Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-178-AC1, 2021

Solid Earth Discussions

Interactive comment on "Anisotropic transport and frictional properties of simulated clay-rich fault gouges" *by* Elisenda Bakker and Johannes H. P. de Bresser

Elisenda Bakker and Johannes H. P. de Bresser

elisendabakker@hotmail.com

Received and published: 21 April 2021

Interactive comment on "Anisotropic transport and frictional properties of simulated clay-rich fault gouges" by Elisenda Bakker and Johannes H. P. de Bresser Anonymous Referee #1 Received and published: 22 January 2021

The paper by Bakkers and de Bresser presents new experimental data on the influence of pressure and slip on permeability (and permeability anisotropy) of clay-rich gouge. The new data are certainly valuable, and those experiments are not easy to conduct. We thank the reviewer for pointing out the value of the new data and appreciate the

recognition of the complexity of our experiments.

The paper is framed heavily around CO2 storage problems, which is just one of the many possible applications of the work; this is somewhat distracting, since a lot of space is devoted (in the introduction and discussion) on the link with CO2 storage, which can only be addressed very generically here, at the expense of more physical/microstructural discussions about the processes that generate changes in permeability. We indeed focus on faults penetrating caprocks of CO2 storage systems, realizing of course that the outcome has wider applications than only CO2 systems. However, the nature of our experiments, namely shear tests with measurement of argon gas permeability, makes that the results help determining (see also the response to referee 2) the upper limit of the fault gouge permeability/frictional strength and trends with increasing shear displacement and increasing normal stress. In other words, it is the limits and trends rather than the absolute values that form the main output. And these may help assessment of leakage risks. So we prefer to keep the framing to CO2 storage systems.

My main concern with the paper is twofold. Firstly, it is not clear at all if the data are useable, since the author repeatedly mention that they could not achieve a proper correction for the Klinkenberg effect. This should be clarified. This point should be viewed in the same light as point 2 of referee 2. With the study we aimed to investigate the trend in permeability with increasing shear displacement and increasing normal stress, for carbonate rich clay materials relevant for caprocks of reservoirs. Similar as to Faulkner and Rutter (2000), and many more studies, we observe a decreasing trend with increasing shear displacement, and normal stress, as well as an anisotropy between perpendicular and parallel to fault. Ideally, we would have preferred to establish a proper Klinkenberg correction, to account for differences in permeability of the gouge to gas as that to water. However, as explained in the paper, a systematic correction could not be applied due to the progressive change of the maturity of the fault during shear. A Klinkenberg correction would have affected the absolute values for permability, lowering the values relative to those observed in or study, but the current results still give the upper limit of the fault gouge permeability/frictional strength and support the trends with increasing shear displacement and increasing normal stress also found by others (on different type of materials).

Secondly, the data interpretation in terms of mechanisms (in absence of other measurements, such as pore volume, or microstructures, or modelling attempts) are very vague and rely almost entirely on comparison and analogies with published literature. In that sense, the paper remains very technical and descriptive. One key question that is not really addressed, for instance, is the role of pre-compaction: if permeability is anisotropic to start with, it means that normal compaction of the layer already produces a texture, which is then altered by shear. But how is this initial texture formed? Is it visible in the microstructure? Is this an experimental artefact or something that we should expect in nature? We recognize the observation of the reviewer that the paper is rather technical and relies on a comparison with literature rather than using own microstructural observations. Unfortunately, it appeared not possible to retain deformed samples such that meaningful microstructural analysis was possible. Regarding the role of pre-compaction, see our response to comment 3 of referee 2. The permeability anisotropy develops due to the physical nature of the play clay minerals, that rotate to an orientation with their basal planes tending to become parallel to shear/fault plane.

