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Solid Earth Discussions



# *Interactive comment on* "Simulating stress-dependent fluid flow in a fractured core sample using real-time X-ray CT data" *by* Tobias Kling et al.

**Anonymous Referee #2**

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The objective of the study is to evaluate a methodology to characterize the flow path geometry in a fracture as a function of stress history using medical CT images of the fracture under varying states of stress. The development of methodologies for digital rock physics workflows remains an active area of research and the investigation. However, the investigation of methodology presented here is incomplete, a local instead of global analysis of the uncertainty in fracture aperture estimation should be conducted. I have one fairly major concern regarding the methodology, and analysis of the methodology. Following this I also have outlined further comments/questions that should be addressed. As it stands this manuscript offers little in the way of novelty, and there are major issues with the methodology.



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Discussion paper

Page 3 lines 23-24, Page 6 lines 28-32, Page 7 lines 20-21: There is a major misconception regarding CT numbers throughout the manuscript, X-ray attenuation is a function of density and apparent atomic number (this is why contrasting agents are often employed!). The variation of the CT numbers for the sample shown is not only a function of density, but also composition. The variation of the CT numbers being a function of density and composition can have a significant effect on the estimation of the subvoxel fracture aperture.

I do not believe the methods used by the authors to extract the fracture geometry from the CT images have been properly assessed. Specifically, the effect a locally varying matrix CT number has on estimated subvoxel fracture apertures. The fracture apertures of interest are far smaller than the voxel dimensions. Therefore, as outlined by the authors, the apertures are instead estimated from the CT numbers of the voxels containing the fracture (CT(i)). To estimate the local aperture the authors use a method that requires assumptions that may be significantly erroneous; Equations (1) and (2) summarize the MA method,

Local Aperture = 1/C*CT(MA)=1/C*(CT(mat)-CT(i)).

The underlying conception of the MA method used by the authors assumes the following weighted volume average contributes to the measured CT(i),

CT(i)=(VolumeLocalMatrix/voxelVolume)*CT(local matrix) + (VolumeWater/voxelVolume)*CT(water),

where the voxel volume and CT(water) are constants, and CT(local matrix) is the CT number of the matrix within that voxel, CT(i). Only CT(i) is known, therefore, unless CT(local matrix) is also known, the volume of water occupying the voxel – and subsequently the estimated aperture of the fracture – cannot be determined. It is apparent from the CT image in figure 3 that the matrix CT numbers, CT(local matrix), vary locally and significantly. The authors address this issue by using a mean for CT(mat) that is varied globally in accordance with the standard deviation of the CT-number distribution of the matrix. This is the equivalent of extracting one realization of the possible fracture geometries within the framework of the method and the known uncertainties, then simply dilating (CT(mat)+standard deviation) and closing (CT(mat)-standard deviation) this geometry. To determine the variance in the simulation results as a result of local variation of the unknown CT(local matrix) would require a local analysis. A local analysis would result in a large number of possible fracture geometries that likely could vary substantially from those estimated by applying a variation in CT(mat) globally. If the objective of the investigation presented by the authors is to develop a methodology for estimating the flow path characteristics – which can conceivably range from a singular main flow path, to multi-stranded flow paths, to uniform flow – as a function of fracture geometry, which is coupled to stress history, then the local variation in aperture will be the determining factor. Bulk measurements - such as the permeability - can easily be fit by a planar plate model, therefore matching bulk measurements to simulated bulk measurements does not provide any information on the flow path geometry characterization. Likewise, if the uncertainty in the aperture variation on the local scale is large, the uncertainty of the flow path characterization will also be large, and thereby defeating the purpose of the proposed methodology. From what is presented here a rather simple conclusion may be reached, if CT(mat) varies locally and significantly then the estimated sub-voxel fracture apertures determined using the MA method may be far too uncertain to be of use in characterizing flow path geometry. Of course, this conclusion could be reached without the investigation presented by the authors. Unless the above outlined concerns are addressed the investigation presented by the authors amounts to a computational exercise that is likely disconnected from the system of interest by non-trivial uncertainty.

Page 6, lines 11-12: The authors should show that five averaged scans reduces noise in comparison to a single image, how much does it reduce noise? Is it significant?

Page 6, lines 17-18: How is the resampling performed? Are the CT number values of the resampled voxels equal to the imaged voxel? Also do you mean one voxel to 16 voxels?

Figure 4: The linear calibration used was matched to known apertures (by use of spacers) that are far larger than those inferred for flow simulation, casting doubt on the extrapolation of the calibration to the apertures inferred.

Page 7, lines 30-32: The authors state that apertures affect 2-3 voxels, but assume only a single voxel measurement is necessary to obtain the aperture. Isn't this information loss? Wouldn't this have an effect on the estimated fracture aperture? Do the 2-3 voxels reference resampled or original image voxels?

Page 7, line 34: The authors state that the MA method works well. Based on what evidence?

Page 8, lines 11-13: Yes, higher porosity should reveal lower CT numbers, if the composition does not affect the CT numbers.

Page 8 lines 10-15: I am confused by this paragraph, are the authors suggesting that the CT numbers of the matrix can be used to determine the pore sizes? Given the matrix has a permeability several orders of magnitude smaller than the fracture, the flow in the matrix can be ignored. Flow in the matrix is then discussed on pg 10-11: Are the authors suggesting that there is significant flow in the matrix? The permeability assigned to the matrix using the non-sequitur method outlined on page 8 are arbitrary, CT numbers do not provide any information beyond an estimate of porosity if one assumes CT(local matrix) is known.

Page 12 lines 31-32: The authors state that "significant material heterogeneities appear to be negligible for the sandstone," which does not follow from the discussion on the significant variation in permeability that the simulations predict with small global changes in CT(mat). This is the opposite of negligible. Page 13 lines 9-10, the authors state that the simulations are unable to reproduce the measured permeability hysteresis. This statement certainly follows from the results, and indicates major issues with the practicality of the proposed methodology.

Page 12 line 13: The authors state, “one may assume stress-dependency of CT(mat)”, why? The densities might change slightly due to compression, but the strain is likely to be very small, and the resulting effect on CT number is likely to be indiscernible.

Page 14-15 bulleted recommendations: None of these recommendations follow from the study outlined in the manuscript, instead they are generalized advice that may be useful, but is disconnected from the study. For instance, bulleted point 1, I agree pore-scale studies of heterogeneous porous rocks may be valuable, but this was in no way investigated in this manuscript. Again, bulleted point 2, I agree to better understand the effect matrix permeability has on fluid flow in fractured porous rock one may want to measure the matrix permeability before inducing a fracture, I don't think anyone would disagree. And, yes it would better to have higher resolution measurements of fracture roughness and aperture distribution.

After reading this manuscript it appears I would have been better off simply reading Huo and Benson, or the Watanabe papers, which are extensively referenced in lieu of analysis in this manuscript.

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