"GENERAL COMMENTS"

The paper addresses the problem of modelling the pore fluid pressure change within a confined horizontally layered aquifer embedded in a layered elastic medium. An axisymmetric source is assumed to inflate suddenly at depth and the deformation of the surrounding medium triggers pore pressure changes, which evolve with time according to a diffusion equation, coupled with the equilibrium equation. The finite element method is employed to solve the system of linear coupled equations, in order to show specifically the importance of elastic layering, including possibly one instance of lateral heterogeneity (centrally interrupted aquifer).

The paper solves several problems of coupled poro-elastic equilibrium and shows very clearly several interesting features of the solution (some of them could be hardly anticipated on intuitive grounds). A detailed parametric study is performed to show the sensitivity of the solutions to the different non-dimensional parameters characterizing the model. In particular I was impressed by the longer evolution of the pore pressure change in the more permeable pyroclastic aquifer: the explanation in terms of the poro-elastic coupling of the fluid with the highly compressible the solid matrix is brilliant!

"SPECIFIC COMMENTS"

First main concern. The magmatic inflation is assumed instantaneous, so that the solutions for the piezometric head apply only to inflation episodes lasting less than a few days (for the lava-flow-aquifer) or a few months (for the pyroclastic aquifer). Longer lasting inflation episodes would probably yield negligible pore pressure changes: this drawback of the model is mentioned only at the very end of the paper (item 4 at page 1700).

Second main concern. I understand that the aquifer layer is bounded by impervious upper and bottom layers (line 8 at page 1680). While this is physically implausible, some solutions (e.g. figure 3a, 12a) show vertical flow components that seem to contradict this assumption … maybe I did not understand correctly, but I cannot find any permeability associated with the cap rock or the host rock in table 2.

Third main concern. The possibility that an input of fluids and/or an input of heat takes place into the aquifer during the inflation is ignored. This could trigger pore pressure changes that may easily overcome the modeled changes passively induced by volumetric strain. Similar effects are e.g. considered (even if 1 way coupling is assumed) in Todesco et al. (2010)

Suggestion 1. A Reference paper should be mentioned as the first comprehensive model of coupled poro-elasticity: this is Rice, J. R., and M. P. Cleary (1976). This paper shows that the undrained response of a poro-elastic medium is most simply obtained from the purely elastic solution with undrained Poisson ratio (depending on the Skempton parameter) while the long-term drained solution is obtained from the purely elastic solution with drained Poisson ratio. It would be interesting to compare the initial and long-term solutions of the present paper with the mentioned elastic equivalent model, providing the value inferred for the Skempton parameter and the undrained Poisson ratio. These comments also seem to contradict the statement (somewhere in the text) that the solutions do not depend significantly from the Poisson ratio.

Suggestion 2. Considering the text in the bottom lines of page 1690 and figure 10, I would expect greater expansion above prolate ellipsoids. In my opinion, the distance from the center of the chamber is more relevant since z_T is fixed (3km) and z_T+b increases with b (from prolate to oblate ellipsoids).

Suggestion 3. The statement at page 1696 considering the acquisition of permeability data via pumping tests may be criticized because it is the effective global permeability of the aquifer that matters, not its local value that may be easily different by orders of magnitude. As a consequence, I think that modelling the pore pressure changes is a method to gain knowledge of this "effective permeability" rather than a convenient " volumetric strain meter" as claimed, since volumetric strain is best computed from surface deformation (obtained from SAR interferometry) provided that the elastic structure inferred from seismic tomography is taken into account: due to the short periods of seismic waves, these elastic parameters may be considered as undrained parameters. As shown by figure 3 in Trasatti et al. (2015), the shape of the source can be conveniently inferred from geodetic data.

"TECHNICAL CORRECTIONS"

- Equation (3) includes the nonlinear term in the strain tensor: finite deformation is considered? this would make the problem non-linear!
- equation (7) provides the relative volume change of the material already resident within the source, while the volume expansion of the source and the total volume available to host additional magma are different (e.g Bonafede, M. and C. Ferrari, 2009)
- The non-dimensional parameters are introduced most easily (and elegantly) as the ratios of different terms in eq. (4): for instance, I suggest defining *Q* in eq. (9) as the ratio between the 4-th and the 1-st term in eq. (4). Furthermore, *F* in equation (10) is the ratio of the 2-nd to the 4-th term in eq. (4) if the time scale $\tau = \mu/E$ is introduced.
- Table 1: *g* should be described as gravity (acceleration of), not as the gravitational constant!
- The first line of table 5 shows a reference value out of the considered range! to be corrected.
- Figure 11: I understand these plots by saying that outflow is insensitive to *L* while inflow is hindered.

References

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Todesco, M., Rinaldi, A.P., Bonafede,M., 2010.Modeling of unrest signals in heterogeneous hydrothermal systems. J. Geophys. Res. 115, B09213. doi:10.1029/2010JB007474.

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