**Sep 17, 2023**

**To:** Editor of Hydrology and Earth System Sciences

**Subject:** Revision of HESS-2023-29

**Dear Editor:**

Thank you for giving us the opportunity to revise our manuscript. We are submitting the revised manuscript titled “Influence of bank slope on sinuosity-driven hyporheic exchange flow and residence time distribution during a dynamic flood event” (HESS-2023-29). All the comments and suggestions from the reviewers and editor have been carefully addressed in the revised version of the manuscript. We show and discuss the accuracy of our modeling approach, revised the results section and extended our discussion section. We have also revised the structure of the manuscript as suggested.

Below you can find an itemized response to the review comments. The original comments are included verbatim in blue text, and our response to each comment follows in black text while the revised texts as they appeared in the revised version of manuscript follow in underlined black text.

We would like to express our heartfelt appreciation to the associate editor and two reviewers for their constructive comments that undoubtedly increased the scientific value of this manuscript. We hope the revised manuscript will meet your expectations. Should you have any further queries, please feel free to contact me.

Again, we thank you very much for your consideration and look forward to your favorable response.

**Zhang Wen, Ph.D.**

Corresponding author on behalf of all authors

Email: wenz@cug.edu.cn. Tel: 86-27-67883159. Fax: 86-27-87436235.

# Point to point response of the comments.

## Comments from Review #1

**Comment 1.1.** Line 27: present tense: general truth.

**Reply:** Agree, we have modified this sentence.

**Revision made:** Please see lines 26-29: "For sloping riverbanks, the hyporheic zone (HZ) encompasses a larger area and penetrated deeper into the alluvial aquifer, especially in aquifers with smaller transmissivity (i.e., due to increased hydraulic conductivity or reduced specific yield)"

**Comment 1.2.** Line 31: Travel time fits better for "residence time"?

**Reply:** Agree, we have replaced the "residence time" by "travel time"

Please see lines 31 in the revised version.

**Comment 1.3.** Line 34: inappropriate: there is not residence time for a flux

**Reply:** Agree, we have rewritten that sentence.

**Revision made:** Please see lines 34-35: "Consequently, this decreases the travel time of water discharging into the river relative to base flow conditions."

**Comment 1.4.** Line 36-37: "in alluvial aquifer" should be "river valleys or floodplains"

**Reply:** We have replaced the "alluvial aquifer" by "floodplians"

Please see lines 37.

**Comment 1.5.** Line 40-41: do not use keywords that are also used in the title; this is meaningless

**Reply:** Agree, we have rewritten the Key words.

**Revision made:** Please see the key words at lines 41-42: "hyporheic exchange, sloping riverbank, deformed geometry, numerical simulation, residence time distribution"

**Comment 1.6.** Line 47-48: it is not only mixing as a cause

**Reply:** Agree, we have modified this sentence.

**Revision made:** Please see lines 47-52: "Mixing and transporting of different water types (groundwater, surface water) and ages in the HZ driven by hydrodynamic and hydrostatic factors cause spatially and temporally varying exchange of water and biogeochemical species between river channel, riverbed and aquifer (Cardenas, 2009b; Hester and Gooseff, 2010; Krause et al., 2011, 2017, 2022; McClain et al., 2013; Boano et al., 2014)."

**Comment 1.7.** Line 54-55: poor terminology of "surface flow components that penetrate and transport", and Line 55: Never heard of hyporheic sediment. I cannot see how it can exist. Hyporheic refers to a zone.

**Reply:** Agree, we have modified that sentence.

**Revision made:** Please see lines 52-55: "The hyporheic exchange flux (HEF) represents the interaction flux between surface water and groundwater in vertical (e.g., bedform-driven) and horizontal (e.g., meander-driven) directions, which can add to general regional groundwater ex-filtration and infiltration."

**Comment 1.8.** Line 56-57: Vague; rephrase. Biochemical > hydrochemical (you refer to water) when it also involves the porous matrix, better talk about biogeochemical but not biochemical

**Reply:** Agree, we have replaced "biochemical" by "biogeochemical" here and throughout the manuscript.

**Comment 1.9.** Line 62: Replace "pressure" by "hydraulic"

**Reply:** Agree.

**Revision made:** Please see line 61.

**Comment 1.20.** Line 64: "demonstrate" should be "demonstrated"

**Reply:** Agree, we have modified it.

**Revision made:** Please see line 64.

**Comment 1.21.** Line 65: "heterogeneity" in what?

**Reply:** We intended to say the heterogeneity of hydraulic conductivity. That sentence has been modified.

**Revision made:** Please see lines 64-66: "For example, Cardenas et al. (2004) demonstrated how riverbed characteristics and especially the heterogeneity of hydraulic conductivity could increase HEF by 17% to 32%."

**Comment 1.22.** Line 68: Delete "interaction of its"

**Reply:** Agree, we have deleted them.

**Comment 1.23.** Line 69: "HEF paths" is erroneous terminology; see earlier. And Line 72: "attenuation capacity" for what?

