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Interactive comment on "The hydraulic efficiency of single fractures: Correcting the cubic law parameterization for self-affine surface roughness and fracture closure" *by* Maximilian O. Kottwitz et al.

Anonymous Referee #2

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This manuscript addresses the hydraulic behavior of geological fractures using a numerical approach. The authors perform low Reynolds CFD inside synthetic fractures and analyse the impact of the fracture closure R and Hurst exponent of the fracture walls, H, on the fractures' permeability. They introduce an effective surface area S which accounts for the change in surface area when changing the Hurst exponent H, and therefore replaces H in the parametrization. They then compute the mean behavior of a large population of geometrically equivalent fractures (i.e., fractures generated with the same geometrical parameters) as a function of R and S. They also discuss how the

value of the correlation length impacts the variability of the hydraulic efficiencies within the population, and show that for the smallest investigated correlation length that variability is negligible. In this case they fit an analytical formula to the dependence of the hydraulic efficiency on R and S and propose that this formula be used as an analytical model in reservoir scale DFN models.

Few studies have so far examined the role of the correlation length, though a previous work (using a Reynolds equation based approach) has shown that it strongly impacts the hydraulic behavior of fractures, but also the variability of hydraulic efficiencies of fractures of identical statistical geometrical parameters. In addition, few studies have addressed systematically the impact of the Hurst exponent. The paper is well written and easy to read. The methods are sound and the interpretation overall convincing. I therefore recommend publication.

I provide below a number of comments which may have to be addressed prior to publication.

1) Main comments:

* Large closures and percolation analysis:

I don't think it makes much sense to investigate closures R much larger than 1. Indeed, the hypothesis of perfect plastic closure (overlapping regions just disappear) is not too bad for configurations in which a moderate proportion of the fracture plane is closed (i.e., for $R <\sim 1$). But for larger closures one would expect the real geometry to be significantly different from that obtained with this crude approximation.

I turns out that the results relative to the hydraulic efficiency are shown only for $R < 1$. The study of percolation, on the contrary, is only interesting for $R > 1$ since the percolation probability starts taking values strictly smaller than 1 precisely for these configurations of large closure ($R > 1$). This section is therefore, in my opinion, rather irrelevant. I would suggest to remove it.

* Figures 5 and 6, and the corresponding discussion (pages 10 and 11):

Instead of showing a plot that is interpolated from the raw data using a Matlab function whose principle is not explained and whose parameters are not given, I would suggest that the authors perform their own box-averaging to show local mean values of S as a function of S and R, but also that they also provide similar information for the fluctuations of the statistics, for example in terms of the standard deviations of values within various (R,S) ranges. Such a figure coud be added following the model of Figure 5, and would complement it.

In a way the information provided in Figure 6 contains this type of information, but in a less straightforward manner, and though the interpretation provided by the authors is correct, the choice of words matters. This is not about the "accuracy of the presented model", this results from the fact that the model corresponds to the average behavior of a population, and that fluctuations in hydraulic are found within the population. These fluctuations are all the larger as the correlation length is larger. The model may be very accurate for the average behavior (and probably is). And the authors could provide a model for the standard deviation around the mean behavior by fitting the data of the figure I am suggesting above.

Similarly I think that the wording used in the sentence of page 15, line 276, is misleading when mentioning "a prediction error of 26.7%".

* I am not quite sure I fully understand how the test on the accuracy of the numerical solution is done.

Firstly, the notion of "uncorrelated part of a fracture" is strange to me, as the uncorrelated vs. correlated feature is a question of scale rather than location. Perhaps it is simply a question of formulation. Similarly, the sentence of line 228, "16 subsets are drawn that focus on the uncorrelated parts of the fractures that corresponds to ..." is not clear to me.

* Last sentence of the paper: "This parametrization could easily be incorporated in a DFN modeling framework to investigate the hydraulic response at reservoir scales".

Yes, it could. It could be interesting to substantiate, though, for two reasons.

First, this model is obtained for fractures of correlation length L/16. Does it still hold whatever the correlation length if it is smaller than L/16 ? If not the model could only be used in models of fractured reservoirs for which all fractures exhibit a ratio L/lc = 16.

