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Comment on hess-2021-573

Anonymous Referee #2

Referee comment on "Spatiotemporal variations in water sources and mixing spots in a riparian zone" by Guilherme E. H. Nogueira et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-573-RC2, 2022

The work presented here by Nogueira et al concerns the evaluation of the reactivity of the hyporheic zone, and more globally of a small stretch of a river-aquifer interface that is in a losing condition. The paper is well written and proposes a new framework for assessing the potentiel reactivity of this interface by coupling a physically based model, that simulates stream water (SW) and groundwater (GW), with a Hydraulic Mixing Cell (HMC) module. However it suffers from various flaws that must be addressed before further publication.

- The system at hand must be detailed and a significant effort must be done for positioning it in a much broader picture (see for instance the review of Flipo et al. 2014). From the title and the abstract, it must be clear that the focus of the study is a 900m stretch of a river connected to a small portion of porous medium (the lateral extent of the model seems too narrow to speak about an aquifer). The river is in a losing condition, which is not the most probable configuration as far as SW-GW are concerned since rivers constitute the water outlet for GW at the catchment scale. Finally the connection of the small portion of river stretch with a broader regional aquifer system must be explained.
- The main conclusions highlighted in the abstract only make sense if it is clearly stated beforehand that the stretch of river is in a losing configuration otherwise readers could be misled at the reading of the abstract. On the one hand, the highlighted results of evaluated water mixing values should be moderated in the abstract considering the remark 3. On the other hand, it seems to me that an important result of the study is not sufficiently reported in the abstract, it is the fact that the potential hot spots of reactivity of such a system in terms of nitrate removal is located at the fringe of the HZ and not directly below the leaking river.
- The GW model set up must be detailed. What is the extent, in the x, y and z directions ? what are the lateral boundary conditions and also at the bottom of the system, as well as for the upstream part of the simulated porous media. If no water flux conditions

are used for the lateral and the bottom of the porous medium system, it has consequences on the presented result, entailing them with a large uncertainty related to the misconception of the connection of the system to the larger regional aquifer system. A discussion on the consequence of the model set-up should be added to the paper.

- One way to clarify the paper is to add a summary of the other Nogueira et al papers
- The added value of using HMC rather than a fully coupled transport model is not clear and is in the current state of the paper an affirmation, not a scientific statement. As it is stated that Nogueira et al in press used the transport module of HydroGeoSphere, a comparative assessment of computational duration should be provided. This quantification is essential because from line 609-618 it seems more efficient to directly use a transport model than a HMC for the quantitative assessment of the stream-aquifer interface in terms of nitrates removal.
- Errors in mathematical formulas are unacceptable and must be corrected :
  - Eq 1. f_(w)^ t-1 not defined, as well as vbc_k^t
  - Eq 2 not homogeneous in terms of units between left hand side and right hand side
  - Eq 4 the denominator seems wrong, please check and either add the original reference or detail the math. L 250 the value of the denominator of eq 4 in case only two pools of water are concerned is 1, root square(2)/2 as stated by the authors.
  - Same problem in eqs 5 and 6
- The discussion about the reactivity of the interface should be enriched with other important references such as Newcomer et al. 2018, especially providing arguments on the added value of a 3D approach.
- Sec 2.4.1 the authors mention that the origin of water from the flood plain can be neglected, then developing eq 4 in that specific case. It is confusing since they use 3 origins in the remaining of the paper. Section 2.4.1 must be reworked l235-271

Other remarks

- L. 127 Please write the explanation of Fig. 2 in a paragraph at the beginning of section 2 Method. It is not currently detailed, only the Figure is in the document.
- L. 141 AT each time step
- Fig 3a. Scales are not readable, especially in the Z direction. Overall the readability of the whole figure must be improved. The reader should be able to read the piezometer names
- L 180 grammar issue
- L 196 what is the integration function of Tecplot, please explain the math instead
- L 204 50% OF stream water
- L210 WHILE most
- 8. Fig 6 and 7 are too small and therefore not very informative. The authors must select more dedicated illustrations that correspond more closely to their message in the text

Refs :

Flipo, N., Even, S., Poulin, M., Théry, S., Ledoux, E. (2007). Modeling nitrate fluxes at the catchment scale using the integrated tool CAWAQS. Sci Total Environ. 375, 69-79. doi:10.1016/j.scitotenv.2006.12.016.

Flipo, N., Mouhri, A., Labarthe, B., Biancamaria, S., Rivière, A., Weill, P. (2014). Continental Hydrosystem Modelling: the concept of nested stream-aquifer interfaces, Hydrol. Earth Syst. Sci., 18 (8), 3121–3149, doi:10.5194/hess-18-3121-2014

Newcomer, M.E., Hubbard, S.S., Fleckenstein, J.H., Maier, U., Schmidt, C., Thullner, M., Ulrich, C., Flipo, N., Rubin, Y. (2018). Influence of hydrogical perturbations and riverbed properties on hyporheic zone respiration of CO2 and N2. Journal of geophysical research Biogeosciences, 123(3), 902-922. doi:10.1002/2017JG004090