Referee #2 (Domenico Doronzo)

The submitted manuscript is an excellent contribution toward making advances in numerical volcanology, accounting for necessary inputs from the field, as done in the present worked example; it definitely deserves publication, in my opinion after minor revision. Three points could be further considered in the revised version of the manuscript, which are the following: 1) It could be clearer why the well-known box model approach has been adopted, considering recent advances in 3D numerical volcanology, even experimentally-validated or field-constrained? (see Sulpizio et al.,2014_JVGR for a review) It is stated in some parts, and I agree with Reviewer 1 (https://doi.org/10.5194/se-2020-138-RC1) that such choice could also be introduced from the very beginning

Thanks a lot for these words and for the comments to our manuscript. We agree with your suggestion, and we think that, after implementing the suggestions from Referee #1, the discussion in the introduction section (LINES 40-50) describes in detail why we have performed our study using the box-model approach. We have also added the reference from Sulpizio et al. 2014 (LINE 32).

2) The box model for sedimentation in entraining density currents of Andrews and Manga (2012)_JVGR could also be considered in the discussion

We agree with this suggestion, and we have added a sentence in the discussion section (LINES 406-408).

3) Agreed with the definition of inertial currents in the present worked example, as anew unconventional terminology appeared eight years ago in volcanology (see Giordano and Doronzo, 2017_Sci. Rep., and the introduction of de' Michieli Vitturi et al.,2019_Geosci. Model Dev. for a review); the original referenced terminology could be considered

Thank you for this suggestion, we have added the references from Giordano and Doronzo 2017 and de'Michieli Vitturi et al. 2019 and we have also added an explicatory sentence (LINES 55-60).