Second, the hydraulic behavior of DFN of rough fractures is not necessarily properly described by that of a DFN of parallel plate fractures of suitably adjusted apertures. There can be coupling between fracture scale heterogeneity and network-scale heterogeneity, that is, fracture scale flow heterogeneity can in some cases modify the flow connectivity at the network scale. However de Dreuzy et al (JGR 2012) have shown that this can only occur if the correlation length is not significantly smaller than one or two tenths of the medium size. At reservoir scale this is clearly never the case. But this is not trivial and could be discussed.

2) Various comments on other points along the text:

* The introduction is rather short, but logically organized, and provides a proper summary of the state of the research on the topic so far. The authors use an approach relying on a large statistics of fracture with identical. They could mention that the first approach of this kind was proposed by Méheust and Schmittbuhl in a JGR paper in 2001, studying populations of synthetic rough fractures with self-affine aperture fields (that is, for lc/L=1).

* In the presentation of Eq. (1), the hypothesis of permanent flow is missing.

* In Eq. (6), the mathematical notation is strange: a is used both for the aperture field prior to negative values being put to 0, and for the the aperture field whose negative values have been put to 0. Of course when coding one may use the same variable name, overwriting the previous variable a, but mathematically they are two different

quantities.

* Page 5, line 104: "leaving H as a measure for the intensity of small scale roughness".

This explanation is a bit caricatural. H is rather a measure of the ratio between larger scale roughness and smaller scale roughness (the ratio being always larger than 1 since h>0, but all the smaller as H is smaller).

* Equation (8): some authors choose to divide the standard deviation of the aperture by the mechanical aperture, which is the mean aperture prior to putting negative apertures to 0 and thus corresponds to the distance between the mean planes of the facing topographies. Is there a particular reason why you chose to use the mean aperture ?

* Page 4, line 113: why don't you express the condition of contact in terms of R (R >= 1/(3\sqrt{2}) ?

* Page 5, line 121: It seems that a simple way of presenting S would be as the ratio of the fracture surface's area to twice that of its projection on the fracture plane.

* Page 6: was the conservation of the total volumetric flow rate tested ? What are the relative flow rate fluctuations between all sections transverse to the mean flow ?

* Table 1: It would be interesting to have the mininum and maximum values of R in the table.

* Page 8, line 184-185, about the inset plot: the contact fraction is only controlled by the PDF of apertures prior to setting negative values to 0; that PDF is mostly independent of lc/L (though if one looks closely one may find a slight dependence), and therefore only dependent on the fracture closure. This is well known.

* Page 12, line 225: Here you probably mean "perpendicular to the fracture plane", i.e., the vertical direction if the fracture is horizontal.

* Equation (8): in this equation, it seems that the norm is simply the absolute value of

the relative error. Why use a square inside a root mean square ?

* Discussion of page 14: here you could mention that the lower the value of lc/L, the larger the impact of the vertical flow tortuosity on the fracture's permeability.

* Page 15, line 262: "correlation lengths that are equal to the size of the fracture .... seem rather unrealistic".

The origin of the correlation length is not generally known, is it ? Is it mechanical ? A fresh fracture without shift along the fracture plane would present a constant aperture field, one with a shift of length l would have a correlation length lc = l in that direction, but then the aperture field would be anisotropic.

* Page 15, line 269: Here and elsewhere I would use "parallel plate equivalent" (which refers to the geometry) rather than "cubic law equivalent", which involves a hydraulic concept. The two fractures are equivalent in that their mean apertures are identical (a geometric feature), not in that their hydraulic behavior is the same (this equality defines the fracture's hydraulic aperture).

3) Writing:

The paper is overall very well and clearly written. Here are a few corrections that could be made:

* Shouldn't the vectorial quantities (including \nabla) appear in bold fonts ?

* Page 4, line 100: I would call the "rescaling factor" simply a "prefactor".

* Page 6, line 137: ")" should be removed after "0.01 Pa".

* Page 6, line 145: here I'd write "with η the fluid's dynamics viscosity".

* Page 8, line 176: I think "build" should be "built" here; please check.

* Page 12, line 219: "multiplied by" rather than "on".

* Page 12, line 225: "the resolution perpendicular to the flow direction".

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