**Reply:** Agree, we have modified that sentence.

**Revision made:** Please see lines 68-74: "This is imperative as the spatiotemporal evolution of HEF, the resulting change in HZ (area) and thus also the residence or travel time (RT) of the exchanged water in the HZ have significant impact on flow dynamics and transient storage along the river continuum and in turn control the capacity for contaminant attenuation (Weatherill et al., 2018) and biogeochemical functions of river corridors (Bertrand et al., 2012; Boulton et al., 2010; Brunke and Gonser, 1997)"

**Comment 1.24.** Line 70: "residence tine (RT)" as indicated earlier, I would prefer travel time, change throughout.

**Reply:** We agree that the meaning of residence time and travel are same. But we used residence time distribution equation (or termed as water age equation in other literatures) to evaluates the mean travel time of pore water in aquifer, thus we think it's better to use that term in our Introduction section. But we have modified the Results section and replace "RTD" by "travel time" when we analyze the results.

**Revision made:** Please see the revised manuscript.

**Comment 1.25.** Line 75: Use ... the river and its floodplain ....

**Reply:** Agree. We have modified that sentence.

**Revision made:** Please see line 75: "Both lateral exchange between river and its floodplain"

**Comment 1.26.** Line 80-83: Delete.

**Reply:** We think that sentence is very important for introducing the controller of hyporheic exchange processes, thus it is better not to delete them.

**Comment 1.27.** Line 95-96: "nitrate forming or reducing reactions" vague. nitrification and denitrification??

**Reply:** We have revised that sentence.

**Revision made:** Please see Lines 95-97: "By comparing RTD with the timescale of nitrification/denitrification reactions, a meander can be classified as a source or sink of nitrate"

**Comment 1.28.** Line 97: Delete "for (de)nitrification activities" and "have"

**Reply:** Agree, we have done that.

**Revision made:** Please see Line 98.

**Comment 1.29.** Line 114: "tilted" should be sloping? Inclined?

**Reply:** Agree, we have replaced "tilted" by "sloping (inclined)".

**Revision made:** Please see Line 113.

**Comment 1.30.** Line 136-137: "riverbank morphological conditions" poor style

**Reply:** We have replaced "riverbank morphological conditions" by "when a sloping river bank exists" at Lines 136.

**Comment 1.31.** Line 147-149: poor formulation of objective/research question/hypothesis. Go in depth on this.

**Reply:** Agree, we have revised that sentence.

**Revision made:** Please see Lines 146-149: "Our results reveal how and when bank slope plays an important role in sinuosity-driven meandering rivers with respect to HEF and RTD, which in turn will lead to an improved understanding of the river channel-aquifer-floodplain system and provide guidance on the placement of monitoring locations in river management studies."

**Comment 1.32.** Line 153-156: repetition from 5 sentences above. Smoothen the text on this.

**Reply:** We have modified that sentence

**Revision made:** Please see Lines 153-156: "The modeling approach and dimensionless parameterization metrics used by Gomez-Velez et al. (2017) can represent most riverbank-aquifer situations and dynamic flood conditions. In our study, we use their conceptual model to set up a baseline case with the same model frame, equations and parameterization metrics”

**Comment 1.33.** Line 266: Change color coding. Blue lines in area with blue background gives poor insight.

**Reply:** As the differences between different slope angles are at times small, not all line colors are distinguishable in all sub-plots as they can lie on top of each other. The blue color is however clearly distinguishable from the background in Fig. 4 (c) so we have not changed it.

**Comment 1.34.** Line 266: Unclear figure that is not self-explaining. Indicate this is plain view.

**Reply:** We have indicated that this figure is in plain view.

**Comment 1.35.** Line 307: “Fig. 2 shows that the bank slope has little impact on the net outflux” why not? Explain.

**Reply:** We have rewritten this section. In general, infiltration through a sloped surface depends on the slope angle much more than during exfiltration. During the infiltration time, the gravity flow (vertical) and non-vertical flow components both are important whereas during exfiltration, vertical flow is much less pronounced.

**Comment 1.36.** Line 319: Unreadable with this many lines (Fig. 5a)

**Reply:** Agree, we have deleted that subfigure as well as the subfigures of hyporheic zone and penetration distance.

**Revision made:** Please see revised Fig. 3 at Line 281, Fig. 6 at Line 345, Fig. 7 at Line 351.

**Comment 1.37.** Line 337: Explain what mound means

**Reply:** We have rewritten this section and removed the term mound.

**Comment 1.38.** Line 390: confusing: 10 - 70 degree is all sloping. Same comment for the other graphs.

**Reply:** The results in Fig. 9 to Fig. 12 in the revised version of manuscript (which are Fig. 8 to Fig. 11 in previous version) indicate the RT ratio between the sloping and vertical river bank condition (($\mu_r^*(\mathbf{x}, t) = \mu_{r\text{-}S}^*(\mathbf{x}, t) / \mu_{r\text{-}V}^*(\mathbf{x}, t)$)), which were used to illustrate the direct impact of bank slope on RT.

