RC1

GC1) The paper is generally well written (although I got lost in a few paragraphs). The main purpose of this paper is to introduce a multi-tracer approach to quantify GW-SW exchange. I fully agree with the authors that complementary information from tracers (and especially natural tracers) can (and should) be used more often in such alluvial river-aquifer contexts to better constrain decision-based model predictions.

Thank you for your thoughtful and constructive review on our paper. We appreciate your positive assessment of the overall writing quality and your acknowledgment of the importance of a multi-tracer approach in quantifying groundwater-surface water (GW-SW) exchange in alluvial river-aquifer systems. We understand your concerns regarding the clarity of certain paragraphs and proceeded to a major revision of those unclear sections to enhance overall readability.

GC2) However, I must admit that I was disappointed to see that the information from the tracers was not really valorized in the modelling exercise. The model was only calibrated against steady state hydraulic head.

The model has been effectively calibrated on hydraulic heads; we have not calibrated any transport parameters. We just added a production parameter to reproduce the natural value (without pumping) of radon in groundwater. With pumping, we just verified that the model was able to reproduce radon observations without any additional calibration (in a parsimonious approach). This validates the global flow and transport model. Regarding stable isotopes of water, they allow identifying a mixing (surface water-groundwater exchanges) which is a major contribution to the conceptualization of the system in support of a numerical model (in terms of boundary conditions).

From above statement and the response to comment GC3 below, we believe that we have exploited the tracers in a relatively efficient way.

GC3) More generally, the paper lacks details on the modeling setup which make it very difficult to understand the main goal of the model (what do they want to predict??). Overall, the so-called approach is not clear to me.

To address this, we now provide more details on our modeling approach in the manuscript (some information also added as responses to the detailed comment's part, see below). Additionally, we clarified our goal and workflow approach in the introduction. The main goal of this study is to determine the origin and proportions of the water sources feeding a pumping well system on an alluvial plain. For this purpose, the following steps were implemented:

i) Identification of the water sources feeding the pumping wells using at least two distinct tracers (radon and stable water isotopes) and the piezometric data.

ii) Analysis of temperature seasonality in the piezometers and Rhône River to estimate pore velocity (u), based on known porosity.

iii) Use of δ18O seasonality in piezometers to constrain dispersivity, a crucial but often poorly known property.

iv) Use of steady-state flow modelling coupled with reactive transport (radon) to confirm the origins and quantify the proportions of the pumped water mixture, using pore velocity and radon spatial distribution only for model validation.

During the study the δ18O seasonality analysis in piezometers presented in the additional content section, was not as robust as expected and the dispersivity value obtained can only considered as a first order estimate. However, this value in the order of 10 m is in excellent agreement with those reported in the literature for similar media (Schulze-Makuch, 2005).

GC4) My last concern is that I also felt that the authors did not perform a complete literature review in the introduction. The authors stated that the use of radon in transport models is rarely discussed, but I strongly disagree. The authors are missing some relevant papers. 222Rn (and natural tracers in general) have been used extensively to study river-groundwater interactions under losing river conditions. Please see the following not exhaustive list of publications: Bertin and Bourg, 1994; Hoehn and Cirpka, 2006; Hoehn and Von Gunten, 1989; Hoehn et al., 1992; Popp et al., 2021; Stellato et al., 2013; Vogt et al., 2010. See also Peel et al., 2022, Gilfedder et al., 2019, Liao et al., 2021, and Delottier et al., 2022 for explicit simulation of tracers.

In the previous version of the manuscript, the novelty approach regarding radon was introduced once with a certain moderation (line 87-90 and see below) and secondly in an inadequate manner (line 133-137), the latter being comprehensibly the only one commented on by RC1. In fact, the moderated paragraph in the previous manuscript was supported by some literature, one of which is mentioned by RC1 (Hoehn and Von Gunten, 1989) and one of which is not (Close et al., 2014). In the revised version we retained the moderated presentation (using "less studied" for losing-river radon use and radon modeling) adding some of the references pointed by RC1, for which we are grateful.

