**Reviewer 1**

The paper studies the creation of viscous fingering using the CVFEM with different element pairs and also dynamic mesh optimisation. I think the paper is very interesting by addressing a very complicated effect as viscous fingering with different numerical methods and formulations while also giving a very good background on the work done by others and I will strongly recommend its publication after some important modifications to the paper, as in its current state it looks like a draft. Therefore, my comments are towards improving the quality of the text and presentation more that the content itself, which I consider good enough for publication.

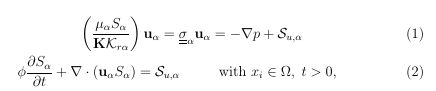
Major comments:

- In the introduction it first appears what I consider to be one the biggest of the paper. They try to explain too many mathematical concepts but they realise that it would take too much effort and they leave it unfinished. What does it mean, for example, “The dual pressure and velocity fields are represented simultaneously in FE and CV spaces”? More importantly, does it matter to the “Numerical Investigation of Viscous Flow Instabilities in Multiphase Heterogeneous Porous media”??? Isn’t it better to send the interested reader that want to know more about the discretisation to the discretisation paper of the method as you already mention in some parts of the paper? I would like to see a paper more focused on what is presented and less diffuse.

The above sentence has been removed to avoid any confusion, section has been reviewed.

- Equation 1. The permeability K is a tensor as far I understand, and therefore if you want to divide by it, it has to be K^-1. Also in equation 1 you say it is define for t > 0. However, in the current state that equation does not depend on time and therefore that assumption is unnecessary.

Equation 1 has been corrected and the time has been removed.



- In the paragraph after equation one, you say S and S\_alpha are this and that. However in the equations you have S\_u,alpha and S\_cty,alpha. This needs to be corrected.

The term Sualpha is the source term of the equation. Both of the aforementioned quantities have been corrected.

- In section 2 you mention that high-order fluxes are limited on CV boundaries, which are limited to yield bounded fields. Again you mention something without enough detail, you say that the saturation is bounded between 0 and 1. This is clearly not enough to guarantee stability of a high-order flux, as starters because if the irresidual oil and water saturation are non-zero then a saturation of a phase of 1 or 0 would be unphysical and therefore wrong.

JG comment?

- You mention in section two that you use P1DGP1 and P1DGP2, however later on you use P1DGP1DG. This leads to two questions: what have you used finally? And second why use an unstable element pair like P1DGP1DG? In Gomes et al 2017 they do not use P1DGP1DG.

P1DGP1 and P1DGP2 were used for the 2D cases as these are listed up to figure 16 while the P1DGP1DG was used for the 3D case onwards. Unlike the 2D cases, the P1DGP1DG element is the one using a linear discontinuous Galerkin approximation for velocity.

- Velocities within and between elements is not described in Jackson et al 2013, as they only use classical CVFEM in that paper. Moreover, in Gomes et al 2017 they explain that they use two different methods for within elements and between elements. In the same paragraph you talk about sigma instead of sigma with two bars and bold, this needs to be corrected. Again this part is not even necessary in this paper, as it confuses from the main objective of the paper.

The reference has been corrected as well as the variable but the sentence has been removed as instructed.

- The mention that the discretised global mass and force balance equations are solved using a multigrid-like approach as described in Pavlidis et al. is not correct as this is not described in that paper. Again this statement feels unnecessary in this paper.

This part has also been removed as instructed.

- In section 4.4 you present and give a lot of information about dynamic mesh optimisation. I think that all this background information should not go in the results section, but in the introduction or the methods section.

This is the paragraph marked in red that I suggest to be totally removed.

- Section 4.4 there are a couple of references to Figures with question marks.

Both of these references have been corrected.

- Figure 12, check the scale of the permeability.

Like the rest of the figures in the text the units of permeability are cm2 and have been corrected. The permeability we are using in this case is synthetic permeability unlike the rest of the permeability values that are based on realistic values.

- Currently the sizes of many numerical test cases are missing, this should be included to allow for reproducibility.

JG comment?

Minor comments:

- In the abstract a “-” is used but it is not closed with a “-”. A comma should be used instead as it is a clarification and the first level for clarification is a comma. If the authors insists to use a “-” then for consistency the sentence should be closed with a “-” as well.

The “-” has been removed and the sentence has been corrected/rephrased to “Due to large viscosity ratios, flow instabilities at fluids' interface may arise leading to the formation of fingers therefore creating an uneven front with elongation at the outside edge of fluids interface with strong impact on the displacement efficiency”.

- It has struck me the fact that despite using dynamic mesh optimisation this does not appear in the title. The authors should consider giving more emphasis to the use of dynamic mesh optimisation for fingering processes.

Dynamic mesh optimisation is not part of the objectives of this work but part of the techniques currently used to run the simulations/cases as these are presented.