**Comment 1.39.** Line 420: "penetration distance of the negative value of μr*area" strange phrasing

**Reply:** Agree, we have revised that sentence.

**Revision made:** Please see Lines 459-461: "Due to the scattered and nested flow paths near the cut bank and point bar, respectively, the area of negative value of $\mu_r^*$ at the cut bank of SWI is larger than that at the point bar."

**Comment 1.40.** Line 422: "to forced groundwater mixing" how established?

**Reply:** Fig. 8 shows that the water in the point bar was more aged in *y* (valley) direction compared to the -*x* (ambient groundwater gradient) direction. The base flow direction (-*x*) will be changed to *y* direction as the river stage arises as shown in Fig. 4b, thus, the water will mix with the water with older water compared to its original flow path. We have added the explanation about that statement to make a clearer statement.

**Revision made:** Please see Lines 461-464: "The change of flow direction near the point bar leads to a prolonged flow path for the water in the river as well as to forced groundwater mixing with the slightly older water (as shown in Fig.8 that the water was more aged in *y* direction compared to -*x* direction in the point bar)."

**Comment 1.41.** Line 456: explain in physical terms. Hard to grasp

**Reply:** Agree, we have added more explanation about that figure.

**Revision made:** Please see Lines 509-510: "$\mu^*_{out}(x, t)$ indicates the difference of flux weighted water RT (travel time) that the aquifer discharges into river compared to the initial condition."

**Comment 1.42.** Line 479: First part is very repetitive; please shorten

**Reply:** Thanks for your suggestion. We have shortened the first paragraph of this section.

Please see revised Section 4.1.

**Comment 1.43.** Line 502: Provide values; be more quantitative.

**Reply:** Agree. We have added the maximum underestimation rate of HEF caused by the neglecting of bank slope.

**Revision made:** Please see Lines 541-543: "We show that not accounting for bank slope and river sinuosity can lead to an underestimation of the infiltration rate of water from the river to the alluvial aquifer (by up to 120%), as well as the area and penetration distance"

**Comment 1.44.** Line 521: Not defined in the same way

**Reply:** Agree. We have revised the manuscript and use the same definition (point bar and cut bank) throughout the paper.

**Revision made:** Please see the revised manuscript and Lines 561-563: "Bank storage versus time for $\Gamma_d = 1$ and $\delta = 90^\circ$ condition at: the peak of point bar ($x = 0$); middle ($x = 0.25\lambda$); peak of cut bank ($x = 0.25\lambda$). Dimensionless bank storage was calculated by $\frac{\int_{Y(x,\,t)}^{Y(x,\,t)+4\lambda}[h\text{-}z_b\text{-}H_0]\mathrm{d}y}{\lambda H_p}$."

**Comment 1.45.** Line 533: "biogeochemical efficiency" meaning of efficiency? Please explain

**Reply:** For a clearer explanation, we have added new figures to prove the importance of bank slope on biogeochemical reactions, please see Fig. 15 in the revised version. The current sentence has been deleted.

**Revision made:** Please see the Section 4.2 in the revised version from Lines 566 - 604.

**Comment 1.46.** Line534-535: please turn around as chemistry follows physics and not opposite. Further, nutrient cycling is vague. Talk about kinetics or so

**Reply:** Same to the Comment 1.45, the current sentence has been deleted. Please see the Section 4.2 in the revised version of manuscript.

**Comment 1.47.** Line 554: rephrase. "It is important to note that ..." is a clumsy way of writing.

**Reply:** Agree, we have rewritten this sentence.

**Revision made:** Please see Lines 618-620: "In our simulations we assume a constant bank slope angle along the entire meandering river while natural riverbanks often change their slope angle from reach to reach as well as with time."

**Comment 1.48.** Lines 585-586, 589-592. explain why

**Reply:** Agree, we have added one more sentence for each of these conclusions.

**Revision made:** Please see Lines 682-686: "Sloping riverbanks can considerably increase HEF during a flood event, especially when the river is connected to an alluvial aquifer with rather high hydraulic conductivity and small bank slope angles as water can more easily infiltrate the connected aquifer. Smaller bank slope angles can lead to an extended hyporheic zone with river water infiltrating deeper (penetration distance) into the aquifer."

Please see Lines 689-691: "During a flood event, the impact of bank slope on residence time distributions (RTD) is more pronounced for high transmissivity aquifers, due to the larger area and deeper penetration distance of the HZ for these conditions"

**Comment 1.49.** The authors present an interesting modelling exercise on hyporheic exchange between river water and groundwater as function of river bank slope. This is an overlooked variable but highly relevant in the outer world. I made many remarks in the manuscript itself that are major to minor. My major overall concern is that the RESULTS chapter is a very, very hard read. It repeatedly took me two times reading to understand the meaning of sentences or text parts. This is largely due to the fact that the authors use the symbols and abbreviations instead of the hydrological terms. I therefore ask for a complete rewriting of this chapter where the features observed are explained in terms of hydrological processes. Attention should also be paid to the related figures and their captions as these are hard to grasp, too. I did make less small remarks in this chapter for this reason.

