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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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The authors would like to express their thanks for the very useful and constructive review provided by Micol Todesco. The manuscript will only be revised and uploaded at a later stage, after all referees' comments have been received. However, we would like to respond to the individual comments beforehand. In the following, the respective comment will be cited in italic, followed by our reply.

"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."

We will add a statement to the main text to clarify that Table 2 states the range of parameters used in the dimensional parametric sweeps, which were also used to calculate the ranges used in the nondimensional studies. As stated in the manuscript, these are based on parameter ranges found in different (cited) literature sources and medians of these ranges were taken as reference values. We attempted to cover a wide range of parameters to account for their natural variation.
Furthermore, we agree that there is a need to expand on the statement on unrealistic combinations. We assume the referee is referring to page 1685, line 5 here, where we state that "The parameter space (Table 5) was derived from ranges in dimensional sweeps and sometimes adapted as not all combinations of nondimensional parameters are physically reasonable." While all dimensional parameter ranges explored are realistic values, their grouping to nondimensional numbers can result in unphysical values. As an example: When we entered nondimensional numbers in the model, the Young's Modulus of the aquifer is calculated on the basis of the Poisson's ratio, the Biot Willis coefficient, the porosity, the fluid compressibility, and the nondimensional parameter Q. For a small Poisson's ratio (i.e., 0.15) and reference values of the other parameters, this results in a negative Young's Modulus. Therefore the minimum for tested Poisson's ratios in the nondimensional sweeps was fixed to 0.275. We can add an appendix where all adapted ranges for nondimensional sweeps are explained. In fact, only the ranges for the Poisson's ratio, ERh and Q - and only in the pyroclastic aquifer case - needed to be adapted. All other nondimensional parameter ranges reflect the whole dimensional range as shown in table 5.

"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."

We agree with this comment and will shorten the sections on the Usu case, and remove mention of it in the abstract, as suggested.

"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."

We agree that other causes for hydraulic head changes - meteorological influences, fracture opening, hydrothermal fluid injection - need to be mentioned earlier; and also that our statement that hydraulic head can be directly inferred from well level changes was somewhat too optimistic. We believe however that, under certain circumstances, the idea of using wells as strainmeters still holds, as we should be able to distinguish different causes for well level changes:
First, the general hydrological behaviour - i.e., the meteorological responses - should be tracked and therefore be reasonably well known if wells are to be included in a monitoring system. If this is given, a water level response to strain will be a transient signal

on top of the background behaviour.
A strain signal can be distinguished from flow processes by time scales - a response to strain is instantaneous, flow processes are slower.
A hydrothermal fluid injection should lead to additional and relatively fast temperature effects compared to heat transferred from an inflating chamber or a dyke intruding through the crust, which would reach the aquifer much later.
Additionally, wells should clearly only form a component of a monitoring system and work in conjunction with seismometers and GPS stations etc. Shallow magma movements can then be identified on the basis of a combination of seismic, deformation and well level signals. Ground deformation will also be of assistance in identifying hydrothermal injections: In the case of an injection, pore pressure will rise and the ground will be uplifted (e.g. Fournier and Chardot, 2012), while strain due to chamber inflation leads to a water level fall together with ground uplift (unless of course the sign of the strain signal is flipped - this is a more complicated case).
All these considerations will be (expanded and) included in the manuscript.

"Page 1676 line 10 It would be interesting to know if and how much ground deformation was recorded at Mt Usu before the eruption."

Yes, there was a radial ground deformation of about 2cm recorded at about 8km SSE from the summit (between the 15th of March and 1st of April; the eruption was on the 31st of March). This will be included in the manuscript.

"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."

This is true; Chiodini et al. was included as an example as they do simulate hydrothermal flow due to an injection and relate it to recorded ground deformation. But we will add the suggested references as well.

"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 col-

SED
7, C890–C903, 2015

leagues exists and dates back to 2002."

This is true; we will include the references and rephrase the relevant text.

"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."

We agree this needs to be mentioned in the text. In fact, all fluid mechanical properties remain unchanged, including permeability and porosity; the poroelastic coupling is achieved by the pore pressure and stress effects, as well as the inclusion of elastic properties of the pore fluid in the storage coefficient.

