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# Comment on hess-2021-338

Anonymous Referee #2

Referee comment on "Reactive transport modeling for supporting climate resilience at groundwater contamination sites" by Zexuan Xu et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-338-RC2, 2021

Review of HESS-2021-338-2: Reactive transport modeling for supporting climate resilience at groundwater contamination sites

This paper reports an interesting coupling of climate projection data with subsurface reactive transport simulations through the Amanzi platform. The study is developed in application to the Savannah River site F area and focuses on the effects of perturbation scenarios on recharge and the distribution of uranium and nitrate concentrations. The key result is that pH is impacted by dilution and remobilization, which influence the sorption of uranium onto sediments. Simply for the novelty of this method and the effort to offer projections into future climate scenarios, this study could represent an advancement in the field. However, I do think there are several areas that require strengthening to lend fidelity to these model results.

Major edits:

- Testable hypotheses: Line 73 refers to testable hypotheses, but I see no such statement offered in the text to this point. It reads like there is a lot of competing information and many factors in play. This is not a testable hypothesis. This needs to be carefully revised – offering a clear and testable statement would greatly strengthen the purpose and scope of the study
- Vegetation: there seems to be no treatment of the role vegetation plays in recharge and near-surface water storage. Everything is limited to assuming that the effects of changing precipitation can be emulated by changes in recharge rate. This would seem to undermine the coupling of these climate models and thus the overall impact of the study
- Prior work: there seems to be a lot of overlap with Libera et al (2019) and Bea et al. (2013) with regard to the reactive transport simulations. Figure 2 seems to be largely reproduced from Libera. The distinction between these models and prior work should be clearly explained. Presently I am left with the sense that this paper is a melding of Bea

et al. (2013) + Libera et al. (2019) + the climate scenarios. Perhaps this is enough to argue that the study is novel, if so, this should be explicitly detailed. Further, it is unclear why this particular location was chosen for the purposes of such a model – is it because the Libera et al. paper already existed or is there some stronger reason why this is the appropriate location to work on the Amanzi development?

- Advection dominated: the authors are well aware of how profoundly important the effects of diffusion and dispersion are when dealing with sorption and solute exchange between high and low flow zones. The assumption that this system is advection dominated, along with the large uncertainty in Kd values, would seem to place significant uncertainty on the present results. This assumption must be clearly explained and justified. At present it seems to be simply stated in section 3.2 without further consideration
- pCO2: Table 2 reports CO2(g) concentrations that appear to be lower than even present atmospheric values, and certainly do not appear to consider changes in pCO2 associated with a changing climate. Why isn't this considered in the model along with recharge variability?

Specific edits:

L40: "significant amounts" clarify what this means

L51: what is meant by "absorb the projected stresses"

L54: this is circular. First the authors argue that we don't know how climate changes and associated stresses may impact contaminated sites, then it states that this information is a critical need. How can it be both unknown and critical?

L55-65: please revise this text. It's quite confused and hard to follow

L70-71: "appear in different phases" what does this mean?

L73: tritium is non reactive?

L33: that's a huge range of variability in a key parameter (Kd). How is this much uncertainty accommodated in the model?

L143: state the criteria for transition from enhanced to monitored natural attenuation

L179: This seems to strongly overlap with the Libera et al. study

L197: state the model used by Bea et al. (2013)

L221: kinetic rate constant. Kaolinite is not a primary mineral

L266-269: does this undermine the study?

L288: this cap failure scenario seems very fictitious and may only be included to essentially get the model to do something noticeable. I'm not convinced this is a strong addition to the study

Figure 5: are these data supposed to suggest model fidelity? The fits appear quite poor.