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Comment on hess-2021-148

Anonymous Referee #3

Referee comment on "The influence of hyporheic fluxes on regional groundwater discharge zones" by Brian Babak Mojarrad et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-148-RC3, 2021

General

The authors of this study used a complex multiscale model to compare the travel time and spatial distribution of catchment scale groundwater flow with and without the influence of hyporheic flow. The results are loosely linked to the fate of nuclear particles that might potentially leak out from nuclear waste disposal in the bedrock of the catchment. The main finding of the study is the fragmentation of the groundwater discharge areas, the reduction of the effective volume that carries groundwater flow and the consequential reduction of the groundwater travel time in the upper 5m of the domain. A monte carlo approach was used to some extend to generate a distribution of possible outcomes where exact model parameters were unknown.

The underlying model is without a question quite sophisticated. However, I wonder if the right model was chosen to answer the specific scientific question. The main finding, that the discharge zone is fragmented, is relatively obvious and has been published various times in model studies from the hyporheic perspective (e.g. Boano et al 2008, Trauth et al 2014, Fox et al 2016) and also investigated experimentally (e.g. Bhaskar 2012). Given that the general effect is well known, I would have expected an in-depth analysis of the correlations of at least some of the parameters involved. I wonder if this complex, multiscale, pseudo-realistic model is the right choice to draw systematic conclusions with results that have a direct use for other scientists. The model has too many unknowns to investigate general findings, e.g. correlating discharge area reduction to the ratio of hyporheic head and groundwater head or something similar.

If the authors didn’t aim to draw general results but rather calculate the effect for a specific case (nuclear waste disposal in this specific catchment), I see two problems. First, case studies do not match the scope of the HESS journal. Second, some of the boundary conditions have been selected without verification or are too unclear to be used for a specific use case (especially a safety relevant topic like nuclear waste disposal). Generally, I think that a lot of choices for boundary conditions are justified only by references to past work of the own workgroup. In some cases, references to independent workgroups would strengthen the trustworthiness of the model (see detailed comments below).

The second major finding is the reduced travel time within the upper 5m of the 500m deep domain. Also here, I miss a systematic investigation of the underlying mechanism. E.g. what is the correlation between the fragmentation and the travel time? What is the correlation with the hyporheic head and groundwater head? Is the conductivity a relevant factor in this correlation? These kind of questions should be answered with specific results rather than vague discussions, because the vague answers to these questions are obvious. Again, it would have been much easier to answer these kind of questions if the model was less complex.

In addition to the missing universal results, I don’t understand why the reduction of travel time in the upper 5m is deemed relevant. The effect of the hyporheic zone decreases exponentially with depth (see eq(5) or Elliot & Brooks 1997a), which is why only the very last segment of a particles travel path will be influenced. In Line 353 it says that the effect of the hyporheic head is strongest, where the overall seepage velocity is low. That means that particles that traveled centuries to millennia (Fig 5) to reach the surface will lose a few years on their last few meters (Fig 7). Even when the different retention coefficients in bedrock and deposits were considered (missing in Fig 7), the effect should be minor. I miss a clear explanation what processes are potentially influenced by the change in travel time in this last stage of the streampath.

Overall, I’m afraid that the study design is not able to answer the research questions to a degree that goes beyond the intuitive and well-published findings.

Specific:

102: I don’t think it is necessary to show the definition of Darcy’s velocity, but if you do, please use the already defined symbol for Darcy’s velocity “W_c” instead of q and q_seepage.

108: The 50-fold difference between retardation factors between bedrock and deposit is a dominating factor for the distribution of particle travel times. It could be justified by a reference to other workgroups who found similar retardation factor ratios between rock and deposit.

147: Extrapolating a decay coefficient from measurements at 3 and 7cm to a depth of 5m is questionable. The assumption that the minimum conductivity is 10^-6 (m/s) probably determines the decay coefficient much more than the actual measurements. The only reference is, again, only a single study from the own workgroup. The authors should be able to find more measurements of sediment conductivities in the literature to strengthen their assumption.

159 – 173: In line 165 you state that the realistic boundary condition would be a recharge-controlled boundary for most of the terrain. However, you choose a head boundary condition instead and use a mesh-coarsening algorithm to fit the recharge. Why didn't you simply use a recharge-controlled boundary? Coarsening the mesh to fit a result is somewhat unorthodox. All discretizing simulation techniques have in common that an infinitely fine mesh resolution results in the exact solution of the underlying differential equations. The boundary condition in this study, however, implies a tradeoff between model inaccuracy and boundary condition inaccuracy, which, in my opinion, should be avoided by choosing mesh-size independent boundary conditions.

177: A figure of the mesh/domain would be helpful. The domain is rectangular? I originally thought it was a whole (sub-)catchment with its natural boarders (and lateral no-flow boundary conditions).

179: Were the particles weighted somehow? In the following particle statistics, what does one particle stand for? A certain fraction of groundwater volume? A certain area/volume of bedrock? Please indicate why your choice is the best choice for the research question.

196: is "c" the same coefficient as "delta" in line 147?

213: Why did you use local regions for downscaling? Couldn't it also be a 100x100m region from somewhere else in the world? Or do you assume that there is a correlation between the catchment topography and its streambed-topograpy? I don't think that Wörman, et al., 2007a proved a local correlation between topographies. I think it should be clarified for the reader if a local correlation is assumed or if the regional topography is simply used as a sophisticated random field generator and the topography data could also be taken from somewhere else.

179/246: Both particle tracings should be described in a single chapter.

248: If intermediate particle traces are those that did not enter the bedrock and deep particles are those that started at 500m depth, you miss the flow that enters the bedrock but not to a depth of 500m. Superimposing two particle tracings with different seeds and without weighting them properly against each other adds some randomness to the results.

260/Fig2/179: The release plane for particles was described as "approximately 500 m below the minimum topography elevation". That describes a flat plane. However, the release surface in Fig.2 two shows a curvy plane. Which one is correct and why is the depth "approximately"?

Fig 3 and Fig 6 and the corresponding text could also be placed in the methods section.

Fig 7 and corresponding text: How many particle traces from the deep fraction entered the 5x5m domain to be superimposed by hyporheic flux? According to Fig4 it could only be a handful. Why are the dashed lines so smooth and the solid lines are not? Do they represent the same amount of particle traces?

377-400: To be honest, I did not understand what you did here. What is on the y-axis of the CDF? The whole topic "coherent area" needs a better explanation. Why did you choose coherence as a measure? What environmental process would coherence be important for? My interpretation is that you created a list of coherent (however coherence was defined precisely) upwelling patches and calculated their surface area. Now you found, for example, that 50% of these patches had surface areas > 400m². Is that correct? If so, I would strongly recommend not to use a CDF for presentation but a PDF. Nothing is cumulating here, CDFs are easily interpreted as "number of particle traces that reached the surface" where 100 means all particles exited the domain or something similar.

409: Why does the scenario assume accumulation? Shouldn't it be steady state in- and outflow at some point?

415: I'm not familiar with dose assessment but if it is based on the idea that groundwater upwelling happens on a large area without fragmentation it is obviously oversimplifying groundwater flow. If so, you should explain the dose model in more detail and propose an improvement to the model.

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