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 minor 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.

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., t_{98}) 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)?

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.

4. Robustness of the v_c Threshold Method: The determination of the critical velocity threshold (v_c) 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.

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."

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.

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.

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

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.