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Interactive comment on "Stress Characterization and Temporal Evolution of Borehole Failure at the Rittershoffen Geothermal Project" *by* Jérôme Azzola et al.

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We thank François Cornet for his comments and review. We appreciate his recognition of the importance of our contribution. Please find below a point by point response to the comments. A pdf file including in black the comments and in blue, our response, is provided as supplement.

Sincerely, on behalf of the authors Jérôme AZZOLA

This paper addresses the important issue of evaluating a regional stress field from images of two different failure processes (borehole breakouts and so-called drilling

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Induced fractures) observed in deep boreholes with different orientations, as well as from results from various water injection tests. The methodology is applied at the Rittershoffen site, located 6km east from the Soultz site, where the stress field is quite well known. This is an important contribution for the understanding of stress field in deep rock masses and the quality of images as well as that of their analysis justify completely its publication.

1. The GRT-2 borehole is inclined 37° to the vertical so that the axial and tangential stress components at the borehole wall are not principal stresses. Authors must write down the equations they are considering, including the role of pore pressure, and that of thermal stresses. Indeed, the principal directions, at the wellbore wall, of stresses resulting from the far field stresses are not the same as those of the thermal stresses resulting from the cooling of the rock. This issue is completely ignored, and the paper cannot be published before this is properly dealt with. I encourage authors to look at paper by Wileveau et al. that provides good illustrations of en echelon breakouts observed in inclined wells. (Wileveau Y, F.H. Cornet, J. Desroches and P. Blumling, 2007; Complete in situ stress determination in an argillite sedimentary formation; Physics and Chemistry of the Earth (vol. 32, pp 866-878)

We acknowledge that the GRT-2 is strongly inclined, with a mean deviation of 37° measured in the section of interest. The equations describing the stress concentration at the borehole wall of a vertical borehole, used in particular for the well GRT-1, are no longer applicable in this case. For the deviated well GRT-2, we used a 3D solution taking into account the geometry of the borehole. The equations in which are involved the geometrical parameters, the far field stresses and the fluid pressure are well documented in the literature and we used the summary proposed in the review from Schmitt et al. (2012) who proposes a complete development of the equations, in the general case. We will include the computation steps leading to the expression of the effective principal stresses at the borehole wall of the deviated well in the revised version of the manuscript and we will cite the work of Wileveau et al., as an additional reference to this approach.

2. For their analysis of the width of borehole breakouts, authors refer to three different failure criteria, including the Hoek and Brown criterion. For the parameters to be considered in these criteria, they refer to laboratory work quoted by Rummel, 1991 and by Valley and Evans, 2006. They should also look at the publication by Villeneuve et al. (Villeneuve M.C., M.J. Heap, A.R.L. Kushnir, T. Qin, P. Baud, G. Zhou, and T. Xu, 2018; Estimating in situ rock mass strength and elastic modulus of granite from the Soultz-sous-Forêts geothermal reservoir (France); Geothermal Energy, 6(11), https://doi.org/10.1186/s40517-018-0096-1), which address precisely this issue.

We used all available published data to parametrize our failure criteria, including data provided in Villeneuve et al., 2018, but also from Heap et al. (2019) (cited on lines 90 and 511 of our original manuscript). To clarify this, we will add the relevant references in section 5 of our revised manuscript.

3. In their table 3 the density value for the granite is said to be 2570 kg/m3, yet in equation (6) the vertical stress is assumed to be equal to 0.024 z-0.83. These differences should be discussed. In addition, given the vertical stress magnitude is taken into consideration in the three-dimensional failure criteria, authors should show how they determine uncertainties on the vertical stress component evaluation.

