**Reviewer #1:**

In overall, the manuscript is well written. I would recommend this manuscript for further consideration if the authors make the following revisions:

1. The nomenclature should be consistent with SPE nomenclature, for example should be used for velocity instead of , should be used for length instead of .
2. When CO2 flow in the fracture and in rock, there are more than one phase because formation fluids also flow, hence the formulation should be for at least two phase.

Why two phase flow? In fracture we suppose the CO2 is in the supercritical sate, then almost most part of fracture will be filled by fluid

How about inside of reservoir?

1. Please use a consistent color set for all figures, scale in some figures changes white to back while others change from blue to white.
2. Please include a short paragraph to show how d and (Eq. 9) are determined?

Check references….

1. Please use more close to reservoir condition for input data. The tensile strength in Tab. 1 is very high for the rock with that Young's modulus. The initial pressure is rather too low.

Check the Wang et al. (2017) paper

1. Please include all necessary inputs for the simulation to allow other people to duplicate the job if needed.
2. There is no data related to in-situ stresses. Although, they are not in the equation but they are the boundary condition.
3. More numerical simulation or case studies may be needed and a comparison with other models may be useful to show the innovation of this model.

How about interaction of two fractures

1. We need more section to discuss the advantage of the model compared to other approaches.

**Reviewer #2:**

This paper presents a very novel approach to model the CO2 fracturing process. I think this topic is very new and definitely worth digging into. The authors have presented enough details about the fundamentals of the solution, and overall I think this paper is very well laid out. I have following minor suggestions for further improvement:

1. One main concern is that this paper seems too mathematical. The layout of the manuscript could benefit from more description about the application of the methodology.
2. The validation is not very clear to me. The author presented three verifications, but are they trying to prove the validity of the proposed method? Honestly I didn't recognize the method that was used for validation. Is it a well-established analytic solution, or results from well-established simulation? I would strongly recommend re-write the validation part.
3. What is the limitation of the current method? It is not very clear to me that whether this method is application to traditional water based fracking or not. And if so, what is the advantage of the current approach?

**Reviewer #3:**

The paper tries to use phase field method to model CO2 fracturing. Some assumptions used in governing equations are not supported with the theory of poroelasticity. Hydraulic fracturing or CO2 fracturing involves strongly coupled processes. But the authors verify their model through non-coupled examples. The coupled behaviors about pressure and aperture evolutions are not demonstrated. This makes the correctness of the model in doubt. I recommend resubmission of the paper after the model is correctly verified through asymptotic analytical solutions for hydraulic fracturing. Without correctly verifying the coupled model, I cannot recommend the acceptance of it. The followings are a few comments:

1. The authors used a phase field depended permeability in their study. The permeability should be determined by the opening or close of fractures. Why could a damage variable be used to determine permeability? The phase field value is distributed over a range, however, a facture creates jump in pressure and displacement. Why could a continuous variable be use to represent discontinuous behaviors, especially for permeability?

A basic question about phase field

1. Eq. 10 is not correct; which casts doubt on the whole sequentially coupled process. The treatment of porosity in Eq. 10 conflicts with the theory of poroelasticity. Change of porosity is not equal to the change of volumetric strain, not even in an approximate manner.

Sheng 2012, but there is no other reference!

1. Could the authors give the spatial and temporal discretization in appendix? Since the weak form is given already, spatial discretization is only one step away. I doubt the spatial discretization for a poroelastic medium could be derived from Eq. B1b or Eq. B2b. Though it is possible that the poroelastic model is ready for use in FEniCS package, the authors are suggested to provide the completely discretized formulations for the benefit of readers.

Yep

1. Fully coupled examples are needed to verify the model. Correctly verifying a tensile test and the pressurization of a fracture do not indicate the model can correctly simulate hydraulic fracturing or CO2 fracturing. The verification about pressurizing a bore hole is not a good example to show poroelastic responses. Actually, no typical poroelastic responses are shown in the example. Mandel's problem is suggested.

We have done it in appendix

1. Please briefly explain the AT1 and AT2 model.

Bourdin 2018

1. line 1-2 Page 1 Are you sure shale or mudstone is the most common sedimentary rock?

We can edit it

**Reviewer #4:**

The authors have proposed a model for CO2 flow and fracturing in shale media. The manuscript has a good order, but needs revision to satisfy publication quality.

