Re: Revised Manuscript Preprint egusphere-2025-5651 (Quantification of Delayed Recharge by Soil Surface and Riverbed Infiltration in a Deep Groundwater Depression Zone in the North China Plain)

Authors' Responses to Comments from Dr. Nima Zafarmomen:

General Comments

The paper addresses an important and very topical problem: delayed recharge in deep vadose zones within a major groundwater depression cone in the North China Plain, comparing precipitation-fed vs riverbed recharge using HYDRUS-1D plus borehole lithology. The regional perspective and explicit focus on lag times and percolation velocities are valuable and fit well within hydrology / groundwater journals. I recommend it for publication after considering below comments.

Response: We sincerely thank Dr. Nima Zafarmomen for the positive evaluation and the encouraging summary of our work. We appreciate your recognition of the importance of this study regarding delayed recharge in the deep vadose zones of the North China Plain. We are also pleased that the regional perspective and the specific focus on lag times and percolation velocities using HYDRUS-1D and borehole lithology were well-received. We have carefully addressed all specific comments and will incorporate the necessary revisions into the final manuscript to enhance its clarity and scientific rigor.

Specific Comments

1. You currently equate "recharge efficiency" mostly with higher percolation velocity and shorter lag time, but sometimes imply it means larger recharge volume. Please give a clear, formal definition early in the paper and stick to it. When you say riverbed recharge is ~4.1× higher "per unit area", clarify this is based on velocity, not on simulated recharge flux volume, or explicitly compute and show fluxes.

Response 1: We sincerely thanks for pointing out the ambiguity regarding the term "recharge efficiency". We agree that a formal definition is necessary to distinguish between the rapidity of the process and the total recharge volume. To address this, we will implement the following modifications in the revised manuscript:

1) We will add a clear definition of “recharge efficiency” in the Introduction section. We will explicitly define it as the rapidity of the vadose zone response, quantified by average percolation velocity rather than the total volume of water.
2) In the Discussion section, we will clarify that the statement “4.1 times higher” refers to the recharge rate based on average percolation velocities, not the total simulated flux volume.

2. Key modeling choices—uniform initial head (–50 cm), 1D vertical flow only, and no root uptake for riverbed cases—are reasonable but need clearer justification. Explain that the long spin-up minimizes sensitivity to the initial profile and that omitting ET in riverbeds makes the riverbed scenario optimistic. Also acknowledge that lateral flow, preferential flow, and riverbed clogging are not represented and discuss qualitatively how this may bias lag times.

Response 2: Thank you for highlighting the need to better justify our modeling assumptions and discuss their implications. We agree that while these simplifications are standard for regional-scale vadose zone modeling, their potential biases should be explicitly addressed. We will revise the manuscript in the “Methods” and “Discussion” sections to address these points and these additions will provide a balanced and transparent interpretation of the model’s capabilities and limitations. The following clarifications and justifications will be incorporated into the revised manuscript:

1) We will clarify that the 6-year spin-up period (2016-2022) is specifically implemented to minimize the sensitivity of the simulation results to the uniform initial pressure head (-50 cm). This ensures that the soil water profile reached dynamic equilibrium before the analysis period. We will add a new figure (Figure B1 in the Appendix B.) that displays the temporal evolution of soil water content at deep layers (20-80 m) for all boreholes during the spin-up phase.
2) We will add an explanation that neglecting root water uptake in the riverbed scenario provides an upper bound estimate of recharge efficiency.
3) We will add a new paragraph in the Discussion section to qualitatively analyze the biases introduced by 1-D flow and preferential flow, and riverbed clogging.

Regarding riverbed clogging, our model setup was based on detailed borehole lithology which was parameterized into seven categories (as described in Section 2.3.1). This detailed parameterization explicitly included low-permeability layers (such as clays and silts) at various depths, and to a certain extent, it can reflect the obstructive effect of low-permeability layers on riverbed infiltration.

3. IDW interpolation of 24 points over ~2,000 km² is appropriate for a first-order picture but provides no uncertainty and may be weak where points are sparse. Clarify that maps of infiltration time and velocity should be interpreted qualitatively, especially in poorly constrained regions. Briefly justify the choice of IDW over kriging (e.g., limited data for robust variogram fitting) and mention this as a limitation.

Response 3: We appreciate the reviewer's insightful comment regarding the spatial interpolation method. We acknowledge that with a limited dataset (num = 24), the resulting maps serve primarily as a regional trend. We will revise the manuscript to address these points explicitly, including:

1) We will add a justification for selecting IDW over Kriging and explain that the limited number of data points was insufficient for robust variogram fitting, making IDW a more appropriate choice for approximating general trends in this context.
2) In the Results section, where the maps are introduced, we will add a cautionary note stating that the maps should be interpreted qualitatively, especially in regions with sparse data coverage.
3) In the Discussion section, we will explicate that the spatial analysis was constrained by the sparsity of points and the lack of uncertainty quantification in the IDW method.

