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Interactive comment on "Characterization of fractures in potential reservoir rocks for geothermal applications in the Rhine-Ruhr metropolitan area (Germany)" *by* Martin Balcewicz et al.

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In their paper, Characterization of fractures in potential reservoir rocks for geothermal applications in the Rhine-Ruhr metropolitan area (Germany), Dr. Balcewicz and co-authors document natural fractures in limestones exposed in quarries some tens of km to the south of the metropolitan areas of Essen, Bochum, and Dortmund, with the goal of extrapolating what kind of natural fracture patterns might be present within the same layers underneath those cities. This is an important topic because fractures do commonly control subsurface permeability, and would be critical to the viability of

the limestones as geothermal energy reservoirs. Moreover, a large amount of data has been carefully collected and presented in the paper, so I think it is potentially publishable in Solid Earth, and would particularly be a good contribution to the special issue on crustal fracturing.

However, I do have a few concerns with the paper as written. Despite a great wealth of data presented, there are some confusing and perhaps contradictory statements given which should be clarified. These mostly concern the orientation of the fractures in the quarries, and their inferred orientations in the subsurface. As well, the conclusions are fairly scant and qualitative, especially in terms of what type of fracture patterns are predicted to be in the subsurface and why. Lastly, and relatedly, I think the authors have the data to weigh in on some long-standing debates about fractures at the surface versus those in the subsurface, the implications of which have important consequences for the hypothetical geothermal capacity. I think to make their study broadly applicable to similar problems elsewhere, and thus to warrant publication in the scientific literature, the authors need to be firmer in their conclusions and explain the implications for their findings on natural fracture patterns in general.

To begin with, the outcrop data show a variety of fracture orientations, and the conclusions are ambiguous about what orientations we should expect in the subsurface. There appear to be three fairly clearly defined (by strike) fracture sets at Wuppertal, two (and perhaps a third) at Hönnetal, and not much systematic strike organization at Hagen Hohenlimburg (judging by the rose diagrams in Figures 3, 4, and 5). Importantly, the fracture strikes in the three quarries do not match one another. Is this because bedding is in a different orientation, accounting for a clockwise rotation at Hönnetal? It is not clear. If so, it may mean the fractures pre-date the folds. The best way to show that is to restore bedding and see what the fracture orientations do. But furthermore, the authors note a correspondence between the observed fracture orientations and the World Stress Map, but that map, in western Germany, has SHmax indicators in many directions. I would agree that NNW is probably the dominant one, but I see several trending NE, WNW, and NNE. So the wide scatter in the outcrop data, combined with that in the map, makes questionable any correspondence between the two. And in any case, it is claimed that the dominant fracture orientations in quarry exposures are NNW and NE, and I suggest that is only true for one of the three quarries (Wuppertal).

Again, in Hönnetal it is unclear whether a third fracture set is present, judging by orientation; we might be able to objectively discriminate sets based on filling, but it is not clear which orientations are filled with what mineral. Fill type is an excellent way to discriminate between fracture relative ages and origins, and of course will have large implications on permeability. As well, partial cementation is a way to keep fractures open in the subsurface, regardless of the current state of stress (Laubach et al., EPSL, 2004).

Assuming the authors are correct and more-or-less N-S striking fractures would dominate the subsurface permeability, can we extrapolate any key parameters of the fracture patterns (length distributions, connectivity, intensity) based on the quarry exposures? These are only discussed very qualitatively. Are the N-S (or thereabouts) striking fractures more porous? Entirely sealed?

The quarry fractures dip 80 to 90 degrees, and yet the beds dip 30 to 40 degrees toward north. This implies a distinct non-orthogonality between the fractures and the beds. Is this true of all fractures of all fill types? All else being equal, I would assume opening mode fractures would form with either a vertical or horizontal attitude. Does the near-vertical attitude of fractures, and the significant dip of beds, mean that the fractures post-date the folding of the beds? Is that true of mineral-filled fractures (perhaps earlier) and clay- or debris-filled fractures (perhaps later-formed)? If so, presumably my above conjecture that the fractures pre-date the folds is wrong. Why then do the strikes in different quarries not match up? It could be any number of reasons (different stress states, different timings) but with a more thorough description of the observations, particularly documenting fracture orientation versus fill type, we could make more sense of the variation of the pattern from quarry to quarry. This is seemingly a prerequisite to

understanding the variation from quarries to subsurface.

Line-by-line comments:

14, also 567. Can omit “shallow” because that is a relative term, and you have already quantified it.

18. See general comment above: why do you focus on N-S fractures and not the other orientations? Especially since the stress map has a variety of SHmax orientations. Is your inferred insufficience of permeability ameliorated any if more fractures are present in the subsurface?

20-1. Is karstification only related to the facies fabric, or does it have anything to do with fracturing? Is there dissolution porosity associated with fractures?

37. “can be” for “be can”

73. “right-handed side” awkward/un-specific

127-9. Please clarify: there is a 150 m thick “carbonate layer” on top of dolomitic carbonates (which or course are also carbonates. Then the limestone beds have 1-5m thickness? The carbonate layer is composed of limestone beds? What are the bed boundaries like–is there fracture stratigraphy?

143. Again, awkward distinction between “dolomite” and “carbonate”–dolomite is a carbonate mineral.

200. Can you describe your method of establishing/categorizing the roughness more? How did you observe them at two scales? It’s fine to just have three qualitative categories of roughness, but please take this opportunity to describe the roughness more. Is it imparted by stylolitization, branching, hooking?

204. Are any fractures filled by quartz? Looks like zero in Table 2-3.

213. What is the “true” spacing between two fractures that are not parallel? Better to

ust use the apparent spacing?

220. A negative exponential function is not, I don't think, the same as a power law. Exponential is y = abˆx whereas power law is y = axˆb.

408. Why does tectonic motion favor the growth of reefs?

417, also 566. Why is the modeled layer 300 m thick? Why not 150, as in Line 127? Without quantifying predicted fracture attributes, what is the purpose of specifying a layer thickness?

429-31. I don't understand this sentence.

480. Unloading stress regimes–yes, highly possible. To determine whether this occurred, some discernment of fracture orientation versus fill would be useful.

487. This is very approximate usage of orientations when discussing potential for openness. Is the difference between "NNW" and N-S" significant?

497. Slickensides a filling material?

534-5: P-waves more sensitive to pore spaces and unfilled cracks? I thought it was precisely because S-waves only travel through the solid rock that S-waves are more sensitive.

589. "comparable"âĂŤany two numbers are comparable; you mean similar? How similar? Can you quantify intersection probabilities, if I drilled a hole at Dortmund?

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