

## ***Interactive comment on “Brine migration along vertical pathways due to CO injection – a simulated case study in the North German Basin with stakeholder involvement” by A. Kissinger et al.***

**Anonymous Referee #1**

Received and published: 17 July 2016

Review of the manuscript "Brine migration along vertical pathways due to CO<sub>2</sub> injection - a simulated case study in the North German Basin with stakeholder involvement" by A. Kissinger, V. Noack, S. Knopf, W. Konrad, D. Scheer, and H. Class

The paper presents a study about the modeling of CO<sub>2</sub> into a Buntsandstein formation in the North German Basin. It consists of 3 parts: 1. a social science based part (participatory modeling - PM), 2. a geological modeling part (GM) and 3. a numerical modeling (NM) part

First of all the paper is very long. The length is far beyond a regular research paper. This makes it difficult to read and to follow. Further more very different aspects are discussed. However, they are not framed in an appropriate way. One could say, that a

C1

red thread is missing.

The paper seems to address different groups: so called stakeholder, natural scientists and social scientists. With respect to the geoscientific content this means that this manuscript addresses many people not familiar with geology, numerical modeling and so on. Keeping this in mind the way the geology is introduced and the modeling is by far too technical. For instance a nice stratigraphic table is missing where the different horizons are summarized and explained. For the modeling figures should show the mesh, the flow, the transport, ... A no go is here that – again unexperienced people are addressed – most figures have no (proper) length scale.

The PM approach is for me as a geoscientist understandable and it seems to make sense.

Very generally, my main concern is: what is this paper about? It addresses too many issues at once. Each issue could be critically discussed. Maybe a social scientist could raise some important critics about the PM approach. The selection of the PM participants biases the result strongly. Who defines who a stakeholder is? Which experts are involved? Which not? Was it an open selection, could people volunteer or was the selection performed by a small group? Following which criteria?

Concerning the geological modeling: do I get it right that you model a formation which does not exist, but which is somewhat similar to existing ones? Why don't you use a real one?

Overall the geological modeling and numerical modeling (everyone is modeling, but means something quite different ...) is presented to some extent as if here the real behavior of nature can be forecast. However, this is for sure not possible. Some valuable information can be obtained, but such a model will never be able to forecast what in reality happens as the simplifications are too many (i.e. each layer has overall the same physical properties), in reality numerous fault zones with very different properties exist. A regional prognosis for the depth in discussion is almost impossible.

C2

I would expect that the issue of uncertainties and the quantification (!) of uncertainties is far more stressed. The different model results are probably a good starting point, but the comparison should be improved. And – again – it should be avoided to overuse the result. Why a single phase, single species model should be sufficient is not clear to me, especially as very little is said about i.e. reactions of the CO<sub>2</sub> brine with the rocks, reactions within the brine, salt dissolution and so on. (From the abstract: “Simplifications in the model setup, such as neglecting variable-density flow or simplifying the complex geometry may prove valid options given sparse data availability.” Why should this be valid? Wouldn't it be far more logical to ask for better data?)

My suggestion would be to split the paper in two, maybe three parts: PM, GM and NM. Each part should be improved considerably especially with the aim that non-experts “get the picture”.

Specific comments: P1, L 21: safety should come first P2, L 25 – 30: what is the importance of these statements? P3, L 25: concerning “public acceptance”, this is true for today, for new technologies and some specific regions of the world. The argument is not true in general. P 4, L 7: what precisely is meant with 'generic' here? P4, L9: which guidelines were used? P6, L6: what is the difference between the science community and independent experts? Is the science community biased? What or who is the science community?

Figure 4 and many more, a proper length scale is missing.

Figure 5: why is the Rupelian penetrated by the Cretaceous? Why is the Rupelian so important? Aren't there more clay formations between Solling and the Cenozoic?

P 10, L 5 – 10: many of these important information should have been given earlier.

Figure 5: why are there no faults visible?

P 11, L6: what is meant with a 'cube mesh'? P 11, L 8: what is the maximum vertical resolution?

C3

Table 4: why is the surface temperature 281,15 K = 8 °C? To my knowledge 10 °C would be more appropriate.

P 27, L 7: a broad agreement does not guarantee that something is in fact reliable.

The given web address <https://git.iws.uni-stuttgart.de/dumux-pub/Kissinger2016a.git> is not accessible (17.07.2016)

So to summarize, my suggestion is - to restructure the manuscript by preparing 3 linked papers - to explain the overall concept, the procedure and the results better with more and better tables / figures

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-281, 2016.

C4