The paper presents a numerical model that solves the thermodynamical problem of lava flowing in channels of complex geometry. The paper presents the differential equations of the problem, the time and space discretization, the numerical tests, the application to a real case study and release the Matlab code and related files. The paper is well-structured but the exposition is not rigorous and I found the reading not fluid and suitable only for very specialized readers.
Moreover in the text you come across changes in the names of the functions/parameters used, and often authors use the same letter to indicate different parameters (h is for mesh and height, p is for polynomial degree and pressure..). This makes reading the article very annoying.
Nevertheless, I suggest the publication of the paper after minor revision. In the following, my comments.
It is not clear whether the proposed method is an adaptation of other methods developed in the fields of atmospheric fluid dynamics or is an original method developed "ad hoc" on the problem of lava flows. The works of Kubakto et al., (2006; 2014; 2015) are repeatedly cited and the origin of continuous and discrete equations is not well distinguishable.
I ask the authors to make an effort to clarify this distinction also by inserting sentences and more references, in the text to guide the reader, i.e. explanations and references before the demonstrative sections. About that, I hope I don't have misinterpreted it, but it seems to me that the layout of the work is inspired by oceanographic studies. In this sense, the modeling of the variations in height of the free surface applied to lava flow is very interesting.
Obviously, what distinguishes an oceanographic fluid dynamics problem from a volcanological one is the complexity lava rheology and its dependence by temperature, bubble, crystal content and composition.
Lava rheology is generally considered non-newtonian and in particular shear thinning or pseudoplastic and the authors include in eq. (3) a non-linear rheology in their modeling. The dependence of viscosity on velocity gradients is expressed by means of the fluid consistency coefficient, modeled following Giordano et al., (2008); the power-law index, on the other hand, does not have an analytical expression. In the application to a real case, the power-law index turns out to be an accommodation parameter whose variations significantly influence the final model. Therefore, the use of a VFT model for consistency with a constant and arbitrary varying n-index should be justified in some way, maybe adding some references. Laboratory experiments showed that n-index is not constant but can vary with temperature too (e.g. Sonder et al., 2006) and authors should add comments on this aspect in section 2.1.
The model verification is achieved by using manufactured solution method but from the tables (from 1 to 3) it is not clear how P0, P1, P2 are defined (also in this case P0 sometimes is expressed with an index some others with a pedis). Also in this case author should justify the choice of exponential functions for their tests or add references.
The results applied to a real case should show also the element size of the chosen mesh to give the idea of the errors in the final model. Then, the authors should explain if the errors in the final model at the boundaries and at the base of the domain are almost an order less than the errors introduced by the variations of the DEM, in order not to risk making the use of the complex geometry of the channel useless. This aspect should be discussed in the text.
Finally, I suggest the authors a re-reading of their manuscript with the aim of clarifying these aspects, adding more references, adjust the nomenclature of functions/parameters and simplify the exposition.