**Reply:** Thank you for your great suggestion and comments, we have revised the manuscript according to your comments left in the PDF file. Furthermore, we have rewritten the Results section by using more hydrological terms, and added more descriptions in the figure captions to make them self-explanatory.

**Comment 1.50.** The DISCUSSION chapter is meagre. The authors do not convince that slope is an important attribute when it comes to associated biogeochemical processes. The authors should provide some examples to illustrate this. They may also realise that many drinking water abstractions are situated in alluvial valleys of hilly or mountainous catchments. What does this mean and what is the impact of an abstraction? This is worth to be discussed under this chapter.

**Reply:** Thank you for your comment. To prove the importance of including bank slope when assessing biogeochemical processes, we added additional information to section 4.2 in the discussion section. We analyzed biogeochemical zonation by using timescales of a common constituent such as dissolved organic carbon (DOC), oxygen and nitrite, and discuss results for vertical and sloping conditions (angle of 10 degrees) as shown in the new Figure 15.

Additionally, we have extended our discussion in section 4.3 regarding the model assumptions and limitations. In this study, we assume an unconfined alluvial aquifer of sufficient extent that is well connected to the river and allows for surface water-groundwater exchange if a sufficient hydraulic gradient is present. We now acknowledge that in other settings where there is no hydraulic gradient between river and aquifer, no large-scale infiltration of river water into the riverbanks will occur, while local turbulent flow (e.g., due to obstacles in the river channel) might lead to localized infiltration over short distances and short time scales. Furthermore, where the unconfined layer is small (e.g., in mountainous headwater streams with a rather small sediment layer overlying a hard-rock aquifer with relatively low hydraulic conductivity), the HZ is limited in its maximum extent, and travel times and distances are considerably shorter. However, in mountainous settings, slope angles are often much steeper due to erosion (here rivers incising into the bedrock) and further simulations are required to better understand the feedback between banks lope angle, hydraulic gradient and maximum extent of the unconfined layer allowing for reasonable river water infiltration. We think that future simulations should be conducted for numerous scenarios where these different settings can be studied individually and the impact on water abstraction can be assessed.

**Revision made:** Please see section 4.3 from Lines 566-590 for the importance of bank slope on biogeochemical process: "Residence time distributions of river water in the alluvial aquifer have been used to evaluate the potential of biogeochemical reactions by comparing the RT with biogeochemical timescales (BTS) for given solutes (Boano et al., 2010b; Gomez-Velez et al., 2012). Locations where the ratio of RT to BTS is small indicate a high reaction potential for that chemical species. It has been documented that the BTS for dissolved organic matters (DOC) is site-dependent and can vary over ten orders of magnitude ($10^{-1}$ – $10^{9}$ d) (Hunter et al., 1998), while BTS for oxygen and nitrite have been found to vary over eight orders of magnitude ($10^{-2}$ – $10^{6}$ d) (Gomez-Velez et al., 2012). Here, we compare the RTD within these two BTS ranges for vertical and sloping riverbank condition ($\delta$ = 10° ) at the peak time of the flood event ($t/t_p$ = 0.25) for different aquifer transmissivity conditions, and show the zonation of residence times by using a BTS range of $10^{-1}$ – $10^{6}$ d (Fig. 15).