"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?"

First, we'd like to note that the limitation of sufficiently small strain stems from the necessity of the material to behave linear elastically as opposed to nonlinear elastically or brittly. So this limitation is not related to the gravity-issue. However, we do indeed not account for the effect of ground deformation on flow patterns. A first order consideration shows:
When not neglecting gravity, hydraulic head is defined as $h = z + p\rho g$. This means that the ground uplift leads to a change in hydraulic head of $\Delta h = \Delta z + \frac{\Delta p}{\rho g}$ (with Δp being due to the poroelastic response of the porous medium), so $\Delta h = \Delta z + \Delta h_{nograv}$, with Δh_{nograv} being the hydraulic head change from the simulations without gravity.
The maximum (central) displacement in the presented reference models is about 4.5cm for the pyroclastic aquifer case and 4cm for the lava flow aquifer. Therefore: $\Delta h = 4.5cm + (-1.5cm) = 3cm$ in the pyroclastic and $\Delta h = 4cm + (-6.2m) = -5.8m$

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Interactive Discussion
Discussion Paper

in the lava flow aquifer.
This means that in the pyroclastic aquifer, the flow induced by the ground deformation will indeed dominate over the strain-induced flow and hence reverse the flow pattern. In the lava flow aquifer, strain induced pressure changes and flow are dominant.
Therefore, the observed time-dependent behaviour of an aquifer with a similar strain sensitivity as our presented pyroclastic aquifer would be quite different from our presented results. As the well will move with the ground, the well water level will still initially fall. But the flow is different when gravity is taken into account and hence water levels will continue to fall (while in our models, they rise).
All presented results concerning the influence of individual parameters on the initial hydraulic head response still hold. However, results on the resulting flow pattern in the pyroclastic aquifer need to be seen under this caveat. We will make this clear in the manuscript. Unfortunately, in the current version of the software, it is not possible to take gravity into account in the simulations.

"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."

While the benchmarking models confirmed that refinement of the mesh around pressurised boundary is necessary, this was already known (Hickey and Gottsmann, 2014) so the appendix can be cut to shorten the manuscript. However, even though COMSOL is commercial software, we prefer to check a benchmarking model - so we would like to see the opinion of the other referees before adapting the manuscript accordingly.

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

We ran the two reference simulations with the suggested changed boundary condition. This required increasing the size of the model domain in the case of the lava flow aquifer, as the imposed hydraulic head differs too much from the simulated hydraulic head change due to the chamber inflation. However, after adjusting this, we found that the difference between these results and those with a closed boundary are negligible - which is what we expected due to the size of the domain. Corresponding figures showing this result are attached.

"Line 20. Is granitic rock appropriate as a host rock for a shallow magma chamber (3 km depth)?"

We do indeed ignore the depth-dependent and temperature-dependent change of Young's Modulus of the crust for simplicity. We chose a Young's Modulus of 30GPa, representing an mean average value for crustal rocks in arcs for depths up to 8 km as derived from seismic velocity data (Gottsmann and Odbert, 2014).

"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."

Vesicular here means a sufficiently connected porosity of the lava to serve as an aquifer. While the "petrological" porosity might be higher than 10%, only the connected pores matter for the fluid flow. The used value of 10% stems from cited literature.

SED

7, C890–C903, 2015

Interactive Comment

"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."

The range of variation for each parameter that was tested in the parametric sweeps is stated in Table 2. As indeed some parameters show a much larger natural variability than others, the x-axis of plots were limited to the range $P* \in (-1, 1)$ to improve comparison. We will make this clearer in the manuscript.

"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"

We agree with this suggestion and will move the paragraph accordingly.

"Reference simulation Please, specify the simulation time for Figure 2 (10 days?)."

This figure shows the initial response; i.e. after $10^{-6}s$ (the pressure on the magma chamber is stepped up over $10^{-8}s$, hence the "initial" time is not 0). We will add this information to the text and caption.

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

We will highlight this in this section and expand in discussion.

"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)."

SED
7, C890–C903, 2015

We will highlight it in the figure.

"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."

This comment is related to a later one, so we would like to reply to them together:

"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."