One thing that could help put the results in perspective with literature data is the make systematic comparison between the permeability anisotropy data obtained here and the other existing datasets. Is there anything general that can be established? What is the order of magnitude of anisotropy that we should expect across the range of materials tested? This is a good suggestion, and we will include such comparison in the revised manuscript. We will focus on studies that have looked into permeability anisotropy, and compare the effect of effective normal stress, composition, shear displacement, dry or wet permeability measurements and the effect of shear and maybe holding on the permeability development. These studies include [Zhang et al., 1999;

Crawford et al., 2008; Okazaki et al., 2013]. Materials used in these studies vary from pure kaolinite/muscovite/serpentinite to pure quartz, and quartzo-feldspatic to granitic rock, allowing us to make general inferences to which we can compare the outcomes of our experiments. One important aspect is that the order of magnitude of anisotropy depends on the nature of the grains. Based on work by Zhang et al. (1999) we know that in quartzo-feldspathic gouges, the initial anisotropy is small, however after some sliding a significant anisotropy develops. The quartz gouges have shown to have no significant anisotropy to begin with and that increases with shear, up to a maximum of one order of magnitude. With permeability perpendicular to shear being the lower. The same is observed for granitic material. Resulting in a permeability anisotropy of 1.5 orders. Okazaki et al., 2013 observed a similar trend, resulting in a permeability anisotropy of $\sim$1. Muscovite on the other hand [Zhang et al., 1999], starts with an initial anisotropy of about 1 order of magnitude. Shearing leads to a decrease in permeability for both directions, however the decrease in along fault permeability is lower, resulting in a smaller anisotropy. This observations is similar to what we have observed with our mixed composition gouges. Text will be modified to include the above in a systematic way.

Detailed comments: l.29: permeable -> permeability Done

l.56: not sure that dilation and compaction (i.e., porosity change) can be so easily linked to increase of decrease in permeability. Maybe moderate the statement? We can do this

l.66: one recent reference that is relevant here is Rutter and Mecklenburgh, JGR 2018, where systematic characterisation of shear and normal stress effects on permeability anisotropy was conducted. Also, Okazaki, Katayma and Noda, GRL, 2013, specifically studied along-fault permeability vs slip in a phyllosilicate gouge. The paper by Rutter and Mecklenburg, 2018, did indeed look at the effect of normal and shear stress on the hydraulic transmissivity of shale, among other rock materials. However, the paper is focussed on thin cracks within/between two rock parts, rather than at fault gouges like we do in our study. However, we will give credit to this study. We will also add the suggestion of Okazaki et al., 2013. They have studied the shear-induced permeability anisotropy of a simulated serpentinite gouge and saw a shear-induced permeability anisotropy of ∼1 order.

l.81: by "transport", it seems that the authors mean "permeability". (Transport is more vague and could refer to hydraulic diffusivity) Done

l.175: does "dynamic permeability" refer to "syn-deformation permeability"? the word "dynamic" means different things to different people, and some clarity would bring everyone on the same page. Changed

l.177: maybe reword as "... a dynamic permeability measurement was conducted" Done

l.209: the symbol on the lhs seems to be "proportional to" instead of "alpha". Done

l.217: is there a way to estimate the potential error produced by this assumption? I don't think it will be huge, but a rough estimate could be helpful, since compaction might lead to artificial increase in permeability (reduction in A). The reduction in A plays a role in the calculation of the permeability along fault. As stated in the paper, we assumed the changes in path length to be equal to the shear displacement at the various stages in the experiment for the along-fault permeability calculations. As such, we have based our reduction in A on the shear displacement. In this way thickness changes do not affect changes in A.

l.243: just say "... a rapid decrease"? Unclear what this is referring to.

l.255: remove first "it" Done

l.270: I do not really understand what was done here, and what was the conclusion. Should we trust the data or not? To avoid argon leakage along the pistons, so to not present a "short-circuit" route for fluid flow outside the gouge layer, we taped the set-up on the outside. This contained the argon gas to the gouge layer, resulting in accurate

permeability measurements.

