Dear Professor Rajaram,

Thank you for your email. While the outcome of the review is regrettable, we very much appreciate the enclosed reviews; they are very constructive and indeed helped us to improve the manuscript.

Pertaining to the review from Reviewer 1 we feel that Reviewer 1’s main critique would hold true if we were presenting work on fracture networks in impermeable media. Indeed, we are fully aware of concepts applied in this context with respect to hydraulic backbone of the network etc. However, for a permeable rock matrix the role of the hydraulic backbone is – as far as we are aware – unclear. This motivates our research. Obviously, we should have pointed this out more prominently to avoid the confusion. This aside, we feel that the revision requests are rather minor. In fact, we have already revised the manuscript accordingly.

1. Issue of novelty: As you pointed out, greater detail is required in the manuscript. Admittedly, we did not adequately delineate our work from previous approaches. In particular, we should emphasize that we are focusing on fractured porous media where flow occurs in both fractures and host rock at different timescales. This is different from the case of flow in a fractured but impermeable media, which the reviewer drew many concepts from in his/her feedback. We have updated our manuscript to highlight this better (Lines 51-124 in ‘Updated Manuscript.pdf’).
2. Lack of sufficient detail: The reviewer pointed out correctly that the Effective Medium Theory (EMT) was poorly defined in the manuscript. We are glad to improve on this (Lines 169-192 and 273-280 in ‘Updated Manuscript.pdf’).
3. Problematic assumption: The reviewer challenged our use of a linear relationship between fracture size and aperture and suggested that randomly assigning apertures will likely lead to different results. While we did not consider this exact approach, we previously did case study with constant apertures and reached the same findings as our other studies. We chose not to include this in the manuscript as we felt that a de-correlated aperture-size relationship is unrealistic. Instead, we provided two extra case studies with square and square root relationships to show that the findings are undisturbed by the choice of correlation. If needed, we are glad to amend the manuscript to include the constant aperture case study. Please see ‘Case 14.docx’ for the results of this case study.  
     
   Two related issues raised were with regards to the aperture and size ranges of the DFNs used. Our aperture range is designed to be around 1mm. This is based on the order of magnitude studied in a paper by Bisdom et al. (2015). We have clarified this in the manuscript (Line 144). In terms of the size range, our goal was to study hybrid modelling in the context of a polydisperse fracture network which can follow any size distribution. While 10-40m is a small range, we feel that this does not invalidate the use of a power-law distribution. We have updated our manuscript to cite Ebigbo et al. (2016) who used a power law distribution on a size range of 8-40m.
4. Various Small comments: The scale of the simulations is now included in the manuscript. While the 3D DFNs have orthogonal fracture sets, we have also studied 2D realistic outcrop trace maps that do not exhibit the same orthogonality. An illustration of the FSU approach is now provided in the supplementary information (See Figure S4 in ‘Updated Supporting Information.docx’). Figures have been enlarged. Well known acronyms like DFN, EDFM, DFM are retained. NFR, EMT and MRST have been removed. The only author defined acronym left is FSU.

With respect to Reviewer 2, we are disappointed that you consider his/her comments not substantive. The accurate summary shows that he/she has fully understood the manuscript and just has “no particular critical remark”.

In consideration of the additional elaboration and case study provided, we are hoping that you might revisit your decision to decline our manuscript.

Regards,

Daniel