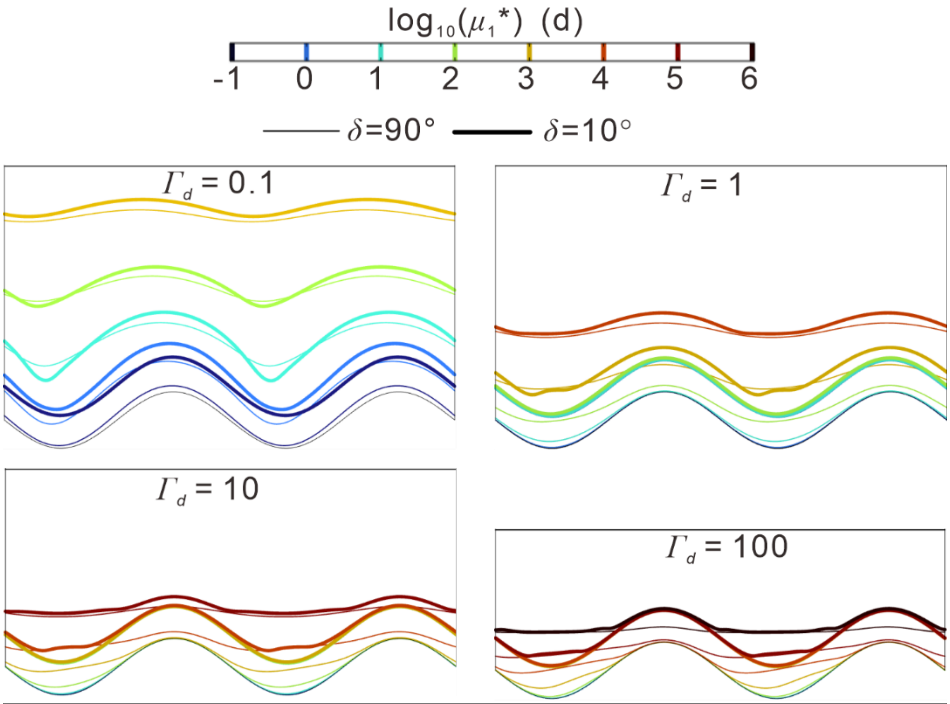


**Figure 15.** Zonation of biogeochemical timescales (BTS, range of $10^{-1}$ – $10^{6}$) for common HZ constituents such as DOC, oxygen or nitrate for different aquifer transmissivities at $t/t_p$ = 0.25. thick and thin lines indicate the comparison of vertical vs sloping riverbank ($\delta$ = 10° ) conditions, while the different colors indicate the different exponents.

Fig. 15 indicates that neglecting bank slope will impact the prediction of reaction potentials during the hyporheic exchange process, especially for locations with short time scales. For sloping bank conditions, the reaction hot spots (areas) expanded into the aquifer, identical to the overestimated areas in Fig. 9 to Fig. 12. Note that we did not aim to include specific reaction models in our study but used RTD as an indicator for various biogeochemical reactions in the aquifer."

Please see Lines 636-651 for the impact of drinking water abstraction: "The current study assumes a perennial stream and unconfined (phreatic) conditions in the connected aquifer as well as changing hydraulic gradients leading to gaining and loosing conditions in the river. Where there is no hydraulic gradient between river and aquifer, no large-scale infiltration of river water into the riverbanks will occur, while local turbulent flow (e.g., due to obstacles in the river channel) might lead to localized infiltration over short distances and short time scales (Sawyer et. al., 2011; Stonedahl et al., 2013; Käser et al., 2013). Where the unconfined layer is small (e.g., in mountainous headwater streams with a rather small sediment layer overlying a hard-rock aquifer with relatively low hydraulic conductivity), the HZ is limited in its maximum extent, and travel times and distances are considerably shorter. However, in mountainous settings, slope angles are often much steeper due to erosion (here rivers incising into the bedrock) and further simulations are required to better understand the feedback between banks lope angle, hydraulic gradient and maximum extent of the unconfined layer allowing for reasonable river water infiltration. These simulations will also help us better understand the impact of bank slope on water supply and water quality to abstraction wells, e.g., used for the production of drinking water."

**Comment 1.51.** Another topic is whether the authors deal with phreatic or confined aquifers when it comes to propagation of hydraulic pressure. This makes a big difference and is relevant in the field but not addressed at all. I also ask them to discuss the other case in the DISCUSSION chapter (in terms of differences and similarities).

**Reply:** We are not quite sure what the reviewer is referring to here. The assumptions for our model scenarios are that the aquifer is unconfined, and flow is only constrained by hydraulic conductivity/transmissivity. We now acknowledge in chapter 4.3 that not all river environments adhere to these assumptions and that some have a rather thin layer of unconsolidated sediment on top of a bedrock aquifer. In such cases the possibility of the water to infiltrate might be limited due to drastic changes in hydraulic conductivity and future studies could focus on defining various scenarios (e.g., layered aquifer with variable hydraulic conductivity values and variable bank slope angles) for specific environments. However, we think that this is beyond the scope of this study as each scenario would require a distinct model discretization.

## Comments from Review #2

### Main Comments:

**Comment 2.1.** The manuscript is generally well-written. I suggest revising the text to improve clarity (see my comments below). For example, the methods introduce equations (line 200) with variables that need to be clearly defined. Also, I suggest making the introduction more succinct to focus on the actual study.

**Reply:** Thank you for your comment. We have revised Line 200 and the Introduction according to your comment.

**Revision made:** Please see Lines 213-219: "They found that the dynamic variations of HEF and RTD are mainly determined by ambient groundwater flow and the ratio of aquifer hydraulic conductivity to the duration of the flood event (referred to as dimensionless constant $\Gamma_d = \frac{S_y\lambda^2}{0.5K(1+n_0)H_0t_d}$, see Table 1 and Fig. S2, where $S_y$ is specific yield [-]; $\lambda$ is wave length of sinuous river; $K$ is hydraulic conductivity [$LT^{-1}$]; $n_0$ is intensity of flood event [-] $H_0$ is base river stage [L]; $t_d$ is duration of flood event [T])"

**Comment 2.2.** After reading the current manuscript, I tried to answer the question: is the work making a significant or incremental contribution to our understanding of hyporheic processes? In other words, did I learn something new that wasn't in Gomez-Velez et al. (2017)? Based on the current version of this manuscript, my answer is "no."