- In the introduction the authors present a problem with classical CVFEM leaking to low permeable regions. Michael Edwards (Edwards, M.: Higher-resolution hyperbolic-coupled-elliptic flux-continuous cvd schemes on structured and unstructured grids in 2-d. (2006)) have done a good job presenting this problem and I believe it would be useful for the interested reader in pointing to this work. What surprised me is that the authors are very humble by not presenting their own solution to this problem. In Gomes et al 2017 I believe that they present their solution as well and I think they should present it as well in this paper.

JG comment?

- You seem to imply that the classical CVFEM requires IMPES, after the citation of Voller. This assumption is wrong and needs to be corrected, or better explained.

The above sentence has been removed.

- In the introduction you cite the work of Jackson et al. 2014 and later on of Jackson et al 2013. I have checked and I think that the correct citation is actually Jackson et al. 2015 in all the cases.

The correct paper has been used and referenced accordingly.

- Section 3 seems like a proper introduction. As a suggestion the authors may think in merging this section with the introduction to also reduce the size of the paper, which is currently “threatening”.

JG comment?

- In section 3 you mention that MR can be reduced to VR. However, it seems that all the studies have been done for MR, why is this reduction done? Maybe this needs to be explained better.

The MR contains the phase saturation while it is clear that during fluid displacement phase saturation, will change in time and space. Therefore, with no lack of generality, the MR can be reduced to the viscosity ratio (VR) as this is also one of the parameters that we are changing while we run the simulations.

- In section 3 again, after explaining in very detail that MR controls fingering you mention all of a sudden that the Peclet number is also important. I believe that this should be presented earlier.

The Peclet number has been removed and the paragraph has been rewritten as:

“In immiscible displacements, viscous fingering occurs when the viscosity ratio is greater than unity. As surface tension becomes weak, the interface is stressed and becomes unstable leading to the formation of fingers. The interface of the main finger collapses and starts splitting into new lobes of fingers. One of these new fingers may eventually outgrow the others and then spreads to occupy an increasingly larger width. In the process, the finger reaches a critical width while the saturation of its front becomes steep as a result of stretching caused by the cross-flow, causing the tip of the finger to become unstable and splits again, and the pattern repeats itself.”

- At the end of section 3 you present what is going to be presented in section 4, like it is normally done in the introduction. As I said before section 3 feels like a second introduction.

JG comment? Should we somehow rephrase it?

- In section 4, again you mention that you don’t use P1DGP1DG, but later on you do. What should I believe?

The P1DGP1DG element is used in section 4.5 for the 3D case.

- In section 4.1 you mention that gravity is zero and the fluids are incompressible. What about Capillary pressure? It is important to mention if this is actually simulated or not, as it has a key effect in stabilising the growth of the fingering processes.

In the simulations demonstrated here we are simulating the saturation front of two immiscible fluids. According to the frontal displacement theory a method for calculating saturation profiles on the basis of the relative permeability, assumes that the effect of capillary pressure between the two fluids and the gravitational effects are neglected. The advance of a saturation front by displacing fluid is largely affected by the permeability of the two fluids (e.g. oil/water) relative to reservoir rock, and to the mobility ratio between the two fluids.

- In 4.2 you present the sizes of the 2D domain in dimensionless units. If one unit is dimensionless then all of the parameters used should be dimensionless. Take a decision on this and stick with it, but currently the permeability values and times in seconds are meaningless.

The units are corrected and also introduced when it was needed (figures and tables).

- In 4.2 you mention that the pressure gradient is discontinuous across the interface. As far as I understand the pressure in porous media flow is never discontinuous. Moreover, in this case you are using a formulation with continuous pressure, so the discretisation used would not allow to have a discontinuous pressure.

JG comment? “The spatial permeability distribution creates a rough pressure field, \ie pressure gradient is discontinuous across the interface between different permeability zones…” – is there something here that we are saying in a wrong way? We both talking about the interface.

- Table 1. You are presenting here 6 columns of constant properties and the permeabilities are using a wrong format for the numbering as this is a document and not a computer code, the exponents should be written as 10 to the power of whichever number in superscript format. This should be reconsidered.

The table has been corrected according to the recommendations above following the corrected format.

- Figure 2 (A) is redundant; P1DGP2 is presented in better detail in Figure 1. I say Figure (A) because you mention triangle A and B while there are no letters below the figures. You should either add letters or reference to the different figures with right and left.

The figure has been corrected and letters A and B have been put below each of the figures.

- In some figures you mention that some boundaries have no slip condition. In porous media flow you cannot have boundary conditions with slip condition so this information is unnecessary.

The above boundary condition has been removed and has been corrected with the no-normal flux boundary condition.

- The scale in the figures should be with a white background, there is no need to waste ink! ( In some figures you have done this already)

**Reviewer 2**

Title: Numerical Investigation of Viscous Flow Instabilities in Multiphase Heterogeneous Porous Media

Authors: Christou et al.

