Review of paper "Numerical Analysis of the Effect of Heterogeneity on CO2 Dissolution Enhanced by Gravity Driven Convection" submitted to HESS

General comments:

It is a pleasure procedure to review this interesting manuscript, which deals with the dissolution trapping of CO2 during geological carbon sequestration, an important means to reduce carbon emissions. In the process of sequestering CO2 in the deep saline aquifer, a concern is that the CO2 is less dense than the saline water, which gives rise to the possibility of upward CO2 leak. It is noticed that dissolution of CO2 in the brine is a crucial mechanism that reduces the risk of leak by transforming the supercritical CO2 into aqueous CO2. The significant challenge lies in the fact that the dissolution process is susceptible to instabilities driven by gravity and permeability heterogeneity, both of which are ubiquitous and involve big uncertainties. Therefore, it is important to characterize the dissolution rate of CO2 in the saline water, which is the objective of this manuscript.

Through reading the manuscript, I can feel that the authors have spent a big effort in preparing this work. They carefully selected the parameters for their numerical model based on the data from a deep literature review. They have conducted a large quantity of numerical simulations to build a data pool for systematic analysis. Finally, they give fruitful analysis and discussions of the results. Based on my own reading of the manuscript, I give my support of publication of this work on HESS. Several revisions are needed before it is accepted for publication.

Major comments:

(1)This manuscript improves the current predictor for enhanced CO2 dissolution due to gravity driven convection. Most of current predictors for enhanced CO2 dissolution address the homogeneous cases, which may have limitations for the real heterogeneous problem. Two representative works, as shown in Table 5 and Figure 6, have offered the preliminary predictor for the CO2 dissolution in heterogeneous fields. However, while one of them simplifies the predictor by neglecting the anisotropic effect, the other may not properly incorporate the ansotropic effect. The authors provided a new explanation of the anisotropic effect on the density driven dissolution. Their numerical results show that for a fixed vertical equivalent permeability increasing the horizontal equivalent permeability can reduce the dissolution efficiency, because increased horizontal permeability can increase horizontal mass exchange, make the horizontal mass distribution more uniform, and thus reduce the instability. My question is: I realize that the gamma_1=0.08 obtained by the data regression is quite similar to that for the 0.09 listed in Table 1. Do they have any relations? This is based on my observation that the alpha_1=1.1 is very close to 1.0 for the homogeneous.

(2)Furthermore, the authors introduced a new predictor using finger velocity, which is particularly intriguing because finger velocity can be measured using optical fiber technology. Given the rapid advancements and expanding applications of optical fibers in the field of geosciences, this novel formula could serve as a valuable tool for monitoring the trapping of CO2 through dissolution. My question is: The regression value for gamma_2=0.34 is quite different from 0.08 or 0.09, could you please explain why the gamma_2 is so different from gamma_1? I am wondering if it is simply a result of the data regression or maybe it have physical explanations.

(3)In the study of density driven instability, the fingers are usually irregular, as shown in the Figure 4 of your manuscript. Could you please explain why the fingers in Figure 3 are quite uniform?

(4)I can understand that the authors and many other researchers use two-dimensional model in both laboratory and numerical simulation, because of the high cost of three-dimensional model. I do agree that the two-dimensional study is very useful, but it would be nice if the authors can provide some discussions on the three-dimensional effect on GDC. I know it is hard to design a new simulation of new parameters, so I would appreciate it if the authors could find some related three-dimensional studies and give a short comparison of the difference of three dimensional simulations and two dimensional simulations. This may give more confidence for readers using the results from this work.

Minor comments:

1-The supplementary materials provided important and comprehensive information about the numerical modeling, but it is not fully referred in the paper. Please, give more clear reference of the supplementary materials in the paper.
2-Line 195 and 205, the appendix is missing.
3-Line 294, it would be better if we say in panel (a) of Figure 4 rather than in the first panel of Figure 4.
4-Line 299, I am wondering if you added white randomness (‘white noise’ maybe) in the heterogeneous fields?
5-Line 306, please remove the comma in ‘dissolution process, shows’.
6-Line 388, please remove the period before the references.
7-The overall structure of this manuscript is very nice, dividing the whole article into logical sections of proper titles. However, it may be better if the authors can reorganize the section ‘6 Conclusions’. We can see that in the section ‘5 Results and Discussion’. The authors first describe the general impact of heterogeneity on the development of instability, and then perform log-linear regressions of the simulation results. However, in the conclusion section the authors do not organize these results in the same order. Moreover, it would be better if the first paragraph is split so that we have a short summary of this work before writing the conclusions. This does not affect the comprehending of this work, but it would be nicer if the authors can reorganize the conclusions.

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