Among omitted literature, the work by Adyasari et al., (2023) allows discussing the real importance of radon used under losing river conditions. Similarly to these authors, alternative requests using the keywords "groundwater discharge", "tracer", "radon" (gaining river); and "river infiltration", "radon" (losing river) performed on Google Scholar and Web of Science led to about 15-20% of the paper dedicated to losing river situations (on a total of between 100 and 130 articles between 2003 and 2023). Based on this analysis, gaining river situation are largely dominating and, in our opinion, the term "extensively" doesn't really apply to the use of radon to losing river situation. This is exactly what is said in the moderated version: "However, the situation of a losing river, i.e., surface water supplying the groundwater is less studied."(Line 87 of the previous discussion version of the manuscript). This is likely related to the fact that radon data interpretation using a standard and simple mixing reactive model approach for surface waters is obviously adapted to gaining rivers (or lakes). More complex calculations (groundwater flow and reactive transport modeling) are required for losing rivers. In the same way, we stated lines 136-137 (previous manuscript)," the use of radon data in transport models is rarely discussed"

In addition to the abovementioned moderation now adopted, the uncommon situation of interest here which can be considered as a novelty was added in the introduction. Previous studies typically focus solely on either "gaining river" or "losing river" situations. However, in this study, alternating gaining and losing river situations occur at the same site allowing the identification of groundwater sources and proportions.

GC5) In the end, I am not really sure where is the scientific contribution of that paper. In the present form, it is not really clear. For these reasons, I cannot recommend publication of that paper in HESS.

We hope that we have answered the major concerns of Rev1 (see responses to GC1 to 5). Overall, we have considered all the constructive comments made by reviewers, which has resulted in a greatly improved manuscript. Hopefully, the revised manuscript will better meet your expectations of HESS.

We believe that the scientific contribution was clarified in the respond to comment GC3 with the goal (subject of public interest due to the common situation considered) and the methodology (workflow) involving the sequential use of tracers to obtain parameters (dispersivity with $\delta^{18}O$), variables (pore velocity with temperature), the conceptual model ($\delta^{18}O$ and radon) to obtain a more robust numerical model enhancing the confidence on model outputs in terms of groundwater balance (mixing proportions in wells). The goal and the workflow are repeatable in this common situation of pumping facilities in alluvial plain system.

Detailed comments

1) Line 28: PEST suite.

Corrected accordingly.

2) Line 29: Is that really reactive transport for Radon ?

Yes, we consider a reactive transport which is the case when a chemical element is transformed or degraded during its transport. In our case, the radon is generated in the aquifer and is degraded by radioactive decay.

3) Groundwater-river; aquifer-surface water etc. Please be consistent in the paper.

Corrected accordingly with groundwater-surface water.

4) Line 108: I would just say a calibrated model. If a model is badly calibrated, it is better to say that it is not calibrated.

Corrected accordingly.

5) Line 114: tracers are observations (not techniques). Here the author refer to method and technique but I think it is observations right ?

Yes, indeed we made measurements that are interpreted using models. This was corrected in the manuscript accordingly.

6) Figure 1: Not really easy to see where are pumping wells and where are piezometers. Need more detailed legend. After reading Part 2.1, I am still not sure about the location of the pumping wells. For the aquifer geometry, a geological cross section would be welcome.

Figure 1 was corrected with a legend for the different symbols used and better style for the surface water. A cross section was added into figure 3 to explain the mechanism of exchanges.

7) Line 226: Specific yield

Corrected accordingly.

8) Line 220: Why do you name it a synthetic model? Is that not a model developed in a real case study?

Corrected using "model". This model is indeed developed using a real case study.

9) Line 236: Is the Rhone river represented with a Dirichlet BC ? If so, this can lead to enormous amount of water in the model. Again, the description of the model is not so clear. Why not used a Cauchy type BC?

The river stages are roughly stable which can be conveniently described using Dirichlet BC. With such conditions, there is no need to calibrate a (generally poorly constrained) conductance coefficient. The "enormous amount" of water behind the Dirichlet BC simply correspond to the supply the pumping wells since the overall mass balance is met (by definition) in the model.

10) Lines 238,239,240: This means that there is only one layer for the entire model? So this is a pseudo 2D model?

. The hydraulic head and the radon activity simulated here are functions of space coordinates x and y but not z (h(x,y), R(x,y)). It is therefore a pseudo 3D model but actually a real 2D model.