**Reply:** Thank you for your comment. We have tried to further clarify the differences between our study and that of Gomez-Velez et al. (2017). We would like to again stress that here we look at the impact of riverbank sloping angle on water flow and solute transport in the banks and connected aquifer. Gomez-Velez et al. (2017) consider the riverbank always vertical in their study. As such, our study presents new findings including:

1) The bank slope angle has an influence on how far water is transported into the aquifer as well as on the residence (travel) times. This impact is more pronounced for smaller bank slope angles. Gomez-Velez et al. (2017) could not have come to similar findings as they have not studied variations in bank slope angle.

2) Aquifer transmissivity also influences the impact time of bank slop on residence time distributions. We found that for a higher transmissivity aquifer, the impact of bank

slope on RTD were observed only during the dynamic flood event while long-lasting impacts can be found after the flood event for low transmissivity aquifers. This again cannot be concluded from Gomez-Velez et al. (2017).

3) Our study finds that the impact bank slope has on RTD is also influenced by the location along the sinuous river where water enters the aquifer. For example, including bank slope will lead to generally shorter residence times around cut banks ($x$ = 0) but longer residence times around point bars ($x = \lambda$). This is a new understanding and has not been addressed by Gomez-Velez et al. (2017) or any other research.

**Comment 2.3.** This work adds additional complexity to capture the effect of the sloping bank; however, the approach includes strong assumptions that might defeat its purpose. For example: The deformed geometry method (DGM) proposed here ignores the primarily vertical fluxes within the wedge cut by the moving boundary, which is still assumed vertical. Is this a reasonable assumption, and what are the implications? The importance of the bank slope highlighted by Liang et al. (2018), Doble et al. (2012), and others for cross-sectional, partially saturated models results from the vertical component of the exchange and the migration of the air-water interface. These processes are ignored here; therefore, I wonder whether the current model addresses these issues. Reproducing the simulations from Gomez-Velez et al. (2017) is helpful for completeness and verification; however, I suggest that the authors use a 3D version of this conceptual model to verify the appropriateness of their 2D reduced-complexity model and the implemented DGM.

**Reply:** Thank you for your comment and suggestion.

The Boussinesq equation as implemented here, ignores the vadose zone. This approach has been used previously (Boano et al. (2014) and Gomez-Velez et al. (2013; 2017)) for vertical riverbanks. However, the vertical flow component exists for both vertical and sloping river bank condition, especially for the conditions with small slope angle or rapidly rising river stage.

We agree with the reviewer that using a 3-D version model to test the accuracy of our assumption (vertical SWI during the fluctuation of river stage) and implementation of the DGM (ignoring vertical fluxes and vadose zone) would be beneficial. However, to go from our 2D model version with 0.5 million mesh elements (to avoid numerical errors) to a 3D model (using the Richard's equation) would lead to an extremely large number of mesh elements and we for the moment simply lack the computing power to run such a model. However, this is one of our future goals.

In order to test the performance of our modeling approach at least on a simple rather scale we compared the hyporheic exchange flux and bank storage difference between a vertical 2-D model (in which the flow field is calculated by the Richard's equation) and a 1-D horizontal model (in which the flow field is calculated by the Boussinesq equation and the DGM is implemented). These two models can be regarded helpful in answering the reviewer's concerns although they ignore the sinuosity of the river and the ambient flow gradient. Model implementation, mathematical statements and results are further outlined in the SI as S4. In short, while we found differences in HEF patterns when comparing simple models using the Boussinesq with those using Richard's equation these differences exist mostly independent of using the DGM. We therefore conclude that our model approach is not more or less capable of representing reduced 3D environments as the models used in previous studies. In principle, most likely, all reduced models used in literature mis-represent 3D flow patterns. As such, further studies should look into running comparative scenarios between a 2D and an actual complex and computationally heavy 3D model for a variety of real world cases to robustly quantify this error.

**Revision made:** Please see Lines 652-664 in the revised manuscript, and the Section S4 in the SI file at Lines 149-234.

**Comment 2.4.** Within the dimensionless context, the metrics show only mild differences with the bank slope. Cases with significant differences require a $\Gamma_d$ of 100, which is relatively uncommon in natural systems (see Figure 2A in Gomez-Velez et al. (2017)). The authors need more clarity about their definition of "significant" differences caused by the bank slope and put this in the context of physical and biogeochemical processes and observations. Based on these results, I would conclude that representing the slope of the banks (with the Authors' conceptualization) does not have significant implications for the exchange flux and other metrics.