The magnitude of the vertical stress Sv is obtained from the weight of the overburden. We apologize for the typo in Eq. (6), which should read 0.248 z – 0.83. This misleading rounding will be corrected in the revised manuscript, which leads to a trend in line with density value 2570 kg/m3 chosen for the granitic layer. Given the fact that the vertical stress is obtained by integrating the density profile from surface to reservoir depth, the uncertainty on density add up and thus the uncertainty on the vertical stress estimation increase with depth. Considering an uncertainty of 50 kg/m3 on the densities leads to a 2.5 MPa uncertainty on the vertical stress at reservoir depth. This uncertainty is not significant compared to other uncertainties involved in the analysis as for example those related to the mechanical parameters chosen in the inversion of the maximum horizontal stress.

4. Similarly, equations used for the evaluation of the minimum principal stress magnitude is not described and this should be corrected. Evaluation of associated uncertainty should be discussed.

We follow approaches largely used in the literature (e.g. Cornet et al., 2017) and estimate the minimum horizontal stress Sh from pressure limiting behavior during hydraulic injections. Since we did not have enough information related to the ECOGI project to compute a complete Sh stress profile, we use measurements carried out at the nearby Soultz-sous-Forêts project. The trend is evaluated by Cornet et al. (2007). This publication does not propose an uncertainty measurement for the minimum horizontal stress Sh. To complete our analysis, we analyzed the wellhead pressure measured during the hydraulic stimulation of GRT-1 and derived an estimate of the Sh at depth from the pressure reached at a maximum flow rate. However, as the pressure shows a gradual but not definitive stabilization for these maximum flow rates, our measurement is discussed as a lower bound of the minimum horizontal stress Sh at depth. We show that our measurement is still consistent with the trend retained from Soultz.

5. Table 2 indicates values for the Poisson's ratio, but no reference is made to Young's moduli nor to thermal expansion coefficients used in equation

We apologize for failing to include these values in the manuscript. They will appear in the revised version. The thermal expansion is chosen to be constant for the different layers of our model, α = 15 x 10-6 K-1, and the Young's modulus will appear in the Table 2.

6. In equation (2) the stress component τoct implies the three principal stress components. This should also apply to the mean stress, as opposed to equation written on line 179.

We would like to refer the reviewer to the original derivation of the equations proposed by Zimmerman & Al-Ajmi (2006) in which they refer to an “effective mean stress”, \sigma_{m,2}=\frac{\sigma_1+\sigma_3}{2} ,for the Mogi-Coulomb criteria. This is not strictly speaking the mean stress, which would also include a contribution of the intermediate stress. We will clarify the terminology and nomenclature in the revised version of the manuscript.

7. In their discussion of results, authors argue that some of the results obtained for the magnitude of the maximum principal stress magnitude do not satisfy the Coulomb stability condition for the rock mass. Interestingly, Cornet (2016) has argued that the large-scale fluid injections conducted at Soultz have generated large scale failure zones that are changing in orientation with depth, a feature consistent with the Hoek and Brown criterion but not with a Coulomb criterion. This issue should be discussed more carefully (Cornet, F.H., 2016. Seismic and aseismic motions generated by fluid injections; Geomech. Ener. Env., 5, pp 42-54). caption of figure 12 has been exchanged with that of fig 13 Because of these many issues, I recommend publication of this paper only after they have been answered, with particular attention to the issue raised on principal stress directions close to inclined boreholes.

We consulted the proposed paper, but it does not refer to these criteria. We would appreciate it if the reviewer could clarify his comment before we review the paper and explain the link between the large-scale failure zones created by the fluid injections and the consistency with the Hoek Brown criterion rather than with the Coulomb criterion. We should also point out that the injection in Rittershoffen were not as “massive” as in Soultz, i.e. the injected volumes were much smaller and thus the transposition of the knowledge from Soultz in this regard may not be directly applicable.

8. How are the various parameters measured? How valid are those measurements for in-situ properties? This should be better discussed.

Please note that we are very cautious in describing the criteria and mechanical

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parameters chosen in the approach. We recognize that the strength parametrization is the main limitation of our approach. We bring this point carefully in the discussion. Given that we do not have “access to direct strength measurements since no cores were collected” (line 549), our results are discussed at the light of the uncertainty on the strength parameters, as stated line 564 or in the discussion.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-72/se-2019-72-AC1-supplement.pdf

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