You are right of course, we made a mistake - if every other parameter (i.e., water viscosity and density) is fixed, the higher viscosity of the liquid water makes it flow slower than the steam. The different speeds observed in the plots, where fluid temperature and hence all three fluid parameters are changed, are indeed due to a combination of effects. In answer to your question how we discriminated amongst the effect of the three parameters: We ran some extra simulations (plots not shown), where only one of the three fluid parameters was changed to its value at a higher temperature and the other two remained fixed at their "cold" value. We understand that this part needs more discussion and clarification and will expand on it in a revised manuscript.

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

Yes, we will change the order of figure 8a and b.

"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?"

In fact, the sign-flip effect occurs for all ERh values - it just is not visible very well in the plots because of the different orders of magnitudes of the head responses: for the larger ERh values, the head response is much smaller. Hence, even though the hydraulic head vs. ERc curves show the same behaviour as the one for the smallest ERh value, they appear as almost constantly zero in the current plot. We are trying to find a way to display this clearer in the plot; possibly a zoom in will suffice. However, an exemplary corresponding geological setting for a set of ER value that would cause a sign-flip will be added (e.g., a lava flow on a softer pyroclastic layer could cause a sufficiently large ERc value).

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

We agree and will replace this term where it occurs in the manuscript.

"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."

We agree and will add all these points to the model limitation discussion.

"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."

SED
7, C890–C903, 2015

As mentioned before, we agree that the other causes for hydraulic head changes need to be considered in the discussion and strain cannot necessarily be inferred directly from hydraulic head. We will also aim to shorten this section.

"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."

As above, we agree on shortening the Usu section drastically as you suggest.

"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."

This is true. As stated above (as a reply to your comment on this issue in the introduction), the other possible causes for hydraulic head changes will be included in the discussion, as well as a "less optimistic" outlook on how wells can be included in monitoring systems.

"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,. . .)."

We will make the suggested changes and double check for errors and missed variables.

"Figures Figure 2. Add the simulation time and specify which variable is shown with dashed line and which with solid line."

We will add this information to the figure caption.

SED

7, C890–C903, 2015

"Figure 4. Specify the meaning of the symbols in the caption. Correct a typo in the lower legend (Piorities instead of Priorities)."

We will add the definition of the symbols and correct the typo.

"Figure 5. Change "Influence of the non dimensional flow and coupling parameters Q and F" with "Influence of the non dimensional flow F and coupling parameters Q""

We will make the suggested change.

"Figure 8. This caption is very difficult to understand. Try to rephrase and add a definition of what a 'strain-flipped simulation' is."

We will rephrase this caption.

Cited literature in this comment:

Fournier, N. and Chardot, L.: Understanding volcano hydrothermal unrest from geodetic observations: insights from numerical modeling and application to White Island Volcano, New Zealand, J. Geophys. Res.-Sol. Ea., 117, B11208, doi:10.1029/2012JB009469, 2012

Hickey, J. and Gottsmann, J.: Benchmarking and developing numerical Finite Element models of volcanic deformation, J. Volcanol. Geoth. Res., 280, 126-130, 2014.

Gottsmann, J. and Odbert, H.: The effects of thermomechanical heterogeneities in island arc crust on time-dependent preeruptive stresses and hte failure of an andesitic reservoir, J. Geophys. Res. -Sol.Ea., 119, 6, 4626-4639, 2014.

