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Solid Earth
Discussions

Interactive comment on "In situ hydromechanical responses during well drilling recorded by distributed fiber-optic strain sensing" *by* Yi Zhang et al.

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We greatly thank the reviewers for their comments and suggestions to help improve this manuscript. Both reviewers showed interest in the monitoring results of DSS but commented on the modeling work for relating the strain changes to pore pressure and formation permeability using a hydraulic diffusion model. We first give a general response as follows.

In an earlier study (of our group), Lei et al. (2019) have shown the corresponding changes between strain and pressure signals in a pumping test in the same field (Figure 1; please see Supplement). They further performed both an analytical hydraulic

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Discussion paper

diffusion model, and a coupled hydromechanical model to explain the aquifer hydromechanical parameters, such as permeability and compressibility. Both models can give a reasonable range of permeability changes and can explain measured pressure and strain changes. The first-order strain changes were linearly related to pore pressure changes and can be interpreted using the hydraulic diffusion model. (We will elaborate this discussion in the revision.)

Therefore, in the current study, we use the hydraulic diffusion model under the first-order approximation and assume a linear relationship between strain and pressure changes with local compressibility (or storage coefficient) to consider the elastic response to pore pressure because we do not have good constraints for the other elastic constants. Moreover, the simplification with the analytical model makes it possible to match the strain or pressure curves by solving an optimization problem. Though the mechanical effect may exist, in a sense of first-order approximation, the analytical results suggest that the trend and pattern of strain changes can be explainable by the hydraulic diffusion mechanism–the main physics.

Regarding the skin effect, we acknowledge that the skin effect may affect the estimation of permeability values (as stated in L233). Though we did aware that the impact of wellbore damage and mud infiltration when doing the analysis, the field test of using DSS monitoring during the well drilling, was the first of such a test, and these parameters related to skin effect, wellbore damage and mud infiltration were not independently evaluated (or not possible). In addition, during the well drilling, the well wall was self-cleaned by the drilling fluid, which was circulated from surface to bottom. Therefore, to clearly analyze the impact of the skin effect is difficult. From an analysis of the responses between the two monitoring wells, we could see the skin effect (larger inverted values of permeability in obs2 than obs1) but not always. (We will explain more about this point in the revision.)

As one of the comments, we will plot both strain and pressure in revised Fig. 4 in the revision. The information may be helpful for readers to know that the hydromechanical strain (of several $\mu\varepsilon$) produced by small pressure changes (of several kPa) can be monitored by using DSS. DSS can be used not only for monitoring of mechanical deformation but also monitoring of pore pressures and fluid flow behavior. Such knowledge can be useful for designing hydraulic tests or monitoring subsurface fluid reservoirs.

Overall, with the main focus of this study is the high-resolution DSS measurement, to interpret the strain changes recorded by high-accuracy DSS during the drilling process, we try to capture the first-order factor–the diffusion of drilling-induced pressure. We acknowledge that a coupled hydromechanial model can be theoretically better; however, practically we lack further constraints besides strain records, and the drilling process maybe not ideal for such a coupled study. Another paper manuscript of us now reviewed by JGR-Solid Earth uses a fully-coupled hydromechanical model to explain the changes in strain for a well-designed and larger-scale hydraulic pumping test.

This manuscript had been previously submitted to another journal (rejected after an external review). Here I give the link of replies to the reviewers' comments (some are relevant to the comments in the current review) and take the opportunity to express my gratitude to them also.

https://docs.google.com/document/d/1HKtZK362WTT9LsQLIn4U4xjU576V1lQY2vYl-jqC_KY/edit?usp=sharing

Reference: Lei, X., Xue, Z., & Hashimoto, T. (2019). Fiber optic sensing for geomechanical monitoring:(2)-distributed strain measurements at a pumping test and geomechanical modeling of deformation of reservoir rocks. Applied Sciences, 9(3), 417.

1. This manuscript documents a quite interesting set of observations of localized deformation during shallow drilling, made with an exciting new fiber optic technology for distributed deformation sensing (based on wave scattering). That there are strains generated in the layered rock system during drilling is, I think, to be expected, but it's exciting to see this demonstrated with relatively high fidelity. I was hoping for some discussion on the frequency response at very long timescales, which would help us

understand the general limitations of signal detection with DSS, but perhaps this is well beyond the scope of such a short paper.

Re: If here I clearly understand the "frequency response at very long timescales", e.g., a long-term deformation behavior with some period (for example, seasonal), I would like to say that the monitoring of it using the DSS system is possible. Beyond this study, we have successfully tested in the same field for long-term monitoring of aquifer deformation due to seasonal agriculture water use or proposed water pumping test (e.g., about 10 days; please see Lei et al. 2019).

2. In terms of how that deformation informs the local permeability structure, I am reluctant to accept the results from the modeling performed here as a definitive demonstration for two main reasons: First, the authors glance off the strong possibility of bias from an unmodeled skin effect, even though this is a known source of permeability heterogeneity; thus, they simply haven't tested whether the estimates they've obtained (or the variability between the two sampling locations) are representative of the layered system and not just related to wellbore damage and mud infiltration. Second, it is perplexing why the authors convert the strain signals to "pressure" in order to use simplistic radial flow models. Unless the timescale of the signal is so short as to cause the system to respond like an undrained medium, strain is not simply proportional to pressure in a fully coupled poroelastic medium (not just the one way coupling they mention). This begs the question: what does this approach offer aside from introducing a whole new set of assumptions that may not hold at such a fine scale? Of course there are very simple yet powerful models of the deformation response in a poroelastic medium that could be used (e.g., Rudnicki, 1986, https://doi.org/10.1016/0167-6636(86)90042-6); using them would permit a way to model strains directly and also remark on the distribution of pore pressure changes. A more sophisticated to replicate the apparent effect of layer contrasts is also warranted.

Re: Thank you for the comments. Please see the general response. We may test the recommended Rudnicki 1986 model.

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Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2020-61/se-2020-61-AC1-supplement.pdf

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