Hydrol. Earth Syst. Sci. Discuss., 7, C3124–C3130, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C3124/2010/

Hydrology and Earth System Sciences Discussions

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

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Received and published: 22 October 2010

Interactive comment on "Simple estimation of fastest preferential contaminant travel times in the unsaturated zone: application to Rainier Mesa and Shoshone Mountain, Nevada"

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Anonymous Referee 2

Received and published: 20 August 2010

Author responses are in **Bold text**

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. 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.

The need for independent testing of the Nimmo (2007) model has been noted previously and is an ongoing project for the authors. Unfortunately, it takes many independent tests to develop a stand-alone comprehensive data set of tracer tests for model performance evaluation. The authors' work on this is not yet complete, but in the revised manuscript we have included a subset of the independent testing data that has been collected thus far. We feel that, while not covering all types of cases, the new independent testing confirms the order-of-magnitude agreement suggested by the original model development by Nimmo (2007).

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. The revised manuscript removes as much of the site detail as is feasible and refers back to the Open-File Report by Ebel and Nimmo (2009) for the reader to find further details. We feel that this better structures the manuscript for the readers of HESS.

Nevertheless, I feel readers of HESS may benefit from the publication of simple models like this that potentially have predictive power.

The authors agree with the reviewer and are grateful for his/her comments which have helped refine and clarify the presentation of the SRPF model application.

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 observing 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.

We thank the reviewer for bringing our attention to relevant research reported in the literature. For the lithologies at Rainier Mesa and Shoshone Mountain, we are convinced that the dominant mechanism of preferential flow is macropore flow through fractures. Finger flow was considered as a remote possibility, but still mentioned as a possible mechanism. The original manuscript did not clearly reflect this, and it could have seemed as if we put fingered flow on an equal footing with fracture flow, which is definitely not the case. In the revised manuscript we have clarified this point and made it apparent that preferential flow via fractures is almost certainly the mechanism that would be responsible for rapid contaminant movement in the unsaturated zone at Rainier Mesa and Shoshone Mountain.

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)?

The authors feel that using the maximum rate ever reported could cause the model to be significantly wrong if an incorrect observed travel time was included in the study. For example, one accidentally contaminated sample by human er-

ror during field work could result in an extremely fast travel time, and if this travel time was the sole value used, thus cancelling out all other observed travel times, it could highly (and incorrectly) bias the travel time model. Using the geometric mean of first arrival times helps minimize the potential for such an error to bias the model by averaging across many data sets. We feel that i0 is a similar case, where using the "global" maximum may result in a model bias towards one specific test. When used in a predictive capacity, this distinction between the geometric mean and the global maximum is extremely important for achieving meaningful results.

We have come to believe that much of the tension between using the geometric mean versus the global maximum results from the terminology used in the earlier papers, with so much discussion of maximum velocities and the prolific use of the symbol Vmax. The revised manuscript changes this terminology to make it clear that we are considering initial breakthrough of contaminant (i.e. first arrival time) and Vmax is changed to VFirst Arrival, shortened to VFA. We feel that "first-arrival velocity" more clearly and specifically implies that what we are talking about is the fastest of a range of transport velocities for a particular instance of transport, as opposed to a more global maximum of all transport velocities possible for a site or for preferential flow in general. With this change we want to bring the focus back to the SRPF model as a tool to estimate first arrival times at sites with minimal data and observed preferential flow, not as a model to estimate the fastest travel times ever observed.

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.

The sensitivity analysis was included at the suggestion of colleague reviewers. We agree with the present reviewer that it does not provide a quantitative metric of uncertainty, what it does show is that the sensitivity to the parameter values is

less than the approximate order-of-magnitude overall agreement of the estimated travel times. We feel that this point is important because of the comparison to a standard unsaturated zone modeling approach using Richards Equation which relies sensitively on highly variable and uncertain unsaturated hydraulic properties for the porous media. We feel no change to the manuscript is necessary.

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 significant 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.

Given that the Nimmo (2007) model estimates Vfirst-arrival (using the new notation as described above) as simply equal to 13 m d-1 for continuous sources, the issue of correlation is just a matter of how much do the measured values deviate from 13 m d-1. From the known deviations of measured Vfirst-arrival from this value, seen in Figure 2 of Nimmo (2007) and for the new test data in Figure 8 of the revised manuscript, we feel it is obvious that the correlation is significant for both the continuous and intermittent cases. Of the total of 80 continuous-supply cases in the two figures, in 70 of these cases the measured value is within an order of magnitude of 13 m d-1. For the intermittent-supply cases, all 25 fall within an order of magnitude of the SRPF model prediction. This may suggest slightly better agreement for the intermittent-supply cases, but we would not make this claim without additional cases, especially intermittent, to support it. As to whether the model's predictive power is significant, the answer is that order-of-magnitude estimates are useful for some but not all purposes, as we explained more fully in response to reviewer 1.

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.

Yes, we have acknowledged the source of the figure appropriately in the caption of the revised manuscript, consistent with the American Geophysical Union rules for use of figures from Water Resources Research.

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

The revised manuscript clarifies that the SRPF model is solely for travel-time estimation, and eliminates any suggestion that the SRPF model is a contaminant transport model and makes it clear that the model can only be used to provide estimates of travel time for conservative solutes.

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. Bauters, T.W.J., Dicarlo, D.A., Steenhuis, T.S., Parlange, J.-Y. Soil water content dependent wetting front characteristics in sands (2000) Journal of Hydrology, 231-232, 244-254 Wang, Z., Jury, W.A., Atac, T. Unstable flow during redistribution: controlling factors and practical implications (2004) Vadose Zone Journal, 3, 549-559

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