11) Line 240: permeability field? Is this considered homogeneous or Heterogeneous? If so, is there zones of piecewise constancy or pilot points? Not clear. How many parameters involved in the model calibration?

The permeability field being heterogeneous, the domain is separated in several zones of piecewise constancy. During the calibration 10 zones were used for 10 observation points. The information was added line 253-256.

12) Line 242: PEST optimization tool. This means that you have used the CMAES global optimisation scheme? Not clear.

We use the module PEST incorporated in our MODFLOW version (processing MODFLOW X). PEST uses a nonlinear estimation technique known as the Gauss-Marquardt-Evenberg method which is a standard gradient-based optimization algorithm.

13) Line 245: Not production of radon in the groundwater? Not clear. How the production of radon can be simulated with an injection well? More information is needed here to better understand how Radon was simulated in the model.

Radon geological production was added using the "injection well" package of Modflow. Injection wells are implemented in all aquifer cells, with a very low injection rate to avoid artificial impact on the water table, and a large mass of chemicals to reproduce radon generation. This injected mass is considered as homogeneous, and its value is set to reproduce natural radon activity. Radon radioactive decay is also implemented. Details on this technical implementation of the production are provided lines 260-264.

14) Line 250: This is not a classical way to simulate radon. Usually an end-member equilibrium activity is needed for that seeks.

Unfortunately end member equilibrium is not available on our version of MODFLOW and MT3D.

15) Figure 3: The use of an inverse distance method to draw a piezometric map is not ideal. The piezometric map seams strange with geometric 90° change of orientation. Is this because of the method or because of strong heterogeneity at the site scale?

The appearance is due to the interpolation method and the software. We change it and the figure 3 was redrawn accordingly. Additionally, we added the piezometric map without pumping.

16) Figure 4: Not clear. What is the meaning of the grey circle in the Figure? The large grey band ? Not easy to follow.

Figure 4 and the description was modified accordingly to a better understanding. The large grey band corresponds to a rupture in the x axis. The grey circle square in this (new manuscript) correspond to piezometer Prg2 during the pumping stopped period.

17) Line 297: explain the meaning of GMWL (global meteoric water line I guess).

GMWL is indeed global meteoric water line the definition was added line 314.

18) Line 303: meteoric groundwater recharge.

Corrected accordingly.

19) Line 335: this method? I should admit that I am getting lost in the end of this paragraph. Section 3.4: Is there any uncertainty on the temperature models used to interpret the data? It would be good to discuss the reliability of the results of these models regarding the uncertainty.

A sensitivity analysis was added in this section to determine the relative importance of the parameters. It appears that he flows velocity is the more sensitive one (line 351, figure 7A). Additionally, we also present the limitation of this model and alternative approaches when an analytical solution is not applicable (lines 205-209).

20) Figure 8: I don’t find the C) section in this figure. This is unfortunate since I also find that the A) and B) are far from being informative to support model calibration.

Corrected accordingly, figure C is restored.

21) Line 350: I do not see how the calibrated model reproduces the observed data. This is not clear at all in the figure. Section 3.5.1: How the results of the model are sensitive to the estimated parameters? At least, a sensitivity analysis should be applied. Section 3.5.2: As I understand, the radon and isotopic information were not added at all in the model calibration? Why?

The calibration result is added in the scattering plot in figure C that was absent in the original version of the manuscript. Additionally, the calibration is effectively only made using hydraulic head data. However, we reproduce the observed radon activity as explained lines

388-390 and figure 9, indicating a good estimation of the flow parameters as explained with more details in our response to GC3.

References

Adyasari, D., Dimova, N. T., Dulai, H., Gilfedder, B. S., Cartwright, I., McKenzie, T., and Fuleky, P.: Radon-222 as a groundwater discharge tracer to surface waters, Earth-Science Rev., 238, 104321, https://doi.org/10.1016/j.earscirev.2023.104321, 2023.

Schulze-Makuch, D.: Longitudinal dispersivity data and implications for scaling behavior, Ground Water, 43, 443–456, https://doi.org/10.1111/j.1745-6584.2005.0051.x, 2005.