



EGUsphere, referee comment RC2
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Comment on egusphere-2022-824

Andreas Busch (Referee)

Referee comment on "Insights into the interaction of a shale with CO₂" by Eleni Stavropoulou and Lyesse Laloui, EGU sphere,
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This article addresses CO₂ interaction with a clay-rich mudrock, originating from the Mont Terri Underground Lab in Switzerland. Authors have taken an imaging approach whereby the long term interaction of the mdrock with CO₂ is investigated and quantified.

The sample has been prepared by saturation over salt solution to obtain near-water-saturated conditions (in my understanding). My main points are detailed below, further comments can be found in the pdf attached.

The long-term interaction of CO₂ with caprocks acting as seals above CO₂ storage reservoirs is important for de-risking CCS in general but also allows for a deeper understanding of the coupled processes taking place in such a rock where chemistry and temperature affect mechanics and fluid transport. Studying such coupled effects is the goal of this paper. Mudrocks are complex rocks, especially when they contain swelling clays, like smectite. Smectite, depending on pH, water content and water composition, can swell or shrink, potentially leading to volumetric expansion or shrinkage of the rock, resulting in crack opening or closure. A careful handling of the geochemical conditions is therefore important to not come up at potentially wrong conclusions. CO₂ interacting with clay-rich material can do similar things. It can dehydrate the sample through water evaporation and can further swell smectite. It dissolves in water, thereby changing pH which again can trigger geochemical reactions (most likely carbonate over short time scales of weeks to months).

Separating all these effects is difficult and not straight-forward. In this study, effects mainly caused by CO₂ have been addressed, disregarding any other implications of water-saturation (change), pH change or other effects. This would be important in order to separate observations caused by CO₂ or by any other process that has nothing to do with the injection of CO₂ but only by handling the samples under lab conditions. I have placed several comments in the text to highlight where I see this being important. I had the impression that the focus of the paper is too much on showing a CO₂ related effect, to confirm a hypothesis that was potentially established before running the lab tests. This can lead to misleading conclusions, then again used by others.

I appreciate the efforts that have been made in this paper to highlight coupled effects using observations in the wider sense. This is a complicated topic that is difficult to address experimentally. Experiments are required to support models for upscaling to reservoir scales. Often a good understanding starts with observations and interpretations. The interpretations made in this paper might be correct but the discussion is far too linear, targeting a single goal which is to relate everything to CO₂-related effects. This needs to be revisited before the paper can be further considered for publication.

Other rather major points:

English language needs to be improved. While the paper is "readable", there are many flaws in the text, like words missing, wrong syntax and partly also wrong tense. Some examples are highlighted in the pdf attached but it is absolutely necessary taking a careful look at this point before potentially re-submitting this article.

Most observations are discussed in Ch 3 and 4 while Ch 5 talks about Discussion and Conclusions. In Ch 5 there is basically no discussion and mainly the image in Fig 10 is introduced to show how coupling works for the case study presented. It would be good to strictly separate results from discussions and conclusions throughout the manuscript. On the same note, besides the introduction (and own citation later on), there has been no inclusion of other literature sources in the "discussion" of the results. Lots of work has been done in terms of volumetric changes of (smectite-containing) mudrocks with changing water contents, pH, CO₂ etc. I do not think the paper can be accepted without a proper discussion including other research outputs. This again might lead to some different conclusions in this study or to a weakening of the statements made about CO₂-related effects. In particular, research published by groups of Eric Ferrage, Andreas Busch, Chris Spiers, Bernhard Krooss, Richard Worden, Eugene Ilton (and others) etc can be useful in this regard.

Please also note the supplement to this comment:

<https://egusphere.copernicus.org/preprints/2022/egusphere-2022-824/egusphere-2022-824-RC2-supplement.pdf>