**Reply:** Thank you for your comment. While we agree that a Gamma of 100 is relatively uncommon, we would like to point out that as shown in Figure 5 in our manuscript, a Gamma of 10 can already show considerable differences in changes to the net flux. For example, for a slope angle of 20 degree which is not uncommon in lowland rivers the net flux can increase by about 20% is compared to a 90 degree slope angle. Additionally, Fig. 9 to Fig. 12 show that the impact of bank slope on residence time distributions can be significant during and after a flood event for high-transmissivity and low-transmissivity aquifers, respectively. As such, we would conclude that (not) representing the bank slope definitely can have implications for the exchange flux and other metrics. In the end, we decided to rewrite the entire section and remove the word significant to avoid confusion.

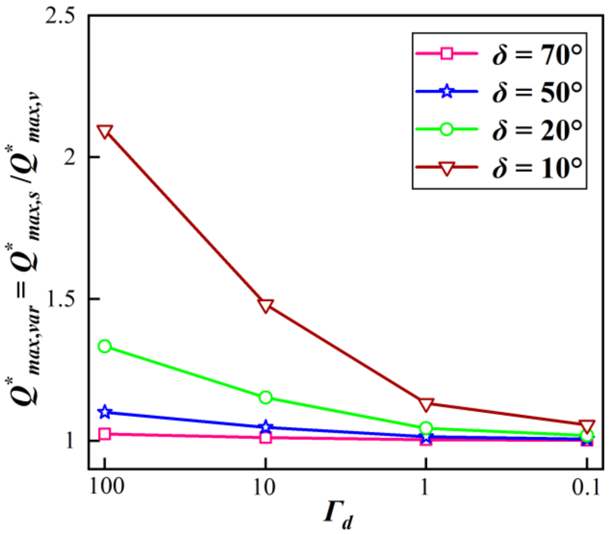


Figure 5 in our manuscript showing Gamma vs Ratio of maximum net flux for slope to no-slope (vertical river bank) conditions $Q^*_{max,var} = Q^*_{max,s}/Q^*_{max,v}$ for four aquifer transmissivities and slope angles.

**Revision made:** Please see the revised manuscript.

__**Comment 2.5.**__ The most novel aspect of this manuscript relates to using a deformed geometry method; however, there is no detail about its implementation.

**Reply:** We have added a statement about the theory and implementation of the DGM as well as additional text in Section 2.1 to further describe the method. As the DGM has not been developed by us we have refrained from adding the respective mathematical backbone and referred the interested reader to the respective sources.

**Revision made:** Please see Lines 195-209 in the revised version of manuscript: "Fig. 1 illustrates the river stage hydrograph of this study (Fig. 1a, calculated by Eq. (S2)) and the diagram of the displacement of the SWI (Fig. 1b) during the flood event after coupling DGM into the model. The colored river boundaries in Fig. 1b are corresponding to the times of colored dots in Fig. 1a. Additionally, solute transport and RTD were simulated based on the extent of the flow field according to Gomez-Velez et al. (2017), as shown in the SI (S2 and S3, respectively).

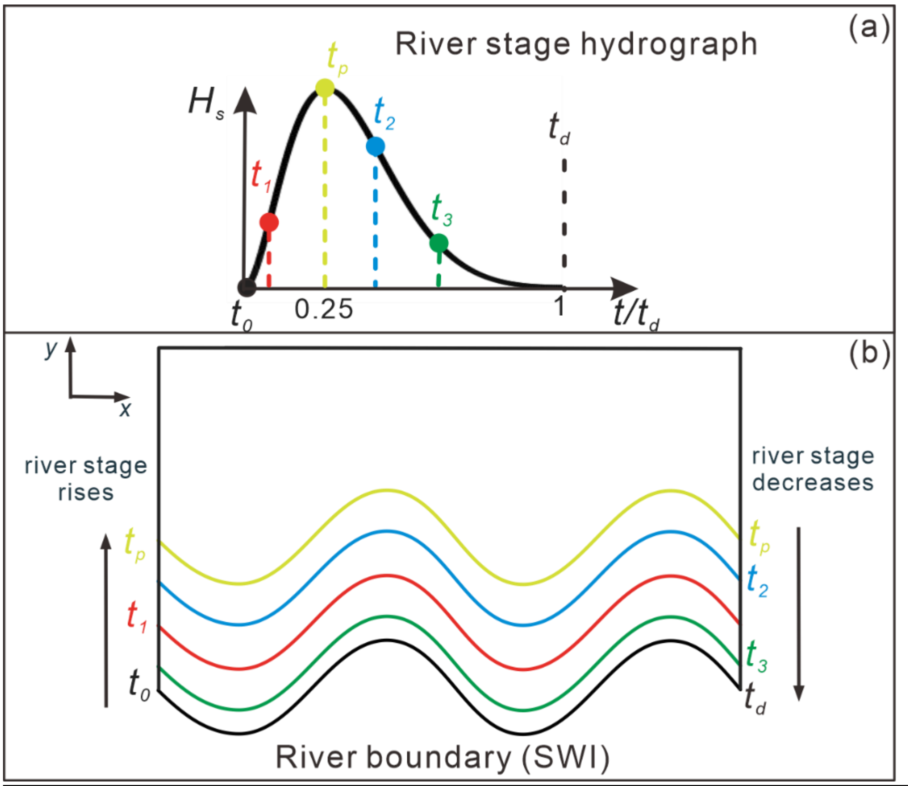


**Figure 1.** (a) River stage hydrograph during the flood event; (b) diagram showing displacement of SWI during the flood event. The colored SWIs in (b) correspond to the times of colored dots in (a). When the river stage increases, the river boundary migrates into the aquifer and recovers to its initial location as river stage decreases. The upward and downward arrow in Fig. 1b indicates the raising and decreasing of river stage, respectively."

