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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Regional-scale brine migration along vertical pathways due to CO_2 injection – Part 2: a simulated case study in the North German Basin" *by* Alexander Kissinger et al.

Anonymous Referee #1

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This manuscript addresses a very relevant topic, which should be of general interest to HESS readers. Novel approaches are presented and substantial and clearly justified conclusions are reached. The methods used are stated completely and clearly (with my more specific remarks to be considered below). I think, some more discussion on individual findings would improve the paper even further. I give some statements concerning this below, which are intended to assist the authors in this point. The abstract and title reflect the type of work and findings, the paper is well written and structured. The figures are required (one exception listed below) and of good quality. The paper contains lots of interesting findings, which allow for a better system understanding of the model considered. Overall, I think this is a good paper very suitable, and with

the modifications indicated below this should be a valuable contribution to the HESS journal.

- Vertical exaggeration too small, I do not see topography

- The main feature is the fault zone and the corresponding parameters. How is this connected to paper 1? Is this the end of the expert discussions? Why such a high value? This is higher than most saline formations. . .

- Mesh too coarse at fault zones. Any grid resolution study? Have you done a grid effect study to test if you capture the right effects? What did you find?

- Justify the reference model. It is missing thermal effects, which provide additional buoyancy.

- Fig. 8: 5 Isolines instead of 6? Please correct number.

- Describe infinite aquifer BC better. I think it is closed, but at 100 km extra (see also comment further below)

- Page 18, line 10 ? I do not see this in the Figure. Please be clearer here.

- The upper boundary condition drives the brine through the fault and hydrogeological window into the shallow aquifers and leads to high salinization (up to 50% of injected brine) in the reference model that is considered as the most likely geological setup (p 12. line 6f). This high leakage rate actually appears as unlikely and already an effect of the BC. Please comment on this a bit more in you manuscript.

- Give Scenario in Figure captions and state varied variable more clearly, that makes it easier to read.

- Sensitivity for Fig. 15 should be calculated using the 2p3c or 1p2c model, not the simplification, which shown to yield wrong results in Fig. 14. Why the model comparison anyway.

- Better scaling of figures (Fig. 14 top left, Fig. 11, Fig. 13, Fig. 16)

- Fig 5 should be drawn using the model area, to indicate the BC clearly. Where are the BC at the salt wall in the 3D model?

- I can see differences in Pressure and fluid mass in 4.2. Please also show salt mass and concentration changes, because that is what is interesting from the point of view of the near surface aquifers. And here, the simulators should be different.

- Make a better connection to the first part of the paper also for the parameters and processes assumed important. Was the model complexity also discussed? What was the outcome of the "expert" discussions?

- Clarify maximum salinity in Table 3. kg_NaCl / kg_brine or kg_NaCl / l_brine ?

- Also concerning the initial salt gradient: I am wondering how the salt gradient at the beginning of the spin-ups can influence the initial salt distribution in the scenario study 1, which is a balance between the impacts of diffusion and gravity? Is there an upper boundary conditions for salt in the spin-ups?

- Scenario study one: Better state Low/medium/High salinity gradient instead of just low/medium/high.

- Show Fig 10 as table. Instead, a plot of the isolines for the different cases like in Fig. 8 would be very interesting, to see the changes. Maybe for one or two cases this should be possible.

- Discussion section: I do not agree that leakage rate is a good indicator. For modellers this is true, but how do you measure and how do you detect? Additionally, as you state yourself, the salt concentrations may be very different, so NaCl-mass flow rates would also be of interest. The same volume of leaked water has a very different impact with high concentration brine as compared to low concentration brine.

- I am unhappy about the term "infinite aquifer" boundary, because obviously that is not

the case. The Dirichlet case is that one, the "infinite aquifer" case has just a higher volume and thus dampens the pressure response by a larger overall compressibility. The assessment of the BC is important, I agree, but just assuming that 100 km are "infinite" is wrong, as Figure 11 shows. Please use different terms, and better justify the assumption on your BCs. Check e.g. Benisch and Bauer (2013) for an estimation of the connected boundary volume, which can be appended to the model as bulk storage, to represent this case better. These authors actually also found that the low permeability sections contribute just as much to compressibility as the aquifers, so storage is omitted from the model by extending just the aquifers.

- Discussion: Please do not just cite the Schäfer paper, but also state their findings. This is interesting here.

- Your paragraph on the model comparison and leakage rates (Page 24, line 22) is confusing. Please state more clearly, that the simple model do not capture the leakage rates correctly, as they do not transport salt. The hydrostatic equilibrium is a function of the BC and of the injection rate in your case. Compare Delfs et al (2016) for this effect – you probably would need to include temperature for this in the simulations and look at the Dirichlet case.

- "The Analytical Model is therefore a useful tool to quickly assess the consequences of changing certain parameters within the geological model or to obtain conservative estimates on leakage rates over the fault zone" Have you done more than the two model comparison runs to justify this statement? For one model run, the analytical model does not work, for the other you get a 30% overestimation. A comparison for a range of parameters as in the parameter study would be helpful, to assess the capabilities of the analytical model fully. However, I do agree that these models are very useful. However, because they have many limitations, a clear understanding of these limitations is helpful (but maybe also beyond scope here).

- I agree with the conclusions, but they are highly repetitive with the discussion section.

How about merging these sections?

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