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Interactive comment on "Modelling stress field conditions of the Colima Volcanic Complex (Mexico) integrating FEM simulations and geological data" *by* Silvia Massaro et al.

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The manuscript entitled "Modelling stress field conditions of the Colima Volcanic Complex (Mexico)" by Massaro et al. investigates the added-value of integrating geological information in numerical models using the example of Colima Volcano, Mexico. The topic is certainly of high-interest but, in my opinion, the study suffers at this stage of several issues that should be addressed before considering publication. I will detail my main concerns below.

1. Main concerns:

* I enjoyed reading the introduction because the topic addressed is very important and this point is pretty well explained. But then I was somehow disappointed by the study itself. It is intended to evaluate the influence of geological data integrated into the modeling but only the stratigraphy and geometry of the plumbing system (see section 4) is considered. There is no real novelty in considering these aspects, which has been done in several studies (e.g. Cianetti et al, GJInt, 2012). I was expecting the authors to also take into consideration the existing faults after the long description of the structural/tectonic context of Colima volcano (fig1, section 2.1). In particular, the profile chosen for modeling cuts 2 faults of the Colima Rift, which are not considered in the models. Only a uniform extension applied at the lateral boundary is considered in some models. However, there are ways to consider a fault plane in 2D using a friction law (see Chaput et al, GRL 2014).

* Model assumptions are not clearly described.

-It is in 2D but it is not explained whether a plane strain or a plane stress approximation is considered, which is a key information. Usually models are performed in plane strain, which means that there is a stress component out of plane. . ..

-The way the gravitational loading is applied remains unclear. When applying body forces lithostatic stress field should also be applied but when a topography is considered, some iterations are required to find the initial state of stress consistent with both the topography, the rheology and the body forces as described in Chaput et al, GRL, 2014 or Cianetti et al, GJInt, 2012. Also with a lithostatic stress field, the load applied at the reservoir boundaries has to be a superposition of the overpressure and the lithostatic component. It is not explained in the manuscript. Also if a lithostatic stress field is applied both the minimum and maximum stress field should incease with depth. From the figures shown in the result section it is not the case for the minimum stress sigma3 and I don't really understand why.

* Illustrations should be improved to help the understanding. In some cases, the di-

mensions of the numerical box represented are not clearly reported, titles are unclear. I will detail later on each figure.

* Regarding the results and discussion of the Young modulus influence on the stress field, what matters are the ratios of the Young modulus considered in various layers and not the absolute value of the Young modulus in one given layer (I mean that if the Young modulus is mutliplied by 10 in each layer, no changes are expected except in the vicinity of the domain external boundaries). This fact is not clearly shown. Also I would recommend to cite the paper by Heap et al. published recently in the Journal of Volcanology and Geothermal Research (https://doi.org/10.1016/j.jvolgeores.2019.106684)

2. Minor points:

*Introduction: line 37, 41, before the chosen references list for numerical models I would put "e.g." because there are plenty of references that could be equally fairly cited here. Also I would add the reference to Cayol & Cornet, GRL, which is really a classical one. Line 57, I would suggest to also cite Albino et al., Geophysical journal international, 2010.

*Section 3.3 Line 213: it would be very helpful to show the mesh used. line 217, the boundary condition applied on the reservoir and dike walls should be explained. Line 229, the way gravity is expected to influence the failure condition is really depend on the rupture criterion considered (see for instance Albino, et al. JGR, 2018) Line 231, in Corbi et al, 2015, the trajectory of magma propagation is not influenced by gravity but by the deviatoric stress field induced by caldera unloading.

*section 4.2: Line 296 “During ascent to the surface, the dykes align themselves with the most energy-efficient orientation, which is roughly perpendicular to the least compressive principal stress axis $\sigma 3$ (e.g. Gonnermann and Taisne, 2015; Rivalta et al., 2019)." this is true providing the magma driving pressure remains small compared to the deviatoric stress (see Pinel et al, JGR, 2017 and Maccaferri et al. G3, 2019)

Section 5.1 Line 345, it would be important to show on a figure the reduced simulation domain selected for the sensibility analysis. Also for each unit, we would need to know the number of nodes considered (size of the vector space X). It is important because the larger variability of Unit B could be only due to the larger domain considered.

Figure 2 Figures labels and title should be improved, information of the number of nodes considered should be added.

Figure 3 : Figures labels and title should be improved, information of the number of node considered should be added. Limits of the different units should be shown. For panel A, it remains unclear to me which stress perturbation is considered as there is no reservoir.

Figure 4 : The topography doesn’t look the same on each panel, which makes comparison difficult. No indication is provided on the orientation of the maximum and minimum compressive stress. I don’t understand the term "distensive". Once again I don’t understand why sigma3 does not increase with depth.

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