**Comment 2.6.** The SI section is almost a copy of the methods from Gomez-Velez et al. (2017). I leave this issue for the Editor to resolve, but there should be a balance between credit and completeness. To some degree, it feels like the SI could be replaced by a sentence like "We use the same methods, equations, and metrics described in Gomez-Velez et al. (2017). The only difference is the implementation of a deformed geometry method to capture the dynamic evolution of the wetting front along the sloping banks," followed by the exact details of the moving boundary equation (y(x,t) in the manuscript) and the explanation of the deformed geometry method.

**Reply:** Thank you for your comment. We agree that some of the SI is similar to Gomez-Velez et al. (2017) and Singh et al. (2019), with respect to parts of the conceptualization and construction of the model. However, as the previous model of Gomez-Velez et al. (2017) (in effect the baseline case with a vertical riverbank) has not been made available by the authors despite our request, we had to build our own version of their case. As the reviewer can see, our model does not reproduce the exact same results for the baseline case as shown by Gomez-Velez et al. (2017) as we had to make decisions regarding model discretization, mesh size and mesh element number that are not based on their study (the respective information was not provided) but that seem to have a slight effect on the results in the baseline scenario. In the interest of full disclosure, we decided to put our methodological approach in the SI.

We have made the following clarifications in the main text and the SI highlighting the connection to Gomez-Velez et al. (2017):

Lines 143-146: "We build on the numerical modeling approach introduced by Gomez-Velez et al. (2017) and consider lateral bank slope by coupling the deformed geometry method (DGM) to the flow (Liang et al. 2020), the solute transport and the residence time distribution equation."

Lines 176-181: "In contrast to Gomez-Velez et al. (2017), the displacement of the SWI caused by the deformation of the model domain ($M(t) = [h(t) - h(0)]/\tan(\delta)$, where $h(t)$ [L] is transient hydraulic head) is added in Eq. (1), which represents the displacement of the river boundary in *y*-direction due to river stage fluctuation and bank slope angle (see the horizontal distance between the vertical red and green solid line in Figure S2c)"

Lines 16-19 in SI: "We use the same methods, equations, and metrics described in Gomez-Velez et al. (2017), however, we implement here a deformed geometry method to capture the dynamic evolution of the wetting front along the sloping banks, while Gomez-Velez et al. (2017) assumed a vertical river bank."

## General comments:

**Comment 2.7.** Line 20-21: "This new model approach serves as the initial step to consider complicated floodplain morphologies in physics-based models for better predictions of HEF…" is inaccurate. This model is a refinement of a reduced-complexity model, but the literature is full of significantly more complex models that capture the complexities of banks and floodplains.

**Reply:** Thank you for your comment. We have revised that unclear sentence.

**Revision made:** Please see Lines 19-22: "This new modeling approach serves as the initial step focusing on the impact of bank slope on the hyporheic exchange flux (HEF) and the residence time distribution (RTD) of pore water in the fluvial aquifer for a sinuosity-driven river corridor."

**Comment 2.8.** I need clarification on this statement, which seems conceptually incorrect. I suggest rewording for clarity.

**Reply:** Agree, we have revised that sentence.

**Revision made:** Please see Lines 52-55: "The hyporheic exchange flux (HEF) represents the interaction flux between surface water and groundwater in vertical (e.g., bedform-driven) and horizontal (e.g., meander-driven) directions, which can add to general regional groundwater ex-filtration and infiltration."

**Comment 2.9.** You need to define the parameters used here. You could use the conceptual figure from the SI.

**Reply:** Agree, we have added the definition of these parameters.

**Revision made:** Please see Lines 213-219: "They found that the dynamic variations of HEF and RTD are mainly determined by ambient groundwater flow and the ratio of aquifer hydraulic conductivity to the duration of the flood event (referred to as dimensionless constant $\Gamma_d = \frac{S_y \lambda^2}{0.5K(1+n_0)H_0 t_d}$, see Table 1 and Fig. S2, where $S_y$ is specific yield [-]; $\lambda$ is wave length of sinuous river; $K$ is hydraulic conductivity [$LT^{-1}$]; $n_0$ is intensity of flood event [-] $H_0$ is base river stage [L]; $t_d$ is duration of flood event [T])"