This manuscript entitled “Dissolution and precipitation of fractures in soluble rock” reports a numerical study of fracture evolution caused by mineral dissolution and precipitation. A 1D reactive transport model was developed to investigate the temporal evolution of fractures at different depths and in different soluble rocks. The study showed that fracture opening caused by mineral dissolution is highly dependent on the kinetical rate laws, and fracture clogging can be caused by precipitation of the dissolved mineral or secondary mineral in the downstream of the fracture. The study also showed that the evolution of fractures can be expedited by the presence of a coating or filling layer of different minerals.

This manuscript addresses scientific questions that are within the scope of HESS. The model framework and main findings are of interest to a broad audience. However, there are some issues to be addressed.

Major comments:

1. The introduction and literature review appear to be disconnected from the main focuses of this study. For example, the paragraph that starts with line 3 on page 2 elaborates on several modeling work, but it is unclear how this study is related to or different from those studies, except for the Kaufmann et al (2014) study. Moreover, while the literatures summarized in section 2.2 showed how fracture evolution is affected by different flow regimes and other factors, most of these factors are not addresses or are simplified in this study. For example, a drop of hydraulic head of 10 m was used in the manuscript, but it was not compared with realistic cases, and it is unclear why this specific flow regime is chosen. Furthermore, these literatures focus on small scale processes, while this study investigates field scale phenomena. Can the authors comment on how these literatures are relevant (or irrelevant) to this study and the scaling issue?
2. Some aspects of the model framework and parameterization need to be clarified.
    a. Page 4 line 10: ‘with a fracture roughness coefficient mimicking small-scale wall irregularities in the fracture’, is this roughness coefficient used in the calculation of reaction rate or flow or both?
    b. Page 5 line 13: what is the threshold Re used in this study?
    c. Page 5: f_l the friction factor for laminal flow was presented in eqn(3) but not used in eqn (1), was it used at all?
    d. Page 5: how is the wall roughness (w in eqn(3)) defined and determined in this study and what is the impact of this parameter?
    e. Page 6: eqn (5) is very different from the advection-diffusion-reaction equation, even if the diffusion term is excluded. Can the authors comment on this and clarify the underlying assumptions? For example
        i. Is steady-state assumed, although it appears not to be the case given the following results?
        ii. Is CFL criterion assumed to be one?
    f. Page 8/29 (table 1):
        i. Only one kinetic coefficient is reported for the calcite reaction, but three reaction pathways were listed in (6), can the authors comment on this discrepancy?
        ii. For the gypsum reaction, the kinetic coefficients for the linear and non-linear rate laws are about one order of magnitude different according the reference

cited, but the authors used the same kinetic coefficient, why and how the results may be affected?

iii. The texts pointed out that different parameters are used in precipitation from dissolution, it should be clarified in table one.

g. Page 9 line 17: step 6 what is the time step?
h. Given the strong dependence of the evolution profile on kinetic rate laws, some sensitivity analysis or discussion of the uncertainties of the kinetic laws and coefficients should be provided.

Minor comments:

1. Page 2: how is section 2.1 fracture widening different from fracture dissolution discussed in section 2.2?
2. There are a couple of typos. For example, Page 4 line 3 should be ‘Jones and Detwiler (2016)’, and ‘where’ should be ‘were’