1. Gas flow in shale is one of the most challenging topics and has been widely investigated. The authors have used a relatively simple model for calculation of gas flow and permeability in shale media. A good model will capture important phenomena like Knudsen Diffusion and adsorption effect in shale rock media. *Please modify this part of your model by providing a more holistic and detailed explanation*.
2. Please refer to series of papers by Javadpour et al. Also see: Seyyed A. Hosseini et al. "Novel Analytical Core-Sample Analysis Indicates Higher Gas Content in Shale-Gas Reservoirs" SPE Journal 2015.

\textit{page 2: Please cite other methods for crack propagation such as phase-field

and a recent paper by Irzal et al. in CMAME in 2013.}

We have included a phase-field method by Bourdin et al.~and the mentioned work in the bibliography as references [7] and [8], respectively.

\textit{page 5: (1a): minus sign missing}

We have assumed no body force, in accordance to the KGD geometry. Hence, there is no difference between $\divergence\bm{\sigma} = 0$ and $-\divergence\bm{\sigma} = 0$.

\textit{page 3: Why a nonnegative pressure field?}

We are aware of the different equivalent conventions of defining the pressure field, and the community of hydraulic fracturing modeling usually adopts the net pressure. We have chosen to use the fluid pressure which is always nonnegative both for the physical meaning and for numerical convenience. We have added a clarification in the introduction.

\textit{page 10 on top: It is not necessary to explain the meaning of the arrows.}

Thanks for the comment. We have removed the explanation.

\textit{page 31: Caption of Figure 12, last sentence: This is a very important

observation. Please refer to the page number, where

exactly the [discrepancy] is described.}

Thanks for the suggestion. We have added the location of the description in now Figure 15.

\section{Comments from Reviewer \#2}

\textit{This is a very interesting piece of work. Ultimately the problem that I have is with the comparison to prior work. The author is a co-author on reference [6] and so he should have direct access to those numerical results such that a more quantitative comparison can be made. I think that greater care needs to be taken in this comparison. Clearly there are differences in the results but there is no discussion of why these differences arise. The convergence study results shown in Table 1 do not seem to demonstrate convergence. For a given time increment mesh refinement leads to contact times of 1.875, 1.875, and 1.833 for the most refined mesh. What are these results converging to? Then, for a given mesh refinement of a0/h = 100, decreasing the time increment leads to contact times of 1.875, 1.667, and finally 2.115. So again, what are these results converging to? }

Thanks for the suggestion. We have re-examined the method and made a small but important change to it by removing all the characteristic functions like $\chi\_{\Gamma\_l^k}$ in the formulation. The resulting method converges very well, but it has a less-ideal performance in conserving mass.

\textit{Due to the differences in the fluid lag assumptions, the comparison in Figure 12 is not a good one. I would recommend making a comparison to the analytical solution for a case where the fluid lag assumptions are in concert. Otherwise you are comparing apples to oranges. Again, overall I think this is a good piece of work, but the details of the demonstration of the method need to be a bit more polished before this should be published in CMAME.}

We agree that the difference is built in in the different assumptions of the lag. However, this is one of the advantages of our method in that it always gives a physical finite pressure solution. Moreover, we are not aware of analytic solutions based on the same assumptions of the fluid lag. So we keep the current comparisons.

\section{Summary of changes}

\begin{itemize}

\item We have re-examined the method and removed all the characteristic functions like $\chi\_{\Gamma\_l^k}$. The resulting method adopts the same solution methodology, but has better convergence property than the original method. Due to this, we have regenerated all the plots.

\item We have rewritten the Introduction from the third paragraph to the end to better motivate the work and to explain the non-negativity of the pressure.

\item We have substantially rewritten Sections 3.3 and 3.4 so that the reader can follow the discretization procedures.

\item We have substantially rewritten Section 4.2 to explain how we propagate the fracture following Griffith's criterion. In particular, we have added Algorithm 1 to explain the procedure and Figure 5 to illustrate some special case in solving for the fracture tip increment.

\item In Section 5.1 we have added a figure to explain the configuration of the numerical example.

\item In Section 5.3, we have put forth the convergence study before the comparison with other methods.

\item In Section 6, we have added a discussion of the applicability of the proposed method and future studies.

\end{itemize}