4. The constant-head lower boundary at the long-term average groundwater level is a strong simplification in a declining groundwater system and likely underestimates true lag times. Justify this assumption more clearly and discuss its effect on results; a short sensitivity discussion would help. Similarly, using a single rainfall station and a single river stage series for the whole area needs explicit justification and acknowledgement of added uncertainty.

Response 4: We sincerely thanks for identifying these critical simplifications regarding the boundary conditions and forcing data. We agree that these assumptions require explicit justification and a discussion of their implications. We will revise the manuscript to address these concerns as follows:

1) Considering that the actual groundwater level in the North China Plain is constantly changing, we agree that assuming a constant groundwater level is a simplification. We will add a discussion in the Discussion section acknowledging the sensitivity of the results to this assumption. In the revised manuscript, we will acknowledge that the groundwater level dynamics are complex, with possibilities for both decline (due to extraction) and rise (due to management), and we will continue to optimize and solve this problem in subsequent studies.
2) We will add text to explicitly justify the use of single-station data. As stated in the revised manuscript, the Baixiang Rain Gauge Station and the selected river stage sequence were chosen for the high continuity of their observational records within the study area. Furthermore, applying these data uniformly across the region serves a specific methodological purpose, i.e., to control meteorological variables. By keeping the surface inputs constant, we can isolate and focus primarily on the influence of vadose zone heterogeneity (thickness and lithology) on infiltration recharge, which is the central objective of this study.

5. I strongly recommend to discuss the paper and deepen your discussion "Assimilation of sentinel ‐ based leaf area index for modeling surface ‐ ground water interactions in irrigation districts".

Response 5: Thank you for the comments. We have carefully reviewed the recommended paper and agree that it offers critical insights into improving the representation of vegetation dynamics in hydrological modeling. We will integrate a discussion of this work into the Discussion section of the revised manuscript.

6. The phrase "two infiltration modes were considered: precipitation-fed and riverbed infiltration" could be tightened to "precipitation-fed soil infiltration and riverbed infiltration".

Response 6: Thank you for the comments. We will modify the phrase in the Abstract exactly as suggested.

7. When mentioning the regression equations, briefly state the key predictors (vadose zone thickness and particle fractions) to give the reader more context.

Response 7: Thank you for the comments. We will revise the sentence to explicitly list the specific independent variables used in the regression analysis.

8. Some paragraphs are quite long and dense (e.g., lines 41–64, 85–110). Consider splitting into shorter paragraphs to improve readability.

Response 8: Thank you for the comments. Clarifications will be made in lines 41-64 and 85-110, Introduction of the revised manuscript.

9. When you review past work (HYDRUS applications, global lag studies), explicitly state the remaining gap you are addressing (combined effect of deep vadose zones, complex lithology, and 'comparison of two recharge sources under identical profiles)'. You do this, but it could be more sharply framed at the end of the Introduction.

Response 9: Thank you for the comments. We will rewrite the paragraph at the end of the Introduction to explicitly state the gap regarding the combined effects of lithology and depth, and the lack of comparisons under identical profiles.

10. It might be helpful to explicitly mention average annual precipitation and reference ET, if available, to characterize the climate quantitatively.

Response 10: Thank you for the comments. We will add the long-term average precipitation and evaporation data to Section 2.1 Study Area.

11. The description of boundaries (Taihang Mountains, Shijiazhuang, Hengshui) is good, but consider adding one sentence stating dominant land use (e.g., double cropping, wheat–maize rotation) to connect with the root uptake assumptions.

Response 11: Thank you for the comments. We will add a description of the dominant cropping system to Section 2.1 (Study Area)

12. "Depth (cm)" is given for boreholes, but values like 8,080 cm (= 80.8 m) etc. Make clear that these are vadose zone thicknesses down to shallow groundwater table or borehole depth; the phrase "Depth (cm)" is ambiguous.

Response 12: Thank you for the comments. We will clarify that the values listed in Table 1 represent the thickness of the vadose zone rather than the total depth of the borehole and will add an explanatory note to the caption of Table 1.

13. You might add a column indicating vadose zone thickness vs. total borehole depth if they differ.

Response 13: Thank you for the comments. Since the focus of this study is on vadose zone infiltration and recharge, the "Vadose Zone Thickness" is the critical vertical parameter. As detailed in our response to the previous comment, we have explicitly clarified in the caption of Table 1 that the listed "Depth" refers specifically to the vadose zone thickness. We believe this clarification effectively removes the ambiguity regarding the vertical dimension used in our models without needing an additional column.