

## ***Interactive comment on “Simulation of rock salt dissolution and its impact on land subsidence” by A. Zidane et al.***

### **Anonymous Referee #1**

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Paper Review for Hydrology and Earth System Sciences

Authors Zidane et al. have submitted the manuscript (ms) entitled “Simulation of rock salt dissolution and its impact on land subsidence” to Hydrology and Earth System Sciences.

The authors investigate flow and salt transport including salt dissolution within a fracture. To do so, a numerical model is developed and applied, which accounts for dissolution by moving domain boundaries (i.e. fracture interfaces) depending on the dissolution rate. Effects of salt dissolution are linked to land subsidence. It is concluded that subsidence rates are highest in regions above fault zones.

This is an interesting ms that I would like to see published. The idea of moving

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boundaries depending on salt dissolution is, to the best of my knowledge, novel. Discontinuous finite element schemes have been used before, but probably never to flow/transport/dissolution problems in a fracture. I do, however, see some technical inaccuracies and several weaknesses in how the test cases are explained and how results are presented. Details are given below in the General Comments section. For these reasons, I recommend acceptance of the manuscript with moderate revisions.

## General Comments

1. Introduction. Other publications relevant to the topic should be included in the introduction. Examples of a model that simulates flow/transport/dissolution/precipitation/variable-density/K-changes is in Graf and Therrien (2007b, Adv. Water Res.), and references therein.

2. Simulation domain. It is unclear to me what exactly the simulation domain is. On P58L25, it is said that only the “fluid” phase is simulated. You cannot simulate a phase, only processes that happen in a phase. Also, you probably mean the “liquid” phase. A “fluid” phase does not exist. Simulating processes in the liquid phase seems to be in accordance of what you say on P66L3f. However, looking at the domain definition (Fig. 1) and on results (e.g. Fig. 5) suggests that you are simulating processes in the Upper Aquifer as well. Is that so? This is very confusing to me because you are showing boundary conditions and fault zones that reach into the Upper Aquifer in Fig. 1, and you are presenting salt distribution in the Upper Aquifer in Fig. 5. The question therefore is, what exactly is your simulation domain, and does it include the Upper Aquifer (as Figs. 1 and 5 suggest) or are you only concentrating on the fracture in the Lower Aquifer (as you appear to state on P58L25 and P66L3f)? These questions also lead to the question of the transport boundary conditions in your model domain, which are never explained. “CH” in Fig. 1 should be replaced by “constant head”.

3. Truncation error control. Referring to P60L5ff, a more recent local truncation error control scheme for variable-density flow in fractured-porous rock was published by

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Hirthe & Graf (2012, Adv. Water Res.), which the authors should reference.

4. Mathematical terms of Eqs. (1) and (2) should be explained right after Eq. (2).

5. Mass fraction. Are you sure,  $C$  is mass fraction? The density equation in Eq. (4) suggests that  $C$  is relative concentration that varies between 0 and 1, while mass fraction would vary between 0 and 0.35. What is the exact meaning of  $\rho_1$  and  $\mu_1$ ? Fluid properties at what concentration, at relative concentration of 1 or at mass fraction of 0.35? There is a technical inaccuracy here, which should be resolved. If your code is actually using exactly Eqs. (4), the inaccuracy may actually be major. Please clarify.

6. Dissolution. You are simulating dissolution, but not precipitation. Dissolved salt may precipitate elsewhere downstream. It is necessary to comment on the effect of that simplification. Also, dissolution parameter  $\lambda$  does not seem to be temperature-dependent, which it really is. Again, the effect of that assumption must be discussed. For a reference of temperature-dependent dissolution and precipitation parameters, you may look at Graf and Therrien (2007b, Adv. Water Res.).

7. Section 3. Definition of the conceptual model should be improved (as said above), and the same is true for the presentation of results. Why do you need a pumping well at the WNW side? Are you actually pumping? If yes, this is not possible because your model is 2D. If no, why the pump? You can simply assume constant head, without mentioning the pump. Also, what is the physical justification for your choice of flow and transport BCs (the latter are not defined)? The flow BCs suggest flow from ESE to WNW, is that so? The flow physics of all 3 test cases must be explained much more clearly. It looks as though much detail is spent on the numerical explanations, and that the physics of the test cases is not explained as clearly. This must be improved so that the reader understand what is happening physically in the 3 test cases. The same is true for the presented salt profiles. An interpretation of presented salt profiles is mostly missing. There is much more science in your results, which should be developed more fully.

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8. Section 4. The two §§from P74L6 to P74L21 belong into the Conclusions section.
9. Fig. 12 should be presented much earlier to set the stage for your investigations.

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