l.350: is the reduction in k with increasing sigma_n reversible? how much of that is elastic closure of pathways vs. permanent collapse? Based on work done by e.g. Faulker and Rutter, 1998, who tested the effect of pressure cycling, the reduction in permeability with increasing confining pressure/ effective pressure is dominated by permanent collapse. With each consecutive pressure cycle they observed a reduction in permeability. This was attributed to the reshuffling of the phyllosilicates with respect to one another, producing enhanced compaction with each pressure cycle. However they also observed that there is a slight difference in permeability with complete or partial depressurization of the sample, for which they suggested the opening or re-opening of microcracks (during complete depressurization). But they assumed that this effect is negligible at higher pressures.

l.370: I understand that the measurement of volumetric strain (or pore volume change) was not possible in the experiments. Would there be a way to back the statements in the discussion using some indirect observations (say, microstructures, or anything else), rather than just relying on literature data (on other rocks!)? No, unfortunately we are not able to back it with microstructures, as these were damaged due to their fragile nature when preserved with epoxy.

l.375: What exactly is the problem here? I am not I follow what is stated. It is not clear to us where this is referring to?

l.389: I do not understand the sentence; rephrase? We will do this.

l.397-415: I am not convinced that such small anomalies deserve to be discussed at such great length, especially since no real explanation is given beyond speculation. Is the stick-slip behaviour reproducible under those conditions? See our response to comment 7 of referee 2. We will follow the suggestion to downsize the discussion given the fact that the anomalies are relatively small..

l.430: this conclusion strongly depends on the state of consolidation of the gouge layer. How do the laboratory condition reflect the in-situ conditions of natural faults? In nature, faults might be overconsolidated or chemically sealed, which would lead to dilation (not compaction) upon slip. Unfortunately, we don't really understand this statement. The conclusion as derived in our paper is based on the fact that our experiments show a reduction in permeability with increasing shear displacement, which is the equivalent of increasing fault maturity, and with static holding and re-shearing. These situations happen in nature are well, i.e. relatively long hold periods leading to consolidation interrupted by short and rapid periods of shearing, all while being under natural stresses. Yes, natural faults might be overconsolidated and might be chemically sealed, but neither are required for a natural fault to be a fault. As for overconsolidation requires an abnormal stress regime and chemical alteration requires oversaturated fluids running through a fault.

l.455: I am not sure what it means to show "uncorrected" values. They may not be meaningful at all, unless some reasonable error bars can be provided. Are the results upper or lower bounds for the actual permeability? In addition, if CO_2 is the focus, the interesting permeability value is that relevant to (possibly gaseous) CO_2, which then implies another Klinkenberg effect. The values documented in this paper represent the upper bounds for clay-rich, quartz-calcite fault gouge permeability. Argon is chosen as medium as it is an inert gas, as such it will not cause any interaction with the fault gouge. Any possible interaction of gas with the fault gouge material, such as clay swelling, will lead to a further reduction of the permeability. As such the argon permeability values represent the upper bound values. As these values point to near impermeable values, CO_2 or even water in the fault zone are expected to lower the permeability values even more. Making leakage even less probable.

l.459: foliation is mentioned but not shown? here again, microstructures would be important to support this point. As we agree with the referee that microstructures would be important to support this point, we were not able to preserve the microstructures in

epoxy due to the fragile nature of the gouge.

l.470: I sense that this point, as stated, could be made independently from the data shown in the paper. I am not convinced this is a solid conclusion that is drawn from the new dataset, rather than a generic point about permeability. This statement is based on our experiments which show that the permeability will be higher for lower effective normal stresses. As this is inversely proportional with an increasing pore fluid pressure as a result of the migration of supercritical CO_2 from a storage reservoir into a fault. Moreover, our data show that flow along a fault into over- or underlying formations is easier than into neighbouring reservoir compartments. We could rephrase this point such as to make it clearer that this is point is derived from the data obtained in this study.

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-178, 2020.