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Interactive Comment

Interactive comment on "Poroelastic responses of confined aquifers to subsurface strain changes and their use for volcano monitoring" *by* K. Strehlow et al.

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This study focuses on level changes in well water that arise from crustal deformation in volcanic areas. In particular, the authors study the response of a confined aquifer to the volumetric strain due to a pressurised magma reservoir. They present the results of a detailed parametric study that explores the sensitivity of hydraulic head changes to various features, including hydraulic and elastic properties of the rocks, the magma chamber volume, shape and position with respect to the aquifer. The study is carried out with the COMSOL commercial software, that couples the elastic deformation of the porous rock and the Darcy's flow of a single-phase pore fluid. The results show

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that, given the appropriate system properties, significant water level changes may arise from volumetric strain. This result is interesting and provide useful insights for the interpretation of monitoring data, although I am somewhat more skeptical than the authors about the direct applicability of well water as strainmeters. The manuscript is well written and mostly clear. It could be shortened for an easier reading.

Being this a parametric study, the major limit I find is the fact that the range of variation explored for each given parameter is not clearly stated, but only expressed in terms of non-dimensional parameters. I would appreciate some more discussion on the choice of parameter values for the reference case and the explored range of variation. As the authors themselves state, not all the combinations of parameters are realistic. While the limits and assumptions of the model are discussed, the range of parameter values explored is not. This makes it difficult to assess the applicability of these results to real cases.

I list below a few suggestions, and some issues that need further discussion or clarification.

Abstract

The Usu volcano case is introduced here, mentioned in the Introduction and further in Section 4.4. I do agree that the results of the present study have implications for the Usu case. However, this case history is not really discussed here: the data are not shown, and no simulation directly applies to it. As such, I do not think it is appropriate to mention it in the abstract. I would leave it in the introduction as an example, and I comment on it again in Section 4.3, discussing the implication for volcano monitoring. Section 4.4 is not really necessary.

Introduction Even though your objective here is to address the hydro-mechanical coupling, you should mention here that deformation may not be the only cause of water level fluctuations, and discuss other common causes. You do that later on, but it is important that you define this aspect clearly as you introduce the topic. The real problem in the interpretation of signals is when different processes cause similar variations of a given observable. Before proposing well waters as “cheap strainmeters”, you should show that strain-induced changes are the dominant signal the wells would record. Or at least, demonstrate that you can distinguish the strain-induced signals based on some particular feature (perhaps a characteristic time scale?). Otherwise, it won’t be possible to draw conclusion on the source of deformation, based on hydraulic head changes only.

Page 1676 line 10 It would be interesting to know if and how much ground deformation was recorded at Mt Usu before the eruption. line 20 Chiodini et al., 2012 did not simulate rock deformation. You may quote Todesco et al., 2004; Hurwitz et al, 2007; Rinaldi et al., 2010. Also, the two-way coupling was never attempted only in volcanological applications, as early work on two-ways thermo-hydro-mechanical coupling by J. Rutqvist and colleagues exists and dates back to 2002.

Methods If I understand, the coupling between fluid flow and rock deformation does not involve changes in permeability, that may accompany porosity changes. If this is correct, it would be worth mentioning it in the text. Page 1678, line 10. Neglecting gravity means that you do not account for the effects of ground uplift and subsidence in contributing to flow magnitude and direction. If this is the case, it should be mentioned in the text. You mention that your approach is only feasible for small strain. It would be interesting to know when the effect of ground elevation change overcomes the effect of strain in terms of fluid propagation. Did you check on that?

Page 1679, line 5 Validating numerical codes is a good practice. However, I presume that the model performance is carefully assessed by COMSOL before releasing the software. If you used this test to define your grid resolution for your simulations, you should mention this in the text (providing information on mesh geometry and discretisation). Otherwise, I find Appendix A unnecessary.

Model set up Page 1680 Line 10. The aquifer you simulate is confined above and

SED

7, C854–C860, 2015

below, and has impermeable boundaries. Under these conditions, the only possible fluid flow is the one required to re-distribute the fluid in the new voids arrangement caused by the deformation. The simulated fluid propagation is driven by strain, but must comply with mass conservation. In this case, the amount of fluid that can flow to re-equilibrate the pressure is fixed. I wonder how your results would change with an open lateral boundary (very far away and at fixed hydrostatic pressure), that would allow fluid to leave or enter the domain according to the evolving pressure gradients. I expect some effect could be emphasised while others could be smoothed. Can you comment on this?

Line 20. Is granitic rock appropriate as a host rock for a shallow magma chamber (3 km depth)? When you mention a vesicular lava, I tend to expect a high porosity (and associated permeability), but obviously this is not what you mean. I imagine you introduced the term vesicular to mean a lava that is not compact and can host an aquifer, but I find the adjective misleading in this context.

Parametric study and sensitivity analysis In your parametric study, each parameter is expressed as a function of its reference value. This is an effective way to show how a given parameter influences the observed response. However, in this way it is very difficult to appreciate the range of variation that you explored for each single parameter, and how realistic are the set of parameters that cause the greater changes. You should provide the range of variation for each parameter.

