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April 20, 2026

Memorandum

To: Xavier Sanchez-Vila, Ph.D. Editor, *Hydrology and Earth System Sciences*

Subject: Final response of **EGUSPHERE-2025-6295**

Dear Editor:

On behalf of all authors, I would like to express our sincere gratitude to you and the reviewers for your time, constructive comments, and the positive evaluation of our manuscript. We have carefully considered all comments and have revised the manuscript accordingly.

We are delighted to note the Editor's conclusion that the manuscript can be accepted for publication without further revisions. Nevertheless, to ensure the highest standard of scientific precision and to fully address the reviewer's concern, we have voluntarily made some minor but important revisions to the manuscript, as suggested. These revisions do not alter any scientific conclusions but improve the clarity and terminological consistency of the paper.

Below we provide a point-by-point response to the reviewer's comment, along with the exact locations and wording of the modifications refers to the revised manuscript.

Response to Reviewer #1:

The authors present a novel 3D quantitative method for characterizing eddy zones and successfully link these physical characteristics to the Mobile-Immobile Model (MIM) parameters. The methodology is sound, and the results are clearly presented. Here are a few questions and suggestions, particularly the simplification and deepening of the introduction section, to help further refine the manuscript:

1. Terminology Consistency (Turbulence vs. Laminar Flow): The title of the manuscript uses the phrase "turbulent channel flows". However, in Section 3.2, the authors note that the Reynolds numbers range from 113 to 1697 and explicitly clarify that the observed eddy formation is a result of "inertial effects within the laminar flow regime, rather than full turbulence". The authors should clarify this terminology choice, which would be helpful to either adjust the title or add a brief explanation in the introduction to ensure consistency with the described flow regime.

Reply: We thank the reviewer for this important observation. We agree that the original wording was inconsistent. The flow regime in our study (*Re* = 113–1697) is indeed dominated by inertial effects within the laminar to transitional regime, not by fully developed turbulence. To eliminate any potential confusion, we have made the following two modifications to the final manuscript:

1) Revision of the title: The revised title is now: "Eddy-driven effects on solute transport in porous media". See line 1.

2) Addition of a clarifying sentence in the Introduction: We have added one sentence to explicitly state the nature of the flow regime investigated. See lines 108-112.

2. Optical Measurement Uncertainty: The authors accurately acknowledge the "increased uncertainty in low-concentration estimation" which is an inherent limitation of the optical calibration method. Given that late-time tailing (e.g., t98) relies heavily on these low-concentration readings to quantify eddy-driven retention, the authors should briefly discuss in the text how this specific limitation might affect the precision of the fitted MIM parameters, particularly the immobile zone ratio (1-β)?

Reply: We greatly appreciate this insightful comment. To avoid any future confusion for readers and to strengthen the justification for our methodology, we made supplementary explanations. See lines 842-848.

3. Inert Tracer Assumption: The numerical simulation and the governing equations of the MIM model assume that Brilliant Blue acts as a perfectly inert solute without adsorption or degradation. While this is a reasonable assumption for the experimental setup, is there any possibility of very slight physical sorption onto the artificial spheres? A brief comment on whether minor sorption could have any compounding effect on the observed non-Fickian tailing would strengthen the discussion.

Reply: We deeply appreciate the reviewer's rigorous perspective. We completely agree that explicitly discussing this theoretical compounding effect and how our numerical setup addresses it will significantly strengthen the rigor of our manuscript. We have added a brief discussion in Section 4 (Solute transport model), immediately following the introduction of the MIM governing equations. See lines 767-777.

4. Robustness of the v_c Threshold Method: The determination of the critical velocity threshold (vc) using the PDF/CDF inflection point is an elegant approach for quantifying the 3D eddy volume in these specific structural packings. How robust do the authors expect this threshold identification method to be if applied to more natural, highly heterogeneous porous media with a wide particle size distribution (unlike the uniform spheres used here)? Adding a short perspective on this in the discussion would highlight the broader applicability of your method.

Reply: We thank the reviewer for this insightful comment regarding the scalability and robustness of the v_c threshold method. As suggested, we have added a short perspective on the broader applicability of this method to heterogeneous media in the Section 2.3. See lines 427-434.

5. The first paragraph of the introduction contains a significant amount of textbook knowledge or general background information, such as the current state of contamination and classification of aquifer media. While such content is necessary, it has not yet established a strong connection with the core keyword "Eddy effects" and therefore requires substantial condensation. Similarly, lines 73–83 are overly verbose in setting the stage for introducing "Eddy."

Reply: We agree with the reviewer that the initial background was too broad and lacked a direct bridge to the study's core focus on "eddy effects." We have revised the introduction part, see lines 47-108.

6. The entire introduction section contains only lines 84–107 that offer an in-depth literature review of the core topic. While the latter part of this section talks about the advantages of the MIM model in characterization from a technical perspective, the first half still fails to clearly articulate the significance of this study. For example, line 93 mentions that current research rarely considers the impact of eddies on solute transport at the pore scale, even though the preceding text has already discussed their influence on pore-scale water flow. However, why bridging the understanding from water flow to solute transport presents such a major challenge or gap remains unclear to the reader. Thus, further elaboration is needed.

Reply: We sincerely appreciate the reviewer's constructive feedback. We agree that the transition from hydrodynamics to solute transport lacked a clear articulation of the fundamental challenges that have historically prevented this connection. We have revised and expanded the manuscript, see lines 135-149.

7. Line 92 mentions that previous studies have not directly observed the eddy region. This raises the question: are there any potential experimental methods or techniques that could address this gap? Given that the authors subsequently conducted detailed laboratory experiments, it is recommended that they provide some background on the current state of research regarding such methods. This would help underscore the necessity and significance of the experimental work presented later.

