Hydrology and Earth System Sciences Discussions

Interactive Comment

Interactive comment on "Simple estimation of fastest preferential contaminant travel times in the unsaturated zone: application to Rainier Mesa and Shoshone Mountain, Nevada" *by* B. A. Ebel and J. R. Nimmo

Anonymous Referee #2

Received and published: 20 August 2010

1 General Comments

This paper presents a case study application of Nimmo's (2007) approach to estimate the first arrival times of contaminants through the unsaturated zone. In that earlier work Nimmo concluded that maximum transport rates for continuous input deviated little (within an order of magnitude) from 13 m/day. Adjustments were also proposed for intermittent input.

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As a case study the work presented here provides significant detail on Rainer Mesa and Shoshone Mountain culminating in conceptual models characterizing the unsaturated zone materials and their potential for preferential flow. I note that no new data is presented which tests the proposed approach, so this paper cannot be seen as a validation or independent test of Nimmo's (2007) model. The lack of additional theoretical development and lack of an independent test of the model retracts from the scientific value of such a study. There is also so much detail and it is presented in such a style that unfortunately the paper reads more like a consultants report than a novel scientific contribution. Nevertheless, I feel readers of HESS may benefit from the publication of simple models like this that potentially have predictive power.

The authors state numerous times that "finger flow" is a potential mechanism at various depths throughout the profiles of their hills. I have serious misgivings about the common reference to "finger flow" in this and many other recent papers particularly where they cite the potential for the process to occur but at the same time fail to address the conditions known to be required for the formation and persistence of finger flow (Glass et al., 1989). A systematic consideration of the numerous conditions required to first form and then sustain finger flow should be discussed, in a similar manner that the fractures and macropore flow pathways were considered in the other lithological units. In addition if the authors are to pursue finger flow as a potential mechanism I'd suggest they also consider the reported physics, and in particular the known relationships governing the velocity of fingers and their spacing in relation to the input flux.

As yet I am unconvinced that finger flow occurs in the field to significant depths. Let me begin explaining this position by giving a summary of the reported conditions for the formation of wetting front instabilities and finger flow. In laboratory studies, wetting front instabilities could not be observed with even small amounts of heterogeneity in the unsaturated materials (Glass et al., 1989; Bauters et al., 2000). The extreme homogeneity in material properties and water content that is known to be required for the development of wetting front instabilities appears to be a significant constraint on ob-

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serving it in natural systems (Glass et al., 1989; Wang et al., 2004). The authors did not present evidence for this extreme homogeneity to occur at their sites. Secondly, when fingers are subjected to repeated wetting and drying cycles they do persist for a number of cycles but gradually their fringes expand leading to a reduction in the transport velocity and a homogenisation in water contents across most of the domain. Cases where fingers have been observed in the field have typically been associated with surface water repellence and have been limited to depths less than 2 m, below which fingers often merge together, leading to much more homogeneous wetting deeper in the profile. In these instances heterogeneity in water repellence at the soil surface, reinvigorated seasonally, is likely a significant contributing factor to finger formation and persistence. It is unlikely such reinvigoration can occur deep in the unsaturated zone.

I by no means reject the potential for preferential flow to occur in those materials the authors claim "finger flow" to occur but rather I am sceptical about the proposed mechanism.

On the issue of risk, why do the authors propose to use the geometric mean of reported fastest travel times for their calculations from continuous sources (section 3.3)? Why wouldn't you use the fastest ever reported rate in order to provide a conservative estimate of the risk, particularly if the model should be "viewed as a worst-case scenario for the first arrival of a contaminant"? Similarly for the case of io, the reference intermittent water flux, why would one not simply use the smallest value published (section 3.4)? Also, the vary-one-at-a-time sensitivity analysis of parameters (section 4.6) actually adds little to the paper, nor provides me with any enhanced understanding of the uncertainties.

On a related point I note from Nimmo's (2007) paper that there is virtually no correlation between simulated maximum transport velocity (Vmax) and measured Vmax for continuous sources, but the authors imply that this is acceptable as errors lie to within plus or minus an "order of magnitude". It seems to me that the model as applied lacks sig-

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nificant predictive power for continuous sources in comparison to intermittent sources despite the additional assumptions in the latter. This issue has not been discussed in either paper and should be addressed.

2 Minor issues

I suggest that for Figure 5 copyright permissions should be obtained from the publishers and submitted with future revisions as it is substantially similar to a figure in Nimmo 2007. In addition the caption should reflect this reproduction of material.

Page 3897, line 12: The model presented is a travel time estimator and not a "contaminant transport model"

3 References

Nimmo, J. R.: Simple predictions of maximum transport rate in unsaturated soil and rock (2007) Water Resour. Res., 43, W05426, doi:10.1029/2006WR005372.

Glass, R. J., J.-Y. Parlange and T.S. Steenhuis, Wetting front instability, 1. Theoretical discussion and dimensional analysis (1989) Water Resour. Res. 25, 1187–1194.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 3879, 2010.