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

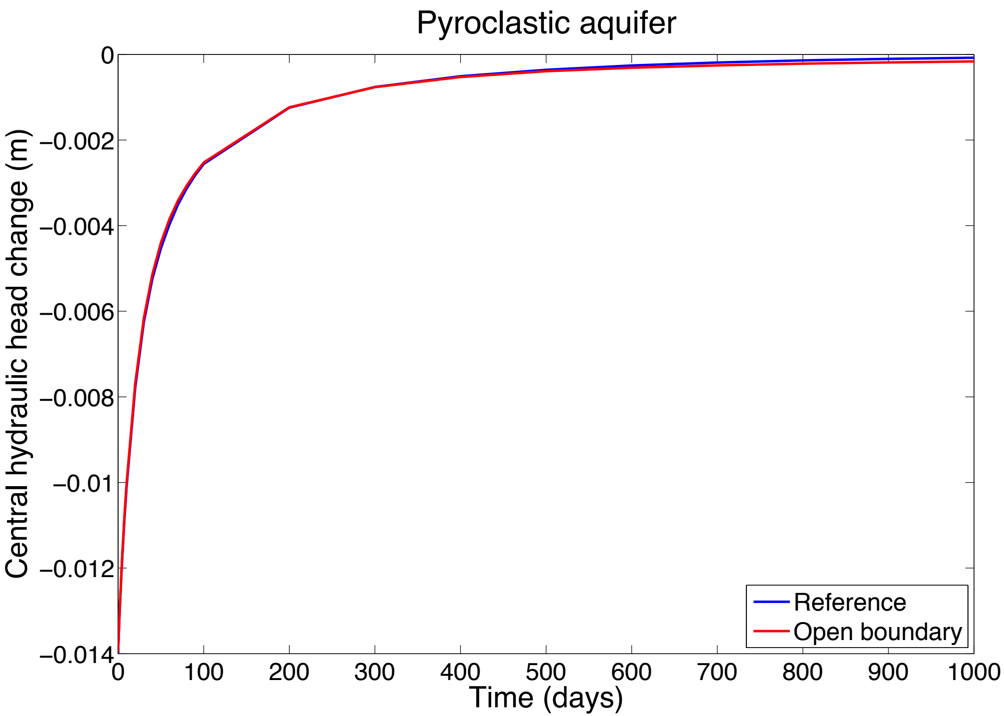

Fig. 1. Difference between the reference simulation and a simulation with a changed lateral boundary condition of the aquifer (as suggested by reviewer): fixed hydraulic head of 0m. Pyroclastic aquifer.

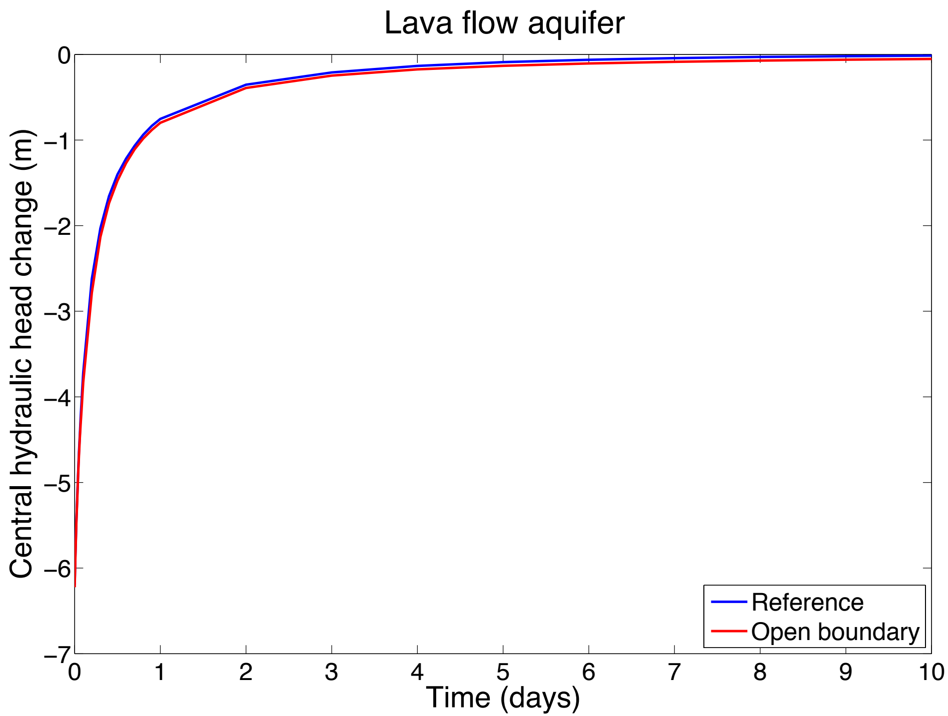

Fig. 2. Difference between the reference simulation and a simulation with a changed lateral boundary condition of the aquifer (as suggested by reviewer): fixed hydraulic head of 0m. Lava flow aquifer.

SED

7, C890–C903, 2015

Interactive Comment