2017). A similar indication – that the values come from already published examples - can introduce the “strange” values used in the equations of line 9 (why 2.2222, 0.8246, -1.6359, etc.?)

- A table of all the variables used and their meaning would be useful.

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