Reply to the review of anonymous reviewer RC#1

Dear Thomas Blenkinsop,

We would like to thank you very much for the review and your comments. Your constructive suggestions and corrections improve the quality of our manuscript. In the attached document, we present our changes and corrections to your individual comments.

Kind regards,

Lisa Winhausen and co-authors

Reviewer: *One of the only significant problems in the paper is the use of the term "ductile". In rock mechanics, brittle and ductile were defined in a very specific way, depending on the amount of strain before failure. Heard (1960) defined deformation as brittle if fracture formation was preceded by less than 3% permanent strain. Griggs & Handin (1960) defined ductility as a material's ability to withstand more than 5% permanent strain before failure. This paper does not seem to be using ductile in these terms, since failure is occurring at < 1.5% strain, which would be brittle in the original definitions. Given that the paper is based on rock mechanics, the original definitions would seem to be relevant.*

Answer: This a good point. Indeed, we missed to include some early but fundamental work of experimental investigations on failure mode transitions. We added some important references (your suggestions and others, which seemed relevant to us) and extended the instruction part by adding missing information about the definition of brittle-ductile terminology. Furthermore, we added some more specific description of microstructural/mechanical deformation behavior based on work by Rutter (1987). We further clarified the use "plastic", only using it in the geo-mechanical sense for inelastic/irreversible, permanent strain.

Reviewer: *The paper uses brittle and ductile in a variety of ways: plastic is also in the mix. It would be good to straighten out the intended use of the terminology early in the paper. It is also surprising that the classic papers by Griggs, Handin etc that identified brittle and ductile behaviour in the 1960's are not cited.*

Answer: This comment is closely related to the first one. Our comment is included in the above answer.

Reviewer: *The large values of Poisson's ratio in table 2 are vastly in excess of the maximum of 0.5 for an elastic solid. That is possible with highly dilatant materials, but it is worth a comment about this in the text, and possibly some comparison with other similar materials in the literature. Poisson's ratio as used in this way is far from an elastic constant for materials. It turns out that there are even problems with the definition of Poisson's ratio when it is less than 0.5 (Dong, L., Xu, H., Fan, P., & Wu, Z. (2021). On the Experimental Determination of Poisson's Ratio for Intact Rocks and Its Variation as Deformation Develops. Advances in Civil Engineering, 2021. https://doi.org/10.1155/2021/8843056).*

Answer: We agree that a discussion on the values for Poisson's ratio is valuable for interpreting the failure behavior. The considerably high values are associated to the anisotropy and the fact that the measurement is taken normal to the bedding plane orientation. Additionally, to our interpretation we included some other works which deal with anisotropic shales and the estimation of the Poisson's ratio.

Reviewer: *The description of the grain and pore alignment in the shear zones in lines 182-185 is oversimplified and partly contradictory to the interpretation that grains may have a P foliation: this would not be parallel to the shear zone boundary, and indeed such grains can be seen in Fig. 9.*

Answer: We agree with your comment. The description of the shear zone structures was partly contradictory and has been corrected. We have added an additional figure showing the structures mapped in the shear zone.

Reviewer: *Finally, the paper did mention the possible use of Opalinus clay as a host rock for nuclear waste. It would really round this paper off if a paragraph in the discussion could consider the implication of these results for waste disposal. Presumably one important additional consideration would be to what extent temperature is likely to change the results presented here.*

Answer: We agree and inserted a small paragraph explaining how the findings can be transferred for the application as a host rock for rad-waste and included some factors, which might change the behavior observed. Actually, this project has now started the second phase, in which creep and the influence of temperature will be analysed.

Reply to the review of anonymous reviewer RC#1

Dear Mike Chandler,

We would like to thank you very much for your review and the comments you raised. They helped to improve the quality of our manuscript. Below, we present our changes and corrections according to your comments.

Kind regards,

Lisa Winhausen and co-authors

SPECIFIC COMMENTS

Reviewer: *My only significant suggestion is that the paper might benefit from some discussion of the influence of the heterogeneous sample material on the Skempton B-checks used to determine the sample saturation. I was not previously aware of the method of B-checks, and having read around it a little, the relationship between B and saturation seems to assume a constant bulk modulus, porosity and permeability in the matrix? In Sections 2.1 and 4.2, the authors draw attention to the heterogeneity of the material presented here, and so I wonder if the authors could comment on how confident they are certain that the Skempton B-check is confirming the full saturation of the sample, or just the saturation of the more porous components? If there was a heterogeneous saturation state within the sample due to lower porosity/permeability regions (presumably with different bulk moduli), would this be likely to affect any of the deformation processes discussed in Section 4.2?*

Answer: This is a very good question! Indeed, the absolute B-value is dependent on the bulk modulus and porosity. To maintain these two parameters in each individual B-value assessment, we increased the back pressure after each undrained loading step to keep the effective stress constant, so that also K and ϕ can be assumed constant. We are quite certain that all specimens have a very high degree of saturation. For lower porosity/permeability regions (of effective, connected porosity), the total stress increase leads to an increase in pore water pressure (bulk compression leads to a pore space compression) and an increase in saturation due to Boyle's law (volume of gas is reduced due to a pressure increase) in combination with Henry's law (possible gas is solved in the liquid phase with higher pressures). We agree that there might be regions of lower and higher porosity/permeability and that these require longer time periods for the pore water pressure diffusion to the specimen's end face. However, we can be certain that these effects are accounted for, since we waited for full pore water pressure equilibration at both top and bottom of the sample (usually less than 1 hour) and the back-pressure phase lasted usually 24 hours. Even if there were some non-saturated regions (e.g., S>99.5%), such as nano-sized gas bubbles trapped in isolated micro-cracks or void corners, their volume would be much smaller than the total volume in the pore space. We infer therefore that their existence does not invalidate neither full saturation nor the deformation behaviour. We included the missing information on the procedure in the text to underline its robustness.

Reviewer: *It might also be worth putting the range of confining pressures in the abstract, as I see this as being quite a large differentiator between this study and the earlier papers of Amann et al.*

Answer: Agreed. We included the range in the abstract.

TECHNICAL CORRECTIONS

Reviewer: *I think there is a rogue "a" on line 294, and it should read something like " … failure on the microscale is less dilatant, forming a broader…"*

Answer: Well-spotted! Redundant word deleted.