This paper presents an interesting study on the numerical simulation of formation and growth of viscous fingers in heterogeneous porous media. The model is based on a control volume finite element method (CVFEM). 2D and 3D cases have been considered in this study and the dynamic mesh adaptivity is presented in the simulations. The paper is a potentially valuable work to be published in ADWR. However, I would like the authors to address the following concerns prior to acceptance for publication:

1. The multiphase porous media model with CVFEM has been published in several papers by the authors and their collaborators. The authors should discuss the novelty of their paper and their contribution in this work.
2. I found that most of the paper is about numerical simulation with different set up without detailed discussion on the physics. As there is nothing new on the numerical aspect (as their method has been published in several papers), I would encourage the authors to focus on the physics of the problem here.

JG comment? Both 1 and 2 asking about physics but as far as the physics we have already described the mechanisms and how the fingers are developed and spread in the control volume. What am I missing?

1. For the model validation, there is a lack of quantitative comparison with any analytical solution or experiments.
2. There is no convergence study for fixed mesh in this paper. As the viscous finger is very sensitive to the mesh as pointed out by the authors, this should be included in the results and discussion.
3. The dynamic mesh adaptivity is used in the simulation. However, there is no discussion about the criterion for the mesh adaptivity.

JG comment?

1. In section 4.5, the discussion mentioned that the simulation was using P1DG-P1DG element-pairs. However, the results for the continuous saturation P1DG-P1 are shown in the figures. The authors should include the results with the discontinuous formulation.

JG comment? I am bit a confused here, what is that we should include? I mean, we have the results for the 3D case using the P1DG-P1DG element type and we have include a comment about the P1DGP2 element type in the last paragraph.

1. The symbol s\_ua is in Eq. (3) is different from the symbol in Eq. (1).

The above symbol has been corrected and explained.

1. In Figure 13, looks like the figures (e) and (f) have been put in the wrong position.

Both of these two pictures have been corrected and been put in the right order.

**Reviewer 3**

Review of ADWR-2018-119

1. The contribution of the paper is the use of a high order CVFEM technique with adaptive grids to solve unstable immiscible flow in heterogeneous porous media. I believe, the numerical method is excellent, but the physical problem solutions need improvement.
2. In standard nomenclature, Hele-Shaw cell is an open gap between two plates, not a porous medium between two plates. I believe, the authors are solving flow through a thin porous medium in most of the problem except in section 4.5. These problems should not be called Hele-Shaw cell problems.
3. Eq. 5 (Young-Laplace eq.) applies to open gap Hele-Shaw problems across interfaces. It applies to a single pore in porous media; but does not apply to macroscopic equations in porous media. In porous media, the saturations in macroscopic equations do not go sharply from phase 1 to 2. What we develop are diffused saturation fronts. Eq. 5 does not apply across these fronts.
4. For porous media, one needs relative permeability terms as well as macroscopic capillary pressure terms as a function of saturation. What capillary pressure function was used? Was it neglected?

Capillary pressure is a non-linear function of saturation which makes the governing parabolic equations highly non-linear. Effect of capillary pressure in immiscible porous media flows is similar to the effect of surface tension in Hele-Shaw flows of two immiscible fluids; Also, conditions under which such capillary slowdown of instability can be enhanced are desirable in many applications. Such conditions, if they exist, may not be easy to find (They can be found in a case of enhancing oil recovery see, P. Daripa, J. Glimm, B. Lindquist, and O. McBryan, Polymer Floods: A Case Study of Nonlinear Wave Analysis and Instability Control in Tertiary Oil Recovery, SIAM J. Appl. Math., 49 (1988), pp. 353-373.).

1. The application of no-slip equation on the side boundaries is incorrect for porous media flow.

The above boundary condition has been corrected to the no-rormal flux BC.

1. Eq, 10: What is the value of b? Assuming b=1 mm, Nc’ is too high. Nc >1 in the problems solved. Typical Nc in water flood of reservoirs is 10e-7.
2. Velocity of the order of 1 cm/s is too high.

JG comment?

1. Describe the term Su**α**, in Eq. 1.

The term has been described and corrected

1. Fig. 4b looks OK, but Fig. 4c looks strange for an immiscible flood in porous media. Show a comparison with experimental figures of Dawe & Grattoni (2008).
2. Consider adding the following references: -Doorwar, S. & Mohanty, K. K., “Extension for Dielectric Breakdown Model for Simulation of Viscous Fingering at Finite Viscosity Ratios,” *Phys. Rev. E.*, 90 (1) (2014). DOI: 10.1103/PhysRevE.90.013028

Doorwar, S. and Mohanty, K. K., “Fingering Function for Unstable Immiscible Flows,” SPE 173290-PA, SPE Journal, (July, 2016). http://dx.doi.org/10.2118/173290-PA.

Luo, H., Mohanty, K.K., Delshad, M., Pope, G. A. (2016). Modeling and Upscaling Unstable Water and Polymer Floods: Dynamic Characterization of the Effective Viscous Fingering. SPE 179648-PA, SPEREE, accepted October 6, 2016