

This manuscript presents a numerical investigation of rainfall-induced enhancement of solute transport across the soil-water interface. The manuscript is generally well written, clearly organized, and the numerical implementation appears careful and comprehensive. The validation against independent experimental datasets is also a strength.

However, I have substantial concerns regarding the physical basis of the proposed mechanism. The manuscript builds upon the rainfall-pumping hypothesis originally proposed by Higashino and Stefan (2014), treating it as the governing physical process responsible for the observed enhancement of solute transport. While the numerical framework developed here is considerably more sophisticated than previous studies, the underlying physical assumptions remain largely unchanged and, in my opinion, have not been sufficiently justified.

My primary concern is therefore not with the numerical implementation, but with the physical assumptions on which the entire model is based.

Major Comments

1. The physical mechanism linking oscillatory pressure to irreversible solute transport is not demonstrated.

The central assumption of the manuscript is that rainfall-induced pressure oscillations at the soil surface generate enhanced upward solute transport. While raindrop impacts undoubtedly generate transient pressure fluctuations, it does not automatically follow that periodic pressure forcing produces net solute exchange. In a porous medium, oscillatory Darcy flow is generally reversible, and purely periodic motion does not necessarily generate irreversible transport. The manuscript assumes that oscillatory flow directly translates into enhanced exchange but does not identify the physical mechanism responsible for breaking this reversibility. For example, no discussion is provided regarding the possible roles of pore-scale heterogeneity, hysteresis, nonlinear hydraulic conductivity, diffusion during oscillatory motion, or other processes that could produce net mass transfer. Since this assumption underlies the entire modeling framework, I believe it requires considerably stronger physical justification.

2. The manuscript inherits assumptions from Higashino and Stefan (2014) without critically evaluating their validity.

The physical model is largely based on Higashino and Stefan (2014), where rainfall impacts are represented as a spatially uniform sinusoidal pressure boundary condition acting on the soil surface. That earlier work presented the mechanism primarily as a conceptual hypothesis, supported by order-of-magnitude arguments. Several key assumptions (including the conversion of raindrop kinetic energy into a uniform pressure signal and the interpretation of oscillatory pore-water velocities as an effective exchange velocity) were introduced with relatively limited physical validation. The present manuscript adopts these assumptions as established without discussing their limitations or providing additional evidence that they are appropriate under the conditions considered. Because the conclusions depend directly on these assumptions, I believe their applicability should be discussed much more critically.

3. The boundary condition represents a highly simplified description of rainfall forcing.

Rainfall impacts are localized, stochastic, and spatially heterogeneous. The model instead assumes a spatially uniform sinusoidal pressure boundary over the entire soil surface. Although such a simplification may be useful for analytical development, it is not obvious that it adequately represents the actual forcing generated by individual raindrop impacts. The manuscript would benefit from a discussion of the implications of this simplification and the conditions under which it may be expected to approximate reality.

4. Is Richards equation appropriate for this frequency?

Richards assumes quasi-static Darcy flow. The applicability of Richards' equation under (relatively) high-frequency pressure oscillations raises questions: local inertial effects, dynamic capillary pressure, non-equilibrium flow. The manuscript never discusses whether Darcy/Richards remains valid at these oscillation frequencies.

One small comment:

The discussion should acknowledge that other rainfall-related mechanisms (surface flushing, preferential flow activation, increased hydraulic connectivity...) may also contribute to the observed enhancement of solute transport.

Recommendation

The manuscript addresses an interesting problem and presents a careful numerical implementation. However, because the conclusions rely heavily on a physical mechanism that I do not believe has been adequately established, I find it difficult to assess the predictive capability of the model without a stronger justification of its underlying assumptions. I therefore recommend revising with emphasis on strengthening the discussion of the physical basis of the rainfall-pumping mechanism, clarifying its assumptions and limitations, and distinguishing between demonstrated physics and modeling hypotheses.