Page 1682, Line 10 and 15. I find this paragraph very confusing and not really useful in this location. I suggest moving it to Section 3.2, where figure 4 makes it more understandable.

Reference simulation Please, specify the simulation time for Figure 2 (10 days?). Page 1686, line 5. The lava aquifer (with lower permeability) has a faster flow. You comment on this later on, in the discussion section, but I would highlight here that this implies pressure gradients high enough to overcome the difference in permeability. This means

SED

7, C854–C860, 2015

that the entire process is governed by the elastic properties of the aquifer rather than by its hydraulic properties. This is an interesting result.

Influence of material properties Page 1687, Line 15. It could be useful to see the Q value of the reference simulation highlighted in the plot (since you mention it).

Page 1688, Line 10. This part is very interesting and would deserve more discussion. It reflects non trivial effects associated with fluid compressibility and how it interacts with rock stiffness. I'm not sure I fully understand the different behaviour of steam and water in different aquifers.

Page 1688, Line 20, make sure that Figure 8a is cited before Figure 8b.

Page 1689, Line 5. It seems to me that the sign-flip effect of ERc is important only for the lowest ERh values, while in all the other cases, ERc values do not affect the response (Figure 7). Which is the corresponding geological setting? For which set of host rock, cap rock and aquifer rock should we expect a flipped response?

I can accept the definition "sign-flipped signal" but "sign-flipped aquifer" does not really sound right: aquifers have no sign.

Model limitations I would mention meteoric recharge among the features that may cause fast and significant changes in hydraulic head. Also, you do not discuss the effect related to significant ground deformation. I understand that your model application is limited to cases of moderate strain, but in real volcanoes ground level changes may severely affect the hydrology. Also I would mention the fact that when heating is important, rock mechanical behaviour may deviate from purely elastic.

Discussion Page 1695, Line 5. I'm not sure I follow here: liquid water viscosity is higher than steam viscosity, which means that that the liquid should be slower. I presume that the faster re-equilibration observed for water is due to the fact that the water aquifer undergoes minor changes only. Given that, as you say, the effects of density, viscosity and compressibility are combined, I'm not sure how can you discriminate among them.

In any case, compressibility undergoes the greatest changes, I expect it to be the principal cause for the different behaviour of water and steam.

Implication for volcano monitoring I agree that monitoring water wells would provide important insights on the evolution of the volcano. They certainly reflect fluid flow pattern associated with head changes but regardless to the originating process (strain, meteoric recharge, magmatic volatiles, ground deformation,. . .). This section is rather long and could be shortened.

The Usu 2000 water level changes revisited As I mentioned above, either you run a simulation describing the two wells showing contrasting behaviour, or you may condense this paragraph in a sentence stating that your results confirm that simple models cannot describe complex processes.

Conclusions

I do not agree that your model show that volumetric strain can be inferred from hydraulic head changes. Your model show that volumetric strain may cause hydraulic head changes. But, in logical terms, this does not imply that hydraulic head changes are due to volumetric strain.

Tables Table 1. Please, keep greek and latin letters separated and in alphabetic order. Possibly, also group vector quantities, scalars, and constants. Please double-check the list for errors: I've found the acceleration of gravity g [ms-2] defined as gravitational constant [N m2 kg-2] and S (storage coefficient or specific storage?) with [Pa-1]. Make sure that names in the list are consistent with names used in the text. Make sure all cited symbols are listed (some seem missing: I, Zch,. . .).

Figures Figure 2. Add the simulation time and specify which variable is shown with dashed line and which with solid line. Figure 4. Specify the meaning of the symbols in the caption. Correct a typo in the lower legend (Piorities instead of Priorities). Figure 5. Change “Influence of the non dimensional flow and coupling parameters Q and F” with

SED
7, C854–C860, 2015

"Influence of the non dimensional flow F and coupling parameters Q" Figure 8. This caption is very difficult to understand. Try to rephrase and add a definition of what a 'strain-flipped simulation' is.

Cited references Hurwitz, S., L. Christiansen, and P. A. Hsieh (2007), Hydrothermal fluid flow and deformation in large calderas: Inferences from numerical simulations, J. Geophys. Res., 112, B02206, doi:10.1029/2006JB004689. Rinaldi, A., M. Todesco, and M. Bonafede (2010), Hydrothermal instability and ground displacement at the Campi Flegrei caldera, Phys. Earth Planet. Inter., 178, 155–161, doi:10.1016/j.pepi.2009.09.005. Rutqvist, J., Wu, Y.-S., Tsang, C.-F., Bodvarsson, G., 2002. A modelling approach for analysis of coupled multiphase fluid flow, heat transfer, and deformation in fractured porous rock. Int. J. Rock Mech. 39, 429–442. Todesco, M., J. Rutqvist, G. Chiodini, K. Pruess, and C. M. Oldenburg (2004), Modeling of recent volcanic episodes at Phlegrean Fields (Italy): Geochemical variations and ground deformation, Geothermics, 33, 531–547, doi:10.1016/j.geothermics.2003.08.014.

Interactive comment on Solid Earth Discuss., 7, 1673, 2015.