Reply: We sincerely thank the reviewer for this excellent suggestion. We fully agree that outlining the current state of experimental visualization techniques significantly strengthens the rationale for our methodological design. We have inserted the following background context into the Introduction, see lines 115-134.

8. Typos in this manuscript such as the figure caption in Fig. 4 ("comparison between") should be checked.

Reply: We apologize for these oversight errors. We have conducted a thorough, line-by-line proofreading of the entire manuscript to identify and rectify any remaining spelling, grammatical, or formatting inconsistencies. See lines 301-306, 244, 545.

9. Figure 4 is significant for the validation of this proposed method, there the authors should better try best to analyze why there are larger fitting errors in the case of "D=5mm"than those in other cases.

Reply: We agree with the reviewer that a deeper analysis of the fitting discrepancies in the D=5 mm case is essential for validating the limits of the proposed method. We have revised and expanded the text, see lines 313-320.

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Response to Reviewer #2:

This manuscript presents a pore-scale experimental and numerical investigation of eddy-driven non-Fickian solute transport in porous media. A three-dimensional numerical method is developed to quantify eddy-zone proportion under varying flow velocities, particle sizes, and packing structures. The relationship between eddy proportion and the immobile fraction of the Mobile–Immobile Model (MIM) is also examined.

The study is scientifically interesting and technically well executed. The topic is relevant, and the attempt to provide a quantitative pore-scale basis for conceptual MIM parameters is valuable. However, several weaknesses need to be addressed before the manuscript can be considered for publication.

Major Concern

My primary concern relates to the validation strategy. The experimental validation is currently limited to comparison of hydraulic behavior (v–J curves). While this confirms the accuracy of the simulated flow field, it does not sufficiently validate the solute transport model.

The breakthrough curve (BTC) behavior—particularly early arrival and tailing—is central to the study's conclusions regarding eddy-driven non-Fickian transport. Therefore, the solute transport results should be validated directly against experimental BTC data.

I strongly recommend including a comparison between experimental and simulated outlet BTCs under representative flow conditions. Even if limited to a subset of cases, such a comparison would significantly strengthen the robustness of the numerical model and the credibility of the conclusions. low validation alone is not sufficient to validate transport behavior.

Reply: We sincerely thank the reviewer for pointing out this critical limitation in our validation strategy. We fully agree that while validating the flow field (*v*-*J* curves) is a foundational step, directly validating the solute transport model against experimental breakthrough curves (BTCs) is absolutely necessary to substantiate our conclusions regarding eddy-driven non-Fickian transport. We have revised the defensive paragraph regarding validation. See lines 286-293, 327-350 and Figure 5.

Additional Comments

Line 24

The statement that pore-scale eddies in solute transport "remain underexplored" is not entirely accurate. Several previous studies have investigated this topic. Please revise the wording to reflect that, although eddies have been studied, their quantitative linkage to upscaled transport parameters remains insufficiently established.

Reply: We sincerely thank the reviewer for pointing out this inaccuracy. We have modified the sentence see lines 23–28.

Line 213

The manuscript mentions a mesh sensitivity analysis but does not present the results. Please include the independent mesh convergence analysis, either in the main text or as supplementary material.

Reply: We thank the reviewer for this constructive suggestion. We agree that presenting the mesh sensitivity analysis is crucial for ensuring the reliability of our numerical simulations. We have revised and expanded the manuscript see lines 270–277 and Figure 4.

Line 224

The authors state that validation is based on previous experimental data. Why was the current experimental setup not used directly for model validation? Please clarify.

Reply: We thank the reviewer for this careful observation. We have revised the sentences see lines 286–293.

Lines 244–260

I disagree with the statement that validation of the flow field alone ensures the reliability of the solute transport simulation. Solute transport models must be validated using experimental transport data. At minimum, the simulated and observed outlet BTCs should be compared.

Reply: We have revised the defensive paragraph regarding validation. See lines 286-293, 327-350 and Figure 5.

Line 262

Subsection 3.1 (Identification of eddy zone in 3D scale) describes methodology rather than results. It would be more appropriate to include this section in the Methods.

Reply: We agree with the reviewer's assessment regarding the manuscript structure. The entire subsection originally titled "3.1 Identification of eddy zone in 3D scale" has been moved to the end of Section 2 (Methods). It is now renumbered as "2.3 Identification of eddy zone in 3D scale". See lines 366-368.

Lines 327–328

Please clarify how the Reynolds number was calculated. Specify the characteristic velocity and length scale used.

Reply: We thank the reviewer for this technical clarification. The Reynolds numbers (Re) reported in the manuscript were calculated to characterize the flow regime across different experimental scales and velocities. We have revised the sentences see lines 439–452.

Line 357

Why were the numerical BTCs not compared with the experimental BTCs? This comparison would substantially strengthen the study.

Reply: We completely agree that a direct comparison between numerical and experimental BTCs is essential for strengthening the study's findings. See lines 286-293, 327-350 and Figure 5.

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Line 687
The manuscript states that DMIM increases with eddy proportion. What is the physical interpretation of DMIM? Does it represent mechanical dispersion, enhanced mixing, or an effective fitting parameter? This should be clearly explained.
Reply: We appreciate the reviewer's comment regarding the physical interpretation of the model parameters. We have revised and expanded the text, see lines 820-830.

Zhongxia Li   Haibo Feng

Sincerely Yours,

Zhongxia Li, Ph.D.
Haibo Feng, Ph.D.