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Comment on se-2021-1

Anonymous Referee #2

Referee comment on "An Analytical Framework for Stress Shadow Analysis During Hydraulic Fracturing – Applied to the Bakken Formation, Saskatchewan, Canada" by Mostafa Gorjian et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-1-RC2, 2021

The manuscript:

An Analytical Framework for Stress Shadow Analysis During Hydraulic Fracturing – Applied to the Bakken Formation, Saskatchewan, Canada

It is well written (as far as my English grammar knowledge allows) and deals with an interesting and up-to-date subject, that involves both the economic development of tight reservoirs and the environment protection in such activities (this might be a suggestion to the Authors, from the general point of view…).

Despite these premises, the manuscript in its present form in not suitable for publications.

It is based on the application of a series of equations that are difficult to be understood (and to easily justify). Results produced by the application of the proposed equations are then compared with well results though time (or space?).

Among my perplexities, here are some substantial ones:

The Authors should make explicit how did they obtain the proposed equations (1-9) from the cited reference (Pollard & Segall 1987). This is important, since it is the base of the work presented in the manuscript.

The meaning of the term “Stage” is not enough explicit: does it refer to different time of application (line 280, Fig. 14) or does it represent a distance measure (Fig. 13)?

Computations are compared with experimental results that, as they maintain, strongly depend from the time lag between “stages”. Yet the presented equations do not take into account for the time variable, with the exception of the Thermo-elastic model, where time is used as a mere computation of the amount of fluid injected (eq.12). Furthermore, the Authors demonstrated that this component is negligible in their computation.

On the other hand, the comparison between equations and experimental results deals with the relevance of the time lag between successive injection changes (stages, did I correctly understood?), that are not included in the used formulas.

The Author should make explicit how did they arrive to the simplified formulas (45-46) from the proposed full form. Did they just summed the results from each stage by ignoring the dissipation/interference between stages? These formula contains fracture dimensions: how were they determined? Were they extracted from the experiment data, and how (line 444-446)?

In my opinion, there is a general questionable point in their analysis: stress produced by fluid injection strongly depends on the rate of injection due to fluid viscosity and rock/fluid interaction (e.g. friction). This factor should be taken into consideration when computing produced stress and stress shadows. Cited Pollard models are based on a static approach, that is change in the fracture dimensions (i.e. L) is not considered during the computations, as they modify stress by the produced work, and geometry. And the prediction of enlargement of fractures is one of the goal of the presented work.

The computed average width of fractures with respect to their extension seems too large for the proposed properties (line 294-295 and Tab. 1), with a width/length ratio of about 5.7 / 162 = 0.035 that is about 3 times what observed in nature (e.g. Walsh results). The authors should compare and comment on this.

As far as what mentioned, the manuscript requires a significant improvement before entering in the stage of a detailed and complete review.