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Interactive comment on "A discontinuous Galerkin finite element model for fast channelized lava flows v1.0" *by* Colton J. Conroy and Einat Lev

Anonymous Referee #2

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In this study, the authors present a numerical model based on discontinuous Galerkin finite element method and apply it to channels with complex geometry: the space discretization is obtained by an unstructured triangular mesh, the time discretization by the Runge-Kutta method, the numerical tests via the manufactured solutions method. Moreover, the authors model high-speed lava flow and overcome the modeling of turbulence by a two-layer model of vorticity and introducing a stress term at the boundary of the domain. The authors present an application to the Kilauea 2018 eruption and retrieve flow velocity and rheology. The manuscript is a model description paper, but the description of mathematical formalism needs corrections. The manuscript is not well-organized in sections and some changes are needed before publication. (1) In the title: what v1.0 refer to? I didn't find any explanation for this alphanumeric symbol.

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Discussion paper

Is this the first version of their software? (2)I found some problems with references. At line 21 authors refer to Neal et al., (2019) to find the value of lava speed of 11 m/s but I didn't find this number in the cited work. They also refer to Re>3000, where they found this information is not explained. (3)In the introduction section (lines 59-75) the method is exposed in detail and this part should be moved in the method section. The whole introduction is short, and references are not enough to introduce their method. The method is novel, it is a combination of many different techniques and so, each of them should be introduced with accurate references in the introduction section, explaining the need to use the chosen methodology. Methodologies that are commonly used for hurricane storms are a novelty for lava flows and should be described better. (4) In the mathematical model the authors do not justify the choice of neglecting viscous dissipation. The effect of viscous dissipation is discussed in other numerical studies (Costa and Macedonio, 2003; Cordonnier et al., 2012) and the effect is to dramatically change the velocity of lava flow at the boundaries, introducing local vorticities and increasing the Reynold's number. Please discuss these limitations. (5) Lines 87-99: this part should be extended because it is unreadable as it is. Formulae need space and explanations. If they are not necessary, remove them. (6) Figure 2 doesn't contain all the geometrical and physical parameters used in the mathematical model. Add all the parameters (v, H, x, y....) in Figure 2. (7) Lines 102-104: in this section the upper and bottom surface boundary condition should be introduced here and not divided in different sub-sections. If authors prefer a different sub-section, they can name it "boundary conditions" instead of "stress term" describing the stress term at the bottom and the moving surface at the top. (8) Fluid viscosity function is divided in two sections and should not be. The choice of the rheology model of Giordano et al (2008) should be motivated. Authors claim in their abstract the they will use a non-linear viscosity function but, in the text, they show a temperature dependent viscosity function. The used viscosity model can be considered non-linear respect to the energy equation and not to the dynamic equations. In this work, non-linearity is obtained by a varying exponent (n-1). How the power-law exponent is chosen in the final model?

There are not many works on power-law rheology experiments that model both the exponent n and the consistency k of the fluid, and their variation with temperature and composition. Starting from the pioneering work of Sonder et al., (2006), Hobiger et al., (2011) developed an interesting model for basaltic lava used for both analytical and numerical modelling of lava flows (Tallarico et al, 2011; Filippucci et al., 2017). It would be more correct if, in the presentation of their solving method, the authors described a temperature-dependent viscosity with the possibility of introducing a non-linearity in the equations by appropriately choosing a constant value of the exponent of the power law. The authors should discuss this limitation in the discussion section and in the description of the viscosity funtion. (9) Eq (4) what is C'? is it a refuse? (10) Lines 177-179: please rewrite the sentence. (11) Given the huge number of different parameters used in their sections, I think that a table of acronyms and parameters would be helpful for reviewers and for readers. For example: QR and Qc in (20) are not defined before but I can imagine that are R stands for radiative and C for conductive heat, is it right? Please, use a unique name for functions. Another example in lines: 220-225: what j rapresents? and the pedix e? Formulae are confused and it seems that they came from a patchwork of other works. The mathematical formalism does not seem to have been written specifically for this article. (12) Model results are very interesting, but the problem of the chosen rheology is evident in lines from 410, where authors describe limitations that I appreciated. (13) It would be interesting if in the numerical verification section, authors add the time costs of each computation also as order of magnitude (minutes, hours, days..). The same information is useful also in the results section for explain the choice of the mesh dimension. Is the non-linear problem more expensive than the newtonian one?

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